

Access to Justice and Entrepreneurship: Evidence from Brazil's Special Civil Tribunals

Guilherme Lichand *Harvard University*

Rodrigo R. Soares *Sao Paulo School of Economics—FGV*

Abstract

Entrepreneurship is usually identified as an important determinant of aggregate productivity and long-term growth. The determinants of entrepreneurship, nevertheless, are not entirely understood. A recent literature has linked entrepreneurship to the development of the justice system. This paper contributes to this literature by evaluating the role of access to justice in determining the incidence of entrepreneurship. We explore the creation of special civil tribunals in the Brazilian state of São Paulo during the 1990s. Special civil tribunals increased the geographic presence of the justice system, simplified judicial procedures, and increased the speed of adjudication of disputes. Using census data and an instrumental variables strategy, we find that implementation of special civil tribunals led to increased entrepreneurship among individuals with higher levels of education. Results do not seem to be related either to other changes in public goods provision at the local level or to preexisting trends.

1. Introduction

Entrepreneurship is usually identified as an important determinant of aggregate productivity and long-term growth (Banerjee and Newman 1993; Aghion and Bolton 1997). The determinants of entrepreneurship, nevertheless, are not entirely understood. A recent literature has linked entrepreneurship to the development of the justice system, paying particular attention to judicial quality as a determinant of access to credit, formality, and willingness to start a venture

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(see, for example, Visaria 2009; Quintin 2008; Chemin 2009a, 2009b). By securing property rights and enforcing contracts, the justice system guarantees that future returns to private investments are appropriated and promises of future payments are fulfilled. These guarantees constitute commitment devices that foster the development of explicit and implicit credit markets and increase the return to entrepreneurial activities.

This paper contributes to this literature by exploring an institutional change that increased the geographical availability of a low-cost judicial technology. We use this episode to evaluate the impact of access to justice on entrepreneurship. Specifically, we explore a unique experience of creation of special civil tribunals (SCTs) in the Brazilian state of São Paulo during the 1990s. Special civil tribunals increased the geographic presence of the justice system in municipalities, simplified judicial procedures, and increased the speed of adjudication of disputes (particularly through facilitation of agreements in early stages of the process). We find that implementation of SCTs was associated with increased entrepreneurship, defined as the probability that individuals are occupied as employers or self-employed.

Analyses of the role of institutions in general and the justice system in particular face two traditional challenges: endogeneity and omitted variables. Wealthier and more dynamic areas are able to afford and may demand better justice systems. And areas with better justice systems may also have more developed institutions in other relevant dimensions. We deal with these potential problems by exploring the institutional change represented by the creation of SCTs and using an instrumental variables (IV) approach. Special civil tribunals have jurisdiction over actions of smaller complexity (up to 40 times the minimum wage) concerning microenterprises, consumer rights, debt execution, neighborhood conflicts, torts, and so on. They have authority to execute extrajudicial warranties—that is, debt contracts enforceable out of court—and also to execute their own decisions (Cunha 2008). The introduction of these new courts in Brazil was perceived as constituting “the creation of a new justice, different from all others, simple, agile, safe and effective” (Tourinho Neto and Figueira 2007, p. 69; authors’ translation), overcoming what local legal authors termed “constrained litigiousness.”

Using census data from the state of São Paulo between 1970 and 2010, we apply an IV strategy to evaluate whether entrepreneurship increased more rapidly in areas that received SCTs. Our instrument explores the fact that municipalities that housed headquarters of judiciary districts were, conditional on several observable characteristics, 52 percentage points more likely to receive a SCT than otherwise similar municipalities. At the same time, the location of headquarters of judiciary districts is extremely persistent through time, remaining typically unchanged over periods of several decades. On the basis of these observations, we construct an instrument that combines the timing of approval of the law creating the SCTs with a dummy variable indicating the presence of headquarters of judiciary districts.

The main result from our empirical analysis indicates that implementation was associated with a 3-percentage-point increase in entrepreneurship for individuals with higher educational levels (high school or more; hereafter, high education), while the impact on individuals with lower educational levels (less than high school; hereafter, low education) was quantitatively small (.4 percentage points) and not statistically significant. Given that educational attainment is highly correlated with wealth, we understand this result as indicating that marginal individuals—those for whom the change was enough to foster a transition toward entrepreneurial activities—were those in the upper socioeconomic strata. Our results are driven by findings for self-employment, which increased by 3.3 percentage points after the introduction of the new courts. Robustness exercises indicate that the results do not seem to be related either to other changes in infrastructure or public goods provision at the local level or to preexisting trends in entrepreneurship. Historically, self-employment in Brazil has been particularly common among low-skilled workers. This pattern seems to be changing in the recent past. The evidence presented here suggests that increased access to justice may have been partly responsible for this change. The data support the idea that the decision to become an entrepreneur is positively affected by the availability of a low-cost litigation technology.

Celebrated papers by Acemoglu and Johnson (2005) and Acemoglu, Johnson, and Robinson (2005) argue that, after controlling for broader property rights measures, narrower contracting institutions play no role in explaining differences in economic development across countries. Nonetheless, a varied array of recent work points to the relevance of the judiciary system for the development of credit markets, entrepreneurship, and overall economic activity. Djankov et al. (2003), Jappelli, Pagano, and Bianco (2005), Laeven and Woodruff (2007), Antunes, Cavalcanti, and Villamil (2008), Quintin (2008), and Casas-Arce and Saiz (2010) use techniques ranging from calibration to cross- and within-country regressions to present evidence pointing in this direction. In the case of Brazil, for example, Naritomi, Soares, and Assunção (2012) construct an instrument for the presence of courts in municipalities with historical variables and find it to be significantly related to long-term development.

Within-country studies exploring discrete institutional changes, which are more relevant to our paper in terms of strategy and objective, reach similar conclusions. Visaria (2009) considers the effects of the creation of debt recovery tribunals in India. From 1993 to 1999, these tribunals were introduced as the institutional arena for legal disputes between banks and borrowers. Visaria (2009) takes advantage of their staggered adoption across Indian states to assess their impact on delinquency rates and interest rates, documenting a negative significant effect on both dimensions.¹ Chemin (2009b) also investigates the impact of

¹ Liliendfeld-Toal, Mookherjee, and Visaria (2009) consider the possibility of an adverse impact of this same reform, through an increase in interest rates due to general equilibrium effects. Evidence based on firm-level panel data reveals that small firms experienced contraction in borrowing and fixed assets, while the opposite was observed for large firms.

judiciary efficiency on development in India by looking at amendments to the Code of Civil Procedures that affected judicial ambiguity and complexity. Using data from 1971 to 1996, he shows that exogenous increases in trial duration have a negative impact on farmers, particularly those with lower collateral. The paper also documents a negative effect of longer trials on credit and manufacturing output. Finally, Chemin (2009a) is probably the closest in spirit to our paper. Exploring a judicial reform in Pakistan that intended to decrease the backlog of court cases by teaching case flow management techniques to judges, the author explores a difference-in-differences strategy to evaluate the impact of the reform on judicial efficiency. He looks at the impact of increased judicial efficiency on demand for credit, on steps taken to open a business, and on probabilities of transition from unemployment to entrepreneurship. The main results are that judges in affected areas disposed significantly more cases and that the reform had a positive impact on investment confidence, on demand for credit, and on probabilities of transition to entrepreneurship.

In the case of Brazil, some papers have analyzed the impact of the judicial system and contract enforcement on economic outcomes. Castelar (2000) provides an informal overview of the relationship between the judiciary and the Brazilian economy. Costa and De Mello (2008) and Coelho, Funchal, and De Mello (2012) investigate the effects of payroll lending on interest rates and credit volume, showing that better protection against default increases lending while decreasing borrowing costs. Finally, Madeira, Rangel, and Rodrigues (2010) find that payroll lending has a positive effect on entrepreneurship. There are no papers in the economics literature exploring the institutional change on which we focus.

Our paper differs from previous contributions in two ways. First, we explore the expansion of the technology of SCTs, which reduced the cost of access to the justice system. Other papers analyze institutional changes within a given geographic distribution of the branches of the justice system without exploring explicitly the issue of cost of access. Second, whereas Chemin (2009a) analyzes the transition of individuals from unemployment or salaried work to entrepreneurship by looking at individuals' willingness to start a venture, we draw on household-level data to explore the actual incidence of entrepreneurship in the population. So our specific contribution is to assess the impact of increased access to the justice system on the incidence of entrepreneurship. Moreover, we conduct some exploratory analysis in an attempt to track down the main mechanisms linking access to justice to entrepreneurship, by looking at the heterogeneity of responses according to initial credit availability and capital intensity. The evidence suggests that the increase in entrepreneurship following the introduction of SCTs was stronger in areas where credit markets and capital-intensive industries were more developed. This suggests that the effects were driven by reduced risk of expropriation and increased capacity to access existing credit markets rather than by increased availability of finance in areas with previously underdeveloped credit markets.

The remainder of the paper is structured as follows. Section 2 describes in

detail the institutional setting and the reform that instituted SCTs. Section 3 describes the data and our empirical strategy. Section 4 presents the results. Finally, Section 5 concludes the paper.

2. Institutional Background

2.1. *Special Civil Tribunals*

The origin of SCTs in Brazil dates back to the 1988 Constitution of the Federative Republic of Brazil, which had as a major concern the broadening of fundamental rights and the adequate delivery of public goods. Although equity in access to justice has been a constitutional principle since the early 19th century, it was seen merely as a formal principle rather than as a guideline to policy implementation, having labor justice as its sole exception. According to Carneiro (2007, p. 41; authors' translation), for "the remaining branches of process law, and especially civil law . . . , until the 1980s the practice of our tribunals remained individualistic . . . [and] elitist." It was individualistic because it was based on the principle of formal equity, but there was no real concern for the effective access of the larger part of the population. It was elitist because it was "expensive, distant, mysterious and unknown, a true arena where the richer, better prepared and with better lawyers, obtained more positive results" (Carneiro 2007, p. 41; authors' translation).

The 1988 constitution specified the creation of special courts to judge and execute civil actions of smaller complexity, with emphasis on procedural simplicity (Carneiro 2007). These principles materialized in Law No. 9.099/1995 of September 26, 1995, which created the SCTs (*juizados especiais cíveis*). Special civil tribunals were eventually regulated by state laws following the federal one, which led to the actual implementation of the courts throughout the 1990s.

Special civil tribunals have jurisdiction over actions of smaller complexity (up to 40 times the minimum wage, or roughly US\$14,000 at the current minimum wage and exchange rate) involving consumer rights, debt execution, neighborhood conflicts, torts, and so on. Law No. 9.099/1995 authorized the execution of extrajudicial titles,² made mandatory the presence of lawyers for actions between 20 and 40 times the minimum wage, and, most importantly, determined the jurisdiction of SCTs over execution of their own decisions (Cunha 2008). In 1999, Law No. 9.841/1999 of October 5, 1999, extended the jurisdiction of SCTs to include microenterprises. In contrast, SCTs cannot judge actions related to family law, labor justice, or bankruptcy.

To highlight the actual role played by these courts and the way they may affect small entrepreneurs, Appendix A lists 10 examples of specific lawsuits settled through SCTs in the state of São Paulo. The cases were selected after an extensive

² These correspond, for example, to financial contracts through which a supplier finances an entrepreneur or, more generally, private debt contracts.

review of processes dealt with by the SCTs, focusing on issues relevant to the discussion in this paper. Because of the availability of information in digital form, the cases listed are from the late 2000s and early 2010s. We present summaries of the disputes involved and the agreement reached by the parties or the decision of the judge together with either the specific case number or the reference from which the case description was obtained. The cases described cover a wide variety of issues, ranging from contract nullification and enforcement to collateral execution, partnership dissolution, and enforcement of state-contingent payments, among others. They present a broad overview of concrete examples of how SCTs actually affect small entrepreneurs. We refer the interested reader to Appendix A for details on the cases. Tourinho Neto and Figueira (2007, p. 69; authors' translation) notice that "[r]egarding the 9.099/1995 law as a simple procedural norm is a serious mistake, since . . . its scope . . . [concerns] the creation of a new justice, different from all others, simple, agile, safe and effective."

As a result of the creation of the new courts, the number of judicial actions increased sharply after the installation of SCTs, overcoming what local legal authors usually term "constrained litigiousness." In São Paulo in 1999, 1 year after the creation of most SCTs in the state, 6.15 percent of all judicial actions (15.4 percent of the civil ones) were handled through the new system, while by 2003 this number had increased to 15.53 percent (41.57 percent for civil cases; values are derived from Tribunal de Justiça do Estado de São Paulo [2003]). In July 2010, SCTs were responsible for 27.9 percent of all trials and 19.8 percent of first-instance decisions in São Paulo's judicial system (Corregedoria Geral da Justiça do Estado de São Paulo 2010).

Still, SCTs were not the first attempt in Brazil to extend access to justice to a larger fraction of the population. During the 1980s, Law 7.224/1984 of October 15, 1984, instituted small-claims courts (SCCs) (*tribunais de pequenas causas*), which intended to decentralize justice, promote extrajudicial conciliation, judge matters that were usually not taken to the official justice system, simplify procedures, and reduce the burden on traditional courts (Carneiro 2007). But SCCs did not have the authority to execute their own decisions, and their rulings were not legally binding. Therefore, they were actually instituted in very few locations.³ The ones that were instituted were described as operating under precarious conditions and being ineffective because the execution of decisions still depended on the traditional justice system (Cunha 2008). In accordance with the 1988 constitution and the creation of SCTs, whenever state laws were passed regulating the functioning of SCTs, previously existing SCCs were immediately converted into SCTs.

Given the limited role played by SCCs, we concentrate our analysis on SCTs.

³ In our sample, less than 3 percent of locations had a small claims court (SCC) in 1991, as opposed to 45 percent of locations having a special civil tribunal (SCT) in 2000.

Still, in some of our empirical exercises, we address explicitly the previous existence of SCCs.

According to the Brazilian Census Bureau (Instituto Brasileiro de Geografia e Estatística), 34 percent of Brazilian municipalities had at least one SCT in 2001. In the state of São Paulo, which constitutes our sample, this value was 47 percent (IBGE 2001). We explore the fact that roughly half of the municipalities in the state did not receive an SCT before 2000 to identify the impact of access to justice on entrepreneurship. The remaining empirical concerns are discussed in detail in Section 4.

2.2. Judiciary District Headquarters

Judiciary districts (*comarcas*) are the lowest level units in the Brazilian justice system. They are composed of sets of contiguous municipalities that share the same judicial resources—both material and institutional—and over which a judge in a first-instance court has jurisdiction. Judiciary districts house the lowest level courts in Brazil, where people first come into contact with the system. Districts composed of more than one municipality have the main institutions of the justice system—judges and courts, most importantly—located in one of the municipalities, which is designated as the district headquarters (*sede de comarca*). The location of judiciary district headquarters plays a particularly important role in our identification strategy, so we dedicate some time to them here.

The organization of the Brazilian justice system into judiciary districts dates back to colonial times. Currently, the creation of new districts is regulated by state laws specifying necessary conditions based on population, area, fiscal revenue, and the electorate, among others (Litschig and Zamboni 2012). In São Paulo, article 12 of the Code of Organization and Judiciary Division of 2003 (Código de Organização e Divisão Judiciárias 2003) specifies three general criteria—electorate, caseload, and fiscal revenues—as minimal requirements for the definition of boundaries of judiciary districts, without giving specific values. Provision 59 (*Provimento* 59 of July 15, 2003) goes one step further and specifies objective criteria for the creation of judiciary districts, based on minimum values of fiscal revenue, population, electorate, and caseload. Before 2003, there were no objective criteria defining necessary or sufficient conditions for the creation of new districts. Nevertheless, it is likely that some of the factors listed above—such as population and revenues—also played a role in the political process leading to the creation of districts before that year.

There is no objective guideline in state laws determining which municipality should house the headquarters of a judiciary district. The typical decision when new districts are created is to assign the headquarters to the municipality with the largest population, so that costs of access are minimized (see Litschig and Zamboni [2012] for a detailed discussion and analysis of the organization of judiciary districts).

From the perspective of our discussion, the most important point is that there

were very few changes in judiciary districts—and judiciary district headquarters—between 1970 and the early 2000s. The location of judiciary district headquarters is highly persistent over time and bears no obvious relationship to short-term changes occurring in a particular place and time. Several municipalities that were headquarters of judiciary districts in the mid-1990s had been so at least since the 19th century.⁴ Of the 224 judiciary district headquarters that existed in 1994, 98 percent had already existed as headquarters in 1969. In addition, only 4.8 percent of the judiciary district headquarters in 1969 lost their positions by 1994 (Decree No. 158 of October 28, 1969).

Despite the fact that the creation of new judiciary districts and the installation of new headquarters may be affected by contemporaneous conditions at the local level, they are, during the period of our analysis, very rare events. The vast majority of judiciary district headquarters that existed in the mid-1990s, when the institutional change we analyze takes place, had been in place for several decades. This dimension of exogeneity in the location of headquarters at the end of the 20th century is essential to justify our identification strategy.

3. Data and Empirical Strategy

3.1. Data

Data on SCT locations were obtained from the Tribunal of Justice of the State of São Paulo (Tribunal de Justiça do Estado de São Paulo 2003). We create dummy variables indicating the presence of an SCT in a given municipality. We also create a dummy variable indicating the presence of headquarters of judiciary districts, using information obtained from the 1994 *Statistical Yearbook* (IBGE 1994) for the state of São Paulo.

We restrict our analysis to the state of São Paulo because of data availability and the fact that, ultimately, SCTs are part of the state justice systems.⁵ Therefore, in principle, the analysis should be conducted at the state level to minimize other dimensions of institutional heterogeneity. The state of São Paulo is particularly relevant because it is the largest economy in Brazil, accounting for more than one-third of the country's gross domestic product and one-fifth of its population.

⁴ A quick search led us to the following examples of judiciary district headquarters created in the 19th century or earlier: Agudos, Barretos, Batatais, Caconde, Campinas, Casa Branca, Espírito Santo do Pinhal, Franca, Jaú, Jundiá, Mococa, Mogi-Mirim, São João da Boa Vista, São José do Rio Pardo, and São Simão. There are probably many more cases that cannot be easily ascertained. There were also several district headquarters created in the first 2 decades of the 20th century.

⁵ It is also worth noting that while in several states SCTs have mainly dealt with complaints against the public sector, especially in what comes to telecommunication services, in São Paulo the share of these actions in the total was among the lowest in the country (Centro Brasileiros de Estudos e Pesquisas Judiciais 2006). Therefore, SCTs in the state played a different role from that seen in other areas, where they have been identified as “instances for complaints against services provided by the public sector” (Marques 2006, p. v; authors' translation).

Except for data on average per capita household income and credit availability,⁶ drawn from IPEADData (the database of the Institute of Applied Economic Research [Instituto de Pesquisa Econômica Aplicada]) and from the Central Bank of Brazil, all other variables were constructed from census microdata files (1970–2010). Since there was a significant change in the number of Brazilian municipalities during this period, we use minimum comparable areas (MCAs; *áreas mínimas comparáveis*) as units of analysis. Minimum comparable areas are territorial units within the census that can be traced through time, allowing comparability of the same location across different periods.

Variables were constructed to be compatible across different census years (there were changes in specific questions during the period). Appendix B describes in detail the procedures used to create consistent variables through time. The variables include individual and household characteristics (age, gender, and schooling; whether the individual is a migrant; presence of water, sanitation, and electricity; ownership of a car; and number of rooms in the household). We restrict the sample to individuals between ages 25 and 65.

We define entrepreneurship—our dependent variable—as occupation as employer or self-employed. Those who are self-employed can often be regarded as small entrepreneurs, especially in industries that involve the outsourcing of services. In addition, different motivations for entrepreneurship have been identified in the literature, and these are usually associated, at least in part, with the distinction between self-employed and employers (see Ardagna and Lusardi 2010). There is also some debate on the different nature of self-employment across developing and developed countries (Mondragón-Velez and Peña 2010). Our analysis may help shed light on this issue, since it seems to uncover an ongoing change in the nature of self-employment in Brazil. In Table B4, we provide a detailed account of the major activities in which both self-employed and entrepreneurs are involved. The distribution of individuals by sector of activity is quite similar among employers and self-employed, which supports the idea that the groups are not very different. In any case, in some empirical analyses, we conduct separate exercises for employers and self-employed.

3.2. Descriptive Statistics

Table 1 presents averages of selected variables in MCAs for census years between 1970 and 2010. In Table 2, the shares of entrepreneurs, employers, and self-employed are considered according to educational level attained. Values are presented separately for MCAs that had received an SCT by 2000 or 2010 and those that had not.

Tables 1 and 2 highlight the main challenges in our empirical exercise. Min-

⁶ Data for total credit by municipality report the location where a loan is taken out, which does not necessarily mean that the money is used within that location or that the borrower is a local resident. This is the only credit variable that we have at the municipality level, so we have no alternative to using it. This variable is used only in some robustness exercises.

Table 1
Descriptive Statistics for Minimum Comparable Areas in the
State of São Paulo, 1970–2010

| | Entrepreneur | | Employer | | Self-Employed | | Population | | High School Education | | With Occupation | |
|------|--------------|-----|----------|-----|---------------|-----|------------|---------|-----------------------|-----|-----------------|-----|
| | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT |
| 1970 | .34 | .25 | .03 | .03 | .32 | .22 | 6,094 | 58,533 | .03 | .11 | .53 | .54 |
| 1980 | .30 | .23 | .04 | .05 | .26 | .19 | 6,232 | 85,018 | .06 | .17 | .57 | .60 |
| 1991 | .26 | .26 | .06 | .06 | .22 | .21 | 7,398 | 107,743 | .14 | .29 | .61 | .62 |
| 2000 | .27 | .28 | .03 | .04 | .24 | .25 | 8,742 | 126,235 | .21 | .38 | .61 | .62 |
| 2010 | .22 | .24 | .02 | .03 | .21 | .22 | 9,953 | 140,388 | .36 | .50 | .68 | .70 |

Note. Data are municipality averages. Of a total of 567 municipalities, 294 did not receive a special civil tribunal (SCT), 257 received an SCT between 1991 and 2000, and 16 received an SCT between 2000 and 2010.

imum comparable areas that received SCTs are quite different from those that did not. Since the beginning of the sample period, MCAs that received SCTs were larger and had a relatively more educated population. The differences persist throughout the period. This pattern should not be surprising, since SCTs were more likely to be implemented in regional centers, which were associated with better infrastructure and greater presence of the state.

In terms of our variables of interest, the share of employers is very similar throughout the period in MCAs with and without SCTs. Starting in 2000, though, the fraction of employers becomes slightly larger in locations with SCTs. It is surprising to note that MCAs receiving SCTs tended to have a lower fraction of self-employed individuals in the beginning of the period while at the same time having a similar fraction of employers. This seeming contradiction results from the fact that, historically, self-employment has been a low-skill occupation in Brazil: in our sample, the incidence of self-employment among individuals with less than a high school education was 30 percent in 1970, compared with 13 percent among those with higher levels of schooling. So areas with lower income and poorer labor market prospects had a historically higher incidence of self-employment. At the same time, self-employment was increasing throughout the period among individuals educated above the high school level, from 13 percent in 1970 to 26 percent in 2000 and then to 21 percent in 2010, when the Brazilian labor market was extremely heated (occupation rates in the sample increase from 61 percent in 2000 to 69 percent in 2010).

As Tables 1 and 2 show, for MCAs that did not receive SCTs, the incidence of entrepreneurship declined almost monotonically between 1970 and 2010, from 34 percent to 22 percent. On the other hand, for MCAs that received SCTs, the incidence of entrepreneurship increased from 25 percent to 28 percent between 1970 and 2000 and then fell back to 24 percent in 2010. The table portrays an overall trend of reduction in the incidence of entrepreneurship, driven mostly by reduced self-employment among individuals with lower educational levels,

Table 2
Descriptive Statistics for Minimum Comparable Areas in the State of
São Paulo, in 1970–2010, by Education Attained

| | High Education | | | | | | Low Education | | | | | |
|------|----------------|-----|-----------|-----|---------------|-----|---------------|-----|-----------|-----|---------------|-----|
| | Entrepreneur | | Employer | | Self-Employed | | Entrepreneur | | Employer | | Self-Employed | |
| | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT | No SCT | SCT |
| 1970 | .17 | .22 | .08 | .07 | .10 | .16 | .34 | .25 | .02 | .02 | .33 | .23 |
| 1980 | .22 | .22 | .07 | .10 | .16 | .14 | .30 | .24 | .04 | .04 | .27 | .20 |
| 1991 | .30 | .26 | .11 | .10 | .22 | .17 | .25 | .26 | .05 | .04 | .22 | .22 |
| 2000 | .32 | .30 | .08 | .08 | .26 | .24 | .25 | .27 | .02 | .02 | .24 | .25 |
| 2010 | .23 | .23 | .04 | .04 | .20 | .20 | .22 | .24 | .01 | .01 | .21 | .23 |

Note. Data are municipality averages. Of a total of 567 municipalities, 294 did not receive a special civil tribunal (SCT), 257 received an SCT between 1991 and 2000, and 16 received an SCT between 2000 and 2010. High education denotes a high school education or more; low education denotes less than a high school education.

combined with an increase in entrepreneurship—and self-employment—among individuals with higher levels of education. This hints at the change in the nature of self-employment that has taken place in recent decades, evolving from a low-skill, informal occupation to something increasingly closer to small entrepreneurship.

Still, given the intrinsic differences between locations with and without SCTs, it is impossible to assess the role of the new courts based exclusively on descriptive data. Therefore, in Section 3.3, we analyze explicitly the process of SCT implementation, to shed light on the challenges of our empirical exercise and on potential strategies to deal with them. In Section 3.4, on the basis of these results, we propose an IV approach to uncover the causal impact of SCTs on entrepreneurship.

3.3. *Special Civil Tribunals Implementation*

The descriptive analysis suggests that the presence of SCTs may have increased the fraction of entrepreneurs among individuals with high educational levels, but the heterogeneity of MCAs that received and did not receive SCTs precludes any causal interpretation of the patterns observed in Tables 1 and 2. Despite the heterogeneity, if the implementation of SCTs is a function of time-invariant characteristics only, the use of fixed effects would be enough to deal with the problem. Otherwise, if implementation is endogenous and correlated with the dynamic behavior of dependent variables, we should seek an alternative to a simple difference-in-differences strategy.

To assess the extent of these concerns, we analyze explicitly the process of SCT implementation (similar to the exercise conducted by Galiani, Gertler, and Schargrodsky [2005] when analyzing the privatization of water services in Argentina). We run MCA-level probit regressions of the presence of SCTs in 2000

on a set of time-changing and predetermined time-invariant characteristics. Although some MCAs received SCTs between 2000 and 2010, 94 percent of the SCTs were instituted before 2000. So, to keep the analysis as simple as possible and to avoid the use of dynamic survival models, in this section we analyze only those SCTs implemented up to 2000.

The probit specification has as a dependent variable a dummy indicating the presence of an SCT in 2000 and independent variables indicating levels for 1980 and differences between levels for 1991 and 1980 for a large set of independent variables. Our independent variables (averages for MCAs) include levels and differences of all controls used later plus our entrepreneurship indicators: self-employment and employer status; age, gender, and schooling; indicators of urban or migrant status; presence of water, sewage, and electricity in the household; ownership of cars by household members; number of rooms in the household; and population (natural logarithm).

In addition, we include a dummy variable indicating whether the MCA housed the headquarters of the local judiciary district. Administrative and logistic considerations suggest that the implementation of SCTs would start in the judiciary district headquarters and then be expanded to the surrounding areas under its jurisdiction. In fact, the state law that regulated SCTs in São Paulo (*Lei Complementar* No. 851 of December 9, 1998) mandated all judiciary districts to institute an SCT within 60 days. Although this obligation was not strictly enforced, still 208 (93 percent) of the judiciary districts had an SCT by year 2000. The other 49 SCTs, corresponding to 19 percent of the total, were implemented in locations that were not headquarters of judiciary districts. The idea that the location of judiciary district headquarters were a main determinant of SCT implementation plays a very important role in our identification strategy.

Table 3 presents the results of our probit estimation (as marginal effects). Columns 1 and 2 exclude population and the dummy variable for judiciary district headquarters and include different sets of variables, column 3 includes population, and column 4 includes all independent variables plus the dummy variable for judiciary district headquarters. The table shows that predetermined characteristics (from 1980) were highly correlated with SCT implementation. In the columns 1 and 2, MCAs with a higher fraction of employers and higher levels of schooling were more likely to receive an SCT in the years between 1991 and 2000. Quantitatively, a higher incidence of entrepreneurship—by 2 percentage points in 1980—or a higher fraction of the population with high school education—by 1 percentage point—was associated with a 5- to 6-percentage-point increase in the probability of receiving an SCT between 1991 and 2000.

In columns 1 and 2, the change in the dependent variables (share of employers and share of self-employed) between 1980 and 1991 plays virtually no role in the implementation of SCTs. The change in the number of employers appears to be significant in the second column but has a small quantitative effect: a 50-percent increase in the fraction of employers from the 1980 average is associated with a 4-percentage-point increase in the probability of SCT implementation.

However, this pattern changes when we include population as an additional control in column 3. Both the initial level and the change in population (logs) appear to be strongly related to SCT implementation. A population that is 10 percent larger in 1980 is associated with a 5-percentage-point increase in the probability of SCT implementation, while population growth that is 10 percentage points higher between 1980 and 1991 is associated with a 9-percentage-point increase. In addition, both the levels and the changes in employers and self-employed become statistically significant. Conditional on the size of an MCA, those with higher fractions of entrepreneurs and with higher growth in entrepreneurship were more likely to receive an SCT. This is worrisome from the perspective of a simple difference-in-differences strategy, since municipalities that were more dynamic—in terms of growth and entrepreneurship expansion—between 1980 and 1991 seem to have been more likely to receive the new courts. Quantitatively, an increase of 50 percent of the 1980 average in the proportion of employers was associated with a 5-percentage-point increase in the probability of receiving an SCT, while an increase of 50 percent in the proportion of self-employed was associated with an 18-percentage-point increase.

In column 4, we include as an additional control the dummy variable indicating the presence of judiciary district headquarters. Housing the headquarters of a judiciary district is by far the main driving force behind SCT expansion, increasing the probability of implementation by 52 percentage points. This holds true conditional on the previously discussed variables. It is important to stress that the introduction of the variable changes the point estimates from column 3 very little, as discussed in the previous paragraph. This means that the variation captured by the dummy variable for judiciary district headquarters is close to orthogonal to that captured by the changes in population size and in entrepreneurship. In other words, conditional on observable characteristics, the implementation of SCTs driven by the presence of judiciary district headquarters was not correlated with growth and entrepreneurship expansion. It is particularly important that this result is conditional on population levels and changes since these are the main variables determining the creation of new judiciary districts.

Together with the fact that judiciary districts are very persistent over several decades, the results from column 4 suggest that the presence of judiciary district headquarters could be used to construct an instrument for SCT creation. This is the strategy that we outline in the next section.

3.4. Empirical Strategy

We explore the institutional change represented by the creation of SCTs to assess the impact of a reduction in the cost of access to justice on entrepreneurship (measured by the probability of occupation as employer or self-employed). We use MCA data (individual averages from census microdata files) from before and after SCT implementation so that we can control for unobserved attributes of MCAs that are fixed through time. Our second-stage specification is similar

Table 3
 Probit Estimation Results (Marginal Effects) of Adoption of a Special Civil Tribunal (SCT) in 2000 in
 Minimum Comparable Areas (MCAs) in the State of São Paulo

| | Population Excluded (1) | Judiciary District Headquarters Excluded (2) | Population as Control (3) | Judiciary District Headquarters as Control (4) |
|-------------------------------------|----------------------------|---|------------------------------|---|
| Judiciary district headquarters | | | | |
| Difference between 1980 and 1991: | | | | |
| ΔEmployer (%) | 1.792 (1.288) | 2.673* (1.339) | 3.341* (1.429) | 2.980+ (1.581) |
| ΔSelf-employed (%) | .765 (.542) | .897 (.578) | 1.678** (.651) | 1.753** (.658) |
| Δln(Population) (%) | | | .858** (.222) | .833** (.234) |
| ΔUrban status (%) | -.65 (.485) | -.394 (.497) | -.944+ (.524) | -.509 (.526) |
| ΔAge (average) | -.0559 (.0522) | -.0554 (.0553) | -.0167 (.0619) | -.0329 (.0589) |
| ΔHigh school education (%) | .21 (.810) | -.138 (.928) | .239 (1.127) | .0538 (1.078) |
| ΔMale (%) | -.36 (.825) | -.479 (.825) | -1.267 (.905) | -.65 (1.937) |
| ΔHousehold water presence (%) | .167 (.510) | .34 (.543) | .354 (.547) | -.0261 (.573) |
| ΔHousehold sewage presence (%) | -.216 (.205) | -.281 (.229) | -.424+ (.255) | -.338 (.242) |
| ΔHousehold electricity presence (%) | -.677 (.701) | .169 (.846) | 1.773+ (1.045) | 1.855 (1.212) |
| ΔMigrant status (%) | | -1.462** (.429) | -.937+ (.519) | -.309 (.547) |
| ΔRooms in household (average) | | .0516 (.121) | -.00682 (.141) | .0289 (.145) |
| ΔHouseholds with car (%) | | -1.322* (.603) | -1.857** (.688) | -1.569* (.688) |

| | | | | | |
|------------------------------------|----------|---------|---------|---------|--|
| Levels in 1980: | | | | | |
| Employer (%) | 2.778* | 3.256* | 3.523* | 3.311* | |
| | (1.394) | (1.496) | (1.592) | (1.643) | |
| Self-employed (%) | .637 | .739 | (.660) | 1.416* | |
| | (.496) | (.576) | | (.672) | |
| ln(Population) (%) | | | .526** | .330** | |
| | | | (.0879) | (.0842) | |
| Urban status (%) | 1.261** | .779 | -.35 | -.0832 | |
| | (.427) | (.503) | (.484) | (.510) | |
| Age (average) | -.152* | -.076 | .0843 | .0709 | |
| | (.0752) | (.0773) | (.0807) | (.0827) | |
| High school education (%) | 6.161** | 6.799** | 3.752** | 1.927 | |
| | (1.045) | (1.121) | (1.234) | (1.330) | |
| Male (%) | -.845 | -.628 | .0987 | .315 | |
| | (.847) | (.878) | (.935) | (.935) | |
| Household water presence (%) | -.00332 | .273 | .576 | .519 | |
| | (.477) | (.532) | (.547) | (.553) | |
| Household sewage presence (%) | .123 | .144 | .167 | .122 | |
| | (.179) | (.216) | (.244) | (.232) | |
| Household electricity presence (%) | -1.499** | -.38 | .971 | 1.065 | |
| | (.561) | (.738) | (.844) | (.989) | |
| Migrant status (%) | | -.224 | -.735 | -.092 | |
| | | (.789) | (.803) | (.831) | |
| Rooms in household (average) | | -.265** | .184 | .0867 | |
| | | (.101) | (.122) | (.126) | |
| Households with car (%) | | .704 | -1.227 | -.845 | |
| | | (.596) | (.760) | (.720) | |

Note. The dependent variable is a dummy indicating the presence of an SCT in 2000. Independent variables for levels in 1980 and differences between levels in 1991 and 1980 are MCA aggregates for individuals between ages 25 and 65. Robust standard errors are in parentheses. Census data are from 1980 and 1991. $N = 567$.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

to a difference-in-differences strategy. It is a regression of entrepreneurship on the fitted values of an SCT dummy, MCA and time fixed effects, and a set of controls. Our benchmark specification is

$$Y_{m,t} = \alpha + (\delta \times \widehat{\text{SCT}}_{m,t}) + \gamma' \mathbf{X}_{m,t} + \beta_m + \vartheta_t + \varepsilon_{m,t} \quad (1)$$

where m and t stand for MCA m and year t ; $Y_{m,t}$ is incidence of entrepreneurship; $\mathbf{X}_{m,t}$ is a vector of covariates; $\widehat{\text{SCT}}_{m,t}$ is the fitted value of a dummy indicating the presence of an SCT obtained from the first-stage regression; β_m and ϑ_t are MCA and year fixed effects; $\varepsilon_{m,t}$ is a random term; and α , δ , and γ are parameters.

A simple difference-in-differences strategy is not enough here since Table 3 suggests that implementation of SCTs depended on municipalities' characteristics, and therefore our treatment variable is endogenous. The use of MCA fixed effects partly helps to deal with this problem given that systematic time-invariant differences across locations are controlled for. But it does not solve it entirely, since there seems to be a dynamic dimension of endogeneity: adoption of an SCT was associated with a particular evolution of the dependent variable through time. Therefore, we resort to an IV approach. Our first-stage regression is

$$\text{SCT}_{m,t} = \sigma + (\rho \times \text{inst}_{m,t}) + \varphi' \mathbf{X}_{m,t} + \lambda_m + \psi_t + \nu_{m,t} \quad (2)$$

where $\text{inst}_{m,t}$ is our instrument (the interaction of a dummy indicating the presence of judiciary district headquarters in 1994 with a dummy indicating the timing of approval of the SCT law, which equals one in 2000–10 and zero before 2000); λ_m and ψ_t are MCA and year fixed effects; $\nu_{m,t}$ is a random term; and σ , ρ , and φ are parameters.

Table 3 suggests that there is one judicial characteristic of MCAs that is virtually invariant over very long periods and that, after the approval of the SCT law, was the main driving force behind adoption: housing the headquarters of a judiciary district. So we build our instrument on the basis of the timing of approval of the law and the location of judiciary district headquarters. Since the law regulating SCTs was approved in 1998, a natural instrument arises from the interaction of these two dimensions: a dummy variable equal to zero before 1998 and one in 1998 and afterward for MCAs that were headquarters of judiciary districts before the approval of the law (in 1994).

For this IV strategy to be convincing, it is important that we remember the discussion about judiciary districts from Section 2. The main concern here is how the locations of judiciary district headquarters are determined and whether they could be endogenous to entrepreneurship or to the judicial reform we are analyzing. The locations of headquarters of judiciary districts change very rarely over time, so it is difficult to associate them with variations occurring at a specific time. The proposed instrument relies on the interaction of a characteristic that is virtually time invariant with the timing of approval of the law that led to the implementation of SCTs. Table 3 suggests that this would be a valid strategy to isolate the exogenous component of variation in SCT implementation. This is particularly true because our main specification includes all the variables from

Table 3: gender, age, and indicators of household wealth, which are likely to condition the decision to become an entrepreneur through wealth constraints; schooling and migrant status variables, to control for potential ability heterogeneity and labor market opportunities; and MCA characteristics related to population and public infrastructure. In some specifications, we go so far as to control for income per capita, to make sure that our results are not simply driven by improvements in overall economic conditions.

Our key identifying assumptions are the following: housing the headquarters of judiciary districts affects the probability of SCT implementation after the approval of the law; conditional on observables, the presence of headquarters of judiciary districts is exogenous to local economic circumstances; and the presence of headquarters of judiciary districts has no other effect on entrepreneurship contemporaneous to the approval of the law, apart from that working through the creation of SCTs. Under these assumptions, the time-varying random term ($\varepsilon_{m,t}$) is not correlated with covariates, and the coefficient of interest gives the causal effect of SCTs on entrepreneurship.

We cluster standard errors at the MCA level to allow for general autocorrelation of observations within an MCA over time (following Bertrand, Duflo, and Mullainathan 2004). In addition, since our data set is made of MCA averages, we weight observations by the inverse of their variance (using the number of census observations used to create each data point). We also analyze the impact of SCTs on entrepreneurship for different educational levels—given the changes in the profile of entrepreneurship alluded to previously—and for different samples (in terms of years and MCA characteristics). In some specifications, we look at the presence of preexisting trends before the approval of the SCT law to make sure that our instrument is not simply capturing differential trends in the dependent variable across MCAs with and without the location of headquarters of judiciary districts.

4. Results

4.1. Benchmark Specification and First Stage

Table 4 presents the results from our benchmark specification. We first use as a dependent variable our composite definition of entrepreneur, and we then use its components—employer and self-employed—separately.⁷ Ordinary least squares (OLS) regression and IV estimates are presented, as are results by level of education. As discussed previously, we include as controls municipality averages (among individuals with occupations used to construct the dependent variable) of dummies for gender (male), education (high school education, when not considering specific levels of schooling), migrant status, age, and number of rooms in the household, as well as overall municipality averages of water coverage, sewage coverage, urbanization, and the natural logarithm of popula-

⁷ When using self-employed as the dependent variable, we exclude employers from the sample since they can be regarded as higher intensity entrepreneurs than self-employed ones. So in that sample, we analyze the incidence of self-employment among employees and self-employed.

Table 4

Effect of Special Civil Tribunals (SCTs) on Entrepreneurship in Results of Ordinary Least Squares Regressions and Instrumental Variable Estimations (Benchmark Specifications) in Minimum Comparable Areas (MCAs) in the State of São Paulo, 1970–2010

| | Ordinary Least Squares Regressions | | | Instrumental Variable Estimates | | |
|----------------|------------------------------------|---------------------|---------------------|---------------------------------|---------------------|--------------------|
| | Overall | High Education | Low Education | Overall | High Education | Low Education |
| Entrepreneur: | | | | | | |
| SCT | -.00232 (.00472) | .0133** (.00488) | -.00265 (.00623) | .00394 (.00817) | .0296** (.00906) | .00415 (.00958) |
| N | 2,835 | 2,819 | 2,835 | 2,835 | 2,819 | 2,835 |
| R ² | .823 | .868 | .802 | .823 | .867 | .802 |
| Employer: | | | | | | |
| SCT | .00175 (.00145) | .00362 (.00309) | -.00028 (.00174) | -.000375 (.00278) | .0013 (.00756) | -.0033 (.00269) |
| N | 2,835 | 2,819 | 2,835 | 2,835 | 2,819 | 2,835 |
| R ² | .803 | .829 | .723 | .802 | .828 | .722 |
| Self-employed: | | | | | | |
| SCT | -.00321 (.00478) | .0130** (.00420) | -.0026 (.00603) | .00359 (.00870) | .0330** (.00904) | .00561 (.00987) |
| N | 2,835 | 2,807 | 2,835 | 2,835 | 2,807 | 2,835 |
| R ² | .801 | .854 | .788 | .801 | .852 | .787 |

Note. Dependent variables are municipality averages of entrepreneur and employer (among those with occupations) and self-employed (among those with occupations, excluding employers). Independent variables (not shown) are municipality averages of male gender, age, car ownership per household, number of rooms per household, migrant status, and high school education (among those with occupations used to construct a dependent variable); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects. The instrument is the interaction of variable indicating the presence of judiciary district headquarters in 1994 with a variable indicating years from 2000 onward. High education denotes those with education at the high school level or above; low education denotes those with less than a high school education. Census data are from 1970 to 2010 and are restricted to individuals between ages 25 and 65. Robust standard errors, clustered by MCAs, are in parentheses.

* $p < .1$.* $p < .05$.** $p < .01$.

tion. The overall municipality levels of public infrastructure, urbanization, and population intend to capture the provision of public goods and local economic dynamics. All regressions include MCA and year fixed effects. We use census data from 1970 to 2010, restricting the sample to individuals between ages 25 and 65.

In Table 4, we present results of OLS regressions for entrepreneurs—equivalent to a difference-in-differences strategy—by level of schooling. These can be seen as the conditional correlation between a dummy for SCT implementation and the incidence of entrepreneurship. We also present results of OLS regressions for the separate components of our entrepreneurship variable: employer and self-employed. In addition, we present analogous results for our IV strategy, using the different dependent variables.

Qualitative results are similar across the OLS and IV estimation strategies. We find a significant positive impact of SCT implementation for individuals with high education, with no significant effect noted for individuals with low education or for the overall population. When we break down the results by employer and self-employed, we see that most of the estimated impact comes from self-employment. This should come as no surprise since the incidence of self-employed is considerably higher than that of employers. The estimated effect of SCTs on employers is positive, but point estimates are quite small and not statistically significant.

The quantitative IV estimates tend to be substantially higher than the corresponding OLS ones. If anything, the IV strategy suggests that the endogenous dimension of variation in SCTs reduces their estimated impact on entrepreneurship. Among individuals with high education, SCT implementation was associated with an increase of 3 percentage points in the incidence of entrepreneurship, driven mostly by self-employment.⁸ This should be expected if local changes correlated with SCT adoption improved labor market opportunities, therefore reducing the relative attractiveness of small entrepreneurship. Remember that self-employment has historically been a low-skilled occupation in Brazil and that improved local economic conditions typically would reduce self-employment, not increase it. So the typical concern present in OLS exercises of this sort—that overall improvements in the economy would be associated with SCT implementation and with increased entrepreneurship—is not really present in our case. Since most of the effect of SCTs on entrepreneurship is coming from the changing nature of self-employment, the OLS bias goes in the opposite direction: improved economic conditions, correlated with implementation of SCTs, usually improve labor market conditions, reducing entrepreneurship on the margin. This is precisely the endogenous dimension of variation in SCTs that we do not want to account for. Our IV strategy deals with this problem and, in the end, leads to a stronger estimated effect of SCTs on entrepreneurship.

Also notice that the results from Table 4 hold conditional on various munic-

⁸ The point estimates for employer and self-employed do not sum to the effect on entrepreneurs because of the differences in the sample (see note 7).

ipality characteristics associated with infrastructure and public goods provision, such as coverage of water and sanitation, urbanization, and overall population level. Therefore, it does not seem to be the case that our instrument—which hinges on the interaction of a dummy for judiciary district headquarters and the timing of approval of the law—is capturing changes in institutional quality or public goods provision. The inclusion of population as an independent variable is particularly important, since Table 3 shows that population levels and changes seemed to be correlated with the implementation of SCTs between 1991 and 2000. So it is reassuring that our IV estimation results are stronger than the OLS regression results, even controlling for population.

Table 5 presents the results from the first stage of our IV strategy, to highlight the strength of our instrument and the type of variation that it is capturing.⁹ In the table, we also include some additional controls and change the sample in various ways, to anticipate part of the discussion that appears in the robustness section. The benchmark first-stage result corresponds to the IV exercises from Table 4.

Column 1 restates the result from Table 3 in a panel setting: the coefficient on the instrument indicates that, for headquarters of judiciary districts, the likelihood of receiving SCTs after the approval of the law was roughly 51 percentage points higher than that for other municipalities, conditional on population and on all other independent variables mentioned previously. The *F*-statistic of the instrument—displayed at the bottom of the Table 3—shows no evidence of a weak instrument problem. Our instrument is indeed a strong predictor of SCT implementation.

Maybe the most surprising results in Table 5 occur in column 2, where we include income per capita (log) as an additional control. Although income per capita may be directly affected by SCT implementation and by entrepreneurship, and therefore does not belong to the right-hand side of this equation, we believe that its inclusion highlights the type of variation captured by our instrument and helps lessen potential concerns related to the exclusion restriction in our IV strategy. The inclusion of income per capita as an additional control has virtually no effect on the first-stage coefficient or *F*-statistic of the instrument. This means that the higher probability of receiving an SCT experienced by judiciary district headquarters was not associated with different economic performances experienced by these localities during the period. In fact, the impact of the instrument on SCT adoption seems to be entirely orthogonal to income per capita.

In columns 3–5, while keeping income per capita as a control, we restrict the sample along the time-series and cross-sectional dimensions, to address concerns

⁹ In reality, the first stage varies slightly in each exercise—from entrepreneurs to self-employed and employers, as well as from individuals with high education to those with low education—since some of the controls are constructed based on the individuals being analyzed. But, in fact, these differences are minuscule, and results are almost identical across the different samples. We present in Table 5 the results for the sample that includes all individuals.

Table 5
 Effect of Judiciary District Headquarters on the Presence of a Special Civil Tribunal (SCT) after 2000 in
 Minimum Comparable Areas (MCAs) in the State of São Paulo, 1970–2010

| | First-Stage Result (1) | Income per Capita as Control (2) | 1991–2010 (3) | MCA Population > 5,000 in 1970 (4) | 1991–2010 and MCA Population > 5,000 in 1970 (5) |
|---|------------------------------|---|-------------------|--|---|
| Instrument | .508** (.0588) | .507** (.0578) | .508** (.0635) | .454** (.0600) | .457** (.0655) |
| Income per capita as control | No | Yes | Yes | Yes | Yes |
| 1991–2010 | No | No | Yes | No | Yes |
| Population in 1970 > 5,000, with state capital excluded | No | No | No | Yes | Yes |
| F-statistic of instrument | 74.61 | 77.12 | 63.95 | 57.4 | 48.59 |
| N | 2,835 | 2,835 | 1,701 | 2,235 | 1,341 |
| R ² | .937 | .938 | .935 | .917 | .914 |

Note. Data are ordinary least squares estimates obtained by first-stage regressions. The dependent variable is the presence of an SCT in a municipality. Independent variables (not shown) are municipality averages of male gender, age, car ownership per household, number of rooms per household, migrant status, and high school education or greater (among those with occupations); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects (not shown). The instrument is the interaction of a variable indicating the presence of judiciary district headquarters with a variable indicating years from 2000 onward. Census data are from 1970 to 2010 and are restricted to individuals between ages 25 and 65. Robust standard errors, clustered by MCAs, are in parentheses.

** $p < .01$.

related to heterogeneity. First, since SCTs were implemented only between 1991 and 2000, we exclude the long baseline period from 1970 to 1980, when there was no actual treatment occurring. Even though the sample is reduced by more than 1,000 observations, the results remain virtually identical. In column 4, we exclude from the sample MCAs that had fewer than 5,000 inhabitants in 1970 and the state capital. The smallest MCAs receiving SCTs in the mid-1990s had slightly more than 5,000 inhabitants in 1970. In addition, the state capital is a national metropolis with greater weight in the estimation and a very peculiar economic context. So by restricting the sample this way, we create a more homogeneous set of control and treatment groups. In this case, the coefficient is reduced by roughly 5 percent of its initial value but stays strongly significant, with an F -statistic of 57.4. Finally, in column 5, we restrict the sample along the time-series and cross-sectional dimensions simultaneously. Although the number of observations falls by more than 50 percent of the initial value, the coefficient on the instrument remains almost identical to that from column 4 and very close to that from column 1.

In short, Table 5 indicates that our IV regression results are not driven by spurious correlation between our instrument and entrepreneurship or by heterogeneity across time or MCAs. This suggests that the identification assumptions discussed before hold within our empirical setting.

4.2. Robustness and Placebo Exercises

We concentrate the remainder of the analysis on the entrepreneurship variable and on individuals with high education since they seem to be the most affected by the implementation of SCTs. Table 6 follows the same variations in the sample and the introduction of an additional control analyzed in the first-stage regression in Table 5. In columns 1–3, we restrict the sample to the years between 1991 and 2010; then to MCAs with a population greater than 5,000 inhabitants in 1970, excluding the state capital; and finally to both criteria simultaneously. Columns 4–6 of Table 6 repeat this same sequence of regressions but add income per capita (log) as an additional control. These estimates should be compared with the coefficients for IV estimations, with high education as the dependent variable (Table 4).

Columns 1–3 of Table 6 show that there is very little change in the estimated coefficient when we restrict the sample, both in the time-series and the cross-sectional dimensions. First, when we restrict to years 1991–2010, the coefficient rises a bit, by roughly 10 percent. When we restrict the sample to MCAs with more than 5,000 inhabitants in 1970, excluding the state capital, the coefficient is reduced by 22 percent, and it then goes back to almost the initial point estimate from Table 4 when the sample is restricted simultaneously in both dimensions.

When we include income per capita as an additional control, coefficients remain very similar but are slightly less precisely estimated. Once again, income per capita may be endogenous to SCT implementation and to entrepreneurship, so it is not clear that it belongs to the right-hand side of this equation. Still, its

Table 6
Instrumental Variable Estimates of the Effect of Special Civil Tribunal (SCT) Presence on Entrepreneurship among Individuals with High Education in Restricted Samples of Minimum Comparable Areas (MCAs) in São Paulo, 1970–2010

| | Entrepreneur | | | High Education | | |
|------------------------------|--------------------|---|---|--------------------|---|---|
| | 1991–2010 (1) | Population > 5,000 in 1991–2010 and Population 1970, with State Capital Excluded (2) | Population > 5,000 in 1991–2010 and Population 1970, with State Capital Excluded (3) | 1991–2010 (4) | Population > 5,000 in 1991–2010 and Population 1970, with State Capital Excluded (5) | Population > 5,000 in 1991–2010 and Population 1970, with State Capital Excluded (6) |
| SCT | .0332** (.0112) | .0229* (.0105) | .0299* (.0127) | .0319** (.0114) | .0197+ (.0104) | .0286* (.0129) |
| Income per capita as control | No | No | No | Yes | Yes | Yes |
| N | 1,701 | 2,230 | 1,338 | 1,701 | 2,230 | 1,338 |
| R ² | .899 | .869 | .903 | .901 | .871 | .904 |

Note. The dependent variable is the municipality average of entrepreneurs among individuals with high education (high school education or above). Independent variables (not shown) are municipality averages of male gender, age, car ownership per household, number of rooms per household, and migrant status (among those with occupations used to construct the dependent variable); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects (not shown). The instrument is the interaction of a variable indicating the presence of judiciary district headquarters with a variable indicating years from 2000 onward. Census data are from 1970 to 2010 and are restricted to individuals between ages 25 and 65. Robust standard errors, clustered by MCA, are in parentheses.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

inclusion helps us understand that our main results are not driven by overall changes in economic conditions correlated both with SCTs and with the location of judiciary district headquarters. Even when we control for income per capita, locations that received SCTs experienced a change in occupational structure in the direction of relatively more entrepreneurship. We restrict ourselves to the more homogeneous sample of MCAs with populations above 5,000 inhabitants in 1970, excluding the state capital, in the remainder of this paper.

Up to this point, we have not yet addressed the issue of the previous existence of SCCs in some of the localities that later received SCTs. In 1991, 17 municipalities had an SCC in operation. Despite the limited role played by these courts, discussed in detail in Section 2, we address explicitly their potential effects on our estimation. It is not clear how one should deal with the SCCs. They may be thought of as previous versions of SCTs, in which case the treatment variable considered should be whether an MCA had either an SCC or an SCT. Alternatively, they may be considered a nuisance in the estimation, in which case one might want to exclude from the estimation localities that had an SCC. Finally, they may be seen as a competing force against which SCTs should be compared, in which case one might want to control for the previous presence of SCCs. Since it is not clear what the best alternative is, we adopt these three strategies one at a time.

Table 7 shows the results from these exercises. In columns 1–3, we define a dummy indicating the presence of either an SCC or an SCT as the treatment variable and follow the same IV strategy discussed previously. Each of the columns considers different sample periods: 1970–2010, 1980–2010, and 1991–2010. The coefficients are estimated less precisely, since our first-stage regression in this case is weaker than before, but they remain positive and statistically significant at the 10 percent level. Point estimates, shown in columns 2 and 3, are substantially higher than those obtained previously, possibly indicating a potential problem of weak instrument. Still, the results support the idea of a causal effect of the combination of SCCs and SCTs on entrepreneurship. In columns 4–6, we exclude from the sample the municipalities that had an SCC in 1991, and we again conduct the same IV estimation adopted previously, considering different sample periods for the analysis. The point estimates are close to those obtained in Table 6 but are slightly less precise. Finally, in columns 7–9, we include an SCC dummy as an additional control and repeat the same IV strategy for the SCT variable. The point estimates on the SCT variable are again similar to those obtained previously and again somewhat less precisely estimated. The SCC dummy appears as negative and significant in two specifications. But we do not have an adequate instrument for the SCC variable, so it is difficult to attach a causal interpretation to this coefficient. In any case, the inclusion of SCCs in the analysis, irrespective of how it is done, does not change much the estimated effect of SCTs on entrepreneurship, despite making estimates slightly less precise.

In Table 8, we conduct an additional set of exercises that tries to deal with any remaining concern about the possibility of violation of exclusion restrictions.

Table 7
Instrumental Variable Estimation of the Effect of Special Civil Tribunals (SCTs) on Entrepreneurship, Accounting for Previous Presence of Small Claims Courts (SCCs), among Individuals with High Education in Average-Sized Minimum Comparable Areas (MCAs) in the State of São Paulo, 1970–2010

| | Treatment as SCC or SCT (Instrumented) | | | Excluding Municipalities with an SCC in 1991 | | | SCC as Control | | |
|----------------|---|-------------------------------|-------------------------------|---|-------------------------------|-------------------|-------------------------------|-------------------------------|---------------------|
| | 1970–2010 (1) | 1980–2010 (2) | 1991–2010 (3) | 1970–2010 (4) | 1980–2010 (5) | 1991–2010 (6) | 1970–2010 (7) | 1980–2010 (8) | 1991–2010 (9) |
| SCT or SCC | .0384 ⁺ (.0201) | .0420 ⁺ (.0216) | .0579 ⁺ (.0339) | | | | | | |
| SCT | | | | .0179 (.0109) | .0215 ⁺ (.0116) | .0313* (.0139) | .0193 ⁺ (.0107) | .0209 ⁺ (.0112) | .0263* (.0131) |
| SCC | | | | | | | -.00882* (.00435) | -.00875* (.00441) | -.00742 (.00499) |
| N | 2,230 | 1,784 | 1,338 | 2,150 | 1,720 | 1,290 | 2,230 | 1,784 | 1,338 |
| R ² | .854 | .863 | .867 | .855 | .869 | .892 | .87 | .883 | .904 |

Note. The dependent variable is the municipality average of entrepreneurship among individuals with high education (high school education or above). Independent variables (not shown) are municipality averages of male gender, age, car ownership per household, number of rooms per household, and migrant status (among those with occupations used to construct the dependent variable); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects (not shown). The instrument is the interaction of a variable indicating the presence of judiciary district headquarters with a variable indicating years from 2000 onward. The placebo for 1980 is a dummy equal to one for the presence of judiciary district headquarters in 1980; the placebo for 1991 is defined analogously. Census data are from 1970 to 2010 and are restricted to individuals between ages 25 and 65 and to municipalities with a population greater than 5,000 in 1970, excluding the state capital. Robust standard errors, clustered by MCA, are in parentheses.

⁺ $p < .1$.
^{*} $p < .05$.

Table 8
Effect of Special Civil Tribunals (SCTs) on Entrepreneurship, According to Instrumental Variables (IV) Reduced-Form (RF) Estimates and Placebo Tests, among Individuals with High Education in Average-Sized Minimum Comparable Areas (MCAs) in the State of São Paulo, 1970–2010

| | RF Estimate | | | | | | | | | | | |
|----------------------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|-------------------|--------------------|--------------------|----------------------|--|--|
| | IV Estimate | | | Without Placebo | | | With Placebo | | | Consecutive Censuses | | |
| | 1980 (1) | 1991 (2) | 1991 (3) | 1980 (4) | 1991 (5) | 1991–2010 (6) | 1991–2000 (7) | 1970–80 (8) | 1980–91 (9) | | | |
| SCT | .0237* (.0111) | .0290** (.0109) | | | | | | | | | | |
| Instrument | | | .0103* (.00513) | .0110* (.00561) | .0133* (.00543) | .0135* (.00673) | .0131 (.00896) | | | | | |
| Judiciary district headquarters: | | | | | | | | | | | | |
| In 1980 | .00128 (.00600) | | | .00253 (.00693) | | | | -.00139 (.0135) | | | | |
| In 1991 | | .00604 (.00421) | | | .00658 (.00472) | | | | .00911 (.00699) | | | |
| N | 2,230 | 2,230 | 2,230 | 2,230 | 2,230 | 1,338 | 892 | 892 | 892 | | | |
| R ² | .869 | .868 | .87 | .87 | .871 | .906 | .915 | .871 | .902 | | | |

Note. The dependent variable is the municipality average of entrepreneurship among individuals with high education (high school education or above). Independent variables (not shown) are municipality averages of male gender, age, car ownership per household, number of rooms per household, and migrant status (among those with occupations used to construct the dependent variable); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects (not shown). The instrument is the interaction of a variable for the presence of judiciary district headquarters with a variable indicating years from 2000 onward. The placebo for 1980 is a dummy equal to one for the presence of judiciary district headquarters in 1980; the placebo for 1991 is defined analogously. Census data from 1970 to 2010 and are restricted to individuals between ages 25 and 65 and to municipalities with a population greater than 5,000 in 1970, excluding the state capital. Robust standard errors, clustered by MCAs, are in parentheses.

* $p < .05$.
 ** $p < .01$.

The main problem in this direction would be if judiciary district headquarters—which are used to construct our instrument—had intrinsically different economic dynamics. In this scenario, we might detect an impact of SCTs on entrepreneurship simply due to a spurious correlation between our instrument and local economic conditions. Although we tried to address this question in Table 6 by introducing income per capita as a control, we go one step further here and explicitly analyze whether district headquarters had a different behavior before the SCTs law was approved in the mid-1990s. If district headquarters were intrinsically different, one should expect this different behavior to manifest itself already before the implementation of SCTs.

We conduct this exercise in three ways. First, we include in our IV estimation placebo dummy variables that equal one for the presence of headquarters of judiciary districts in years before the approval of the law: 1980 and 1991. For 1991, the placebo equals one only for those MCAs that did not have a SCC since we do not want to confound the placebo effect with the issues discussed in Table 7. If our instrument displays a spurious correlation with local economic conditions, these dummy variables should appear to be statistically significant when included in our regressions. We run a reduced-form regression—where entrepreneurship is regressed directly on our instrument—and also include these same placebo variables as controls. Finally, also using reduced-form specifications, we restrict the sample to consecutive years and analyze when the behavior of entrepreneurship in headquarters of judiciary districts started being different from other municipalities: we restrict the sample to 1991–2010 and 1991–2000 and regress entrepreneurship on our instrument; we then restrict the sample to 1970–80 and regress entrepreneurship in our 1980 placebo; and, finally, we restrict the sample to 1980–91 and regress entrepreneurship in our 1991 placebo variable.

Table 8 presents these results. Columns 1 and 2 show that, in the setting of an IV estimation, placebo variables that identify headquarters of judiciary districts in years before the SCT law was approved do not appear to be statistically significant. Both placebos have small and not statistically significant point estimates, while our instrumental SCT variable remains positive and significant.

In column 3, we run a reduced form of entrepreneurship on our instrument. Not surprising, the instrument appears as positive and statistically significant, indicating that headquarters of judiciary districts experienced relative increases in entrepreneurship after the approval of the SCT law. Next, in columns 4 and 5, we again include the placebo variables for 1980 and 1991 as additional controls. As in columns 1 and 2, neither placebo variable appears to be statistically significant, while the coefficient on the instrument has the same order of magnitude and significance. In column 6, when we analyze years adjacent to the SCT implementation, our instrument appears positive and statistically significant, with a point estimate very similar to that shown previously. Finally, in column 7, when the sample is restricted to 1991 and 2000, which encompasses a relatively short period of SCT operation, the coefficient remains with almost identical magnitude but is much less precisely estimated. In columns 8 and 9, where we

restrict the sample to 1970–80 and 1980–91, respectively, the placebo variables again do not appear to be statistically significant.

In other words, the different results from Table 8 indicate that headquarters of judiciary districts were associated with increased entrepreneurship after the approval of the SCT law in the mid-1990s but not before that. Unless one has in mind a story of spurious correlation between our instrument and entrepreneurship that works specifically between 1991 and 2000, the evidence indicates that the validity of the exclusion restriction should not be a concern.

4.3. *Mechanisms*

We documented in the previous sections a relationship between the introduction of the low-cost litigation technology represented by SCTs and entrepreneurship. Still, there are many potential mechanisms behind this link. Although we cannot directly address this issue in this paper, we present some suggestive evidence of the heterogeneity of the effect of SCTs across locations. We look at how the response of entrepreneurship to SCTs varies with initial characteristics of MCAs, hoping that this may shed light on the potential channels at work.

The literature on the economic effects of contract enforcement considers several channels through which courts' operation can affect firms' expected profits and access to external financing, the two dimensions that interact to determine the equilibrium level of entrepreneurship. These can be summarized by the following: access to formal credit; access to trade credit or other informal financial arrangements; decreased probability of expropriation, be it of physical assets or intellectual property; and general equilibrium effects.

Regarding access to formal credit, a number of empirical papers document that better functioning judiciary systems are positively correlated with volume of credit, entrepreneurship, and firm size (see, for example, Rajan and Zingales 1998). Furthermore, Lilienfeld-Toal, Mookherjee, and Visaria (2012) find that the introduction of tribunals that enforce debt repayment increases credit availability. However, SCTs are not entitled to enforce debt contracts with banks, which rules out a direct effect of their introduction on formal access to credit.¹⁰ Nonetheless, because they can be used to enforce contracts among different parties along the production chain, firms' future cash flows could be more credibly offered as collateral to suppliers after SCTs were in place, therefore increasing access to trade credit and informal financing. This might also end up indirectly increasing the availability of financing through banks. Similarly, one might expect to see an increase in the incidence of more general state-contingent contracts (as discussed in Anderlini, Felli, and Postlewaite 2007).

In addition, the probability of expropriation falls with better enforcement of contracts. Because there is lower risk that those renting or using capital—or even

¹⁰ According to Law No. 9.099/1995 (art. 8, sec. 1), only individuals, microenterprises, and small firms can start a litigation process in an SCT.

employees—steal resources, either physical or intellectual, returns to entrepreneurship are higher and rental markets in physical capital are more developed (Kumar, Rajan, and Zingales 2001). Kumar, Rajan, and Zingales (2001) find this type of effect in non-capital-intensive industries in a sample of European countries, arguing that physical capital is already guaranteed in Europe by a minimum rule of law. In contrast, Laeven and Woodruff (2007) show that, in Mexico, capital-intensive industries are the ones that benefit the most from better judicial protection, supposedly because the lack of property rights is more pervasive in the developing world.

Finally, general equilibrium effects in this context are unpredictable. If the increase in access to credit allows new suppliers to enter the market, then one would expect reduced price of inputs and positive effects on entrepreneurs' expected profits. Analogously, if new consumers or downstream firms entered the market, larger market size might also lead to higher profits (see the discussion in Laeven and Woodruff [2007]). But it could also be the case that increased access to credit would allow new competitors to finance their way into the market or existing competitors to expand their operations, adversely affecting expected profits of existing firms.

Our data do not allow us to isolate each mechanism at work. Since we use household data, we have no information on entrepreneurs' access to financial services or informal credit arrangements. Moreover, we do not have data on individual experiences with litigation. Instead, we conduct an exploratory exercise by analyzing the potential heterogeneity in responses across areas with different initial conditions. We look at the availability of credit and sectoral composition of employment just before the institutional reform and analyze whether entrepreneurship grew relatively more in MCAs that had more or less availability of credit and more or less employment concentrated in capital-intensive sectors. Although only exploratory, this analysis reveals the characteristics of regions that benefited the most from increased access to justice.

We explore this exercise in Table 9. In columns 1 and 2, we split the sample at the median value of credit per capita in 1997, a year before implementation of SCTs in São Paulo. In columns 3 and 4, we split the sample at the median share of occupation in capital-intensive industries in 1991. We adopt the simplest possible definition of capital-intensive activities, comprising transformation industry, retail, and transport and communication and excluding activities linked to agriculture, mineral extraction, construction, personal services, services to firms, social activities, and education.¹¹ Next, in column 5, we look only at

¹¹ In Table B3 we detail our choice of sectors and describe how we make industries compatible across censuses. We base our classification on aggregate figures from the Brazilian Census Bureau (Instituto Brasileiro de Geografia e Estatística) concerning the ratio of intermediate consumption related to raw materials and inputs to the gross value of production (gross revenues, for services). There is no readily available measure of the relative importance of physical capital by industry for Brazil. Some sectors that might be seen as capital intensive in certain developed countries—such as construction and agriculture—are typically not considered as such in Brazil, where labor is cheap

Table 9

Effect of Special Civil Tribunals (SCTs) on Entrepreneurship, According to Instrumental Variable Estimates, Heterogeneity, and Other Impacts, among Individuals with High Education in Average-Sized Minimum Comparable Areas (MCAs) in the State of São Paulo, 1970–2010

| | Credit per Capita in 1997 | | Capital Intensity in 1991 | | Credit and Capital above Median in 1991 (5) | With Occupation (6) |
|-----------------------|------------------------------|------------------------|------------------------------|------------------------|---|---------------------------|
| | Above Median (1) | Below Median (2) | Above Median (3) | Below Median (4) | | |
| SCT | .0396* (.0161) | .0203 (.0168) | .0321+ (.0182) | .0142 (.0119) | .0438+ (.0259) | .0164+ (.00889) |
| <i>N</i> | 1,315 | 915 | 1,315 | 915 | 905 | 2,235 |
| <i>R</i> ² | .874 | .858 | .894 | .735 | .894 | .92 |

Note. Dependent variables are municipality averages of the entrepreneurship dummy among individuals with a high education (a high school education or more) or with the occupation dummy. Independent variables (not shown) are municipality averages of gender, age, car ownership per household, number of rooms per household, and migrant status (among those with occupations used to construct the dependent variable); municipality averages of water coverage, sewage coverage, and urban status (among the overall adult population); and natural log of population. All regressions include MCA and year fixed effects. The instrument is the interaction of a variable for the presence of judiciary district headquarters with a variable indicating years from 2000 onward. Census data are from 1970 to 2010 and are restricted to individuals between ages 25 and 65 and to municipalities with a population greater than 5,000 in 1970, excluding the state capital. Robust standard errors, clustered by MCA, are in parentheses.

+ $p < .1$.

* $p < .05$.

municipalities above the median values of both credit and capital intensity. Finally, in column 6, we briefly consider one manifestation of general equilibrium effects, by looking at the impact of SCTs on occupation rates (for the overall population between ages 25 and 65).

The results in columns 1 and 2 show that entrepreneurship increased more where the initial levels of credit per capita were higher. The point estimate for the effect of SCTs is .040 and statistically significant for MCAs above the median and .020 and not significant for those below. Our reading of this pattern is that SCTs may have facilitated prospective entrepreneurs' relationship with finance providers where there was already a higher supply of credit. This would suggest that increase in supply of credit was not the main driving force behind the expansion in entrepreneurship. Rather, entrepreneurs seem to have become better able to access credit where it already existed.

Columns 3 and 4 show an analogous pattern in terms of share of occupation in capital-intensive sectors. Minimum comparable areas with initially higher occupation in capital-intensive sectors seem to have benefited the most from SCTs. The estimated coefficient equals .032 and is significant at the 10 percent level for MCAs above the median in 1991, while it equals .014 and is not significant for municipalities below the median. In column 5, when we look only

(although there is a large capital-intensive agricultural sector, when seen from the perspective of employment rather than production, agriculture is dominantly labor intensive).

at municipalities above the initial medians of credit per capita and capital intensity simultaneously, the coefficient becomes larger in magnitude (.044) and remains significant at the 10 percent level. This suggests that capital-intensive sectors were most harmed by lack of contract enforcement and, where there was credit available, benefited the most from increased access to justice. In other words, entrepreneurship increased more in areas that had initially higher levels of credit and a higher incidence of capital-intensive activities. Generally, these patterns are consistent with the idea that SCTs reduced the risk of expropriation of physical assets, similar to the findings of Laeven and Woodruff (2007).

Finally, column 6 suggests that SCTs may have had a positive effect on overall economic activity. The point estimate implies that SCT implementation was associated with an increase of 1.6 percentage points in the occupation rate for the population between ages 25 and 65. Still, the estimate is significant only at the 10 percent level, and additional research would be needed to further explore this possibility.

Our interpretation is that the implementation of SCTs affected entrepreneurship mainly through property rights enforcement. The finding that entrepreneurship increased more in areas with capital-intensive activities—the ones that were likely to benefit from the new litigation technology—supports the idea that a lower probability of expropriation increases expected returns in activities for which physical assets are critical resources.

5. Concluding Remarks

This paper analyzes the impact of easier access to the justice system on entrepreneurship. We explore the creation of SCTs in the Brazilian state of São Paulo during the 1990s. Special civil tribunals increased the geographic presence of the justice system, simplified judicial procedures, and increased the speed of adjudication of disputes (mainly through facilitation of agreements in early stages of the process). We find that implementation of SCTs increased entrepreneurship among more educated individuals, when entrepreneurship was defined as the probability that individuals are employers or self-employed. Preliminary evidence suggests that the increase in entrepreneurship was mainly driven by reduced risk of expropriation of physical assets. These results are particularly robust for the case of self-employment and do not seem to be related either to other changes in infrastructure or public goods provision at the local level or to preexisting trends in self-employment.

Historically, self-employment in Brazil has been particularly common among low-skilled workers. This pattern seems to have been changing in the recent past. The evidence analyzed here suggests that access to justice increased the return to high-skilled self-employment, therefore being a potentially important determinant of the observed change.

Appendix A

Examples of Cases Resolved by the Special Civil Tribunals

A1. Contract Nullification

The defendant, a contractor, had verbally agreed with the plaintiff about the delivery of constructions services—construction material and labor—for a fixed amount, at least part of which was to be paid in advance. The plaintiff claimed that the defendant had not fulfilled his contractual obligations in exchange for her payment in advance. Based on the defendant's absence on the day of the trial, the judge presumed the plaintiff's allegations to be true, ruling the contact void and that the paid amount (in real terms) should be transferred back to the plaintiff.

The process was concluded on December 15, 2010 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0014181-42.2010.8.26.0006).

A2. Contract Enforcement

The defendant, a company that provides food-only debit card transaction terminals, had signed a standard-form contract with the plaintiff, a restaurant, to provide such services and to transfer the payments made through the system, net of a two-part tariff—a fixed fee and a variable fee—to be returned to the plaintiff within 30 days of each payment. The plaintiff claimed that the defendant did not reimburse the plaintiff after transactions of were made by plaintiff's patrons using the defendant's debit card terminal over a specific period. However, the plaintiff failed to provide evidence (other than their own verbal account of the incident) for the accusation, whereas the defendant provided receipts for the reimbursements processed over the period. The judge ruled against the claim because of a lack of evidence.

The process was concluded on March 16, 2010 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0210690-80.2009.8.26.0005).

A3. Limited Liability

The plaintiff, a business owner, had signed a debt contract with the defendant, a commercial bank, on behalf of the plaintiff's firm, with the goal of financing working capital. The plaintiff claimed that, upon failure to repay company's debt, the defendant had forcefully transferred resources from the latter's personal account—in the same bank—to cover for the corporate debt of the plaintiff's firm. On the basis of evidence that the loan was taken to finance working capital, and on the basis of the principle of limited liability, the judge ruled that the defendant should refund the amount and financially compensate the plaintiff for damages (*danos morais*).

The process was concluded on May 31, 2010 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0346693-48.2009.8.26.0100).

A4. Enforcement of Transaction: Example 1

The plaintiff, a salesperson, had acquired a vehicle from the defendant, a car dealer, with the purpose of reselling it. The plaintiff claimed that, despite having paid for it and received the car in exchange, the defendant had failed to transfer it under plaintiff's name, preventing him from reselling it. On the other hand, the defendant claimed that, at the time the contract was signed, the standard procedure was to leave such transfer under buyer's responsibility when the purchase was made with the purpose of resale. The judge ruled that the transfer be made effective immediately by the defendant, rejecting the argument that the dealer should undertake different procedures when selling a car for a reseller as opposed to a final customer, considering the transfer to be an integral part of the transaction.

The process was concluded on September 1, 2010 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0004225-05.2010.8.26.0005).

A5. Enforcement of Transaction: Example 2

The plaintiff, a taxi driver, had acquired a vehicle from the defendant, another taxi driver, as verbally agreed. The plaintiff claimed that, along with the physical transfer of the vehicle, the parties had verbally agreed to the transaction of the defendant's affiliation to a cab service in exchange for an extra payment. As the defendant disputed such agreement, neither the extra payment was made nor the affiliation transaction made effective. Moreover, due to the litigation between the parties, the defendant refused to pay the final installment of the vehicle before the issue on the affiliation transaction was settled. The mediation session, which precedes the judgment, reached an agreement concerning the payment of the final installment but not about the affiliation transaction, which would be judged by a special civil tribunal judge in a future trial.

The process was concluded on June 4, 2006 (Chasin 2007, p. 139).

A6. Collateral Execution

The plaintiff, a creditor although not a financial institution, had signed a debt contract with the defendant, who provided in writing a set of assets as collateral. The plaintiff claimed that the defendant, who failed to repay his debt, denied transferring the collateralized assets, claiming those were not his property but rather his mother's. On the basis of the principle that the defendant, should that be the case, was obliged to fulfill his commitment with different means, and on the basis of the evidence presented by the plaintiff about the signed contract and the lack of evidence of debt repayment, the judge ruled that—with the exception of a refrigerator, the only asset with receipt under the defendant's mother's name—all collateralized assets be transferred to the plaintiff.

The process was concluded on December 11, 2012 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0005821-58.2010.8.26.0220).

A7. Enforcement of State-Contingent Payment

The plaintiff, a creditor although not a financial institution, had signed a debt contract with the defendants, who provided a blank check as collateral should principal plus interest not be repaid, as verbally agreed. The plaintiff claimed that, on defendants' failure to repay the debt, the former filled in the check with the verbally agreed amount, but on deposit found out that the latter's bank account had insufficient funds. On the other hand, the defendants claimed the checks were filled in with an abusive amount, due to an implied interest rate higher than the one verbally agreed. On the basis of the principle that the claim of abusive interest rates could be supported only by evidence or by witnesses' accounts—which the defendants had failed to provide—the judge ruled that the defendants transfer the amount to the plaintiff, with checks recognized as valid instruments of state-contingent payments.

The process was concluded on December 17, 2012 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0010057-82.2012.8.26.0220).

A8. Compensation for Undue Charges

The plaintiff had signed a debt contract with the defendant, a nonbank financial company. The plaintiff claimed that, despite having paid back the contracted amount within the period specified in writing, the defendant had sued the plaintiff, which prevented the latter from accessing new loans (with this financial company or elsewhere). The plaintiff requested not only that the defendant remove the plaintiff's name from the system of unmet debt obligations (credit bureau Serasa) but also that the company financially compensate the plaintiff for the damages (*danos morais*) suffered as a result of the public display of such undue charges. The mediation session, which precedes the judgment, reached an agreement concerning the defendant taking action to remove the plaintiff's name from SERASA but not about the amount to be paid in compensation for plaintiff's moral losses. In the subsequent judgment concerning the unsettled issue, the judge ruled the defendant to compensate the plaintiff for damages (*danos morais*) in an amount in between that demanded by the plaintiff and that offered by the defendant during the mediation session.

The process was concluded on March 13, 2007 (Chasin 2007, p. 134).

A9. Enforcement of Good Faith

The plaintiff had signed a contract with the defendant, an online provider of monthly discount coupons, in exchange for a fixed monthly fee. The contract specified access not only to the specified coupons but also to an array of prizes in exchange for enrolling additional customers to the defendant's business model. The plaintiff claimed that the contract delegated to the customer all costs of enrolling additional customers, including assistance to the defendant's new patrons, and that the prizes offered constituted delusive advertising since their attainment was based on a condition that is impossible to meet at some point—a Ponzi scheme. The defendant, who was faced with multiple law suits of similar

nature,¹² failed to attend the trial, leading the judge to presume that the plaintiff's allegations to be true. The judge ruled the contract void, and ordered the defendant to financially compensate the plaintiff for damages (*danos morais*) due to lack of good faith.

The process was concluded on June 5, 2012 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0030050-44.2011.8.26.0577).

A10. Partnership Dissolution

The plaintiffs had a partnership with the defendant, a house-building cooperative. The plaintiffs sought to dissolve their relationship with the defendant and to be reimbursed for previous investments. An agreement was reached during the mediation session, through which the parties agreed to the financial amount to be paid back to the plaintiffs. The judge ruled that the partnership be dissolved and that the amount be paid in installments, as agreed by the parties, setting a 30 percent penalty if the defendant failed to make installment payments.

The process was concluded on November 22, 2010 (São Paulo Justice Tribunal, Special Civil Tribunal's Procedure No. 0023158-20.2010.8.26.0007).

Appendix B

Census Definitions and Distributions of Activities

B1. Definitions of Variables across Censuses

Since the questionnaires for Brazilian censuses change every survey year, variables were built so as to guarantee consistency over time. In particular, the indicators of access to water, electricity supply, and sewage collection denote whether the household has access to the general distribution network. The variable for migrant status indicates whether the individual has always lived in that municipality.

For schooling, matching categories over time is not straightforward because the classification for elementary school and high school changed in Brazil between 1970 and 1980. In the latter censuses but not the earlier ones, both the old and new classification systems coexist. We account for changes in the classification system by inputting schooling years.

Last, dependent variables always reflect individuals' position in their main occupation. The specific census questions used in our analysis and the coding of our variables are described in detail in Tables B1 and B2, and the list of schooling definitions follows.

¹² See, for instance, Processo No. 526.01.2011.007391-3/000000-000, Order No. 1781/2011, Declaratory (in general), Manoel Castorino Biao x Mundialline Dos Santos Ltd, Visas, Diário Oficial dos Estados São Paulo [D.O.E.S.P.], 02.12.2011, 291 (Braz.) (<http://www.jusbrasil.com.br/diarios/32836386/djsp-judicial-1a-instancia-interior-parte-iii-02-12-2011-pg-291>).

Table B1
Definition of Independent Variables by Year, by Census Codes

| | 1970 | 1980 | 1991 | 2000 |
|-------------|----------------|---------------|----------------|-----------|
| Male | VAR23 = 0 | V501 = 1 | V0301 = 1 | V0401 = 1 |
| Age | VAR27 | V606 | V3072 | V4752 |
| Urban | VAR4 = 0 or 1 | V598 = 0 | V1061 = 1 or 3 | V1006 = 1 |
| Water | VAR12 = 1 or 2 | V206 = 1 or 6 | V0205 = 1 or 4 | V0207 = 1 |
| Sewage | VAR13 = 1 | V207 = 2 | V0206 = 1 | V0211 = 1 |
| Electricity | VAR14 = 1 | V217 = 2 or 4 | V0221 = 1 or 2 | V0213 = 1 |
| Car | VAR19 | V221 | V0218 | V0222 |
| Rooms | VAR20 | V212 | V0211 | V0203 |
| Migrant | VAR32 < 8 | V513 = 8 | V0314 = 2 or 3 | V0415 = 2 |

Table B2
Definition of Dependent Variables by Year, by Census Codes

| | 1970 | 1980 | 1991 | 2000 |
|---------------|----------------|-----------------|-------------------|------------------|
| Employer | 1 if VAR46 = 5 | 1 if VAR533 = 7 | 1 if VAR0349 = 10 | 1 if VAR0447 = 5 |
| Self-employed | 1 if VAR46 = 3 | 1 if VAR533 = 8 | 1 if VAR0349 = 9 | 1 if VAR0447 = 6 |

Note. For all census years, entrepreneur = employer + self-employed.

B1.1. Definition of Schooling in the 1970 Census

- Equals 1 if (VAR38 = 1 and VAR37 = 1) or (VAR38 = 1 and VAR37 = 2)
- Equals 2 if (VAR38 = 1 and VAR37 = 3)
- Equals 3 if (VAR38 = 1 and VAR37 = 4)
- Equals 4 if (VAR38 = 1 and VAR37 = 5)
- Equals 4 if (VAR38 = 1 and VAR37 = 6)
- Equals 4 if (VAR38 = 1 and VAR37 = 7)
- Equals 5 if (VAR38 = 2 and VAR37 = 2)
- Equals 6 if (VAR38 = 2 and VAR37 = 3)
- Equals 7 if (VAR38 = 2 and VAR37 = 4)
- Equals 8 if (VAR38 = 2 and VAR37 = 5)
- Equals 8 if (VAR38 = 2 and VAR37 = 6)
- Equals 9 if (VAR38 = 3 and VAR37 = 2)
- Equals 10 if (VAR38 = 3 and VAR37 = 3)
- Equals 11 if (VAR38 = 3 and VAR37 = 4)
- Equals 11 if (VAR38 = 3 and VAR37 = 7)
- Equals 12 if (VAR38 = 4 and VAR37 = 2)
- Equals 13 if (VAR38 = 4 and VAR37 = 3)
- Equals 14 if (VAR38 = 4 and VAR37 = 4)
- Equals 15 if (VAR38 = 4 and VAR37 = 5)
- Equals 16 if (VAR38 = 4 and VAR37 = 6)

B1.2. Definition of Schooling in the 1980 Census

- Equals 1 if (VAR523 = 2 and VAR524 = 1) or (VAR523 = 4 and VAR524 = 1)
 Equals 2 if (VAR523 = 2 and VAR524 = 2) or (VAR523 = 4 and VAR524 = 2)
 Equals 3 if (VAR523 = 2 and VAR524 = 3) or (VAR523 = 4 and VAR524 = 3)
 Equals 4 if (VAR523 = 2 and VAR524 = 4) or (VAR523 = 4 and VAR524 = 4)
 Equals 4 if (VAR523 = 2 and VAR524 = 5)
 Equals 4 if (VAR523 = 2 and VAR524 = 9)
 Equals 5 if (VAR523 = 3 and VAR524 = 1) or (VAR523 = 4 and VAR524 = 5)
 Equals 6 if (VAR523 = 3 and VAR524 = 2) or (VAR523 = 4 and VAR524 = 6)
 Equals 7 if (VAR523 = 3 and VAR524 = 3) or (VAR523 = 4 and VAR524 = 7)
 Equals 8 if (VAR523 = 3 and VAR524 = 4) or (VAR523 = 3 and VAR524 = 5)
 or (VAR523 = 3 and VAR524 = 9) or (VAR523 = 4 and VAR524 = 8)
 or (VAR523 = 4 and VAR524 = 9)
 Equals 9 if (VAR523 = 5 and VAR524 = 1) or (VAR523 = 6 and VAR524 = 1)
 Equals 10 if (VAR523 = 5 and VAR524 = 2) or (VAR523 = 6 and VAR524 = 2)
 Equals 11 if (VAR523 = 5 and VAR524 = 3) or (VAR523 = 5 and VAR524 = 4)
 or (VAR523 = 5 and VAR524 = 9)— (VAR523 = 6 and VAR524 = 3)
 or (VAR523 = 6 and VAR524 = 4) or (VAR523 = 6 and VAR524 = 9)
 Equals 12 if (VAR523 = 7 and VAR524 = 1)
 Equals 13 if (VAR523 = 7 and VAR524 = 2)
 Equals 14 if (VAR523 = 7 and VAR524 = 3)
 Equals 15 if (VAR523 = 7 and VAR524 = 4)
 Equals 16 if (VAR523 = 7 and VAR524 = 5)
 Equals 17 if (VAR523 = 7 and VAR524 = 6)

B1.3. Definition of Schooling in the 1991 Census

Equals VAR3241

Equals 0 if (VAR3241 = 20 or VAR3241 = 30)

B1.4. Definition of Schooling in the 2000 Census

Equals VAR430

Equals 0 if (VAR4300 = 20 or VAR4300 = 30)

Table B3
Definition of Capital-Intensive Activities in Censuses, 1970–2000

| | 1970 | 1980 | 1991 | 2000 |
|--|-----------------------------|----------------------------|------------|-----------------------------|
| Non-capital-intensive sectors: | | | | |
| Agriculture, fishery, and vegetal extraction | VAR45 = 111–306 | V532 = 11–58 | V3461 = 1 | V4462 = 11–42 |
| Civil construction | VAR45 = 341–342 | V532 = 340 | V3461 = 3 | V4462 = 340 |
| Other industrial activities | VAR45 = 351–352 | V532 = 351–354 | V3461 = 4 | V4462 = 50–59, 351–354 |
| Auxiliary services to the economic activity | VAR45 = 921–928 | V532 = 571–589 | V3461 = 7 | V4462 = 571–589 |
| Personal services | VAR45 = 511–518 | V532 = 511–552 | V3461 = 8 | V4462 = 511–552 |
| Social | VAR45 = 711–721 | V532 = 610–632 | V3461 = 9 | V4462 = 610–632 |
| Public administration | VAR45 = 811–827 | V532 = 711–727 | V3461 = 10 | V4462 = 711–727 |
| Other activities | VAR45 = 911–916, 931–934 | V532 = 451–464, 801–902 | V3461 = 11 | V4462 = 451–464, 801–903 |
| Capital-intensive sectors: | | | | |
| Transformation industry | VAR45 = 311–334 | V532 = 100–290 | V3461 = 2 | V4462 = 100–300 |
| Commerce | VAR45 = 411–424 | V532 = 410–423 | V3461 = 5 | V4462 = 410–424 |
| Transports and communication | VAR45 = 611–620 | V532 = 471–482 | V3461 = 6 | V4462 = 471–482 |

Note. Other industrial activities include mineral extraction and industrial services of public utility. Auxiliary services to the economic activity include technical-professional and auxiliary services. Personal services include lodging and food catering, maintenance and conservation, personal services to households, and entertainment. Social activity includes social activity, medical activity, dentistry, and teaching.

B2. Distribution of Activities

Table B4
Distribution of Self-Employed and Employers by Activity, Brazil

| | Percent | Cumulative |
|---|---------|------------|
| Self-Employed: | | |
| Other or undefined | 18.74 | 18.74 |
| Civil construction industry | 14.84 | 33.58 |
| Food catering services | 7.93 | 41.51 |
| Informal commerce | 6.98 | 48.49 |
| Personal hygiene services | 4.72 | 53.21 |
| Road transport of passengers | 3.59 | 56.8 |
| Road transport of cargo | 3.58 | 60.38 |
| Commerce of food and beverages | 3.27 | 63.65 |
| Repair and maintenance of vehicles | 2.87 | 66.52 |
| Clothing industry | 2.66 | 69.18 |
| Legal assistance | 1.83 | 71.01 |
| Agriculture—other cultures | 1.75 | 72.76 |
| Private medical services | 1.61 | 74.37 |
| Private teaching | 1.31 | 75.68 |
| Cattle raising | 1.02 | 76.7 |
| Furniture industry | 1 | 77.7 |
| Commerce of clothing | .98 | 78.68 |
| Horticulture and floriculture | .95 | 79.63 |
| Cleaning and building conservation services | .95 | 80.58 |
| Employer: | | |
| Other or undefined | 13.31 | 13.31 |
| Food catering services | 8.33 | 22.14 |
| Commerce of food and beverages | 6.8 | 28.94 |
| Civil construction industry | 4.09 | 33.03 |
| Commerce of clothing | 3.99 | 37.02 |
| Commerce of construction and metallurgic material | 3.58 | 40.6 |
| Repair and maintenance of vehicles | 3.2 | 43.8 |
| Commerce of vehicles and accessories | 2.63 | 46.43 |
| Private medical services | 2.32 | 48.75 |
| Commerce of machinery and electrical material | 2.28 | 51.03 |
| Personal hygiene services | 2.25 | 53.28 |
| Legal assistance | 2.22 | 55.5 |
| Metallurgic industry | 2.12 | 57.62 |
| Clothing industry | 2.11 | 59.73 |
| Commerce of chemical and pharmaceutical products | 2.06 | 61.79 |
| Accounting and auditing | 1.95 | 63.74 |
| Food industry | 1.76 | 65.5 |
| Road transport of cargo | 1.54 | 67.04 |
| Private teaching | 1.34 | 68.38 |
| Consulting services | 1.26 | 69.64 |
| Commerce of fuel and lubricant | 1.19 | 70.83 |
| Editorial industry | 1.15 | 71.98 |
| Furniture industry | 1.06 | 73.04 |
| Informal commerce | 1.01 | 74.05 |
| Road transport of passengers | .99 | 75.04 |
| Engineering and architecture services | .98 | 76.02 |
| Dentist services | .97 | 76.99 |
| Agriculture—other cultures | .95 | 77.94 |
| Commerce of furniture, tapestry, and art | .95 | 78.89 |
| Commerce of office material | .95 | 79.84 |
| Cattle raising | .94 | 80.78 |

Source. Census data from 2000.

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