Essays on Finance and Development

Jacopo Ponticelli

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DIRECTOR DE LA TESI
Nicola Gennaioli, Departament of Economics and Business

To Ona

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Abstract

This thesis investigates various aspects of economic development. In the first chapter I study the effect of financial reform on firms. In particular, I study to what extent this effect depends on enforcement's quality. I find that – when court enforcement works poorly – financial reform increasing creditor protection is ineffective in fostering firm access to credit and productivity. In the second chapter I exploit the adoption of genetically engineered soybean seeds in Brazil to study the effects of agricultural productivity on industrial development. I find that areas more affected by technical change in soy production experienced faster manufacturing growth due to increased supply of cheap labour and larger credit availability. In the third chapter I study the relationship between fiscal consolidation and social unrest in Europe using cross-country data between 1919 and 2008. Results show that expenditure cuts are particularly potent in fuelling protests; tax rises have insignificant effects.

Resum

Aquesta tesi investiga varis aspectes del desenvolupament econòmic. En el primer capítol estudio l'efecte de una reforma financera a les empreses. En particular, estudio fins a quin punt aquest efecte depèn de la qualitat de l'execució de la llei. He trobat que – quan l'execució jurisdiccional no funciona – la reforma finançaria que augmenta la protecció del creditor és poc efectiva a l'hora de fomentar l'accés al crèdit i la productivitat de les empreses. En el segon capítol utilitzo l'adopció al Brasil de llavors de soja genèticament modificades per estudiar els efectes de la productivitat agricultora en el desenvolupament industrial. Trobo que les zones més afectades pel canvi tecnològic derivat de la producció de soja han experimentat un creixement productiu més ràpid a causa del augment de ma d'obra barata i més disponibilitat de crèdit. Al tercer capítol, estudio la relació entre la consolidació fiscal i el malestar social a Europa utilitzant dades de varis països entre els anys 1919 i el 2008. Els resultats demostren que les retallades son potents detonants de protestes; les pujades d'impostos, per altre banda, tenen un efecte insignificant.

Foreword

My doctoral thesis is a collection of three self-contained essays that study various aspects of economic development, with an emphasis on the role played by financial markets.

In the first chapter I study the impact of a financial reform on firms. Financial reform designed to improve creditor protection is often encouraged as a way to increase credit access for firms, especially in developing countries. 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.

In the second chapter, co-authored with Paula Bustos and Bruno Caprettini, we study the effects of agricultural productivity on industrial development. Classical models of structural transformation propose several channels through which productivity growth in agriculture can speed up industrial growth. However – as showed by Matsuyama (1992) – in open economies a comparative advantage in agriculture can slow down industrial growth. We provide direct empirical evidence on the impact of agricultural productivity on industrial development by studying the effects of adoption of new agricultural technologies in Brazil. In particular, we use the widespread adoption of genetically engineered soybean seeds as a shock to the productivity of Brazilian agriculture and measure its effects on manufacturing firms. To establish causality, we exploit exogenous differences in soil and weather characteristics across geographical areas leading to a differential impact of the new technology on yields. We find that areas more affected by technical change in soy production experienced faster manufacturing growth. Our findings imply that if technical change is strongly labour saving, increases in agricultural productivity can lead to industrialization even in an open economy.

The third chapter, co-authored with Joachim Voth, focuses on the relationship between fiscal consolidation and social unrest. Using cross-country evidence for the period 1919 to 2008, we examine the extent to which societies become unstable after budget cuts. The results show a clear correlation between fiscal retrenchment and instability. Expenditure cuts are particularly potent in fueling protests; tax rises have only small and insignificant effects. We test if the re-

lationship simply reflects economic downturns, using a recently-developed IMF dataset on exogenous expenditure shocks, and conclude that this is not the case. While autocracies and democracies show broadly similar responses to budget cuts, countries with more constraints on the executive are less likely to see unrest after austerity measures. Growing media penetration does not strengthen the effect of cut-backs on the level of unrest. We also find that austerity episodes that result in unrest lead to quick reversals of fiscal policy.

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Chapter 1

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

1.1 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 de-

[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.

lays (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 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 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,

[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%.

[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.

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 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

[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)).

[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.

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 1.2 I describe the Brazilian bankruptcy reform and how the efficiency of the judicial system influenced its impact on creditors. Section 1.3 presents the data on the Brazilian judiciary. In section 1.4, I present a model of heterogeneous firms which delivers qualitative predictions on firm level outcomes. Section 1.5 describes the firm-level data used to test these predictions. In section 2.4, I present the identification strategy and the empirical results.

1.2 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,

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

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 end, the new law introduced several important innovations (see Table 1.1 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

[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.

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

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 1.1 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 1.1 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 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

[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 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

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]

1.2.1 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 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, dis-

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)).

[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.

tricts 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.

1.3 Data on the Brazilian Judiciary

In section 1.2 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 1.3.1) and the high persistence over time (subsection 1.3.2) of court congestion across Brazilian judicial districts.

1.3.1 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 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]

[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.

[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.

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 1.2 shows descriptive statistics of the main judicial variables. The unit of observation 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

[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.

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 1.5 (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 1.3) 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.

1.3.2 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 1.2 displays the strong stability of the ratio of pending cases per judge between 2004 and 2008 in Brazil as a whole. Figure 1.3 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 1.2 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 1.4 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 2.4 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 1.6.4, I then propose an instrument that is predetermined with respect to the 2005 reform for all judicial districts.

1.4 A Model of Heterogeneous Firms with Financial Frictions

In section 1.2 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 1.3 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.

1.4.1 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

[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%.

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:

$$y(\omega) = \left(\frac{p(\omega)}{P}\right)^{-\sigma} \frac{E}{P} \tag{1.1}$$

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.

1.4.2 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:

$$(1 - p\tau)\bar{\pi}^H - (1 + r)b \geqslant \left(1 - \frac{1}{\psi_j}\right)\bar{\pi}^H \tag{1.2}$$

The left-hand side of equation 1.2 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 1.2 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 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

[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).

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 1.2 solved with equality. When $p > 0$ and assuming for simplicity that $r = 0$, this is given by:

$$\max\{b\} = \left\{ \left(\frac{1}{\psi_j} - p\tau \right) \bar{\pi}^H, 0 \right\} \tag{1.3}$$

Equation 1.3 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 1.7 shows in red the optimal technological choice as a function of firm productivity.

1.4.3 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):

$$f_e = [1 - G(\varphi^*)] \frac{\tilde{\pi}}{e} \tag{1.4}$$

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:

$$\tilde{\pi} = p^u \tilde{\pi}^u + p^c \tilde{\pi}^c \tag{1.5}$$

[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:

$$\tilde{\pi}^u = \int_{\varphi^*}^{\varphi^h} \pi^L(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} + \int_{\varphi^h}^{\infty} \pi^H(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} \tag{1.6}$$

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

$$\tilde{\pi}^u = (1-\tau)\tilde{\varphi}^{\sigma-1}\left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\frac{EP^{\sigma-1}}{\sigma} - f\left(\frac{G(\varphi^h)-G(\varphi^*)}{1-G(\varphi^*)}\right) - \eta f\left(\frac{1-G(\varphi^h)}{1-G(\varphi^*)}\right) \tag{1.7}$$

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 (1.7) 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:

$$\tilde{\pi}^c = \int_{\varphi^*}^{\infty} \pi^L(\varphi)\frac{g(\varphi)}{1-G(\varphi^*)} \tag{1.8}$$

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 (1.6) and (1.8) into equation (1.5), yields the expression for expected profits in an industry with financial frictions.

This expression is then substituted into equation (3.1) to find the equilibrium cutoff productivities to stay in the market, switch to the new technology, and be unconstrained.

1.4.4 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.

1.5 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 1.6 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 1.4 displays the descriptive statistics of both the PIA and PINTEC variables.[34]

1.6 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 1.6.1. In section 1.6.2 I show the baseline results on capital investment. I show that the results are robust to controlling for a full set

[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.

of initial conditions at the judicial district level and to using different samples of firms.[35] In section 1.6.2 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 1.6.2 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 to introduce new products in the market. In section 1.6.2 I present the baseline results on firm productivity, wages and exit. In section 1.6.2 I look at heterogeneous effects across firm size distribution. In section 1.6.2 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 1.6.4 I show that the main results hold when using an instrumental variable strategy.

1.6.1 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:

$$y_{ijt} = \alpha_j + \lambda_t + \beta(\psi_j \times \text{post}_t) + \varepsilon_{ijt} \tag{1.9}$$

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 1.9, 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

[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.

[37]As discussed in section 1.3.2, 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

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 idiosyncratic 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 1.9 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 1.9 as follows:

$$\Delta y_{ijt} = \alpha + \beta \psi_j + u_{ijt} \tag{1.10}$$

When estimating equation 1.10 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 1.10 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]

for court congestion in the pre-reform period. In Table 1.18 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.

[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

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 1.5, 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 (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 1.10 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 1.6.4. 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:

$$\Delta y_{ijst} = \alpha + \beta\psi_j + \gamma\delta_j + \xi' X_{j,t=0} + \sigma_s + u_{ijst} \tag{1.11}$$

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.

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

1.6.2 Differences-in-Differences Results

Baseline Results on Capital Investment

Table 1.6 shows the main result of the paper, obtained from the OLS estimation of equation 1.11. 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 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

[43] Along with two-digit sector dummies.

[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 1.3 show that specialized courts tend to be more congested than normal courts.

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 1.6 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]

Investigating the Financial Channel

In this section I test whether the effect of court congestion on investment found in section 1.6.2 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 1.7, 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:

$$\begin{aligned}\Delta y_{ijt} &= \beta_1(\psi_j \times d_{ef=high}) + \beta_2(\psi_j \times d_{ef=low}) \\ &+ \gamma_1(\delta_j \times d_{ef=high}) + \gamma_2(\delta_j \times d_{ef=low}) + d_{ef=high} + \xi' X_{j,t=0} + \epsilon_{ijt}\end{aligned} \tag{1.12}$$

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 1.7, I exploit variation in terms of asset tangibility at the sectoral level (in the corresponding US sector). The data on asset tangibility

[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 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.

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 1.8 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 1.8 displays the beta coefficients and 90% confidence intervals from this regression in a graph.

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 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:

$$y_{izt} = \alpha_z + \lambda_t + \beta(\psi_z \times \text{post}_t) + \xi'(X_{z,t=0} \times \text{post}_t) + \varepsilon_{izt} \tag{1.13}$$

where y is a firm-level outcome of firm i operating in state z at time t, and $post_t$ is

[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 1.12.

[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.

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 1.9, 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 1.9 displays the results of estimating equation 1.13 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.

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

In Table 1.10 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

[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 1.5 presents descriptive statistics.

[54] Results are robust to the use of different methodologies to estimate firm productivity, such as

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 1.6.2 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 1.11, 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 1.12. 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 1.9 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.

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.

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 one proposed by Levinsohn and Petrin (2003).

[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.

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:

$$\Delta y_{ijt} = \alpha + \sum_{q=1}^{4} \beta_q(\psi_j \times \text{size}_q) + \sum_{q=2}^{4} \nu_q(\text{size}_q) + \gamma\delta_j + \xi' X_{j,t=0} + \sigma_s + u_{ijt} \quad (1.14)$$

The coefficients of interest in regression 1.14 are the β_qs on the interaction between court congestion and firm-size quartile dummies (size_q). Table 1.12 displays the results of estimating equation 1.14 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.

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 congestion 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

[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.

has more predictive power on firm-level outcomes when I pick the right or the wrong timing of the reform.[58] Table 1.13 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:

$$\begin{aligned} y_{ijt} &= \alpha_i + \lambda_t + \sum_{t=2000}^{2009} \beta_t(\psi_j \times \lambda_t) + + \sum_{t=2000}^{2009} \gamma_t(\delta_j \times \lambda_t) \\ &+ \sum_{t=2000}^{2009} \xi'(X_{j,t=0} \times \lambda_t) + u_{ijt} \end{aligned} \tag{1.15}$$

Figures 1.10 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 1.10 is a robust sanity check that the data are picking the right timing of the reform and that the results presented in Table 1.10 are not driven by background trends.

As an additional robustness check, I test whether these trends are driven more by high financially dependent sectors versus low financially dependent sectors. I run a regression similar to equation 1.15 but with a triple interaction of court

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

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 1.11 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.

1.6.3 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 1.14 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.

1.6.4 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 1.5 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.

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.

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,

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

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 population 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.

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} = subset of neighbors of municipality m below min.req. at time $t =$ 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:

$$\log\left(\frac{backlog}{judge}\right)_{mj} = \alpha + Z + log\left(\sum_{n=1}^{N_m^{all}} \text{pop}_{t=\text{year law}}\right)_m + \varepsilon_m \qquad (1.16)$$

Table 1.15 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 1.16,

[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

i.e., I add as a control the log population 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 1.16, I show the results. Column 1 repeats the regression run in column 3 of Table 1.15. 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.

Second Stage Results

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

$$\Delta y_{imt} = \beta \hat{\psi_m} + \xi' X_m + \varepsilon_{imt} \tag{1.17}$$

where:

$$\hat{\psi_m} = \hat{\rho} Z_m + \hat{\xi}' X_m \tag{1.18}$$

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.

(*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.

[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.

Table 1.17 shows the results of estimating equation 1.17 and compares them with the estimates obtained from an OLS regression on the same sample. The 2SLS coefficients 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.

1.7 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 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.

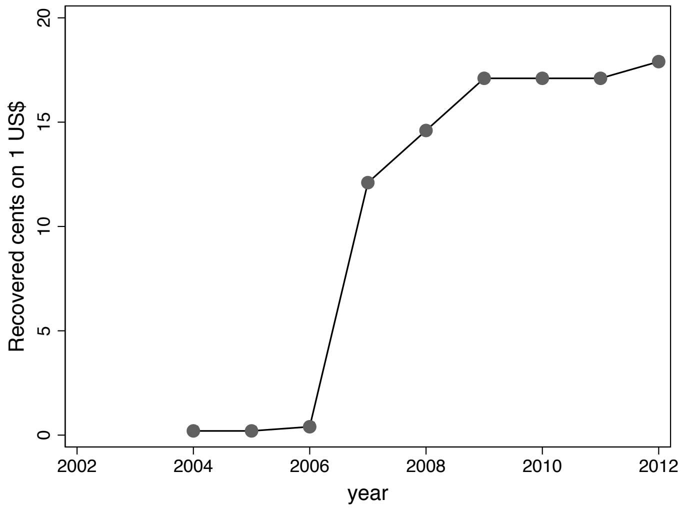
This paper also informs the debate on the sequencing of economic reforms in developing countries. Caselli and Gennaioli (2008) advocate that financial reforms — like bankruptcy law reforms — should come first[65] because they favor the reallocation of resources to their more talented users. This paper 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.

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

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

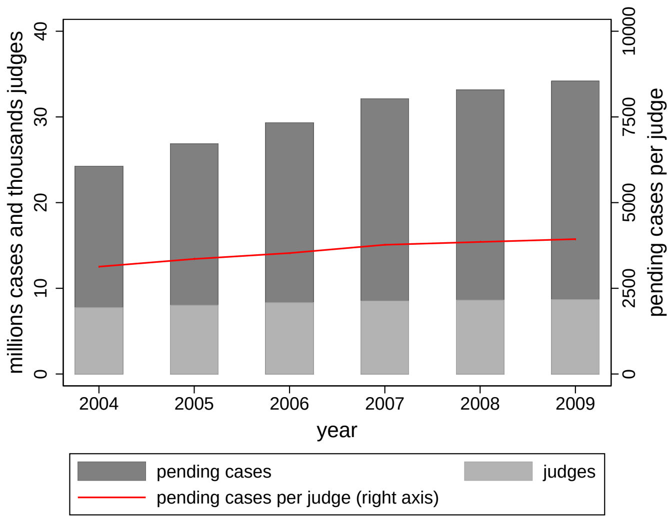
Figures and Tables

Figure 1.1: Recovery Rate in Brazil over time



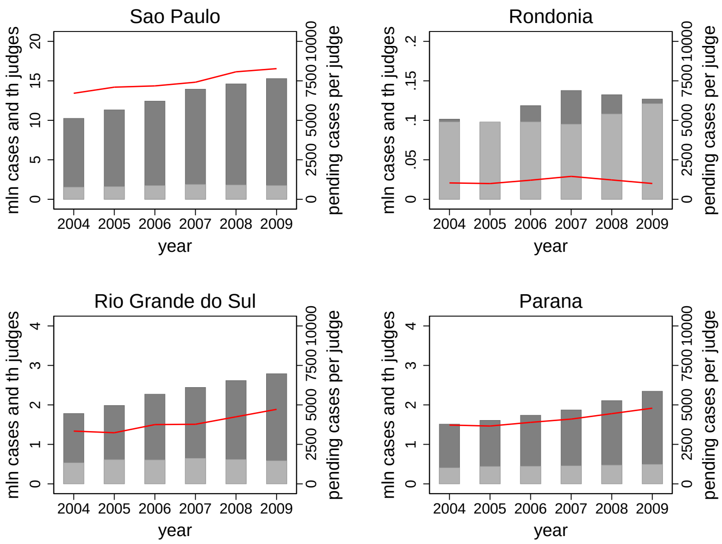
Notes: Data from the World Bank Doing Business database.

Figure 1.2: Trend in Pending Cases per Judge in Brazil



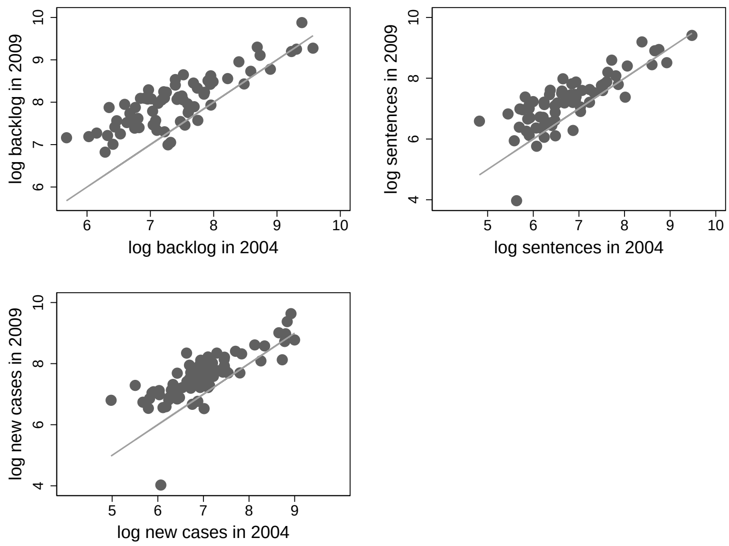
Notes: Data from the Conselho Nacional de Justica (CNJ).

Figure 1.3: Pending Cases per Judge across Federal States



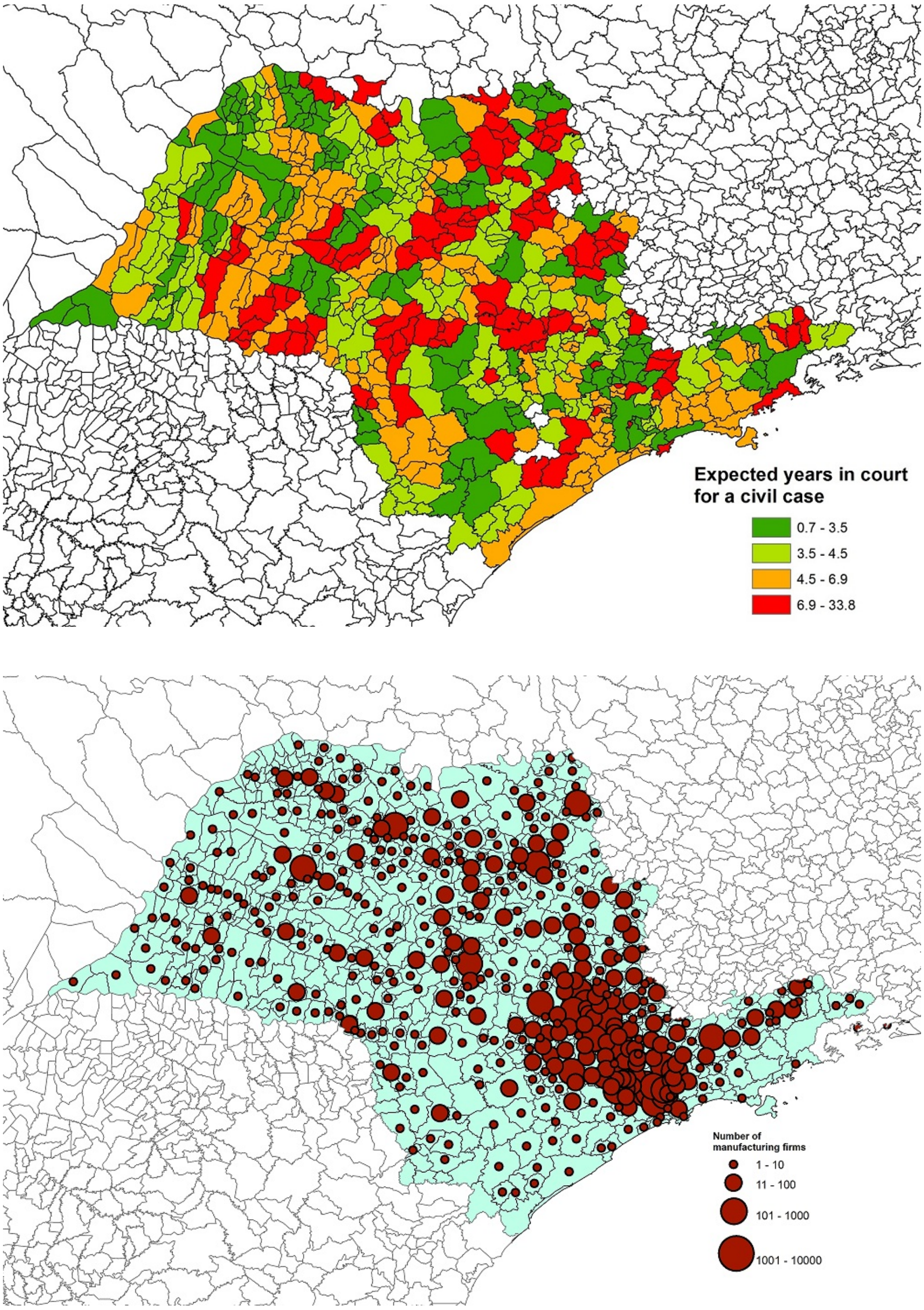
Notes: Data from the Conselho Nacional de Justica (CNJ).

Figure 1.4: Comparing Judicial Variables in 2004 and 2009



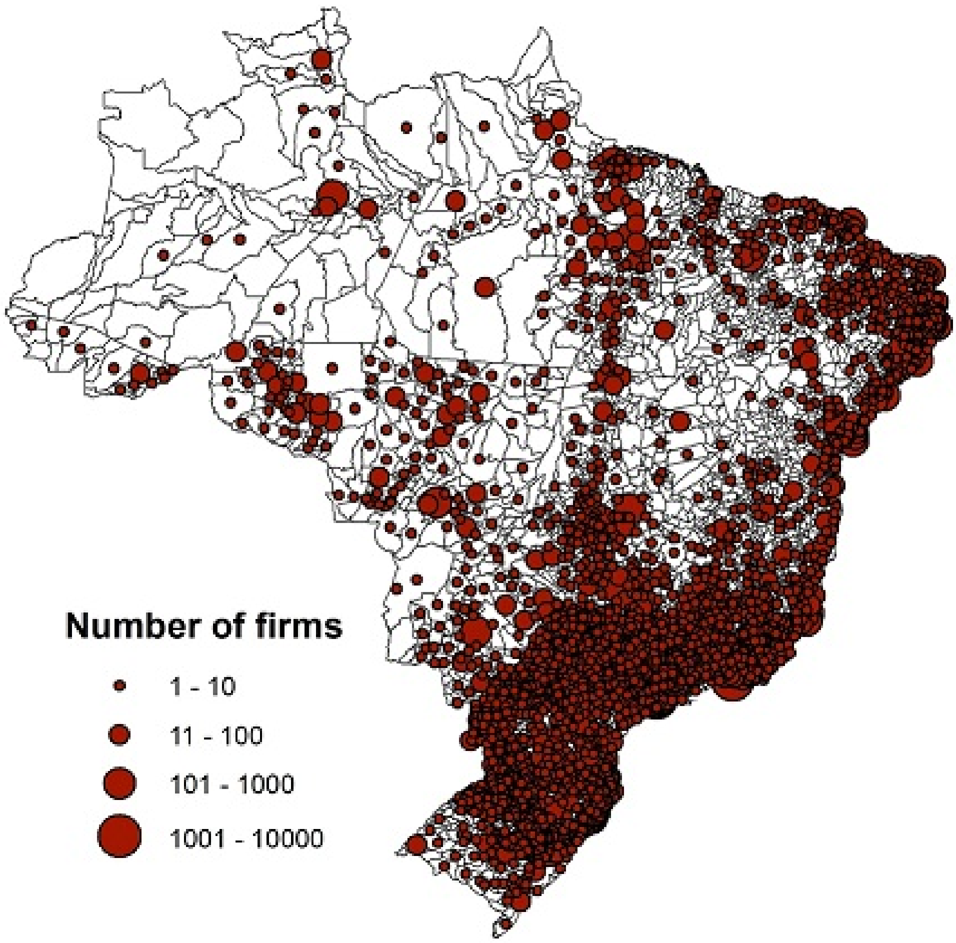
Notes: Data from the Conselho Nacional de Justica (CNJ) and State Tribunals of Rondonia and Mato Grosso do Sul.

Figure 1.5: 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 1.6: Location of Firms across Brazil



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

Figure 1.7: Profit Functions and Productivity Thresholds

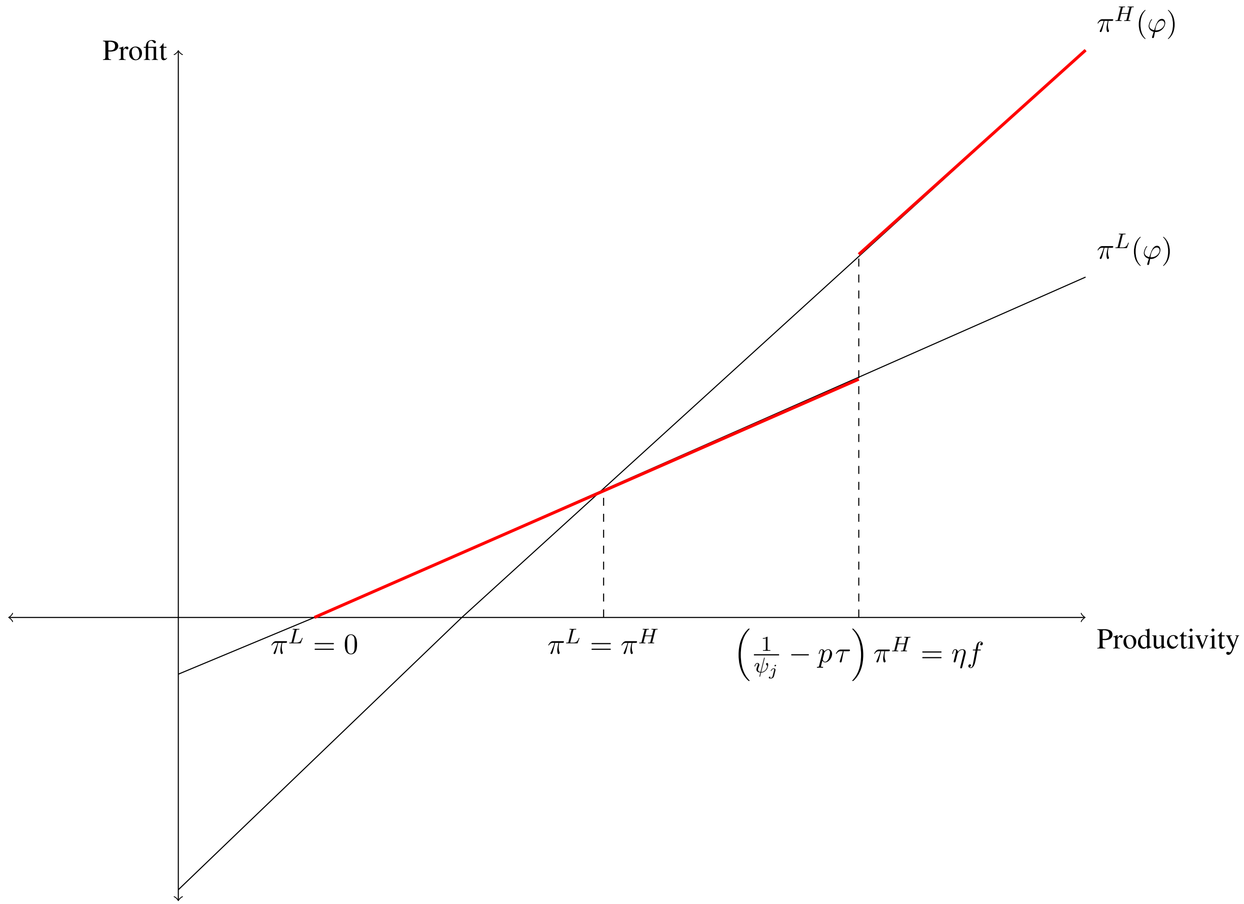
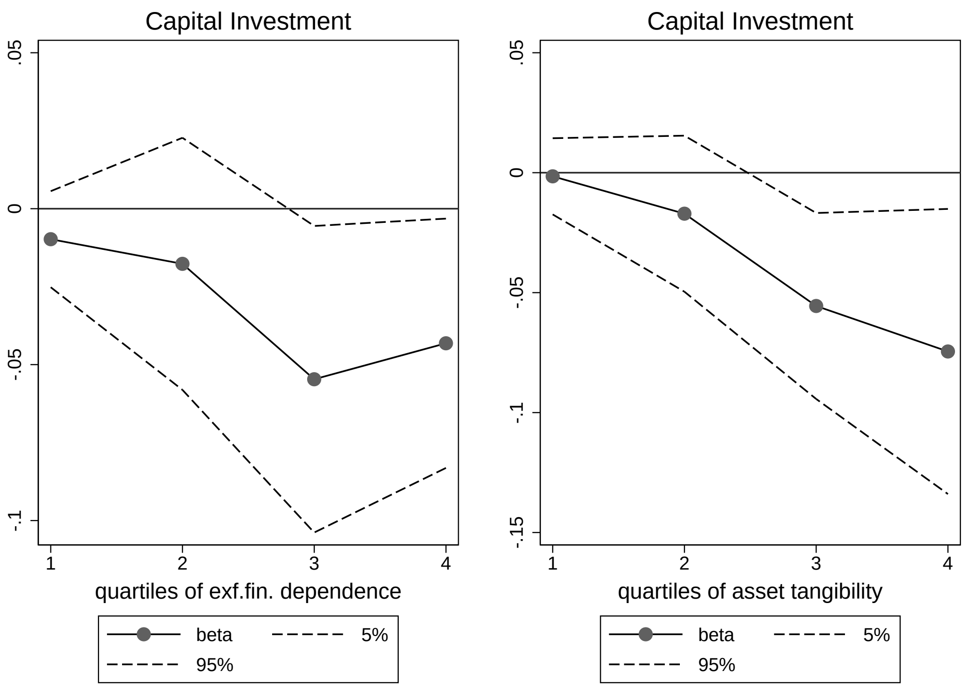
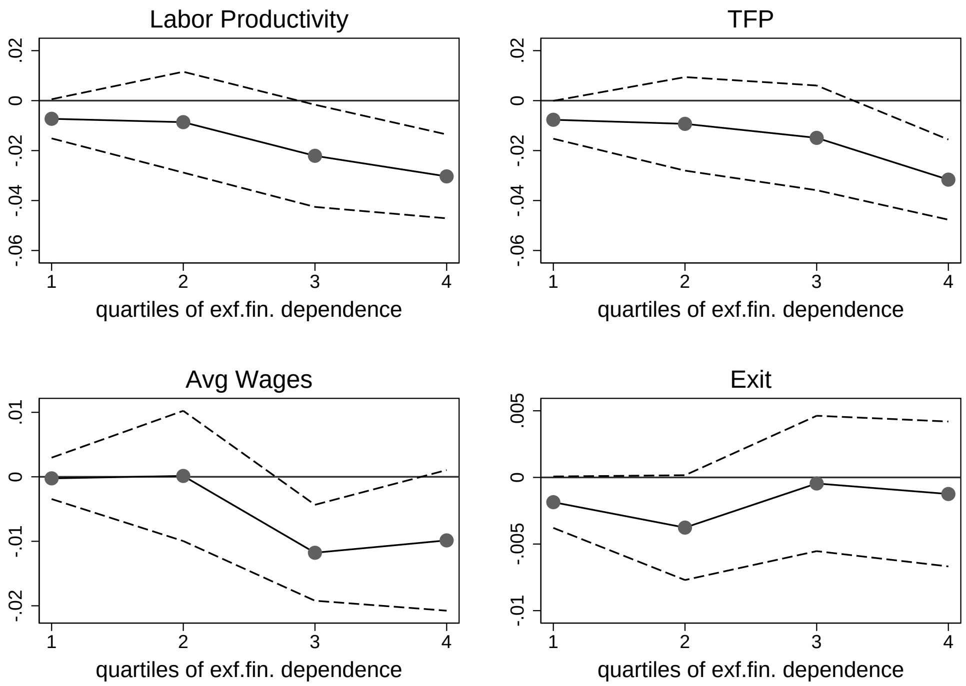


Figure 1.8: The Effect of Court Congestion on Investment across Quartiles of Financial Dependence and Asset Tangibility



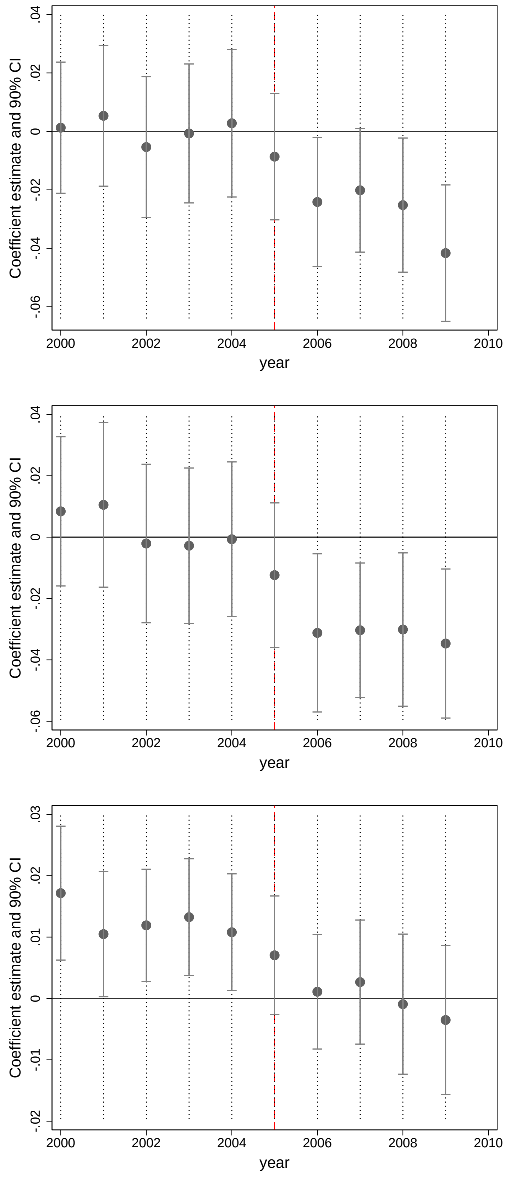
Notes: Dots are beta coefficients and dashed lines are 90% confidence intervals of two regressions where the dependent variable is growth in capital investment and the independent variable is court congestion interacted with quartiles of sector financial dependence (left figure) and asset tangibility (right figure). Both regressions include all the controls at judicial district level used in Table 1.6.

Figure 1.9: 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 1.6.

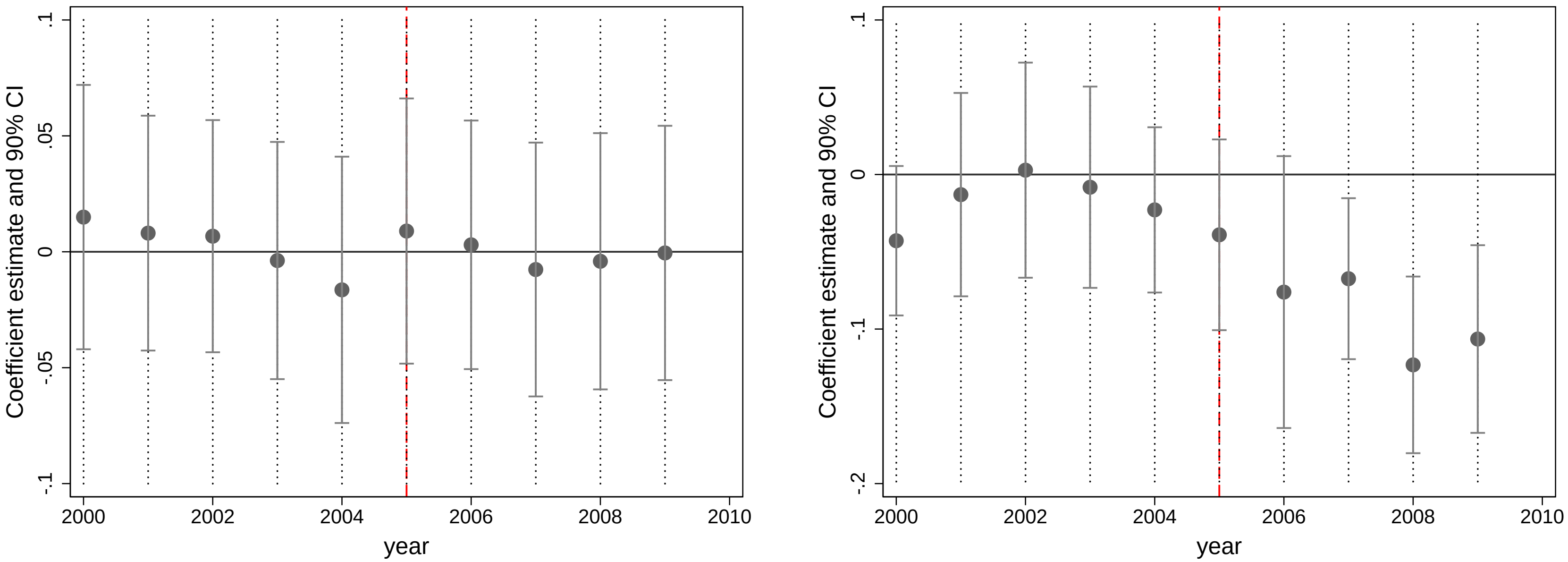
Figure 1.10: The Effect of Court Congestion on Labor Productivity, TFP and Wages over Time



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 1.6 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 1.11: Effect of Court Congestion on Labor Productivity over Time and across Industries

Low Dep. on External Finance : Food and Beverages **High Dep. on External Finance: Electrical Machineries**



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 1.6 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 1.1: 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 1.2: 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 1.3: 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 1.4: 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 1.5: 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 1.6: 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 1.7: 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 $_j \times$ 1(ext.fin. = high)	-0.026	0.034		
	[0.047]	[0.068]		
bankruptcy court $_j \times$ 1(ext.fin. = low)	0.063	0.123*		
	[0.040]	[0.065]		
bankruptcy court $_j \times$ 1(asset tang = high)			-0.008	0.050
			[0.043]	[0.064]
bankruptcy court $_j \times$ 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. $1(\text{ext.fin.} = \text{high})$ 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 1.8: 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.

Table 1.9: Firm Technological Investment and Access to External Finance

Dependent variable indicated in columns	access ext. finance (1)	(2)	log(*Tech I*) (3)	(4)	New product (5)	(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 1.10: 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 1.11: 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 1.12: 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 1.13: 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 1.11 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 1.14: Average Growth Rates under Different Counterfactual Scenarios

	avg in data between 2003-05 and 2006-09	**predicted under counterfactuals** 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 1.15: 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 1.16: 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 1.17: 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 1.18: 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 1.11 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.

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Chapter 2

Agricultural Productivity and Structural Transformation: Evidence from Brazil

Joint with Paula Bustos and Bruno Caprettini, CREI and UPF

2.1 Introduction

The early development literature documented that the growth path of most advanced economies was accompanied by a process of structural transformation. As economies develop, the share of agriculture in employment falls and workers migrate to cities to find employment in the industrial and service sectors (Kuznets, 1957). These findings suggest that isolating the forces that can give rise to structural transformation is key to our understanding of the development process. In particular, scholars have argued that increases in agricultural productivity are an essential condition for economic development, based on the experience of England during the industrial revolution (Nurkse, 1953 and Rostow, 1960). Classical models of structural transformation formalized their ideas by noting that productivity growth in agriculture increases income per capita, generating demand for manufacturing goods (Murphy et al., 1989, Matsuyama, 1992, Gollin et al., 2002). However, Matsuyama (1992) notes that the positive effects of agricultural productivity on industrialization occur only in closed economies, while in open economies a comparative advantage in agriculture can slow down industrial growth. This is because labor reallocates towards the agricultural sector, reducing the size of the industrial sector and its scope to benefit from external scale economies.

The sharp prediction that increases in agricultural productivity do not lead to industrial growth in open economies needs to be qualified in two dimensions. First, in the presence of transport costs and increasing returns to scale, local demand can shape specialization through home market effects even in open economies (Krugman, 1980). Second, note that classical models of the effects of agricultural productivity on industrial development such as Matsuyama (1992) assume that technical change is hicks-neutral. Thus, they predict that in an open economy an increase in agricultural productivity would induce increases in agricultural employment and a reduction in the size of the industrial sector as labor reallocates towards agriculture and wages increase. However, technical change in agriculture is often labor-saving. In this case, increases in agricultural productivity can lead to industrialization even in an open economy. This is because if the reduction in the labor intensity of agricultural production is strong enough, employment in agriculture and wages might fall, inducing an expansion of industrial employment. Then, whether technological change in agriculture induces industrial development in an open economy depends on the factor bias of technical change.

In this paper we shed light on the effects of factor biased technical change in agriculture by studying the widespread adoption of new agricultural technologies on Brazilian manufacturing firms. In particular, we study the effects of the adoption of a new agricultural technology, namely genetically engineered (GE) soybean seeds, on industrial development in Brazil. During the ten years after the technology was invented in 1996 the output of soy doubled in Brazil, becoming the most important crop in the country. GE soybeans seeds have a gene that makes them herbicide tolerant: a feature that allows farmers to adopt a new package of techniques that lowers production costs, in particular it requires less labor per unit of land to yield the same output. The adoption of GE soyben seeds can thus be characterized as labor saving technical change.

To identify the causal effects of this new technology, we use two sources of exogenous variation in the profitability of technology adoption. First, as the technology was invented in 1996, we use the introduction of the new technology as our source of time variation. Second, as the new technology had a differential impact on yields depending on geographical and weather characteristics, we use differences in soil suitability across regions as our source of cross-sectional variation.

We start by reporting that during this period the municipalities where soy expanded experienced an increase in agricultural output per worker, a reduction in labor intensity in agriculture and an expansion in industrial employment. This could respond to the adoption of labor saving agricultural technologies reducing labor demand in the agricultural sector and thus inducing a reallocation of labor towards the industrial sector. Alternatively it could be due to other shocks to local labor markets. For example: an increase in labor demand in the industrial sectors could increase wages, inducing agricultural firms to switch to less labor intensive crops, like soy. To establish the direction of causality we exploit the timing of adoption and the differential impact of the new technology on potential yields across geographical areas.

We construct a municipality-level measure of the potential profitability of technology adoption using data on potential soil yields from the FAO-GAEZ database. These yields are calculated by incorporating local soil and weather characteristics into a model that predicts yields for each crop given certain climate and soil conditions. Potential yields are a source of exogenous variation in agricultural productivity because they are a function of weather and soil characteristics, not of actual yields in Brazil. In addition, the database reports potential yields under different technologies. Thus, we exploit the predicted differential impact of the high technology on yields across geographical areas in Brazil as our source of cross-sectional variation in agricultural productivity. This research design allows us to investigate whether exogenous shocks to local agricultural productivity lead to changes in the size and efficiency of the local industrial sectors. Note that this identification strategy relies on the assumption that although Brazil is an open economy, the existence of transport costs implies that local markets are important sources of labor for industrial firms.

In a preliminary analysis of the data, we find that municipalities where the new technology is predicted to have a higher effect on potential yields of soy did experience a higher increase in the area planted with GE soy. These preliminary findings show that our instrument (the potential impact of new agricultural technologies on yields given soil and weather characteristics) is a good predictor of the profitability of GE soy adoption. In addition, these regions experienced increases in the value of agricultural output per worker and reductions in labor intensity measured as employment per hectare. Finally, these regions experienced faster employment growth and wage reductions in the industrial sector. Interestingly, the effects of technology adoption are very different for maize, a labor intensive crop that also experienced technical change during this periods as new seeds were introduced by the Brazilian government's agency of agricultural technology development (*EMBRAPA*). Regions where the FAO potential yields are predicted to increase the most when switching from the low to the high technology, did indeed experience a higher increase in the area planted with maize. In addition, they experienced increases in the value of agricultural output per worker and labor intensity. Finally, they experienced increases in wages in the industrial sector.

The differential effects of technological change in agriculture documented for GE soy and maize indicate that the factor bias of technical change is a key factor in the relationship between agricultural productivity and industrial growth in open economies. Our purpose in this paper is to investigate these effects further to isolate the channels and mechanisms through which structural transformation takes place.

The remaining of the paper is organized as follows. Section 2.2 gives background information on agriculture in Brazil. Section 2.3 describes the data used in the empirical analysis. Section 2.4 presents the empirical strategy and results. Section 2.5 concludes.

2.2 Agriculture in Brazil

In this section we provide background information about recent developments in the agricultural sector in Brazil. In the last decade, Brazilian labor force has been shifting away from agriculture. In 2004, industry and agriculture employed 18 millions workers each. By 2011, industry was employing 5 millions workers more than agriculture (see Figure 2.1). In the same years of this process of structural transformation, Brazilian farmers started introducing on a large scale the most relevant innovation in agricultural technology since the green revolution: genetically engineered (GE) crops.

Table 2.1 shows how the area of farms specialized in seasonal crops – i.e. crops produced from plants that need to be replanted after each harvest – increased by 11.8 millions hectares between 1996 and 2006. Out of these 11.8 millions, 8.3 millions hectares were part of farms specialized in soybean production. Figure 2.2 shows the time variation of the actual area planted with soy in Brazil since 1980. This area doubled in the last 10 years, with a clear breaking point in the early 2000s, in correspondence with the approval of GE soybean seeds for commercial use.

GE soy, in its Roundup Ready (RR) variety, was patented in 1996 by the multinational agricultural biotechnology corporation Monsanto. It was tested in Brazil by Monsanto since 1998, then temporarily legalized in 2003, and definitely approved for commercialization in 2005.[1] The adoption of GE soy by Brazilian farmers has been fast. According to the last Agricultural Census, in 2006 already 46.4% of Brazilian farmers were using GE soy (IBGE, 2006, p.144). The Foreign Agricultural Service of the USDA reports that GE soybean seeds were used in 85% of the area planted with soy in the 2011/2012 crop year (USDA, 2012).

Brazilian farmers opted for GE soy with the objective of reducing production costs. GE soybeans seeds have a gene that makes them herbicide tolerant: a feature that allows farmers to adopt a new "package" of techniques that lowers

[1] In 1998 the CTNBio, the National Technical Bio-Safety Committee gave permission to Monsanto to conduct field tests of the RR soy (Comunicado n. 54/1998). In 2003, law 10.688 allowed the commercialization of RR soy for one season, requiring users to burn all unsold stocks after the harvest. Finally, in 2005 law 11.105 – the New Bio-Safety Law – authorized production and commercialization of RR soy (art. 35). Since Monsanto holds the exclusive rights to the commercialization of RR soy, Brazilian farmers pay royalties for its use after each harvest.

labor intensity for several reasons. First, since GE soybeans are resistant to herbicides, weeds control can be done more flexibly, because the chemical can be applied at any time during the season, also after the emergence of the plant (Duffy and Smith, 2001). Second, the new GE soybeans are resistant to a specific herbicide (glyphosate), which needs fewer applications: on average fields cultivated with GE soybeans require 1.55 sprayer trips against 2.45 of conventional soybeans (Duffy and Smith, 2001; Fernandez-Cornejo et al., 2002). Third, herbicide resistant soybean ease the adoption of no-tillage techniques, a system that substitute plowing with the application of chemical products that supplement the ground with the nutrients removed by the previous season's crop. The system reduces labor requirement, because application of chemicals needs fewer and shorter trips than plowing, and because no-tillage allows greater density of the crop on the field (Huggins and Reganold, 2008). Finally, farmers that adopt the new GE soybeans report also gains in the time to harvest, because combine harvesters tend to clog less when herbicides are applied later in the season, after the plant has emerged (Duffy and Smith, 2001).

These gains explain why the technology spread so fast, even though experimental evidence reports no improvements in yield with respect to conventional soybeans (Fernandez-Cornejo and Caswell, 2006)[2].

The reduction of labor intensity of soybean cultivation is apparent also in Brazilian aggregate statistics. The first four columns in table 2.2 report total area and total workers employed in agriculture from the Agricultural Censi of 1996 and 2006 broken down by farms' principal activity. Column 5 and 6 report the number of workers per thousand hectares across the different activities in the same two years, and the last column report the relative change. From the table it is clear that soybean production experienced the largest reduction in labor intensity, with a drop of workers per unit of land of -37%. By contrast, labor intensity in other activities either did not change (as permanent crop cultivation) or increased (as in cattle ranching and in forestry).

Table 2.2 also shows another important point. Soybean production is the least labor intensive agricultural activity, employing on average 22 workers per thousand hectare against 153 of other seasonal crops, 27 of cattle ranching and 39 of forestry.

Tables 2.3 shows correlations between the expansion of soy and the movement of other agricultural activities. It is constructed from Census data at the municipality level by dividing all Brazilian municipalities into three groups: the first (in column 1) with municipalities where the total area reaped with soybean increased

[2]In particular, in the U.S. the main advantage enjoyed by farmers that adopted GE soybeans is not in terms of higher yield, but in terms of time saved: in 2000 off-farm income of GE soybean adopters was 28% higher than that of non-adopters, while farm income was roughly the same (Fernandez-Cornejo et al., 2005).

between 1996 and 2006, the second (in column 2) with municipalities where area reaped with soybean decreased between 1996 and 2006 and the third (column 3) with those municipalities that did not produce soybean neither in 1996 nor in 2006. Panel A of table 2.3 reports the average change in the area of farms whose principal activity is the cultivation of seasonal crops (except soybean), permanent crops, cattle ranching and forestry in each of these 3 groups of municipalities. The last 2 columns of panel A report the difference between the averages in column 1 and 2 and the p-values from a test of equality of the means of these two groups. Panel B of table 2.3 shows the total changes in these areas.

Table 2.3 shows that soy expanded primarily on areas previously occupied by cattle ranching. Moreover, it also shows that in municipalities where soy expanded, other seasonal crops expanded at the same time. Table 2.4 looks deeper into the changes of other seasonal crops, and report the average change in the area reaped with the main crops in Brazil across the three groups of municipalities (those that expanded soybean cultivation, those that retrenched and those that do not cultivate soybean neither in 1996 nor in 2006). From table 2.4 it is clear that soybean cultivation expanded together with maize and (to a lesser extent) with wheat cultivation, while it does not show a strong correlation with the other main seasonal crops. Overall, the evidence shown in tables 2.3 and 2.4 is consistent with anecdotal evidence, that reports soy cultivation expanding on areas previously devoted to cattle ranching, and soybean and maize being cultivated on the same fields during different seasons of the year.

In order to study the the effects of the recent technological changes on labor intensity in agriculture it is important to understand why soybean and maize expanded together. Maize used to be cultivated as soy, during the summer season (August to December in Brazil). At the beginning of the 1980s few farmers in the South-East started producing maize during the fall (from March to July), and introduced what is now know as *milho safrinha* ("maize from the small season"). Adoption of a second season of maize is especially convenient in places where soy is cultivated over the summer, because maize can be planted right after the harvest of soybean. Nevertheless, a second season of maize is not feasible everywhere in Brazil, because only where the rain season ends relatively late can a second season be planted: it is for this reason Southern states like Santa Caterina and Rio Grande do Sul can not produce maize during the fall.

Cultivation of a second season of maize requires the use of very modern techniques. A second season removes much nitrogen from the soil, and this needs to be re-added with fertilizers (EMBRAPA, 2006). Herbicides are also needed to remove rapidly the residuals of the previous soy crop, and to allow the farmers to plant the second season in time (if planting is done after march 15th it is less productive). Time also forces the planting of the second season to be carried out faster than the first one (one month window against two months window), which means that it is difficult to do it without modern tractors (CONAB, 2012). Finally, *milho safrinha* puts a lot of stress on the soil and EMBRAPA advises to use no-tillage techniques, as tilling twice a year would accelerate the "compactification" of soil, reducing land productivity (EMBRAPA, 2006). As already explained no-tillage techniques require the use of modern fertilizers and tractors.

Finally, even with advanced techniques, maize is still more labor intensive than both soy and other agricultural activities like cattle ranching. In the USDA Agricultural Resources Management Survey (ARMS) labor cost of maize cultivation in 2001 and 2005 were on average 1.8 and 1.4 times higher than the labor cost for soy cultivation[3].

These considerations are relevant when trying to isolate the effect of the adoption of GE soybean on labor intensity in agriculture. Between 1996 and 2006 soybean became much less labor intensive and occupied areas that were devoted to activities that were more labor intensive. For these reasons we expect the change in soybean cultivation to be negatively correlated with labor intensity in agriculture. At the same time, in many areas soybean expanded together with the cultivation of a second season of maize. Maize cultivation is an activity that is relatively more labor intensive than cattle ranching; and so when ranches are replaced by the cultivation of soybean in the summer and maize in the fall the net effect on labor intensity is ambiguous. It is for this reason that when we correlate labor intensity with the change in area devoted to soybean, we need to control for the change in the area devoted to maize. For the same reason, we also need to find an instrument for the change of area cultivated with maize when we try to identify the causal effect of the GE technology changes on agricultural and industrial outcomes.

2.3 Data

In this paper we use three main data sources: the Agricultural Census for data on agriculture, the Yearly Industrial Survey (PIA) for the data on manufacturing and the FAO Global Agro-Ecological Zones database for potential yields of soy and other crops.

The Agricultural Census is released at intervals of 10 years by the IBGE, the Brazilian National Statistical Institute. We use data from the last two rounds of the census that have been carried out in 1996 and in 2006. This allows us to observe agricultural variables both before and after the introduction of genetically engineered soybean seeds, that were legalized in Brazil in 2003). The census data

[3]Maize (corn) survey years are 2001 and 2005, soybean producers were surveyed by the USDA in 2002 and 2006.

is collected through direct interviews with the managers of each agricultural establishment and is made available online by the IBGE aggregated at municipality level. The main variables we use from the Census are: the value of agricultural production, the number of agricultural workers and the area devoted to agriculture in each municipality. Out of the area devoted to agriculture in each municipality we are able to disentangle the area devoted to each crop in a given Census year. This allows us to monitor how land use has changed between 1996 and 2006. The upper panel of Table 1 reports the total area reaped (in millions hectares) for the three major crops by cultivated area produced in Brazil: soybean, maize and sugar. Among these three crops, soybean registered the largest absolute increase in terms of area reaped between the last two Census years. The area devoted to soybean increased from 9.2 to 15.7 millions hectares between 1996 and 2006, more than half of the total increase in all seasonal crops. The lower panel of Table 1 reports total employment (in millions workers) in seasonal crops production and in agriculture as a whole. As reported in the last row, there are around 17 millions Brazilians - around 20% of the Brazilian active population - whose main working activity is agriculture. Notice that between 1996 and 2006, although the area cultivated with seasonal crops has increased by a third (from 36.8 to 48.2 millions hectares), the number of workers employed in seasonal crops' production decreased from 6.8 to 6.4 millions. This might be due to 2 reasons: technological change in the production of single crops (within-crop effect) and the switch from more to less labor intensive crops (across-crops effect). The introduction of genetically engineered soybean, whose production requires less workers per hectare than normal soybean, is an example of a within-crop effect. The across-crop effect derives from the fact that the production of some crops is less labor intensive to start with. Data from the 1996 Agricultural Census suggests that soy production in Brazil employs on average 42 workers per thousand hectares, while maize and sugar production employ on average respectively 106 and 138 workers per thousand hectares. Part of the reduction in the number of workers employed in seasonal crops production could be due to agricultural establishment switching from more to less labor intensive crops, e.g. from maize to soy production.

Figures 1 to 3 compare the distributions of average actual yields (tons per hectare) across Brazilian municipalities in 1996 and 2006 for, respectively, soy, maize and sugar. As for soy and maize, there was a clear shift to the right in the distribution of average yields, indicating some type of technological improvement taking place. As for sugar, on the contrary, the distribution of average yields looks very similar in 1996 and 2006.

Our second source of data is the Yearly Industrial Survey (PIA), produced by the IBGE, that monitors the performance of Brazilian firms in the extractive and manufacturing sectors. We focus on the manufacturing sector as defined by the Brazilian sector classification CNAE 1.0 (sectors 15 to 37). We use yearly data from 1996 to 2007. The population of firms eligible for the survey is composed by all firms with more than 5 employees registered in the national firm registry (CEMPRE, 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*), the second includes all firms having 30 or more employees (*estrato certo*). For all firms in the survey the data is available both at firm and at plant level (when firms are composed by more than one plant). Our unit of observation is, for most outcomes, the plant. At plant level the survey includes information on: number of employees, wage bill, revenues, costs, capital investment and gross value added. Finally, we use data on potential yields for soy and other crops from the Global Agro-Ecological Zones database produced by the FAO. Potential yields are the maximum yields attainable for a crop in a certain geographical area. They depend on the climate and soil conditions of that geographical area, and the level of technology available. The FAO-GAEZ database provides estimates of potential yields under three levels of technology: low, intermediate and high. Each of these levels is captured by the availability of certain inputs like machines and fertilizers. When the level of technology is assumed to be low, agriculture is aimed at subsistence. It is mostly labor-intensive, it uses traditional cultivars and does not use nutrients or chemicals for pest and weed control. When the level of technology is assumed to be intermediate, agriculture is partly market oriented. Production is partly mechanized, it uses improved varieties and some fertilizers and chemicals for pest and weed control. When the level of technology is high, agriculture is market oriented. Production is fully mechanized, it uses improved or high yielding varieties and "optimum" application of nutrients and chemical pest, disease and weed control. The database reports potential yields for each crop under low, medium and high technological levels available in agriculture for a worldwide grid at a resolution of 9.25 x 9.25 km. Figure 4, 5 and 6 show the potential yields for soybean in Brazil under, respectively, low and high technology. The same type of maps are also available for maize and sugar. In order to match the potential yields data with agriculture and industry variables we superimposed each of the potential yields' maps with a political map of Brazil reporting the boundaries of each municipality. Then we took the average of the potential yield, weighted by the area, within each municipality. We repeated this operation per each crop and per each of the three levels of technology. Finally, we measure technological change within each municipality by computing the difference between yields under the high and the low technology. Figure 7 illustrates the resulting measure of technological change in soy.

2.4 Empirics

In this section we study the effect of the adoption of a new agricultural technology, genetically engineered (GE) soybean seeds. The GE soy seeds were first commercially introduced in the U.S. in 1996 and legalized in Brazil in 2003. The advantage of this seeds relative to traditional ones is that they are herbicide resistant which implies that no-tillage planting techniques can be used.[4] The planting of traditional seeds is preceded by soil preparation in the form of tillage to kill the weeds in the seedbed that would crowd out the crop or compete with it for water and nutrients. In contrast, the planting of herbicide resistant GE soy seeds requires no tillage as the herbicide kills the weeds. Then, the GE soy seeds can be applied directly on last season's crop residue. This new technology is then expected to save on production costs, in particular requires less labor per unit of land to yield the same output. Then, the adoption of GE soy seeds can be characterized as labor saving technical change.

Note that traditional models of the effects of agricultural productivity on industrial development like Matsuyama (1992) focus on hicks neutral technical change. They predict that in an open economy an increase in agricultural productivity would induce increases in agricultural employment and a reduction in the size of the industrial sector as labor reallocates towards agriculture and wages increase. The type of technical change we study is instead labour saving. As a result, new forces emerge: if the reduction in the labor intensity of agricultural production is strong enough, employment in agriculture and wages might fall, inducing an expansion of industrial employment. Then, whether technological change in agriculture induces industrial development in an open economy depends on the factor bias of technical change. In this section we exploit the adoption of GE soy seeds to asses the relative importance of these two forces.

For this purpose, we first study the effect of the adoption of GE soy on the factor intensity of agricultural production and agricultural labor markets. Next, we asses its impact on industrial employment. We start by reporting simple correlations between the expansion of the planted area with soy relative to other crops and agricultural and industrial labor market outcomes. Next, to establish causality, we exploit the timing of legalization and the differential impact of the new technology on potential yields across geographical areas, which depends on local weather and soil characteristics.

[4]Genetic engineering (GE) techniques allow a precise alteration of a plant's traits and permit targeting a single plant trait. This facilitates the development of characteristics not possible through traditional plant breeding and decrease the number of unintended characteristics. In the case of herbicide resistant GE soy seeds their genes were altered to include those of a bacteria that was herbicide resistant.

2.4.1 Basic Correlations in the Data (OLS)

We start by documenting how soy and maize expansion during the 1996-2006 period relates to changes in the agricultural production and in the industrial labor market. Here we present a set of OLS regressions in which agricultural and industrial outcomes are regressed on the percentage of farm land cultivated with soy and maize, inserted one at a time in separate regressions. Note that these results are intended to introduce to the basic correlations in the data, but do not claim to uncover any causal relation between variables.

The basic form of our equation is:

$$y_{jt} = \alpha_j + \alpha_t + \beta \left(\frac{Crop\ Area}{Agricultural\ Area} \right)_{jt} + \varepsilon_{jt} \tag{2.1}$$

where $\left(\frac{Crop\ Area}{Agricultural\ Area} \right)_{jt}$ is total area reaped with either soy or maize divided by total farm land[5] and y_{jt} are agricultural or industrial outcomes of interest. The units of observation are Brazilian *smallest comparable areas* (*AMC*: Área Mínima Comparável in Portuguese)[6] and we wish to control for both AMC and year fixed effects (α_j and α_t). Our source for agricultural variables is the decennal Agricultural Census, which means that for both the independent variable and the agricultural outcomes we have only two observations over the last 20 years: one in 1996 and the other in 2006. With only two periods, fixed effects and first difference estimates are identical, so we estimate (2.1) in first differences:

$$\Delta y_j = \Delta\alpha + \beta\, \Delta \left(\frac{Crop\ Area}{Agricultural\ Area} \right)_{j} + \Delta\varepsilon_j \tag{2.2}$$

where changes are defined between 1996 and 2006 throughout.

Table 2.5 reports correlations between changes in area reaped and changes in agricultural production: the first panel presents the results using changes in area reaped with soy while the second using changes in area reaped with maize. Together they suggest that although technological progress was fast in both crops, soy production became much less labor intensive and that this in turn drove labor out of agriculture.

[5]Total farm land includes areas devoted to crop cultivation (both permanent and seasonal crops), animal breeding and logging.

[6]Municipalities are the smallest administrative areas for which many Census variables are recorded in Brazil. Over the years however, many municipalities split and reorganized to accomodate population growth and migration, with the result that municipalities are not always comparable across years. *Smallest comparable area* is the smallest geographical breakdown for which consistency is warranted overtime.

The first column of Table 2.5 reports the relationship with productivity, and shows that in places where soy and maize cultivation expanded, also the value of agricultural production per worker increased. Value per worker is defined here as the total value of crop production divided by total number of workers and refers only to seasonal crop production[7]. Column 2 confirms that labor intensity in agriculture decreased where soy cultivation expanded and increased where maize cultivation did. Agricultural labor intensity is measured here as number of workers per acre in seasonal crops cultivation. These results support the notion that changes in agricultural productivity have come with major changes in the mode of production, and in particular that in 2006 soy is being produced with relatively less labor per acre and maize with relatively more. This evidence in turn, is consistent with the qualitative description of technological change we gave above, and in particular with the fact that in 2006 much of soybean was produced with GE seeds that need much less labor. Column 3 shows that the share of workers employed in agriculture decreased in places where soy expanded and did not change significatively in places cultivated with maize. Share of workers employed in agriculture is defined as total number of workers in agriculture (from the Agricultural Census) divided by total number of workers (calculated as total number of workers in non-agricultural sectors from CEMPRE plus total number of workers in agriculture from the Agricultural Census).

Table 2.6 reports results from regression (2.2) using industrial labor market outcomes as dependent variables. We focus only on manufacturing plants (CNAE 1.0 codes 15 to 37) owned by firms that employ at least 30 employees: the sample for which the PIA Empresa survey contains the population of Brazilian firms. Industrial outcomes come from the yearly plant-level survey: we aggregate these data at AMC level and then collapse all years in 2 periods: one before 2003 and one after it, in order to maximize the number of observation used in the regressions[8]. Total employment includes both production and non-production workers; plant size is calculated as total number of workers within an AMC divided by total number of plants; wage is aggregate wage bill within an AMC divided by total number of workers there.

The first column of Table 2.6 shows that total employment in manufacturing significantly grew where soy expanded, but not where maize did. The second and third columns qualify this result, and show that the increase came from both the intensive and the extensive margin: column 2 shows that places where soy expanded had on average more plants, while the third column shows that these

[7]Both soy and maize are seasonal crops.

[8]Brazilian manufacturing activity is very concentrated, and not all AMCs host at least 1 plant from a large firm every year. Collapsing observations from more than one year allows us to use all municipalities for which we observe 1 plant at least once before and after the introduction of GE soy seeds.

plants were on average larger. Finally, the last column of table 3 shows that wages did not move in a statistically significant way where soy expanded while they grew where maize did.

Overall, the results on employment are consistent with a rightward shift of the labor supply schedule driven by soy expansion: although the positive sign on wages does not support this story, notice that this result might be driven by omitted variable bias, because the sign is reversed when the change in soy and maize area are inserted together in the regression.

2.4.2 Causality

In this section we provide direct empirical evidence on the effects of the widespread adoption of a new agricultural technology, GE soybean seeds, on industrial development in Brazil. The basic correlations in the data reported in the previous section show that areas where soy expanded experienced an increase in output per worker and a reduction in labor intensity in agriculture and an expansion in industrial employment. This could respond to the adoption of labor saving agricultural technologies reducing labor demand in the agricultural sector and thus inducing a reallocation of labor towards the industrial sector. Alternatively it could be due to other shocks to local labor markets. For example: an increase in labor demand in the industrial sectors could increase wages, inducing agricultural firms to switch to less labor intensive crops, like soy. To establish the direction of causality we exploit the timing of adoption and the differential impact of the new technology on potential yields across geographical areas. First, the new technology was commercially introduced in the U.S. in 1996, and legalized in Brazil in 2003. Thus, we use the periods before and after 2003 as our "pre and post-treatment" periods. Second, the new technology has a differential impact on potential yields depending on soil and weather characteristics. Thus, we exploit these exogenous differences on potential yields across geographical areas as our source of cross-sectional variation in the intensity of the treatment.

To implement this strategy, we need an exogenous measure of potential yields for soy and other crops, which we obtain from the FAO-GAEZ database. These potential yields are estimated using an agricultural model that predicts yields for each crop given climate and soil conditions. As potential yields are a function of weather and soil characteristics, not of actual yields in Brazil, they can be used as a source of exogenous variation in agricultural productivity across geographical areas. In addition, the database reports potential yields under different technologies or input combinations. Yields under low inputs are described as those obtained using traditional seeds and no use of chemicals, while yields under high inputs are obtained using high yielding varieties and optimum application of chemicals for weed control. Thus, the difference in yields between the high and low technology captures the effect of moving from traditional agriculture to a technology that uses optimum weed control, among other characteristics. [9] We expect this increase in yields to be a good predictor of the profitability of adopting herbicide resistant GE soy seeds. Thus, we can then exploit the predicted differential impact of the high technology on yields across geographical areas in Brazil as our source of cross-sectional variation in agricultural productivity. This research design allows us to investigate whether exogenous shocks to local agricultural productivity lead to changes in the size and efficiency of the local industrial sectors. Note that this identification strategy relies on the assumption that although Brazil is an open economy, the existence of transport costs implies that local markets are important sources of labor and demand for industrial firms.

More formally, our basic empirical strategy is to estimate an equation of the following form:

$$y_{jt} = \alpha_j + \alpha_t + \beta A^{soy}_{jt} + \varepsilon_{jt} \tag{2.3}$$

where y_{jt} is an outcome that varies across municipalities and time, j indexes municipalities, t indexes time, α_j are municipality fixed effects, α_t are time fixed effects and A^{soy}_{jt} = potential yield of soy under high (low) inputs if $t \geqslant$2003 ($t <$ 2003).

Note that a potential problem with this identification strategy is that the productivity of land is positively correlated across crops, thus we could be capturing the effect of overall technical change instead of the labor saving technical change associated to GE soy. For example, during this period there were also increases in yields of maize, a labor intensive crop. Thus, we need to control in the above regressions for the changes in yields of maize when switching from the low to the high technology. We then include the following variable as a control: A^{maize}_{jt} = potential yield of maize under high (low) inputs if $t \geqslant$2003 ($t <$ 2003). In addition, we want to control for changes in the prices of crops, that can also have an influence on the expansion of soy relative to other agricultural activities. Note that the overall effect of price changes would

[9]FAO-GAEZ description of each technology is as follows:

Low-level inputs/traditional management

"Under the low input, traditional management assumption, the farming system is largely subsistence based and not necessarily market oriented. Production is based on the use of traditional cultivars (if improved cultivars are used, they are treated in the same way as local cultivars), labor intensive techniques, and no application of nutrients, no use of chemicals for pest and disease control and minimum conservation measures."

High-level inputs/advanced management

"Under the high input, advanced management assumption, the farming system is mainly market oriented. Commercial production is a management objective. Production is based on improved high yielding varieties, is fully mechanized with low labor intensity and uses optimum applications of nutrients and chemical pest, disease and weed control."

$$y_{jt} = \alpha_j + \alpha_t + \beta A_{jt}^{soy} + \gamma A_{jt}^{maize} + \sum_z \theta_z p_t^z A_{j0}^z + \varepsilon_{jt} \tag{2.4}$$

where z = soy, maize and sugar, p^z is the international price of crop z, A_{j0}^z is potential yield of crop z under low inputs for maize, medium for sugar.

First we estimate the effects of the agricultural technology shock A^{soy} on the share of agricultural land devoted to soy. As the increase in yields resulting from adoption of the high technology ($A_{high}^{soy} - A_{low}^{soy}$) is expected to be correlated with the profitability of technology adoption, we can use changes in A^{soy} as an instrument for changes share of agricultural land devoted to soy. Then, we can perform an instrumental variables estimation of equation (2.1) where the first stage is given by equation (2.3). Alternatively, we can estimate reduced form equations of the form of (2.3) where we study the direct effect of the change in potential yields driven by technology adoption ($A_{high}^{soy} - A_{low}^{soy}$) on the set of outcomes we are interested in, namely: value of output per worker and labor intensity of agricultural production and industrial employment. In the following subsections, we report our first stage, reduced form and instrumental variable estimates.

2.4.3 First Stage

We document here the relation between the change in agricultural area cultivated with soy and maize on our technological shock described above. The purpose of these regressions is twofold. On the one hand we see them as a "sanity check" on the information content of our technological shock; on the other hand these regressions represent the first stage used to address the endogeneity of the OLS regressions reported above.

The equations we estimate are:

$$\Delta \left(\frac{Crop\ Area}{Agricultural\ Area} \right)_j = \Delta\alpha + \beta \Delta A_j^{soy} + \Delta\varepsilon_j$$

and

$$\Delta \left(\frac{Crop\ Area}{Agricultural\ Area} \right)_j = \Delta\alpha + \beta \Delta A_j^{soy} + \gamma \Delta A_j^{maize} + A_j^{sugar} + \Delta\varepsilon_j \tag{2.5}$$

where $\Delta \left(\frac{Crop\ Area}{Agricultural\ Area} \right)$ is the change in share of farm land reaped with either soy or maize between 1996 and 2006 and it is defined as in section 3.1. A_j^{soy}, A_j^{maize} and A_j^{sugar} are potential yields in AMC j for soy, maize and sugar while the technological shocks are defined as explained in the previous section: $\Delta A^z = A^z_{high} - A^z_{low}$, with z =soy, maize. We control for general suitability to sugar production with intermediate inputs (A_j^{sugar}) because sugar is the other major seasonal crop in Brazil. However, we do not use its change in potential yield because sugar production does not seem to be more productive in 2006 relative to 1996 (see graph 3).

Column 1 and 3 in Table 2.7 show that the soy and maize shock correctly predict soy and maize expansion over the period: when inserted alone, an increase in soy (maize) potential yield is associated with significantly greater farm land reaped with soy (maize). Column 2 and 4 strengthen this results: they show that controlling for both shocks and for the potential yield of sugar, increase the effect of both shock: on soy, the effect more than doubles, while on maize the effect goes up by one third. Also, the fact that the maize shock has a significantly negative effect on the share of farm land cultivated with soy and that the potential yield of sugar has a significantly negative effect on both shares, is also consistent with optimal behavior of farmers, who choose which crop to cultivate based on the specific suitability of their plots. Note that all of these suitability measures tend to be positively correlated (this is especially true for maize and soy), so the result that the shocks correctly predict expansion or retrenchment of specific crops means that they capture the dimensions that are more relevant for the decisions of farmers.

All in all, Table 2.7 support the choice of our instrument. The effect of the shocks are extremely significant (the F-tests for the joint significance of the regressors range from 141.8 to 15.51) and also economically relevant. The estimated coefficient on soy implies that municipalities with a one standard deviation above the mean increase in potential soy yields increased the share of soy in planted land area by 36% of a standard deviation.

2.4.4 Reduced Form

Once we have established the relevance of our instruments, we turn to the study of the effect of technological change in agriculture on production and employment in agriculture and manufacturing: our reduced form. In this section we show that the change in potential soy yield is associated with a reduction in the use of labor in agriculture and with a rightward shift of the labor supply schedule in manufacturing.

We start with agricultural production: table 5 reports the results of running an equation similar to (2.5) on the agricultural outcomes used in table 2:

$$\Delta y_j = \Delta\alpha + \beta\Delta A_j^{soy} + \gamma\Delta A_j^{maize} + A_j^{sugar} + \Delta\varepsilon_j$$

where y_j is value produced per worker in seasonal crops, labor intensity or share of workers employed in agriculture all defined as in section 3.1.

Table 2.8 reinforce the results on agriculture highlighted in section 3.1: productivity increased in places where potential soy yield increased relatively more, while in the same places labor intensity in agriculture dropped and agriculture shrunk in terms of total employment. At the same time, in places where potential maize yield increased relatively more, the number of workers per acre increased, and the share of population employed in agriculture grew. Also, the maize shock seems to be negatively associated with value produced by worker in seasonal crops, but this effect is not significantly different from 0. It is interesting to note that these results do *not* hold when we run these regressions only including AMC in the North-East of the country, the region where sugar has been cultivated historically, and neither soy nor maize are widespread (results available upon request).

Overall, Table 2.8 is consistent with our interpretation of the technological change brought about by GE soy seed. The new technology seems to require much less labor per acre, and this in turn seems to have driven labor out of the agricultural sector.

We now turn to the regressions on manufacturing outcomes. Here we are able to exploit the full panel structure of our data: the equation we estimate with these data have the form:

$$y_{jt} = \alpha_j + \alpha_t + \beta A_{jt}^{soy} + \gamma A_{jt}^{maize} + \sum_z \theta_z P_t^z A_{j0}^z + \varepsilon_{jt}$$

where y_{jt} are industrial outcomes of interest; A_{jt}^{soy} (A_{jt}^{maize}) are potential yield of soy (maize) under low inputs for all years before 2003 and under high input for all years from 2003 on. We observe all years from 1996 to 2007 and control for the real price of soy, maize and sugar times potential yield for these crops in 1996: A_{j0}^z is potential yield under low inputs when $z =$ soy or maize and it is potential yield under medium inputs when $z =$ sugar. These controls are intended to make sure that changes are truly driven by technological change rather that the evolution of commodity prices. Although international commodity prices will affect all Brazilian AMC in the same way, they might still have heterogeneous effect in places that were more suitable to the cultivation of some particular crop at the beginning. Controlling for the interaction of these prices with potential yield in 1996 makes sure that our results are not driven by the heterogeneous effects that commodity prices have on different places in Brazil. In all specification we control for both AMC and year fixed effects (α_j and α_t) and cluster standard errors at AMC level to avoid that serial correlation in our shock make the precision of our estimates artificially high (Bertrand et al. (2004)).

Table 2.9 shows reduced form results with industrial variables. All variables are defined as above and are built as averages at AMC level starting from plant level data. Again we focus on manufacturing plants owned by firms with at least 30 employees.

The first column of Table 2.9 reproduces closely the patterns highlighted in section 3.1. In particular, it confirms that industrial employment (both production and non-production workers) grew in places where soy potential yield increased more. The effect of maize potential yield is negative (as in the OLS) but not significantly different from 0. Contrary to the OLS results however, the effect on employment seems to come exclusively from the growth of existing plants: in places where the change in potential soy yield increased more, average plant size grew significantly, while the total number of plant did not change. Average wage fell in places where the potential yield of soy increased more, and grew in places where the change in potential yield of maize was greater.

Overall, the results shown in Table 2.9 are consistent with rightward shift of the labor supply curve in areas where the change in potential yield of soy was relatively greater, and a (less clear) leftward shift of the labor supply schedule in the places where the change in potential yield of maize was relatively greater. Together with table 2.8, these results support our story of a labor-saving technological change in soy production promoting a growth of industrial employment by freeing up labor in agriculture.

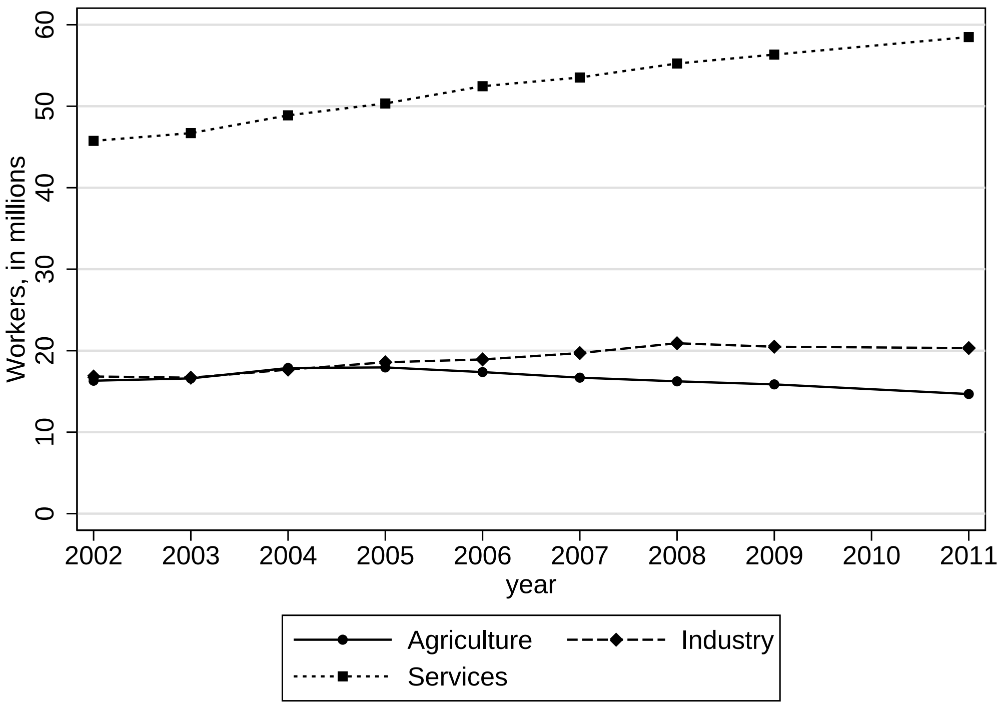
2.5 Final Remarks

The process of modern economic growth has been accompanied by a reallocation of economic activity from agriculture to industry. Isolating the forces behind structural transformation is therefore key to our understanding of economic development. Based on the experience of England during the industrial revolution, economists have argued that increases in agricultural productivity can be one of these forces. However, Matsuyama (1992) notes that the positive effects of agricultural productivity on industrialization occur only in closed economies, while in open economies a comparative advantage in agriculture can slow down industrial growth. Despite the importance of the question, there is so far scarce micro evidence on the channels through which increases in agricultural productivity can shape the reallocation of economic activity across sectors in an open economy.

In this paper we contribute to the debate by isolating the effects of the adoption of new agricultural technologies on Brazilian manufacturing firms. In particular, we study the effects of the adoption of genetically engineered (GE) soybean seeds on industrial development in Brazil. To identify the causal effects of this new technology, we exploit the timing of adoption and the differential impact of the new technology on potential yields across geographical areas. We find that municipalities where GE soy is predicted to have expanded more experienced faster employment growth and wage reductions in the industrial sector. Interestingly, we find opposite effects when looking at municipalities where maize – a labor intensive crop that also experienced technical change during this periods – is predicted to have expanded more. The different effects documented for GE soy and maize indicate that the factor bias of technical change is a key factor in the relationship between agricultural productivity and industrial growth in open economies.

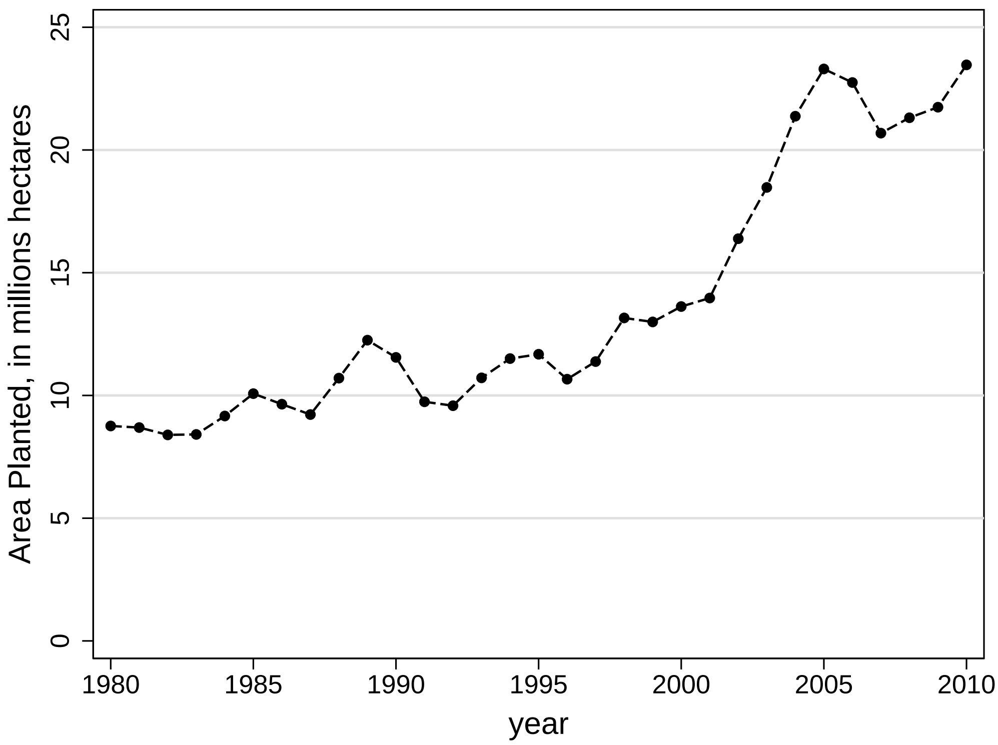
Figures and Tables

Figure 2.1: Labor Force in Agriculture, Industry and Services, 2002-2011



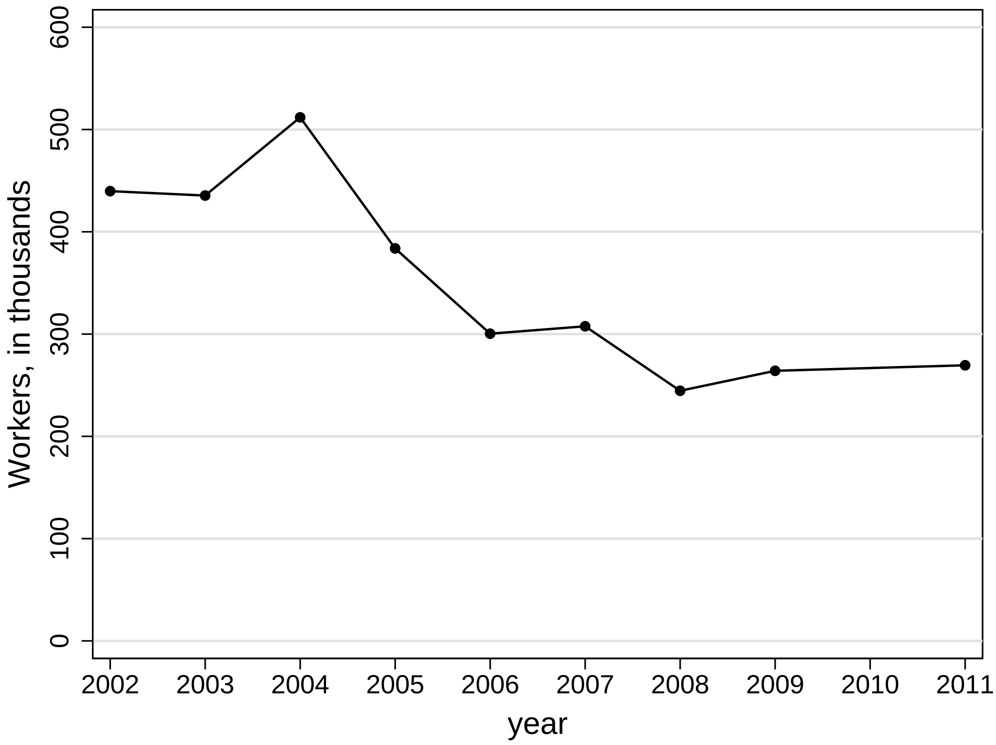
Notes: The Figure depicts the evolution between 2002 and 2011 of the total number of workers (expressed in millions) employed by sector in Brazil. The sectors are: Agriculture (including pasture, fishery and forestry), Industry (including manufacturing, construction and extractive industries) and Services. Data come from PNAD, a national household survey representative at country level and carried out yearly (with the exception of the population census years) by the Brazilian National Statistical Institute. The sectoral classification used is the CNAE-Domiciliar.

Figure 2.2: Area Planted with Soy, 1980-2010



Notes: The Figure depicts the evolution between 1980 and 2010 of the total area planted with soy in Brazil (expressed in millions hectares). Data come from monthly surveys carried out by CONAB, Companhia Nacional de Abastecimento, an agency created by the Brazilian Ministry of Agriculture. Data is constructed by interviewing on the ground farmers, agronomists and financial agents in the main cities of the country.

Figure 2.3: Labor Force in Soy Production, 2002-2011



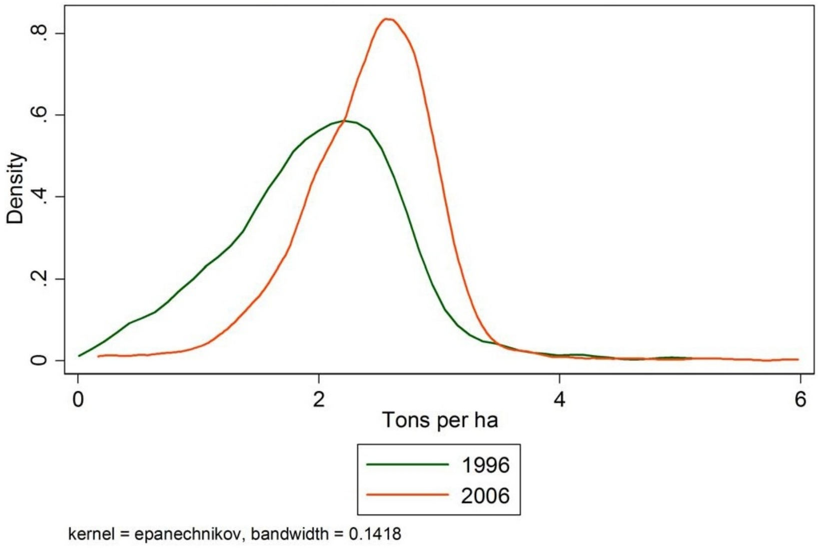
Notes: The Figure depicts the evolution between 2002 and 2011 of the total number of workers employed in soy production (expressed in thousands) in Brazil. Data come from PNAD, a national household survey representative at country level and carried out yearly (with the exception of the population census years) by the Brazilian National Statistical Institute. The sectoral classification used is the CNAE-Domiciliar (soy production has the code 01107).

Table 2.1: Land use (millions ha)

	1996	2006	Change	% change
Permanent crops	7.5	11.7	4.1	55%
Seasonal crops	34.3	44.6	10.4	30%
Cattle ranching	177.7	168.3	-9.4	-5%
Forest	110.7	91.4	-19.2	-17%
Not usable	15.2	8.2	-6.9	-46%
Other	8.3	9.0	0.7	8%
Total	353.6	333.2	-20.4	-6%

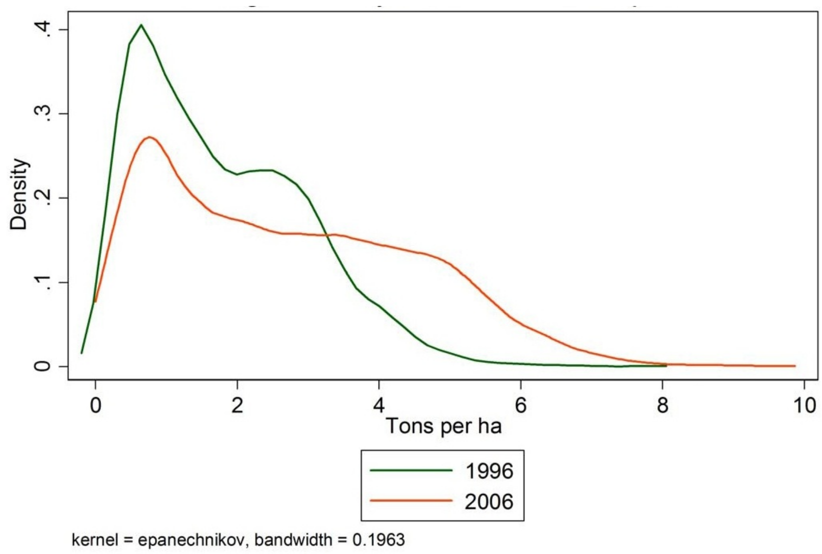
Notes: The Table reports the total land use in Brazil (expressed in millions hectares). Data is available for 1996 and 2006 and come from the last two Brazilian Agricultural Censi carried out by the Brazilian National Statistical Institute and it is sourced from the the IBGE Sidra repository (table 317 for 1996 and table 1011 for 2006). Seasonal crops include (among others) cereals (e.g. maize, wheat and rice), soybean, cotton, sugar cane and tobacco. Permanent crops include (among others) coffee and cocoa. Not usable land includes lakes and areas that are not suitable for neither crop cultivation nor cattle ranching. Other uses is not exactly comparable across years: in 1996 it includes resting area for seasonal crops; in 2006 it includes area devoted to pasture, flowers and buildings.

Figure 2.4: Distribution of Actual Soy Yields across Brazilian Municipalities in 1996 and 2006



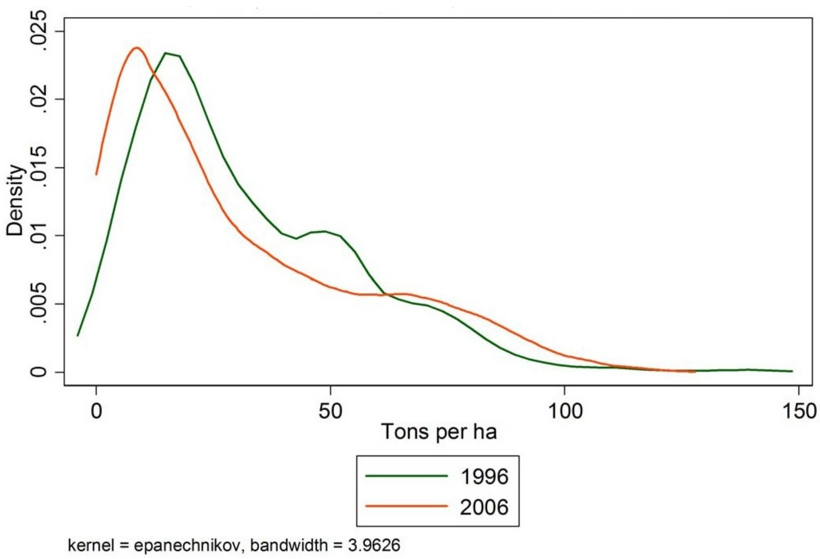
Notes: All data from Brazilian Agricultural Censi of 1996 and 2006, IBGE.

Figure 2.5: Distribution of Actual Maize Yields across Brazilian Municipalities in 1996 and 2006



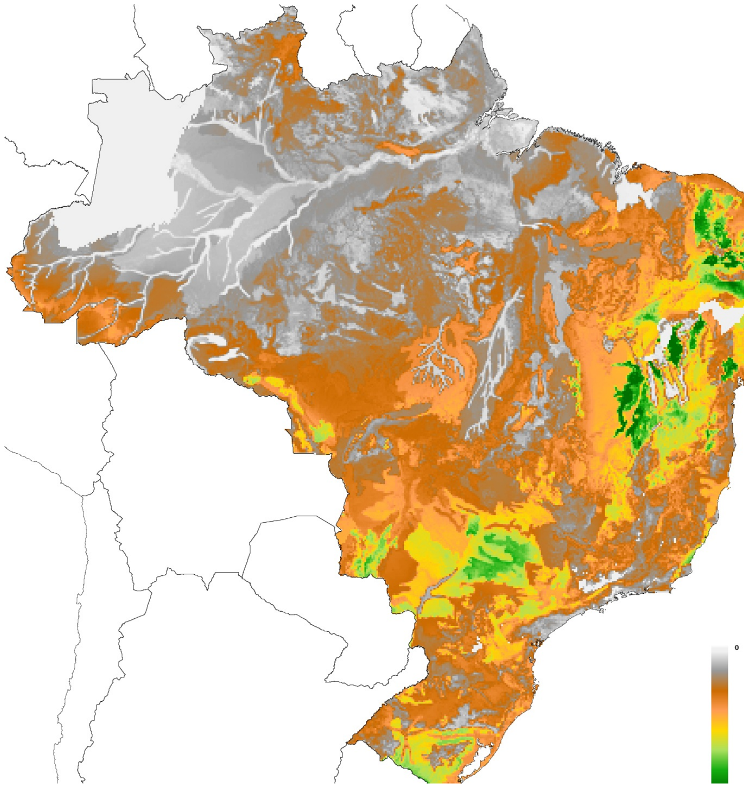
Notes: All data from Brazilian Agricultural Censi of 1996 and 2006, IBGE.

Figure 2.6: Distribution of Actual Sugar Yields across Brazilian Municipalities in 1996 and 2006



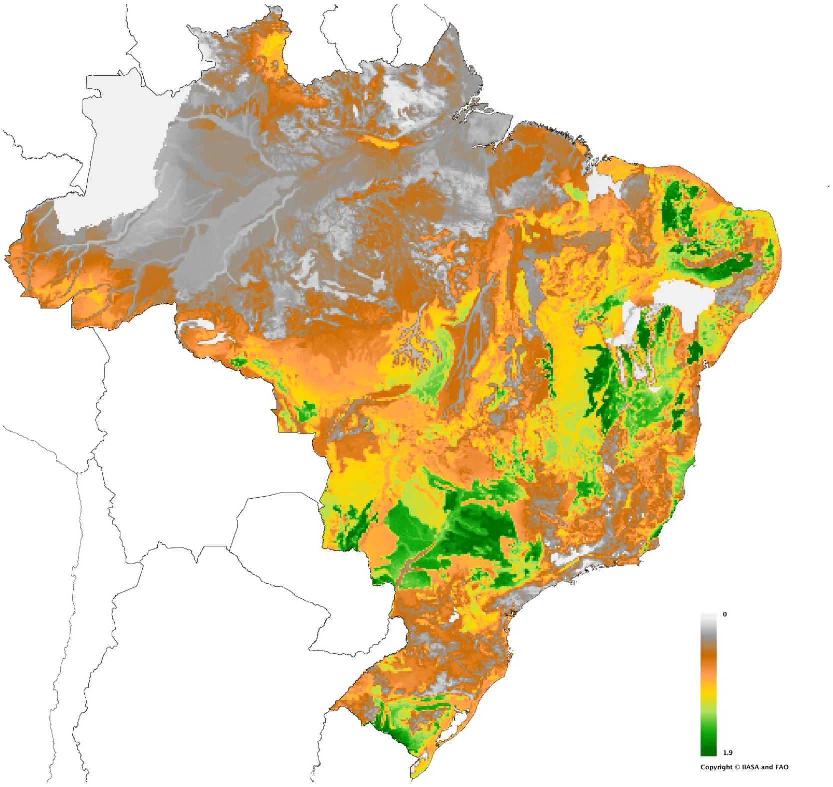
Notes: All data from Brazilian Agricultural Censi of 1996 and 2006, IBGE.

Figure 2.7: Potential Soy Yield Under Low Agricultural Technology



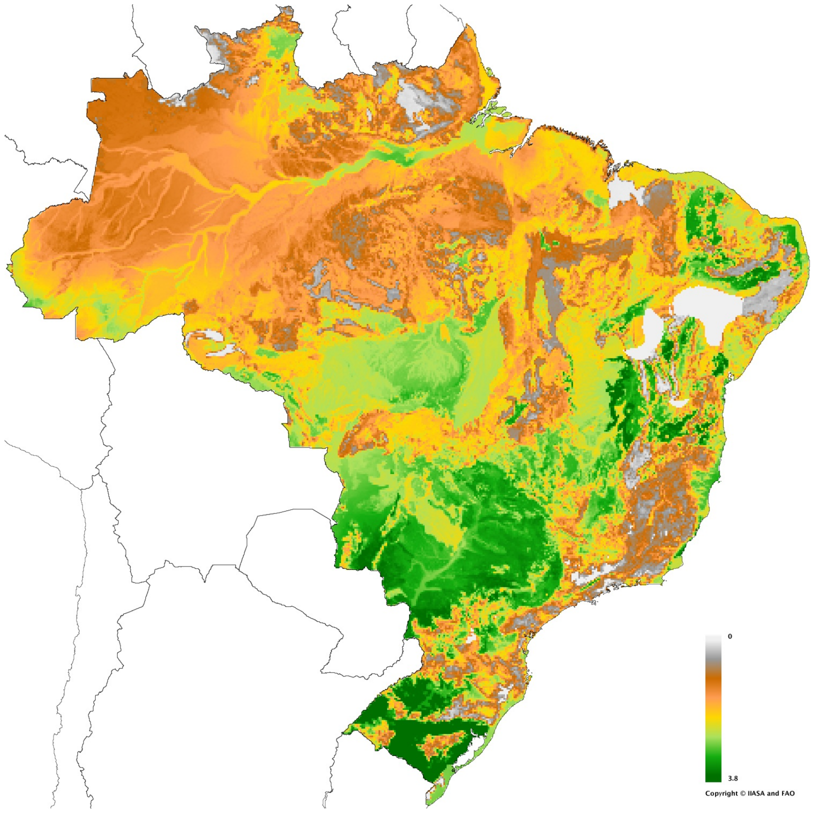
Notes: All data from FAO GAEZ.

Figure 2.8: Potential Soy Yield Under Intermediate Agricultural Technology



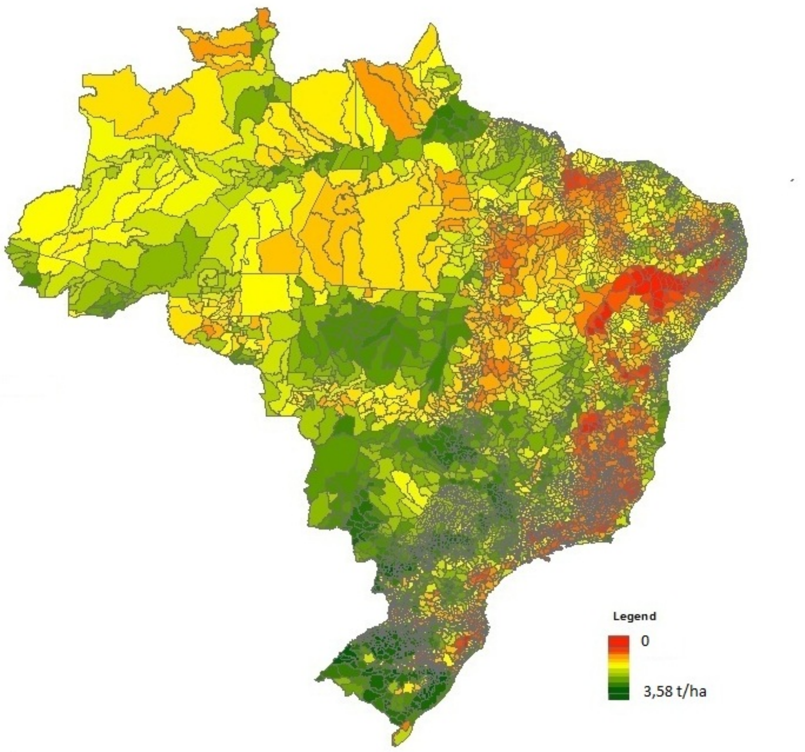
Notes: All data from FAO GAEZ.

Figure 2.9: Potential Soy Yield Under High Agricultural Technology



Notes: All data from FAO GAEZ.

Figure 2.10: Technological Change in Soy: Potential Yield under High Technology minus Potential Yield under Low Technology



Notes: Authors' calculations starting from FAO GAEZ data.

Table 2.2: Factor Intensity in Brazilian Agriculture: 1996-2006

	Area		Workers		Labour intensity			Change in labour intensity	
Principal activity:	1996	2006	1996	2006	1996	2006	Average 1996-2006	Absolute	Relative
Seasonal crops	63.0	74.8	6.8	6.3	107.6	83.7	94.6	-23.9	-22%
soy	*15.1*	*23.4*	*0.4*	*0.4*	*28.6*	*17.9*	*22.1*	*-10.7*	*-37%*
all cereals	*19.1*	*23.8*	*1.8*	*1.8*	*92.4*	*76.8*	*83.7*	*-15.6*	*-17%*
other	*28.8*	*27.6*	*4.6*	*4.0*	*159.2*	*145.4*	*152.5*	*-13.8*	*-9%*
Permanent crops	17.2	17.3	2.2	2.2	126.8	127.4	127.1	0.6	0%
Cattle ranching	213.9	221.8	4.8	6.8	22.6	30.6	26.7	8.1	36%
Forest	16.0	13.0	0.5	0.6	33.9	46.1	39.4	12.2	36%

Note: The table reports total land in farms and total number of workers employed by principal activity of the farm. Data are sourced from the IBGE Sidra repository. Land in farm by principal activity in 1996 comes from table 491 and for 2006 from table 797. Total number of workers in 1996 is reported in table 321 and in 2006 in table 956. Cereals are rice, wheat, maize and other cereals. Labor intensity is computed as number of workers per 1000 hectares. The definition of "principal activity" of the farm changed somehow between 1996 and 2006. In 1996 higher specialization was required for farms to be classified under one of the categories reported, and those that did not produce at least 2/3 of the value within a single category were classified under the "mixed activity" category. In 2006 farms were classified according to the activity that accounted for the simple majority of production and no "mixed activity" category existed. Source: Agricultural Census 1996 and 2006.

Table 2.3: Correlations between changes in land by activity: 1996-2006

Panel A. Average change in land used (ha)

	Land reaped with soy				
	increased (1)	decreased (2)	no soy (3)	Difference (1) - (2)	p-value
Soy	6’912	-1’295		8’206	0.00***
Seasonal crops, except soy (area reaped)	2’188	335	703	1’853	0.00***
Permanent crops	1’054	919	629	135	0.48
Cattle ranching	-8’326	-3’882	760	-4’443	0.02**
Forest	-157	-8’911	-3’165	8’754	0.00***
Unusable land	-2’516	-1’882	-727	-633.9367	0.03**
Observations	1’069	848	3’647		

Panel B. Total change in land use (millions ha)

	Land reaped with soy		
	increased (1)	decreased (2)	no soy (3)
Soy	7.39	-1.10	
Seasonal crops, except soy (area reaped)	2.34	0.28	2.56
Permanent crops	1.11	0.77	2.25
Cattle ranching	-8.90	-3.29	2.77
Forest	-0.17	-7.56	-11.50
Unusable land	-2.69	-1.59	-2.64

Note: The table reports the change in land of farms by their principal activity. Panel A reports the average change across all municipalities that experienced an increase in the land reaped with soybean (column 1) a decrease (column 2) or were not producing soy neither in 1996 nor in 2006 (column 3). The last 2 columns report the difference in the change in land across the municipalities in the first 2 groups and the p-value from a test of equality of means. Panel B reports the total change for the three groups of municipalities. The first row in both panels reports the change in land reaped with soybean, the second line reports the area reaped with seasonal crops except soybean. All data are sourced from the IBGE Sidra repository. Total area reaped comes from table 501 in 1996 and table 1823 for 2006. Land in farm by principal activity in 1996 comes from table 491 and for 2006 from table 797. The definition of “principal activity” of the farm changed somehow between 1996 and 2006. In 1996 higher specialization was required for farms to be classified under one of the categories reported, and those that did not produce at least 2/3 of the value within a single category were classified under the “mixed activity” category. In 2006 farms were classified according to the activity that accounted for the simple majority of production and no “mixed activity” category existed. Source: Agricultural census 1996 and 2006.

Table 2.4: Correlation between changes in area reaped with seasonal crops: 1996-2006

Panel A. Average change in land reaped (ha)

	Land reaped with soy				
	increased (1)	decreased (2)	no soy (3)	Difference (1) - (2)	*p-value*
Soy	6’912	-1’295		8’206	0.00***
Maize	1’272	-483	97	1’754	0.00***
Wheat	517	-30	0	547	0.00***
Tobacco	194	34	15	160	0.00***
Rice	-265	-283	-17	18	0.87
Cotton	180	182	-44	-2	0.99
Beans	-120	27	91	-148	0.20
Cassava	102	283	350	-180	0.06*
Sugar	356	602	127	-246	0.19
Other seasonal crops	592	916	576	-325	0.24
Observations	1’069	848	3’647		

Panel B. Total change in land reaped (millions ha)

	Land reaped with soy			
	increased (1)	decreased (2)	no soy (3)	Total
Soy	7.39	-1.10		6.29
Maize	1.35	-0.40	0.34	1.29
Wheat	0.48	-0.02	0.00	0.46
Tobacco	0.19	0.03	0.05	0.27
Rice	-0.24	-0.20	-0.05	-0.49
Cotton	0.17	0.14	-0.14	0.17
Beans	-0.13	0.02	0.33	0.23
Cassava	0.10	0.22	1.17	1.49
Sugar	0.32	0.44	0.38	1.15
Other seasonal crops	0.63	0.78	2.10	3.51

Note: The table reports the change in area reaped with the main seasonal crops in Brazil. Panel A reports the average change across all municipalities that experienced an increase in the land reaped with soybean (column 1) a decrease (column 2) or were not producing soy neither in 1996 nor in 2006 (column 3). The last 2 columns report the difference in the change in area reaped across the municipalities in the first 2 groups and the p-value from a test of equality of means. Panel B reports the total change for the three groups of municipalities. Data are sourced from the IBGE Sidra repository. Total area reaped comes from table 501 in 1996 and table 1823 for 2006. Source: Agricultural census 1996 and 2006.

Table 2.5: OLS Regressions: Changes in Agricultural Production on Changes of Area Reaped with Soy and Maize.

	Δ Value per Worker	Δ Labor Intensity	Δ % Agri Workers
Panel A			
Δ % Soy Area	3.303***	-0.630***	-0.0734**
	(0.281)	(0.210)	(0.0358)
N	3,841	3,838	3,921
Panel B			
Δ % Maize Area	2.907***	0.679***	0.0204
	(0.209)	(0.160)	(0.0252)
N	4,062	4,053	4,112

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.6: OLS Regressions: Changes in Industrial Labor Market on Changes of Area Reaped with Soy and Maize.

	Δ Total Employment	Δ Number of Plants	Δ Plant Size	Δ Wage
Panel A				
Δ % Soy Area	1.087***	0.547**	0.662**	0.066
	(0.379)	(0.268)	(0.334)	(0.155)
N	2,048	2,063	2,048	2,048
Panel B				
Δ % Maize Area	0.117	0.132	0.046	0.250**
	(0.260)	(0.175)	(0.188)	(0.122)
N	2,172	2,187	2,172	2,172

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7: First stage: changes in area reaped with soy and maize on potential yield shocks.

	Δ % Soy Area		Δ % Maize Area	
ΔA^{soy}	0.012***	0.025***		0.002
	(0.001)	(0.002)		(0.003)
ΔA^{maize}		-0.003***	0.003***	0.004***
		(0.001)	(0.001)	(0.001)
A^{sugar}		-0.007***		-0.006***
		(0.001)		(0.001)
N	3,921	3,921	4,112	4,112
R-squared	0.054	0.074	0.006	0.013
F-test for joint significance	141.28	52.77	23.95	15.51

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Reduced form: changes in agricultural production on potential yield shocks.

	Δ Value per Worker	Δ Labor Intensity	Δ % Agri Workers
ΔA^{soy}	0.143***	-0.088**	-0.027***
	(0.044)	(0.035)	(0.005)
ΔA^{maize}	-0.025	0.049***	0.010***
	(0.016)	(0.013)	(0.002)
A^{sugar}	-0.036*	-0.027	0.002
	(0.021)	(0.017)	(0.002)
N	4,150	4,146	4,254
R-squared	0.003	0.007	0.013

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9: Reduced form results: changes in industrial labor market on potential yield shocks.

	Total Employment	Number of Plants	Plant Size	Wage
A^{soy}	0.087***	-0.006	0.094***	-0.046***
	(0.029)	(0.016)	(0.025)	(0.013)
A^{maize}	-0.021	-0.001	-0.020	0.019***
	(0.015)	(0.008)	(0.013)	(0.006)
$P^z A^z$ controls	Yes	Yes	Yes	Yes
AMC & year FE	Yes	Yes	Yes	Yes
N	25,258	25,517	25,258	25,235
R-squared	0.922	0.948	0.809	0.777

Standard errors clustered at AMC level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

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Chapter 3

Austerity and Anarchy: Budget Cuts and Social Unrest in Europe, 1919-2008

Joint with Joachim Voth, UPF

3.1 Introduction

From the French Revolution to the Arab Spring, social unrest has led to key turning points in history. Marx saw it as the driving force of the transition of societies from feudalism to capitalism and, eventually, communism. The power of unrest as a catalyst for change manifests itself explicitly in regime changes, or it operates through expectations: The extension of the franchise in Western societies can be seen as a bid to reduce the threat of revolution (Acemoglu and Robinson, 2000).[1] What leads to social unrest is less clear. Economic shocks are one possible contributing factor: The demise of the Weimar Republic at the height of the Great Depression is a prominent example of economic hardship translating into political instability and social unrest (Bracher, 1978).[2]

In this paper, we examine one of the possible determinants of unrest and violent protests - fiscal policy. How do budget measures affect the level of social instability? The extent to which societies fracture and become unstable in response to drastic retrenchment in the government budget is a major concern for policymakers tackling large budget deficits: To name but two examples, in both

[1]In a related exercise, Boix (2003) models the incentives of the populace to resort to violence as a function of the wealth distribution and economic development.

[2]The French Revolution has also been interpreted in these terms (Soboul, 1974; Doyle, 2001). The view is controversial (Hunt, 2004; Cobban, 1964).

Argentina in 2001 and in Greece in 2010-11, austerity measures often coincide with waves of protest and civil unrest. Economic conditions can deteriorate further and faster if political and social chaos follows attempts to reign in spending.[3] Consequently, sustainable debt levels for countries that are prone to unrest may be lower than they otherwise would be.

Using a panel dataset for 25 European countries covering the period 1919 to 2008, we show a clear link between the magnitude of expenditure cut-backs and increases in social unrest. With every additional percentage point of GDP in spending cuts, the risk of unrest increases. This finding is surprising in the light of the recent political economy literature on expenditure cuts. Alesina et al. (1998, 2010) show that governments typically do not lose votes in elections after they implement austerity programs (and relatedly, there is no electoral benefit for free-spending parties (Brender and Drazen, 2008). The fact that austerity may lead to unrest - even without penalties at the ballot box - may help to explain why governments are typically reluctant to cut spending, even if this might be economically beneficial.[4]

Europe over the last century is an an ideal testing ground for our hypthesis. The continent went from high levels of instability in the first half of the 20th century to relatively low ones in the second, and from frequently troubled economic conditions to prosperity. It thus provides a rich laboratory of changing economic, social and political conditions. In terms of outcome variables, we focus on riots, demonstrations, political assassinations, government crises, and attempted revolutions. These span the full range of forms of unrest, from relatively minor disturbances to armed attempts to overthrow the established political order. We compile a new index that summarizes these variables, and then ask - for every percentage cut in government spending, how much more instability should we expect?

As a first pass at the data, Figure 3.1 shows the relationship between fiscal adjustment episodes and the number of incidents indicating instability (CHAOS). CHAOS is the sum of demonstrations, riots, strikes, assassinations, and attempted revolutions in a single year in each country. The first set of five bars show the frequencies conditional on the size of budget cuts. When expenditure is increasing, the average country-year unit of observation in our data registers less than 1.4 events. For moderate budget cuts, there is no increase in the frequency of unrest. However, as austerity intensifies, the frequency of disturbances rises. Above 3%, the frequency of events rises markedly. Once austerity measures involve expenditure reductions by 5% or more, there are around 4 events per year and country - more than twice as many as in times of expenditure increases or mild cuts.

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[3] Bloom (2009) documents how uncertainty shocks can reduce output.

[4] Alesina and Ardagna (2010); Alesina et al. (2002); Guajardo et al. (2011). An early example in the literature is Giavazzi and Pagano (1990). The finding is controversial – cf. Devries et al. (2011).

The same relationship can be observed in each of the four main subcategories of CHAOS. The frequency of riots, demonstrations, political assassinations, and general strikes is greatest for the biggest cuts. While small cuts have few effects, beyond a certain threshold, unrest explodes.

The strength of the link between austerity measures and unrest is our first important finding. Is the link causal? Other factors, such as generally depressed economic conditions, have the potential to drive up unrest and the need for cut-backs simultaneously. Controlling for economic growth does not change our results. This suggests that we capture more than a general association between economic downturns and unrest. Also, controlling for ethnic fragmentation and unemployment does not change our result. Findings are robust to adding other possible tools available to governments to finance public deficit as controls: inflation and public debt.

To demonstrate that causality runs from cut-backs to unrest, we use recently compiled data on changes in the government budget that follow directly from discretionary policy decisions (Devries et al., 2011).[5]. In addition, we draw on the work by Alesina et al. (2012) that decomposes changes in the government budget that follow directly from discretionary policy decisions into their anticipated and unanticipated components. For both types of additional evidence, we find clear indications that the link runs from budget cuts to unrest.

Our findings are robust to a wide range of alternative specifications and further tests. Different measures of unrest do not affect our conclusions. We examine if the link between austerity and unrest changes as countries institutions improve. For most values of the Polity2 score of institutional quality, results are broadly constant. However, countries with very high levels of constraints on the executive show a somewhat weaker degree of association. Further, we examine if the spread of mass media changes the probability of unrest. This is not the case. If anything, higher levels of media availability and a more developed telecommunications infrastructure reduce the strength of the mapping from budget cuts to instability. We also test which part of the distribution of unrest is responsible for our results, using quantile regressions: The higher the level of unrest, the bigger the relative impact of additional budget cuts. Finally, we test for asymmetries in the relationship between unrest and austerity. Reductions increase instability, but spending increases do not cut the number of incidents to the same extent.

Earlier papers have typically focussed on case studies, on relatively short time-periods, or on subsets of the developing world.[6] DiPasquale and Glaeser (1998) analyzed race riots in the US in the 1960s and early 1990s.[7] They find that ethnic

[5]The recent work by Devries et al. (2011) follows in the footsteps of Romer and Romer (1989, 2010). For a critique of the application to US fiscal policy after 1945, cf. Ramey (2011)

[6]Theoretical work on unrest includes Kuran (1989), Tullock (1971), and Grossman (1991).

[7]The authors also analyse international data for the period 1960-85. They find that higher GDP

heterogeneity and unemployment rates are a strong predictors of riots, and that poverty is relatively unimportant. Bohlken and Sergenti (2010) argue that riot probabilities in India over the period 1982-95 dropped sharply when growth rates accelerated. Field et al. (2008) examine religious violence in Ahmedabad, India, in 2002, and argue that tenancy rights created neighborhoods that were more integrated, and hence more prone to violence.

Comparative work by Alesina and Perotti (1996) suggests that inequality leads to more unrest, and this instability can adversely affect investment. Work on 23 African countries during the 1980s found that budget cuts had typically no effect on political and social stability. IMF interventions, on the other hand, often led to more frequent disturbances (Morrison et al., 1994). Paldam (1993) examines current account crises in seven South American countries during the period 1981-90, using high-frequency (weekly) data. He finds that the run-up to new austerity measures is associated with higher levels of unrest, but that actual implementation is followed by fewer disturbances. Similarly, Haggard et al. (1995) find that IMF interventions and monetary contractions in developing countries led to greater instability. Analyzing the period 1937-1995, Voth (2011) explores related issues for the case of Latin America. He finds that austerity and unrest are tightly linked in a majority of cases.[8] Martin and Gabay (2012) analyze fiscal protests in a set of European countries after 1980, and argue that incompatibilities between tax and spending policies are responsible for popular opposition. Andronikidou and Kovras (2012) focus on cultural determinants of differential inclinations to protest.

Relative to these papers, we make several contributions. To the best of our knowledge, ours is the first systematic analysis of how budget cuts affect the level of social instability and unrest in a broad cross-section of developed countries, over a long period. We also examine the exact causes of instability in a subset of the data where more detailed information is available, and find a strong link with austerity. This also allows us to perform placebo tests. Using detailed evidence on the motivations for government expenditure changes, we strengthen the causal link still further. Finally, we examine the link between instability and media penetration.

Other related literature includes work on the political economy of fiscal consolidation and its economic effects, as well as the logic of conflict between politicians and society. Recent work has emphasised that cutting entitlement programs tends to produce persistent improvements in the budget balance, while revenue measures and capital expenditure cuts have only temporary effects (Alesina and Perotti, 1995). The timing of stabilization measures has been explored in war-

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reduces the incidence of riots, while urbanization rates are positively associated with them.

[8]There is also a rich literature on economic instability and political radicalization, which we cannot survey here. For a recent analysis of the interwar period, cf. De Bromhead et al. (2012).

of-attrition models, which view relative bargaining strength of different groups as crucial (Alesina and Drazen, 1991). A rich literature has examined the macroeconomic effects of budget cuts. Giavazzi and Pagano (1990) and Alesina et al. (2002) find that cuts can be expansionary. Amongst the reasons suggested for this finding are a reduction in uncertainty about the course future spending (Blanchard, 1990a), and a positive wealth shock as a result of lower taxes in the future (Bertola and Drazen, 1993).[9] Recently, work by the IMF has suggested that austerity measures may be less expansionary than previously thought; they may well have the standard negative Keynesian effects as a result of lower demand (IMF, 2010; Guajardo et al., 2011).

We also relate to the rich literature on conflict between the government and society at large. Persson et al. (1997) argue that the separation of powers in modern constitutions is deliberately designed to engineer conflict, preventing the abuse of power. It could be argued that ”direct action” in the form of demonstrations and extra-legal means such as riots act as an extreme form of ”checks and balances” on the executive. Other related work that also treats the issue of conflict between government and governed includes the work of Buchanan and Tullock (1962), Ferejohn (1986), Shleifer and Vishny (2002).

The paper is structured as follows: Section 2 presents our data, and section 3 summarises our main results. Robustness checks and extensions are discussed in section 4; section 5 concludes.

3.2 Data

In this section, we briefly describe our data and summarize its main features. Five main indicators of domestic conflict in the long-term data form the main basis of this study - general strikes, riots, anti-government demonstrations, political assassinations, and attempted revolutions. These data are part of the Cross National Time Series Dataset, compiled by Banks (2010) and his collaborators. The main source of data on unrest episodes are the reports of the *The New York Times*, while the variables' definition is adopted from Rummel and Tanter (1971). In addition, we use data on GDP, government revenue, expenditure, and the budget balance from a variety of sources.[10] The dataset contains information on 25 European countries and covers the years from 1919 to 2008.[11]

[9]Once the response of labor supply and capital formation is fully taken into account, these effects may not go through (Baxter and King, 1993).

[10]See the Data Appendix for a detailed description of the sources of each variable

[11]The countries included are: Austria, Belgium, Bulgaria, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Netherlands, Norway, Poland, Portugal, Russian Federation, Slovak Republic, Spain, Sweden, Switzerland, United Kingdom,

Table 3.1 gives an overview of the main variables and their descriptive statistic for the long-term data. The average number of assassinations and general strikes was quite low in our sample, with less than 2 events in each decade. There were more riots and more demonstrations, 5-6 per decade. Attempted revolutions are quite rare, but some countries registered high levels of instability. The record in our sample is Germany in 1923, with 5 recorded attempts at overthrow (with communist insurgencies in Saxony and Thuringia, the Hitler Beer Hall Putsch, and a separatist movement in the Rhineland). Assassinations and riots similarly show a broad range of observed values. Almost a century of data includes some extreme observations.[12] For example, Austria and Germany saw major output declines in 1945 and 1946, respectively. The biggest reduction in governments spending in our data occurred in Poland, in 1982; the second-largest, in Finland, in 1947. The start of wars is often associated with big increases in expenditure. The record-holder in our dataset is Hungary in 1940, with an increase of over 30 percent.

To obtain a single measure of instability, we calculate CHAOS by taking the sum of the number of assassinations, demonstrations, riots, general strikes, and attempted revolutions. While a crude way of aggregating indicators, it has high predictive power.[13] In the robustness section, we show that alternative methods of reducing data complexity such as principal components analysis do not change our results.

For CHAOS, the average country in our sample registers 1.6 incidents per year. Instability was not constant over time. The maximum is higher - Italy in 1947 saw a total of 38 incidents, including 7 general strikes, 19 riots, and 9 anti-government demonstrations. Figure 2 gives an overview of the evolution over time, plotting the average of CHAOS as well as the maximum number of incidents observed. While there is no clear-cut pattern over time, some features emerge. The interwar period showed relatively high levels of unrest, with an average of 1.8 incidents per year, compared to 1.3 in the post-war period. The immediate post-World War II period, and the period form 1968 to 1994 also show unusually high levels of unrest. Comparatively speaking, the years since 1994 have been unusually tranquil (average CHAOS = 0.68).

In compiling information on expenditure and the budget balance data, we need to trade off the accuracy of information against availability over a long time span. We rely on standard data sources on the central government revenue and expenditure relative to GDP from Mitchell (2007), augmented by data from the OECD

Yugoslavia.

[12]In the data appendix we explain in detail how we deal with outliers.

[13]One alternative is the weighted conflict index (wci), as compiled by Banks (2010). It gives fixed weights determined to different forms of unrest: Demonstrations have a weight of 200, while political assassinations have a weight of 24.

when available (mostly from 1970 onwards). Expenditure changes will serve as the main explanatory variable. Figure 3.3 graphs changes in expenditure/GDP from one year to the next. The distribution is almost symmetric around the mean, with similar numbers of country-years witnessing expenditure increases and declines (787 vs 662). In an average year and country over the period, central government expenditure relative to GDP rose by 0.3%. The vast majority of observations is in the range of changes by less than 3%, with a few outliers in the tails of the distribution (typically driven by the beginning and end of wars).

When focusing on the sub-period from 1970 to 2007, we use cyclically adjusted fiscal data constructed by Alesina and Ardagna (2010).[14] In particular we employ their data on primary expenditure, government investment, total revenues and the primary budget balance. In this way we correct for both changes in interest payments and the immediate effect of the economic cycle, which drives both expenditure and revenue without any additional policy decision being taken. For a subsample of the data (1978-2008, 17 countries), we also use data by Devries et al. (2011). These authors examine in detail the policy changes that led to changes in a country's fiscal stance. Only expenditure cuts or revenue increases motivated by a decision to press ahead with fiscal consolidation are considered.[15] Overall, Devries et al. (2011) find 173 periods of fiscal policy adjustment.

As a first pass at the data, we repeat the exercise in Figure 3.1 for output growth (Figure 3.4). We subdivide the sample into terciles, and examine how much the incidence of various indicators of unrest declines as growth accelerates. For the summary indicator (CHAOS), there are a little more than 2 incidents when growth is in the lowest tercile. This falls to 1.3-1.5 incidents as growth accelerates. There is also a clear pattern of decline for demonstrations and for assassinations. In the case of riots, the differences are smaller overall, whereas in the case of general strikes, there seems to be little pattern at all. Based on a first, visual inspection of the data, it seems that the link between budget cuts and unrest is clearer than the one with growth.

Next, we examine the correlation structure of our data in Table 3.2. Assassinations, general strikes, riots, revolutions and demonstrations are all positively and significantly correlated with each other. This supports our assumption that they reflect a broader underlying pattern of social instability and unrest. CHAOS is also positively correlated with the weighted conflict index (wci). Finally, Table 3.2 suggests that higher levels of expenditure and faster growth are associated with less unrest. The simple correlation of CHAOS with changes in the budget balance is positive and significant. Higher taxes and lower expenditure are associated with more unrest, but the relationship is not significant.

[14] Alesina and Ardagna use the method of Blanchard (1990b)

[15] The approach is similar to the "narrative approach" pioneered by Romer and Romer (1989)

In the case of output changes, the coefficient is negative, but insignificant (Table 3.2). The simple correlations suggest that these co-movements do not extend to all indicators of unrest equally: riots, revolutions, and demonstrations decline as expenditure rises, but assassinations and strikes seem - at a first pass - uncorrelated. Similarly, output growth seems to correlate negatively with assassinations, riots, revolutions, and demonstrations, but not with strikes. Next, we examine the connection between budget position, expenditure, and unrest more systematically.

3.3 Results

The graphical evidence in Figures 3.1 and 3.4 suggests a link between "hard times" - low growth and budget cut-backs - and unrest. Next, we examine if there is a systematic relationship between budget measures and social instability. We present evidence from the last century of budget cuts and turmoil, and then show that these hold if we use a more narrow but cleanly defined measure of austerity. The dynamic nature of responses to spending reductions is also not responsible for our results. Next, we look at interaction effects with institutional factors. Do countries with more accountable governments weather the storms of austerity better? We find an inverse relationship between government spending and unrest at all levels of institutional quality, but the link appears stronger the more authoritarian a regime is. Finally, we examine the impact of media penetration.

3.3.1 Baseline Results

We estimate simple panel regressions of the type:

$$I_{it} = \alpha + \alpha_i + \alpha_t + \beta \Delta B_{it} + \Gamma X'_{it} + \varepsilon_{it} \quad (3.1)$$

where I_{it} denotes the level of instability in country i at time t, ΔB_{it} is a measure of the change in the budget position, α_i is a country-specific intercept, α_t is a time-specific dummy, and X_{it} is a vector of control variables.

We use CHAOS as the dependent variable in our baseline specification, and test the robustness of findings to alternative specifications later. CHAOS is a count variable. To avoid issues of overdispersion, we estimate a negative binomial model clustering standard errors at the country level.[16] This takes the highly skewed distribution of the number of incidents into account. Table 3.3 gives the main results. We find that expenditure cuts are strongly and significantly associated with the number of unrest incidents in any one country and year (column

[16]In the robustness section, we show that Poisson Quasi-Maximum Likelihood estimation yield nearly identical results.

1). The same is true of overall improvements in the budget position (column 3). We find a positive effect of tax rises, but the coefficient is small and the estimated standard error is large (column 2).

Under OLS with fixed effects and year-dummies, we find the same pattern: expenditure increases reduce instability in a powerful way (column 4). A one standard-deviation cut in expenditure raises the number of incidents (CHAOS) by 0.4 per year and country. Tax increases have a positive sign, but the effect is not significant at standard levels (column 5). It is also small - a one standard deviation rise in the tax/GDP ratio increases unrest by less than 0.01 events. Overall, we find that improvements in the budget balance raise the level of unrest (column 6).

To illustrate the magnitude of effects, we present the predicted number of incidents per year conditional on different values of expenditure changes and GDP growth. Table 3.4 performs such an analysis. We see that for any given level of GDP growth, the implied number of unrest episodes rises rapidly as expenditure is cut. At negative growth of 2%, for example, going from expenditure cuts of 1% to 10% implies almost a doubling of the frequency of unrest. Compared to this, the effects of poor economic performance are relatively mild. Going from negative growth of 2% to a collapse by 10% only increases the predicted number of incidents by 0.18 p.a. [holding expenditure cuts constant at 1%]. The higher the level of expenditure cuts, the higher the effect of low growth. At expenditure cuts of 10%, the same change in growth rates produces 0.29 extra incidents.

3.3.2 Full Set of Controls, 1970-2007

The downside of using the full sample, for the years 1919-2008, is that many important covariates are not available. For example, consistent and comparable data on unemployment rates is rare for earlier periods. To address this issue, we estimate the same regressions but with a full set of controls, for a subset of 16 countries over the period 1970-2007.

We add changes in output per head, unemployment, ethnic fractionalization, polity scores, the debt/GDP level, as well as changes in inflation and the primary budget balance to our set of explanatory variables. For this subset of the sample, our fiscal variables are also more cleanly defined. We can use primary expenditure - net of debt servicing cost - which is conceptually superior. Some of the variation in primary expenditure and total revenue could, however, also reflect changes that are driven by automatic stabilizers. To take this into account we cyclically adjust all fiscal variables using the Blanchard methodology (as in Alesina and Ardagna, 2010), in an attempt to clean as much as possible the change in fiscal variables from their cyclical component.

As Table 3.5 shows, the link between expenditure cuts and unrest remains unaffected, as is the predictive power of the budget position overall. In contrast to the results with the full sample, tax increases appear to be also strongly associated with instability. We will analyze this issue further when using information on the cause of each unrest event. Finally, in column 4, we examine if adjusting the budget position for debt service (using the primary surplus as the explanatory variable) changes our findings; it does not. In addition, we find positive coefficients on ethnic fractionalization and unemployment, which is in line with the results by DiPasquale and Glaeser (1998). Inflation is strongly and negatively associated with unrest, giving credence to theories that see it as a way to preserve "social peace" in economies under pressure (Feldman, 1997).

3.3.3 Results with Exogenous Retrenchment Measures

The obvious challenge in interpreting equation (3.1) is the potential for omitted variable problems. It is possible that the economic cycle is simultaneously driving both unrest and the need for budget cuts. Above, we already control for GDP growth rates, and our main finding remains unaffected. However, the omitted variable problem would only be solved if we measured the effect of economic output on instability perfectly. Since this is unlikely, we present another type of analysis. We use a recently-developed database on the motivations behind expenditure changes to separate cyclically-driven austerity from deliberately-targeted measures that aim at restoring sustainability over the long run.

In Table 3.6 we use the IMF measure of policy-action based changes in expenditure, revenues and the budget balance to strengthen the argument for a causal link. Here, the source of variation is identified as cleanly as possible: Only changes in the budget balance motivated by concern about the long-run fiscal position are used. The downside is that we only have information on episodes of fiscal retrenchment (deficit reductions) and not on periods of fiscal expansion. Using this action-based measure as explanatory variables for unrest produces a negative, large and significant coefficient for change in expenditure, a positive and non significant coefficient for change in revenues and a positive and significant coefficient for the budget balance.

In columns 4 to 6 we use the Alesina et al. (2012) decomposition of anticipated and unanticipated changes in fiscal variables to refine the analysis further. For each year, we use the unanticipated share of expenditure, revenues and budget position as an additional explanatory variable. As the regression results show, unrest rises the most when spending cuts are unanticipated – rioting and other forms of protest occur in direct response to new policy measures. The closer we get to measuring the impact of policy measures, the stronger the link between austerity and unrest becomes. This strengthens the case for a causal link between unrest and austerity.

3.3.4 Accounting for Dynamics

So far, we have ignored the time-series dynamics in our data. Unrest shows positive and significant serial correlation for up to 2 years. In addition, we have abstracted from anticipation effects – events on the street may be caused as much by anticipated expenditure cuts, for example, as by those actually taking place. In Table 3.7, we show results for a number of dynamic specifications. In column 4, we examine the effect of expenditure cuts from t-2 (two years previous) to t+2. We find that there is a relatively small (and insignificant) effect of expenditure cuts in the previous year. In contrast, the effect of current-year changes remains strong and highly significant. Austerity in the next year also has predictive power for unrest at time t – unrest is just as likely to erupt in response to current as to future austerity. In column 5, we examine the effect of including lagged social instability. While highly significant, the estimated coefficient for the other variables are largely unaffected. The same conclusion emerges from the final column (6); future positive GDP growth is associated with less growth, but the effect is not well-estimated.

3.3.5 Institutions and the Sensitivity of Unrest to Austerity

Greater constraints on the executive and more democracy should on the one hand reduce social conflict, producing less of a need to take to the streets.[17] On the other hand, there will be less repression by the authorities in more democratic countries with higher Polity scores, making it easier for protests to be organized. Which effect dominates is not clear ex ante. Table 3.8 demonstrates that in countries with better institutions, the responsiveness of unrest to budget cuts is generally lower. Where constraints on the executive are minimal, the coefficient on expenditure changes is strongly negative - more spending buys a lot of social peace, and expenditure cuts powerfully fan the flames of discontent. As we limit the sample to ever more democratic countries, the size of the coefficient declines slightly. For mature democracies with a full set of civil rights, the coefficient is still negative, but much smaller and no longer significant. The link with growth is also relatively stable. Higher output generally dents the tendency to riot, demonstrate, assassinate, or strike in countries with low institutional quality. The only exception is in full democracies, where the connection is weaker but still negative, and in the most authoritarian regimes, where there is no clear link.

[17]For reflections on the relationship between institutions and social conflict, cf. Acemoglu (2003).

3.3.6 Media Penetration

Does greater media penetration increase or reduce unrest? Events in the Arab world in 2010 and early 2011 have led many observers to argue that greater media availability tightens the link between discontent and unrest. Data on media penetration is available in the Banks dataset.Three indicators are relevant - phone penetrations per capita, radio and television take-up.[18] Radio and television are unidirectional forms of media, allowing typically government-controlled messages to be broadcast to the population. If anything, they should make it easier for authorities to reduce unrest. Phones, on the other hand, allow peer-to-peer communication. All else equal, the expected effect is that they facilitate organized protest. To analyze the data, and to avoid confusing results with the growing availability of broadcasting and telecommunications over time, we rank penetration rate in our sample in each year. We do so separately for each category, and then sum the ranks for each country-year. This gives a rank ordering of media penetration in country x in year y. We then divide the sample at the median. Table 3.9, col. (1) and (2) presents the results of estimating standard regressions for these two subsamples. We find that below-average media penetration is associated with a strong effect of expenditure cuts on unrest. Above the median, the effect disappears. In col. (3)-(6), we differentiate between uni-directional information media (info-media) and peer-to-peer telecommunications (peer-media). The attenuation of the effect of expenditure changes is milder for peer-media, and strong for info-media. For both types, the effect of economic conditions is always important above the median for media penetration, but below (in the case of peer media) the effect is small and poorly estimated. These results do not suggest that countries which, at any one point of time, have greater availability of mass media (relative to their neighbors) experience generally higher level of unrest.[19]

3.4 Robustness and Extensions

In this section, we examine the sensitivity of our results. We test if a more complete set of controls - available for a part of our original sample - can overturn our result, if our main finding holds for sub-periods, and which individual components of unrest are particularly affected by austerity. We also examine if the effect is driven by outliers, whether positive or negative changes in expenditure matter more for the effect on unrest, and whether the effect is constant in all parts of the

[18]We disregard data on telegrams since they are unavailable after 1980.

[19]The obvious alternative is to condition on the absolute level of, say, phone penetration. Most of the variation in phone penetration, however, simply reflects GDP growth and the declining cost of telephones relative to all other goods; no clear pattern emerges.

distribution of the dependent variable. Finally, we test if greater unrest leads to sharper reversals in fiscal policies.

3.4.1 Results by Subcomponent of CHAOS

Which component of CHAOS is most affected by austerity measures? In Table 3.10 , we use the same specification as in Table 3.3 using a negative binomial model, looking at the effect of expenditure cuts on each of the components of our aggregate indicator of instability in the full sample: general strikes, demonstrations, riots, and assassinations, and attempted revolutions. Of the five outcome variables, all show the expected sign, and three of them - demonstrations, riots and attempted revolutions - are statistically significant. The variables that do not show a large, significant coefficient are assassinations and general strikes. On average, years with expenditure increases saw fewer general strikes, but there are numerous general strikes that are not an immediate reaction to economic conditions and budget measures (such as, for example, the 1926 general strike in Britain). For the other variables, the coefficients are large, indicating that austerity measures coincide with significant increases in demonstrations, attempted revolutions, riots, and assassinations. In all specifications, the effect of GDP growth on unrest is negative. In contrast to the results for expenditure changes, the effect is not tightly estimated.

3.4.2 Results by Subperiods

Table 3.11 takes this analysis one step further, by breaking the period 1919-2008 into four sub-periods. We distinguish the interwar years from the period of post-World War II reconstruction, the period of slowing growth in the 1970s and 1980s, as well as the years after the fall of the Berlin Wall after 1989. For the first two sub-periods, we use expenditure relative to GDP as the explanatory variable; thereafter, as a result of greater data availability, it is primary (non-debt) expenditure. In each subperiod, we find the same pattern as in the long-run data. The only exception is the penultimate period, 1970-1989, when the effect of expenditure changes is not as tightly estimated as it is in the other sub-periods. The effect of changes in budget expenditure on unrest is strongest in the period after the fall of the wall, when the estimated coefficient is twice as large as in the earlier periods. The effect of GDP growth is negative except during the post-war boom: More growth was associated with more unrest. While it is difficult to test for the causes of this reversal exactly, it seems plausible that high rates of output growth may have encouraged worker militancy. At a time when many countries reached full employment, this effect seems to have dominated. The normal pattern of GDP growth reducing unrest reasserts itself after 1970. From 1970 onwards, we can control for additional variables, and do so. Inflation seems to have reduced unrest in the 1970s, in line with some of the political economy literature on the origins of high inflation (Samuelson and Solow, 1960). The fall of the Berlin wall saw the spread of Western-style democracy eastwards. The overall connection between austerity and social instability is the same in the expanded sample, and it is highly significant.

3.4.3 Alternative Measure of Unrest

How much does our main finding depend on the way in which we aggregate unrest? CHAOS is the simple sum of incidents. Instead, we can use the weighted conflict index, as compiled by Banks (2010). It encompasses a larger set of domestic conflicts including, in addition to the components of CHAOS, purges, major government crisis and guerrilla warfare. It also assigns different, fixed weights to each individual component. The correlation coefficient of the variable with CHAOS is 0.75, significant at the 1% level. Another alternative is to use the first principal component of the five indicators that go into CHAOS. They all enter with a positive weighting. The first principal component explains 0.42 of the overall variance. The correlation coefficient with CHAOS is 0.98.

In Table 3.12, we use both wci and the first principal as dependent variables. Since the dependent variable is no longer a count variable, we use panel OLS, and obtain large and mostly significant coefficients for expenditure changes and the budget position. For the principal component, expenditure increases cut unrest significantly; for the wci, the standard error is slightly above the typical cut-off for significance. As before, there is no clear pattern for tax changes. The results are largely identical in terms of magnitude and significance with the baseline results in Table 3.3.[20] We conclude that the way in which we measure unrest does not affect our main finding.

3.4.4 Outliers and the Magnitude of Effects in Different Parts of the Distribution

An additional factor that can be questioned involves the use of a count variable for unrest in the baseline results. The variable CHAOS is designed to capture the intensity of unrest, but it may be that it is influenced by a number of outliers with a high count of incidents. For example, Italy in 1947 saw a decline in expenditure (by 0.6%) and also the highest frequency of unrest in our sample, with 38 incidents

[20]A decrease of 1 GDP point in public expenditure is associated with an increase of 0.04 standard deviations in CHAOS, 0.03 standard-deviations in first principal component of chaos and 0.02 standard-deviations in wci.

in a single year. Extreme observations such as this one might make it easier to find significant effects. To examine this potential issue, we transform CHAOS into a simple dichotomous variable, with unrest coded as equal to unity if there are one or more incidents in a country in a single year. In Table 3.13, we re-estimate the baseline regression with panel logit using country- and year-fixed effects. We find the same results as before - expenditure cuts wreak havoc, tax increases do so only to a small extent and insignificantly. Overall, the budget balance matters for predicting unrest. We conclude that the role of outliers is not decisive in underpinning the relationship we established in baseline results.

3.4.5 Symmetry of Expenditure Increases and Cuts

Which part of the variation in the explanatory variables is responsible for the link between austerity and unrest? Do increases in expenditure do as much to reduce unrest as cuts increase them? In Table 3.14, we examine this issue. Column (1) shows the results for expenditure increases. The coefficient is negative, but not large, and not significant. In contrast, if expenditure changes are negative, they matter a great deal for unrest, driving up CHAOS by 0.24 incidents for each standard deviation of expenditure cuts. Next, we repeat the exercise for output changes. Increases in output do much to cut unrest (col. 3), with a one standard deviation increase in output (3.77%) reducing CHAOS by 0.2 incidents on average. In contrast, declines do not set off major disruptions to the same degree (but the sample is relatively small). Overall, the results in table 12 confirm that the relevant identifying variation for expenditure changes comes from cuts; for output changes, it comes from positive growth, not recessions.

3.4.6 Fiscal Reversals

Does unrest lead to fiscal reversals? Are the politics of the "street" powerful enough to change the minds of politicians implementing austerity programs? Here, we examine the time path of fiscal adjustments, comparing periods of austerity with and without unrest. Figure 3.5 shows the change in expenditure relative to GDP from year 1 (when the cuts are implemented and unrest either occurs or does not) to year 4. We compare two groups – countries with austerity programs with and without unrest in year 1. Unsurprisingly given our earlier findings, consolidation episodes associated with unrest on average see more severe budget cuts. In both groups, budget cuts are scaled back after year 1, and spending increases again in year 2. However, the reversal is much more marked during episodes where austerity coincided with unrest. The change in budget position amounts to an almost 3 percentage point swing, from cuts of over 2 percent to increases of more than 0.75 percent. The reversal in countries without unrest is only half as large. In years 3 and 4, expenditure continues to expand in countries that saw unrest, but at a slower pace. In contrast, countries without unrest grow their spending in a more gradual fashion, reaching a higher rate of expansion by year 4. In this sense, our results suggest that austerity programs that are so severe that they fan the flames of unrest can be self-defeating – they lead to a quick reversal of fiscal policy.

3.5 Conclusions

We find a close association between unrest and budget cuts in Europe during the period 1919-2008. Because unrest is a low-frequency event, its incidence and association with other factors is best examined in a long-run setting, using data from a large number countries. The link is strong in almost every single sub-periods, and for all types of unrest. [21] When we use recently-developed data that allows clean identification of arguably exogenous, policy-driven changes in the budget balance, our results continue to hold. Finally, these findings are not affected by using alternative measures of unrest. Contrary to what might be expected, we find no evidence that the spread of mass media facilitates the rise of mass protests.

The link between fiscal retrenchment and unrest may help to resolve a paradox in the political economy literature. There appears to be no significant punishment at the polls for governments pursuing cut-backs (Alesina et al., 1998; Alesina et al., 2010), and no evidence of gains in response to budget expansion (Brender and Drazen, 2008). Also, the empirical evidence on the economic effects of budget cuts is mixed, with some studies finding an expansionary effect, and others, a contractionary one.[22] Why, then, is fiscal consolidation often delayed, or only implemented half-heartedly? Our findings suggest one possible answer why austerity is often avoided - fear of instability and unrest.[23] Expenditure cuts carry a significant risk of increasing the frequency of riots, anti-government demonstrations, general strikes, political assassinations, and attempts at revolutionary overthrow of the established order. While the type of instability evnts we analyze are infrequent in normal years, they become much more common as tougher austerity measures are implemented. Anticipated properly, they may act as a potent brake

[21] Strikingly, where we can trace the cause of each incident (during the period 1980-95), we can show that only austerity-inspired demonstrations respond to budget cuts in the time-series. Cf. Appendix I.

[22] Alesina and Ardagna (2010); Alesina et al. (2002); Guajardo et al. (2011). An early example in the literature is Giavazzi and Pagano (1990)

[23] Alesina et al. (2010) also suggest that implementation of budget measures may be harder if the burden falls disproportionately on some groups. War-of-attrition models of consolidation are one alternative (Alesina and Drazen, 1991).

on government policy. Once unrest erupts, governments quickly reverse course and increase spending in the following year, according to our results – the politics of the street are effective in reversing austerity. The close link between austerity and instability may also help to explain why countries with higher levels of unrest are on average also more indebted (Woo, 2003).

EPCD Dataset and Results (For Online Publication Only)

The European Protest and Coercion Database (EPCD) developed by Francisco (2006) can be used to pin down the main motive behind each public demonstration. We first describe the dataset and then present results.

European Protest and Coercion Data (1980-1995):

The EPCD codes daily data on all reported protest events occurred in 28 European countries between 1980 and 1995. The data is constructed using the full-text reports from more than 400 newspapers in the Lexis-Nexis database. We restrict our attention to the same types of protest events covered in the long-term data: riots, demonstrations, political assassinations, general strikes, and attempted revolutions.[24] The main advantage of the EPCD over the Arthur Banks' database is that the former records the issue behind each protest, allowing us to test the relationship between austerity and unrest in a very precise way, even if only for a small subset of the overall dataset.[25]

There are relatively few protests that are caused by austerity measures. At the same time, when they happen, they involve a large number of participants - by far the largest number of protesters of any category, as Table 3.15 illustrates. These protests tend to be relatively peaceful, with few protesters arrested, injured or killed, and relatively few members of the security forces involved.

We define as a single protest event an event recorded with the same entry in the following EPCD variables: $event$, $protester$ and $location$. The variable $event$ provides a short description of the main issues of each protest and it is the variable we use to sort protest events into different categories (expenditure cuts, tax increase, economy, peace, labour, education and ecology), $protester$ indicates the protesting group or type (e.g. ferry workers, teachers, truck drivers etc.), $location$ is the geographical location in which the event took place. Events

[24] We define as a single protest event an event recorded with the same entry in the following EPCD variables: "event", "protester" and "location". The variable "event" provides a short description of the protest and its reasons, "protester" indicates the protesting group or type (e.g. ferry workers, teachers, truck drivers), "location" is usually the city in which it took place. Events that last more than one day are collapsed in this way in a unique observation, where the number of protesters and security forces members are averages across days. Notice that we only consider protest events whose number of participants is above 100 for riots and demonstrations and above 1000 for general strikes (no threshold is used for assassinations and attempted revolutions). These are the same threshold used in the Banks (2010) database.

[25] For this data we are also able to distinguish among austerity-inspired protests between those whose primary cause is expenditure cuts and those mainly related to tax increases.

that last more than one day are collapsed in this way in a unique observation, where the number of protesters and security forces members are averages across days. Our dependent variable is the number of events in each category occurred in each country-year. Notice that we only consider protest events whose number of participants is above 100 for riots and demonstrations and above 1000 for general strikes (no threshold is used for assassinations and attempted revolutions). These are the same threshold used in the Arthur Banks database.

Data cleaning: We exclude from our estimates one clear outlier, France in 1995, where we register 301 events, against the 2.1 protest events related to expenditure cuts per year that we register on average in Europe. This peak is due to the approval of the Juppé austerity plan that entered into force in 1996.

Results

We examine if the public assemblies that are motivated by complaints against austerity - as determined by the newspaper records in Lexis-Nexis - are significantly affected by actual changes in fiscal policy. Our approach here is similar to what has been called the "narrative approach" (Romer and Romer (1989)). Table 3.16 gives the results. If we use the same specification as in Table 3.3, we find similar results. Increasing expenditure lowers levels of unrest (column 1). In column 2, we examine the responsiveness of anti-tax demonstrations to revenue increases, and find a weakly positive relationship. The main finding, that anti-austerity protests surge when expenditure is cut, survives controlling for a host of additional variables, including country- and year-fixed effects. We can strengthen this result further by conducting a placebo test. In Table 3.17, we look at other protests, and test if they can be predicted by the same explanatory variables as in Table 3.16. Labour disputes and demonstrations about the state of the economy are less frequent when expenditure is expanding, but the link is not strong or statistically significant. Peace rallies, ecological issues, and unrest events in response to education policy are actually more frequent in times of fiscal expansion. Overall, the placebo test shows that only in the case of anti-austerity demonstrations is there a strong and significant link with changes in government expenditure.

Data Appendix (For Online Publication Only)

Variable name	Definition	Source	Time period
1. Fiscal variables (not cyclically adjusted):			
Δ(exp/GDP)	Annual change in total central government spending as a share of GDP (percentage points)	Mitchell (2007) from 1919 to 1969; OECD from 1970 to 2008 (GTE).	1919-2008
Δ(rev/GDP)	Annual change in total central government revenue as a share of GDP (percentage points)	Mitchell (2007) from 1919 to 1969; OECD from 1970 to 2008 (GTR)	1919-2008
Δ(budget/GDP)	Annual change in total central government budget as a share of GDP (percentage points)	Mitchell (2007) from 1919 to 1969 ($\frac{(rev-exp)}{GDP}$); OECD from 1970 to 2008 ($(GTE-GTR)/B1\_GA$)	1919-2008
2. Fiscal variables (cyclically adjusted):			
Δ(primary exp/GDP)	Annual change in cyclically adjusted current expenditure as a share of GDP (percentage points)	Alesina and Ardagna (2010)	1970-2007
Δ(tot rev/GDP)	Annual change in cyclically adjusted total revenue as a share of GDP (percentage points)	Alesina and Ardagna (2010)	1970-2007
Δ(primary budget/GDP)	Annual change in cyclically adjusted primary budget as a share of GDP (percentage points)	Alesina and Ardagna (2010)	1970-2007
3. Fiscal variables (exogenous retrenchment data)			
Δ(exp/GDP)^{IMF}	Annual change in government expenditure as a share of GDP (percentage points)	Devries et al. (2011)	1978-2008
Δ(rev/GDP)^{IMF}	Annual change in taxes as a share of GDP (percentage points)	Devries et al. (2011)	1978-2008
Δ(budget/GDP)^{IMF}	Annual change in government budget as a share of GDP (percentage points)	Devries et al. (2011)	1978-2008
Δ(exp/GDP)$^{IMF}_u$	Unanticipated component of the annual change in government expenditure as a share of GDP (percentage points)	Alesina et al. (2012)	1978-2008
Δ(rev/GDP)$^{IMF}_u$	Unanticipated component of the annual change in taxes as a share of GDP (percentage points)	Alesina et al. (2012)	1978-2008
Δ(budget/GDP)$^{IMF}_u$	Unanticipated component of the annual change in government budget as a share of GDP (percentage points)	Alesina et al. (2012)	1978-2008
4. Social unrest variables:			
CHAOS	Sum of general trikes, demonstrations, riots, assassinations, revolutions	Banks (2010)	1919-2008
PCA	First principal component of CHAOS	Banks (2010)	1919-2008
WCI	Weighted Conflict Index, weighted sum of: general strikes, demonstrations, riots, assassinations, revolutions, guerrilla warfare, government crises and purges	Banks (2010)	1919-2008
General strikes	Any strike of 1,000 or more industrial or service workers that involves more than one employer and that is aimed at national government policies or authority	Banks (2010)	1919-2008
Demonstrations	Any peaceful public gathering of at least 100 people for the primary purpose of displaying or voicing their opposition to government policies or authority (excluding anti-foreign nature demonstrations)	Banks (2010)	1919-2008

Variable name	Definition	Source	Time period
Riots	Any violent demonstration or clash of more than 100 citizens involving the use of physical force	Banks (2010)	1919-2008
Assassinations	Any politically motivated murder or attempted murder of a high government official or politician	Banks (2010)	1919-2008
Revolutions	Any illegal or forced change in the top governmental elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government	Banks (2010)	1919-2008
5. Controls:			
Δ (GDP/POP)	Annual growth in GDP, PPP adjusted	Maddison (2010)	1919-2008
Unemployment rate		OECD Outlook n. 84	1970-2007
Inflation rate		OECD Outlook n. 84	1970-2007
Primary budget	Level of cyclically adjusted primary budget as a share of GDP	Alesina and Ardagna (2010)	1970-2007
Public debt/GDP	Level of public debt as a share of GDP	OECD Outlook n. 84	1970-2007
Ethnic Fragmentation	Probability that two randomly selected individuals in a country belong to different ethnic groups. Data is available for 1985 and 2000. We assign the 1985 values to years from 1970 to 1990, and the 2000 value for years from 1991 to 2007	Patsiurko et al. (2011)	1985, 2000
Polity2	Revised Combined Polity Score	Marshall and Jaggers (2010)	
Media	Sum of the rankings for each country in number of telephones (including cellular phones) per capita, radios per capita and televisions per capita	Banks (2010) ($phone6$, $media2$ and $media4$)	1919-2008
Info-Media	Sum of the rankings for each country in number of telephones (including cellular phones) per capita	Banks (2010) ($phone6$, $media2$ and $media4$)	1919-2008
Peer-Media	Sum of the rankings for each country in radios per capita and televisions per capita	Banks (2010) ($phone6$, $media2$ and $media4$)	1919-2008

Note on data cleaning of fiscal variables: To take deal with outliers we use as a general rule to drop all observations for which the change in the budget position with respect to the previous year is larger than 10% in absolute value. This procedure excludes 17 observations from the sample: Belgium in 1948 and 1949, Czech Republic in 1921, 1924 and 1996, United Kingdom in 1946 and 1947, Greece in 1930 and 1931, Ireland in 1965, Italy in 1920, 1923, 1936 and 1937, Netherlands in 1946 and 1948, Yugoslavia in1932.

Figures and Tables

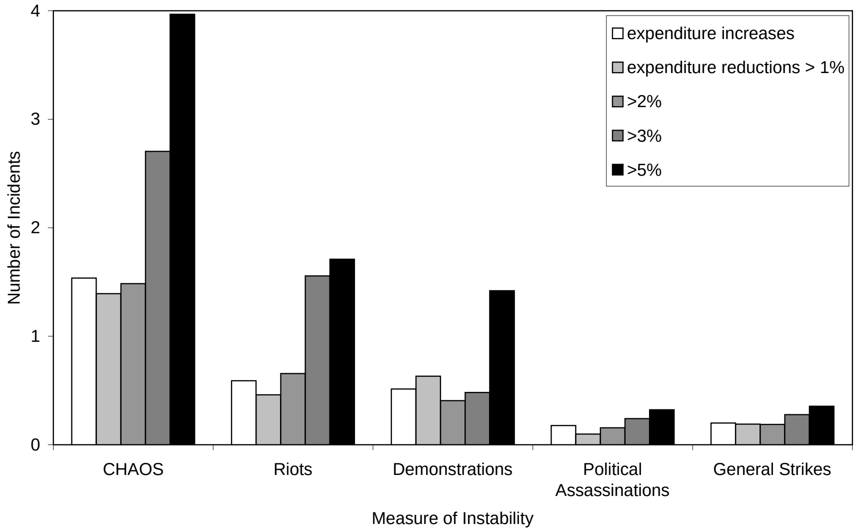

Figure 3.1: Frequency of Incidents and the Scale of Expenditure Cuts

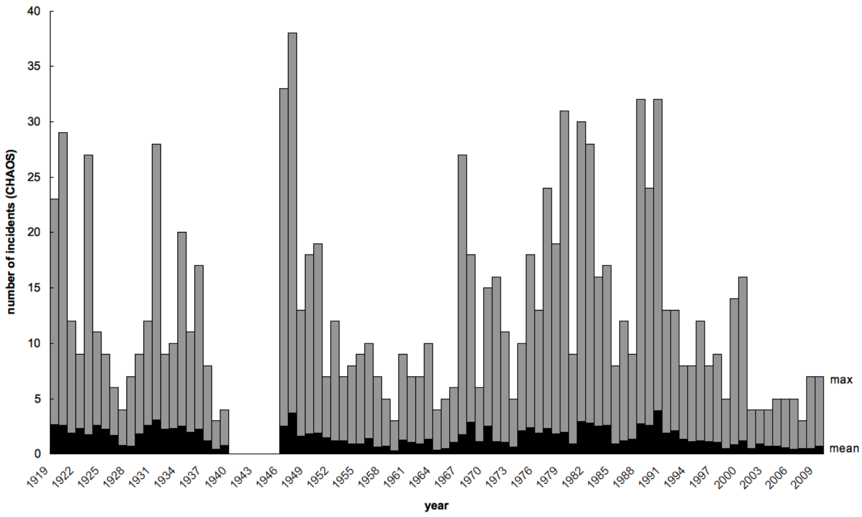

Figure 3.2: CHAOS over Time

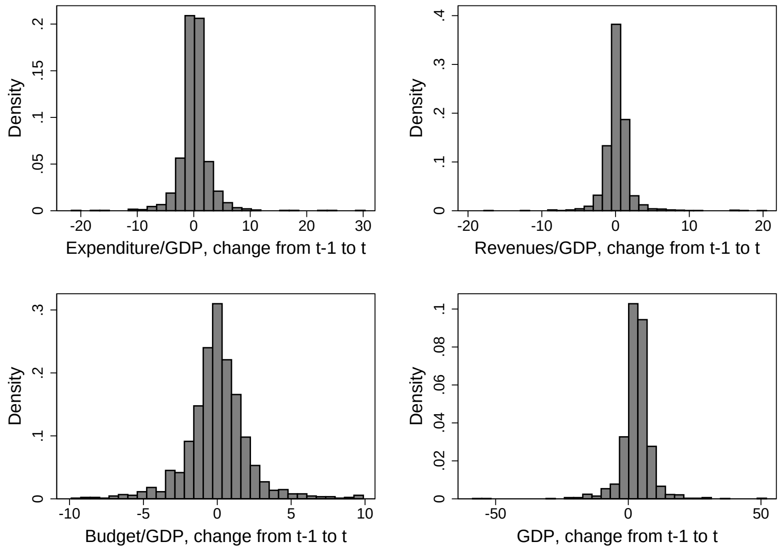

Figure 3.3: Changes in Fiscal Variables and GDP Per Capita (% points)

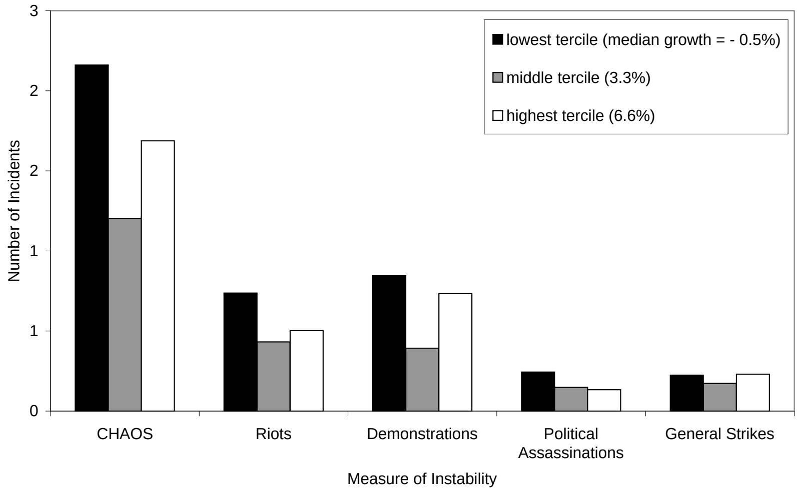

Figure 3.4: Frequency of Incidents and Economic Growth

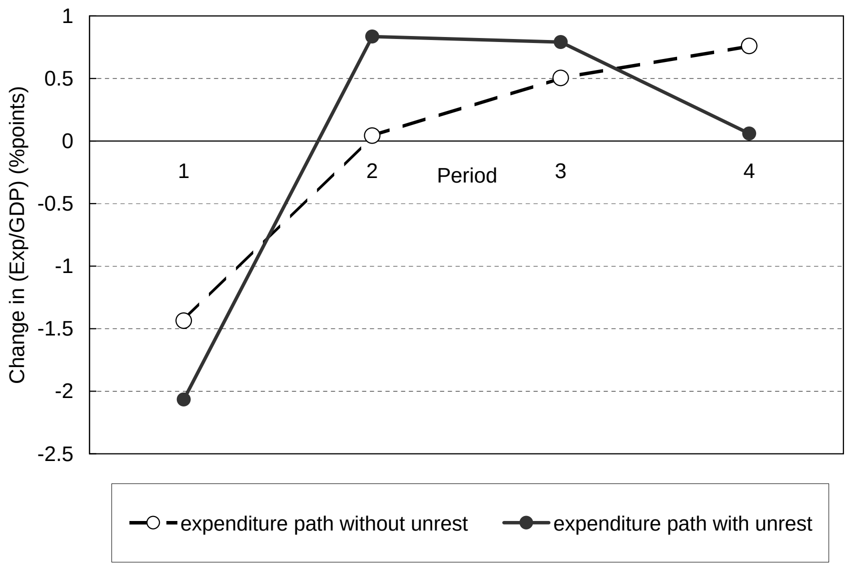

Figure 3.5: Average Change in Expenditure/GDP in Years After Expenditure Cuts

Table 3.1: Descriptive Statistics

	Mean	Std. Dev.	Min.	Max.	Observations
Δ(exp/GDP)	0.2	2.8	-21.7	30.3	1449
Δ(rev/GDP)	0.2	1.9	-17.8	20.5	1398
Δ(budget/GDP)	0.0	2.2	-9.9	9.9	1392
Δ(GDP)	3.2	5.7	-58.7	52.1	1788
CHAOS	1.636	3.753	0	38	1797
Riots	0.635	1.997	0	25	1797
Demonstrations	0.535	1.53	0	17	1797
Political Assassinations	0.177	0.762	0	15	1797
General Strikes	0.196	0.654	0	7	1797
Revolutions	0.093	0.361	0	5	1797

Notes: Fiscal variables and GDP growth expressed in % points.

Table 3.2: Cross-correlation Table

	$\Delta\left(\frac{exp}{GDP}\right)$	$\left(\frac{rev}{GDP}\right)$	$\left(\frac{budget}{GDP}\right)$	Δ(GDP)	CHAOS	Riots	Demonstr.	Political Assassin.	General Strikes	Revolutions
Δ(exp/GDP)	1.000									
Δ(rev/GDP)	0.627	1.000								
	(0.000)									
Δ(budget/GDP)	-0.711	0.102	1.000							
	(0.000)	(0.000)								
Δ(GDP)	-0.151	-0.036	0.200	1.000						
	(0.000)	(0.195)	(0.000)							
CHAOS	-0.062	0.036	0.065	-0.079	1.000					
	(0.021)	(0.193)	(0.017)	(0.001)						
Riots	-0.065	0.027	0.079	-0.030	0.867	1.000				
	(0.016)	(0.317)	(0.004)	(0.224)	(0.000)					
Demonstrations	-0.052	0.032	0.020	-0.098	0.763	0.464	1.000			
	(0.054)	(0.239)	(0.466)	(0.000)	(0.000)	(0.000)				
Political Assassinations	0.006	0.015	0.006	-0.064	0.439	0.213	0.188	1.000		
	(0.816)	(0.583)	(0.835)	(0.010)	(0.000)	(0.000)	(0.000)			
General Strikes	-0.030	0.039	0.060	-0.009	0.599	0.447	0.344	0.147	1.000	
	(0.259)	(0.150)	(0.028)	(0.718)	(0.000)	(0.000)	(0.000)	(0.000)		
Revolutions	-0.027	-0.025	0.029	-0.091	0.354	0.259	0.111	0.220	0.175	1.000
	(0.311)	(0.371)	(0.284)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	

Table 3.3: Baseline Result

Dependent variable: CHAOS						
Estimator:	Negative Binomial			OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ(exp/GDP)	-0.049**			-0.146**		
	[0.022]			[0.057]		
Δ(rev/GDP)		0.003			-0.003	
		[0.020]			[0.048]	
Δ(budget/GDP)			0.075***		0.131*	
			[0.027]			[0.065]
Δ(GDP)	-0.030**	-0.025*	-0.030**	-0.006	0.019	0.007
	[0.012]	[0.013]	[0.013]	[0.070]	[0.082]	[0.083]
Observations	1,310	1,259	1,253	1,310	1,259	1,253
R-squared				0.119	0.108	0.114
Number of countries	24	24	24	24	24	24

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.4: Predicted Number of CHAOS Episodes Under Different Expenditure Reductions and GDP Growth

	GDP Growth			
	3%	-2%	-5%	-10%
Expenditure Reduction = 1%	0.58	0.69	0.75	0.87
5%	0.71	0.84	0.92	1.06
10%	0.91	1.07	1.17	1.36
15%	1.16	1.37	1.50	1.74

Table 3.5: Full Set of Controls, 1970-2007

Dependent variable: CHAOS Estimator: Negative Binomial	(1)	(2)	(3)	(4)	(5)	(6)
Δ (primary exp/GDP)	-0.128**	-0.204**				
	[0.057]	[0.104]				
Δ (tot rev/GDP)			0.095*	0.165***		
			[0.055]	[0.042]		
Δ (primary budget/GDP)					0.074*	0.129**
					[0.040]	[0.055]
Δ(GDP)	-0.133***	-0.247**	-0.088**	-0.136	-0.111***	-0.193*
	[0.044]	[0.112]	[0.044]	[0.112]	[0.041]	[0.099]
Lag unemployment rate		0.039		0.054		0.043
		[0.075]		[0.079]		[0.080]
Ethnic fragmentation		3.909***		3.722***		4.063***
		[0.832]		[0.847]		[0.813]
Polity2 index		0.239		0.223		0.203
		[0.352]		[0.325]		[0.342]
Log (POP)		4.274		3.958		3.010
		[6.308]		[5.536]		[5.733]
Public debt/GDP		-0.642		-0.380		-0.560
		[1.109]		[1.173]		[1.170]
Δ inflation		-0.058**		-0.050		-0.062**
		[0.027]		[0.034]		[0.030]
Primary budget		-0.034		-0.044		-0.063
		[0.055]		[0.050]		[0.058]
Observations	522	434	522	434	522	434
Number of countries	16	16	16	16	16	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects. All fiscal data are cyclically adjusted using the Blanchard methodology.

Table 3.6: Exogenous Fiscal Adjustment

Dependent variable: CHAOS Estimator: Negative Binomial	(1)	(2)	(3)	(4)	(5)	(6)
Δ (exp/GDP) ^{IMF}	-0.805***					
	[0.199]					
Δ (exp/GDP) $_u^{IMF}$				-0.920***		
				[0.236]		
Δ (rev/GDP) ^{IMF}		-0.038				
		[0.245]				
Δ (rev/GDP) $_u^{IMF}$					0.280	
					[0.248]	
Δ (budget/GDP) ^{IMF}			0.296**			
			[0.123]			
Δ (budget/GDP) $_u^{IMF}$						0.414***
						[0.119]
Δ (GDP)	-0.760**	-0.738*	-0.767**	-0.758**	-0.690**	-0.736**
	[0.362]	[0.383]	[0.373]	[0.334]	[0.333]	[0.344]
Lag unemployment rate	0.211*	0.298**	0.313***	0.204*	0.333***	0.281***
	[0.116]	[0.135]	[0.103]	[0.120]	[0.111]	[0.100]
Ethnic fragmentation	8.348**	7.019*	7.121*	7.896**	8.910**	6.917*
	[3.735]	[4.020]	[4.000]	[3.338]	[3.664]	[3.681]
Polity2 index	1.693	1.529	2.082	1.744	1.832	2.376
	[2.390]	[2.375]	[2.506]	[2.161]	[2.279]	[2.394]
Log (POP)	8.980	13.367	11.933	12.277	6.039	11.255
	[29.190]	[27.430]	[30.662]	[29.229]	[25.179]	[30.940]
Public debt/GDP	-10.191***	-9.551***	-10.337***	-10.437***	-9.950***	-10.274***
	[1.650]	[1.801]	[1.782]	[1.895]	[2.031]	[2.005]
Δ inflation	-0.379**	-0.287*	-0.298*	-0.444***	-0.271*	-0.334**
	[0.156]	[0.166]	[0.164]	[0.150]	[0.138]	[0.152]
Primary budget	0.091	0.094	0.120*	0.083		0.119
	[0.081]	[0.073]	[0.071]	[0.089]		[0.079]
Observations	105	105	105	105	105	105
Number of countries	12	12	12	12	12	12

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.7: Accounting for Dynamics

Dependent variable: CHAOS Estimator: Negative Binomial	(1)	(2)	(3)
Δ(exp/GDP) $_{t-2}$	0.012	1.668	1.726
	[1.655]	[1.694]	[1.700]
Δ(exp/GDP) $_{t-1}$	-1.943	-1.206	-1.318
	[1.761]	[1.279]	[1.289]
Δ(exp/GDP) $_{t}$	-0.050**	-0.043*	-0.044*
	[0.020]	[0.024]	[0.024]
Δ(exp/GDP) $_{t+1}$	-5.097**	-5.148***	-5.288***
	[2.254]	[1.917]	[2.013]
Δ(exp/GDP) $_{t+2}$	-0.956	-1.233	-1.280
	[2.196]	[1.864]	[1.777]
CHAOS $_{t-1}$		0.083***	0.084***
		[0.009]	[0.009]
Δ (GDP) $_{t}$	-0.014	-0.026	-0.026
	[0.018]	[0.017]	[0.017]
Δ(GDP) $_{t+1}$			-2.070
			[2.029]
Observations	1,199	1,193	1,193
Number of countries	23	23	23

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.8: Institutions and the Sensitivity of Unrest to Austerity

	Polity2 Index			
Dependent variable: CHAOS	<-6	<5	<10	=10
Estimator: Negative Binomial	(1)	(2)	(3)	(4)
Δ (exp/GDP)	-0.117**	-0.109***	-0.095***	-0.033
	[0.054]	[0.029]	[0.022]	[0.023]
Δ (GDP)	0.003	-0.004	-0.029	-0.016
	[0.034]	[0.027]	[0.026]	[0.039]
Observations	149	248	383	915
Number of countries	8	11	17	19

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.9: Media Penetration and Unrest

	Media		Info-Media		Peer-Media	
Dependent variable: CHAOS	<median	>median	<median	>median	<median	>median
Estimator: Negative Binomial	(1)	(2)	(3)	(4)	(5)	(6)
Δ (exp/GDP)	-0.067***	0.019	-0.077***	0.011	-0.071***	-0.050*
	[0.024]	[0.037]	[0.027]	[0.034]	[0.024]	[0.027]
Δ (GDP)	-0.074*	-0.084	-0.085**	-0.091	-0.035*	-0.062***
	[0.040]	[0.072]	[0.042]	[0.073]	[0.020]	[0.019]
Observations	281	499	306	474	510	800
Number of countries	16	19	15	17	18	18

Notes: Standard errors clustered at country level reported in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. Media includes phones, radio and TV. Infor-Media includes radio and TV. Peer-Media includes phones. All regressions include country and year fixed effects.

Table 3.10: Results by Subcomponent of CHAOS

	Subcomponents			
Dependent variable indicated in columns	Riots	Demonstrations	Political Assassinations	General Strikes
Estimator: Negative Binomial	(1)	(2)	(3)	(4)
Δ (exp/GDP)	-0.040*	-0.041*	-0.059***	-0.045
	[0.021]	[0.022]	[0.021]	[0.031]
Δ (GDP)	-0.013	-0.034	-0.019	-0.002
	[0.014]	[0.022]	[0.021]	[0.027]
Observations	1,310	1,310	1,310	1,310
Number of countries	24	24	24	24

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.11: Results by Subperiods

	Subperiods			
Dependent variable: CHAOS	1919-1939	1946-1969	1970-1989	1990-2007
Estimator: Negative Binomial	(1)	(2)	(3)	(4)
Δ (exp/GDP)	-0.039	-0.107***		
	[0.035]	[0.029]		
Δ (primary exp/GDP)			-0.067	-0.185***
			[0.083]	[0.068]
Δ (GDP)	-0.046***	0.066***	-0.096*	-0.291***
	[0.016]	[0.023]	[0.051]	[0.081]
Observations	238	346	239	283
Number of countries	17	16	14	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.12: Alternative Measures of Unrest

Dependent variable indicated in columns	Principal Component Analysis			Weighted Conflict Index		
Estimator: OLS	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta(exp/GDP)$	-0.054**			-31.983		
	[0.022]			[24.649]		
$\Delta(rev/GDP)$		0.000			16.266	
		[0.019]			[33.410]	
$\Delta(budget/GDP)$			0.051**			49.802
			[0.024]			[31.073]
$\Delta(GDP)$	-0.002	0.007	0.002	-5.107	-1.955	-8.118
	[0.028]	[0.033]	[0.032]	[24.745]	[29.395]	[30.790]
Observations	1,310	1,259	1,253	1,310	1,259	1,253
R-squared	0.123	0.115	0.121	0.128	0.123	0.126
Number of countries	24	24	24	24	24	24

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.13: CHAOS as a Dichotomous Variable

Dependent variable: CHAOS dummy Estimator: Logit	(1)	(2)	(3)
$\Delta(exp/GDP)$	-0.040		
	[0.030]		
$\Delta(rev/GDP)$		0.020	
		[0.049]	
$\Delta(budget/GDP)$			0.088*
			[0.045]
$\Delta(GDP)$	-0.048*	-0.039	-0.050*
	[0.026]	[0.028]	[0.028]
Observations	1,295	1,244	1,238
Number of countries	22	22	22

Notes: Robust standard errors in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a dichotomous variable equal to 1 when chaos is greater than 0, and equal to 0 otherwise. All regressions include country and year fixed effects.

Table 3.14: Unrest, Expenditure Cuts and Growth

Dependent variable: CHAOS	Subsamples			
Estimator: Negative Binomial	Δ Exp>0	Δ Exp<0	Δ GDP>0	Δ GDP<0
	(1)	(2)	(3)	(4)
$\Delta(exp/GDP)$	0.015	-0.078**	-0.047**	-0.008
	[0.023]	[0.035]	[0.023]	[0.029]
$\Delta(GDP)$	-0.041**	-0.033*	-0.017	-0.070
	[0.017]	[0.019]	[0.020]	[0.061]
Observations	702	608	1,147	160
Number of countries	24	24	24	22

Notes: Standard errors clustered at country level reported in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects.

Table 3.15: Descriptive Statistics, EPCD Dataset

Main issue:	Exp Cuts	Taxes	Economy	Labour	Ecology	Peace	Education
Number of events	901	246	8,677	8,117	797	1,987	531
Averages per event:							
Protesters:							
Total	192,557	142,347	49,479	33,839	5,732	20,092	52,784
Arrested	.76	2	4.2	4.4	2	7.2	2.4
Injured	.36	.54	.3	.28	0.7	1.1	1
Security forces:							
Total	81	108	36	34	44	152	144
Injured	.35	.094	.12	.11	.092	.12	1.3

Table 3.16: Protest events from EPCD, 1980-1995

Dependent variable: N events with main issue = Exp Cuts Estimator: Negative Binomial	(1)	(2)	(3)
Δ (primary exp/GDP)	-0.214*		
	[0.124]		
Δ (tot rev/GDP)		-0.164	
		[0.111]	
Δ (primary budget/GDP)			0.021
			[0.051]
Δ (GDP)	-0.134	-0.106	-0.097
	[0.104]	[0.096]	[0.102]
Lag unemployment rate	0.111	0.129	0.115
	[0.103]	[0.100]	[0.100]
Observations	230	230	230
Number of countries	16	16	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** p<0.01, ** p<0.05, * p<0.1. All regressions include country and year fixed effects. In all specifications we excluded one clear outlier: France in 1995.

Table 3.17: Placebo test, 1980-1995

Dependent variable	Protest Event Main Issue:						
indicated in columns	Exp Cuts	Economy	Peace	Labor	Education	Ecology	All
Estimator: Negative Binomial	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ (primary exp/GDP)	-0.214*	0.007	0.031	0.006	-0.238	-0.052	0.001
	[0.124]	[0.036]	[0.081]	[0.037]	[0.252]	[0.107]	[0.024]
Δ (GDP)	-0.134	0.013	0.075	0.025	0.150	-0.006	-0.003
	[0.104]	[0.026]	[0.089]	[0.024]	[0.127]	[0.056]	[0.022]
Lag unemployment rate	0.111	0.052	0.063	0.051	-0.298*	0.014	0.026
	[0.103]	[0.033]	[0.063]	[0.034]	[0.155]	[0.080]	[0.030]
Observations	230	230	230	230	230	230	230
Number of countries	16	16	16	16	16	16	16

Notes: Standard errors clustered at country level in parentheses. Significance levels:*** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include country and year fixed effects. In all specifications we excluded one clear outlier: France in 1995.

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