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Estimating model's predictions

Through the next subsections, model's predictions are taken to the data exploring an exogenous change in litigation costs under the creation of Special Civil Tribunals (Juizados Especiais Cíveis, JECs henceforth) in Brazil during the 1990's. JECs stand for a small claims' litigation technology that extended access to justice to the least favored individuals by providing legal assistance along with rapid conflict resolution. Drawing upon Census data to identify individual's occupational choice, we evaluate the impact of the creation of JECs on the probability of becoming an entrepreneur, as well as on firm size – what we take as a measure of investment – through a differences-in-differences approach, since we have information for municipalities before and after the institutional change, for control and treatment units.

The dimensions of contractibility and contract enforcement that index courts' operation in the model are not readily available for an empirical assessment: they are not objectively observed, are relationship-specific and may not vary within a broad institutional environment ¹. Conversely, a reduction in litigation costs can be interpreted as a transition from no courts to some contractibility for the individuals whose constraint is relaxed by such reduction in costs of access to justice. Accordingly, we expect for these individuals an increase in entrepreneurship whenever the ex-post bargaining power belongs to the entrepreneur, and an increase in investment when it is the other way round.

Next, we briefly summarize the empirical literature on the effects of litigation costs on economic outcomes before describing in some detail the institutional change represented by the creation of JECs in Brazil, as well as the previous institutional environment. Then, we describe our dataset and empirical strategy, followed by discussion of the results and some robustness exercises.

¹For example, in Brazil, there is no relevant variation in municipal-level legal standards in what comes to which states of nature should not allow excused performance for soybean producers concerning trade credit contracts with suppliers. In countries that abide by common law, jurisprudence at the national level has an additional compelling role over local-level decisions.

4.1 Review of literature

The empirical literature has mainly focused on particular mechanisms through which the legal system might discourage entrepreneurship – essentially looking at the credit market – without considering the decision to start a new venture itself (with the sole exception of Chemin, 2009a).

Japelli, Pagano e Bianco (2005) develops a model akin to Antunes, Cavalcanti and Villamil (2008) in what comes to regarding courts as capacity to execute foreclosure rights - in other words, to execute debtor's collateral on the event of default. The authors offer an alternative interpretation of the model, framing imperfect foreclosure as the monetary losses associated to litigation costs. Even though Chemin (2009a, 2009b) rely on this framework to assess the effects of a change in litigation costs on entrepreneurship and related economic outcomes, it stands as a theoretical stretch since general equilibrium effects of this change on entrepreneur's return as well as on equilibrium market wage are not assessed.

Their model's predictions are taken to the data using a panel of 95 Italian provinces merged with data for 27 judicial districts. The measures of judicial efficiency are (1) length of civil trials from 1984 to 1988 and (2) number of pending civil suits by thousand inhabitants. The paper carries out a differences-in-differences strategy in order to assess the impact of a variation in these measures of judicial efficiency (litigation costs) on loans-to-GDP ratio, on a measure of credit constraints and on interest rates.

Results are that the stock of pending trials is negatively correlated with the loans-to-GDP ratio and with the alleviation of credit constraints, displaying a positive correlation with the rate of non-performing loans to total lending. Nevertheless, not only the identification strategy is not clean in what comes to exploring an exogenous change in litigation costs but, as the own authors acknowledge, their indexes account for equilibrium - as opposed to supply - measures of litigation technology. In this sense, the backlog could be as high in inefficient districts as in efficient ones since lenders take the expected costs of litigation into account while setting their credit parameters. Finally, theory's prediction concerning the contingent effect on interest rates depending on market structure is not explicitly tested.

Chemin (2009a)'s empirical strategy is the closest to ours. Exploring a judicial reform in Pakistan that intended to decrease the backlog of court cases by teaching case-flow management techniques to judges in 6 pilot districts out of 117, the author explores a differences-in-differences strategy to evaluate the impact of the reform on judicial efficiency, and then of the former on the

transition probabilities from unemployment to self-employed or entrepreneur and from employment to self-employed or entrepreneur.

First, using panel data on judges' performance based on the annual reports by the Lahore and Peshawar High Courts and the High Courts of Balochistan and Sindh from 2001 to 2003, the paper assesses whether the mean number of cases disposed by judge has increased more in the districts where the reform was implemented. In fact, results are that judges in affected areas dispose significantly more cases than the ones where the reform has not taken place, even after controlling for mean reversal because of non-random adoption of the reform – pilot districts were chosen essentially because of poorer judicial performance.

Then, authors move on to evaluate the impact of the reform on entrepreneurship. Chemin uses the Labour Force Survey for Pakistan, which contains information on working conditions and firm characteristics, to account for several individual attributes that should theoretically be correlated with entrepreneurial status, including additionally several indicators of municipal development.

The estimated effect stands for the marginal effect on the transition probability from employment (or, alternatively, unemployment) to entrepreneurship or self-employment – defined as an affirmative response to survey questions corresponding to willingness to start a new venture or to become self-employed, respectively. As such, the author does not have information on the actual status of individuals on the labor market.

After documenting a positive and significant effect of the reform on personal perceptions concerning investment confidence and on the demand for credit, the paper reports positive and significant marginal effects of the reform on transition probabilities, although non-random adoption comes at stake once more: pilot districts were also the ones where willingness to set up a business or to become self-employed were already rising by the time of reform adoption.

The Indian context, characterized by a remarkably inefficient judicial system, has motivated three relevant papers: Visaria (2009), Mookherjee et al. (2009) and Chemin (2009b). The first two consider the effects of the creation of Debt Recovery Tribunals on the credit market, aligned with the common view that regards courts as capacity to execute collateral. From 1993 to 1999, DRTs were introduced in India as the institutional arena for legal disputes between banks and borrowers.

While Visaria (2009) takes advantage of their staggered adoption across Indian states in order to assess their impact on delinquency rates as well as on interest rates (using a loan-level database from a private Indian bank),

documenting a negative significant effect on both dimensions, Mookherjee et al. (2009) considers the possibility of an adverse impact of the institutional reform – through an increase in interest rates – because of general equilibrium effects due to inelastic supply of credit.

Indeed, econometric estimation based upon a firm-level panel data for the entire Indian economy acknowledges that small firms experienced a contraction in borrowing and fixed assets, whereas credit, fixed assets and profits increased for large firms. Interest rates are reported to have risen for all categories of firms; the contrast with Visaria (2009)'s results accrues to the choice of database – the former considers loans of every Indian bank, while the latter takes only one private bank into account.

Last, Chemin (2009b) investigates the impact of judiciary on development by looking at local amendments to the Code of Civil Procedures in Indian states between 1971 and 1996. The author identifies the amendments in the period enacted through Acts of Central and State Legislatures that can be labeled as 'Court/defendant/plaintiff red tape' – if they required additional procedures for the Court, the defendant or the plaintiff, respectively –, amendments that favored specific group's interests – poor, agriculture, business, Government, debtors –, among other.

In particular, Chemin labels amendments that leave discretion to the court to set time limits to trials, and those that were likely to decrease incentives to file a complaint. Amendments enacted with the specific purpose of speeding the litigation procedures were excluded from analysis because of endogenous adoption. Alternatively, the paper looks at every violation to precedent decision in the period in order to create a measure of conflicting judicial decisions.

The identification strategy consists of two steps: first, a differences-in-differences estimation assesses the impact of several types of amendments and of violation of precedents on the average duration of trials. This step relies on the assumption that these events are not correlated with unobserved features not fixed in time and correlated with the duration of trials at the state level. Although results show a positive and significant effect of 'Court red tape' amendments on average trial duration, for instance, amendments of other nature, as well as violation of precedents, are not reported to have an overall significant effect on the outcome of interest. When looking at the number of cases filed per year as an alternative dependent variable, results are even less robust through specifications.

The second step relies on evaluating the impact of trial duration on economic outcomes, also through a differences-in-differences approach, but

using as instrumental variables the amendments and conflicting decisions motivated in the first step. Although entrepreneurship is not directly assessed, some outcomes of interest are dealt with, such as the number of farmers that are able to obtain advancement of resources with a bank, much in the spirit of our theoretical framework.

Results are that trial duration impacts more negatively those farmers with lesser collateral, although this effect is just slightly significant. Concerning industry-specific effects, the paper also documents a negative effect of longer trials on manufacturing output of registered activities, as opposed to non-registered ones (which are not affected by instrumented duration, what is reassuring).

Therefore, our paper contributes to the empirical literature in three ways: first, it relies on an institutional change that affected the supply of the lower-cost litigation technology as identification strategy to assess the impact of an litigation costs on the outcomes of interest – although this institutional change might have not been completely exogenous, we perform a series of robustness exercises to show that the estimated effects can be indeed regarded as causal ones –; second, it uses actual entrepreneurial status as opposed to intention to start a new venture, and includes firm size as a measure of investment, what has been overlooked hitherto; and, third, it is the first one to evaluate the impact of the creation of a small claims' judicial technology on entrepreneurship and investment.

4.2 TPCs and JECs

The 1988 Constitution had as a major concern the broadening of fundamental rights and the design of the adequate mechanisms to guarantee them, including, among other goals, those of broadening the right of legal assistance to the needy - involving information, consulting, legal and extra-legal assistance -, and predicting the creation of special tribunals to judge and execute the civil actions of smaller complexity, with emphasis on procedural informality (Carneiro, 2003). These principles were materialized by the 9.099/1995 law that created the Special Civil Tribunals (JECs).

JECs have the competencies to judge actions of smaller complexity up to 40 minimum wages that concern consumer rights, debt execution, neighborhood conflicts, torts, etc.; conversely, JECs do not judge actions related to family law, labor justice, actions of bankruptcy, among others. Law 9.099/1995 authorized the execution of extra-legal executive bonds ² up to the

²These match exactly the kind of financial contract through which a supplier finances an entrepreneur, or informal debt contracts, more generally.

allowed value, made mandatory the presence of lawyers for actions between 20 and 40 minimum wages and, most importantly, designed their competence to execute their own decisions (Cunha, 2008). Moreover, in 1999, law 9.841/1999 extended JECs' competencies to included micro-firms, which from this date on could file complaints under the small claims' technology. After 1999, legal provisions concerning JECs stabilized (Cunha, 2008).

JECs were locally backed-up by state laws following the federal one; for instance, they were implemented starting in 1996 in Rio de Janeiro but only from 1998 on in São Paulo.³ Whenever the state law was passed, previous TPCs were converted into JECs, and from then on only JECs could be installed at the municipal-level.

Tourinho Neto and Figueira Júnior (2007) remark: "Regarding the 9.099/1995 law as a simple procedural norm is the major and most serious mistake one can make, since (...) its scope (...) [concerns] the creation of a new justice, different from all others, simple, agile, safe and effective" (Tourinho Neto and Figueira Júnior, 2007, p. 69). Besides that, the number of actions sharply increases after the installation of JECs, overcoming what the authors call "constrained litigiousness".

According to the National Database of the Judiciary (BNDPJ), in 1999 there were 2564 JECs operating in the country. In several states their load was even superior than that of common justice. The small claims' system expanded quickly, such that while in 1999 the proportion of actions submitted to JECs stood for 13.71% of the national total, this number was already 17.92% in 2003.

One might wonder that Special Civil Tribunals were not the first mechanism in Brazil to extend access to justice to the least favored ones. However, although equality in access to justice has been a constitutional principle in Brazil ever since the early 19th century, these concept has not been brought into practice until very recently (Carneiro, 2003). Even most of the 20th century in Brazil is denoted by a distorted notion of access to justice, based upon a mere formal concept of equity, having in the labor justice its sole exception (Carneiro, 2003).

Says Carneiro: "For the remaining branches of process law [excluding the labor justice], and especially civil law (...), until the 1980's the practice of our tribunals remained individualistic (...) [and] elitist(...)" (Carneiro, 2003, p. 41). Individualistic because grounded on the principle of formal equity but without concern to the effective access of the poor to the legal instruments of litigation. Elitist because "expensive, distant, mysterious and unknown, a true

³As informed by each State Tribunal of Justice.

arena where the richer, better prepared and with better lawyers, obtained the more positive results” (Carneiro, 2003, p. 41).

The 1980's testified the beginning of a legislative transformation. Law 7.224/1984 instituted Small Claim Tribunals in Brazil, which, amongst other goals, intended to decentralize justice, to privilege extra-legal conciliation as a conflict resolution device, to judge matters that practically were not taken to the traditional justice - avoiding therefore the creation of a parallel or unofficial justice -, to be costless and fast, debureaucratized, informal, equanimous and effective, and, finally, to loose the burden of traditional justice (Carneiro, 2003).

Between 1984 and 1988, though, few states actually implemented the physical and material structure of the Small Claims Tribunals (Cunha, 2008). In Rio de Janeiro, for instance, although the first Small Claims Tribunal was put in place in 1987, it was only in 1994 that the system started to grow effectively; this lack of expansion of the system until mid 1990's is a common feature with São Paulo (Cunha, 2008). Small Claims Tribunals (TPCs henceforth), informal instances of conciliation that handled actions which value did not exceed 20 minimum wages, were reported to operate under precarious conditions and to be ineffective, in a nutshell because "they lacked the competence to implement their legal decisions, which execution accrued to the common justice" (Cunha, 2008, p. 52).

With the 1988 Constitution, besides the prediction of creation of Special Civil Tribunals (JECs henceforth), stood up the principle of extending the competence of the tribunals in order to execute the actions of their own responsibility. In fact, between 1988 and 1995, when JECs were legally instituted, the number of TPCs increased considerably over the country, and some states - Mato Grosso do Sul, Santa Catarina e Paraíba - anticipated to the 9.099 law, implementing the competencies of state JECs even before the federal law (Cunha, 2008).⁴ In some of the empirical exercises of next subsections we try to disentangle the effects of JECs of those of TPCs on entrepreneurship and investment.

The National Institute of Geography and Statistics (IBGE) has numbers for the proportion of municipalities in each main region that had a JEC. In only 2 years, from 1999 to 2001, the proportion of Brazilian municipalities with at least one JEC increased from 28% to 34%. Nevertheless, this expansion has somewhat led to pronounced regional disparities. Looking for each main region in 2001, the number of municipalities with at least one JEC was 46% in Center-West, 43% in South, 40% in the Southeast, 28% in North and 20% in Northeast

⁴This anticipation was not without costs: the Supreme Court overruled some decisions in Paraíba, declaring it unconstitutional in 1994.

(IBGE, 2001). Moreover, the geographic distribution of JECs by state is very uneven: while 92% of the municipalities in Rio de Janeiro had a JEC by 2001, this number was 47% in São Paulo, 27% in Minas Gerais and 5% in Piauí (IBGE, 2001).

This motivates the study of the economic impacts of this institutional change for two reasons: first, heterogeneous adoption allows for impact evaluation ⁵, and second, the majority of Brazilian municipalities still lacks this technology, such that documenting a positive effect on the outcomes of interest might be an important part of formulation public policy.

4.3 Data and empirical strategy

Data from TPCs and JECs were provided by the Tribunal of Justice of São Paulo and by the Tribunal of Justice of Rio de Janeiro. ⁶ São Paulo and Rio de Janeiro are two of the biggest states in Brazil, adding up to 15% of Brazilian AMCs⁷ and 30.3% of Brazilian population in 2000, according to Census data.

Except for data on industry participation in GDP, drawn from IPEA-DATA⁸, all other variables were drawn from Census data (1970, 1980, 1991 and 2000). Working variables were always built to be compatible between years; Appendix 6.5 presents in detail the relevant compatibilization procedures. The schooling variable was created based upon Rigotti (2004)'s methodology. Average per capita income was deflated by the index proposed by Corseuil and Foguel (2002). We restrict attention to occupied individuals within 25 and 50 years old. Information on firm size is only available for 1991 and 2000. TPCs and JECs' variables were created having in mind that the state law which approved its creation in Rio de Janeiro dates from 1996, whereas in São Paulo the equivalent law dates from 1998.

⁵Nevertheless, endogenous treatment adoption is a concern, especially because there is no clear rule for the creation of TPCs or JECs. We come back to this in the robustness section.

⁶Unfortunately, there is not a centralized source of data concerning the dates of installation of small claims tribunals in each municipality and, surprisingly, these information are not widely available at the state Tribunals of Justice. For instance, the Tribunal of Justice of Minas Gerais was able to provide the dates of installation of new JECs from 1996 on, but could not identify which municipalities had a TPC that was converted into a JEC by the time of the institutional change. Furthermore, all other states in the South and Southeast region were not able to meet our inquiry.

⁷Minimum Comparable Areas (AMC) are territorial units defined by the Brazilian Institute for Geography and Statistics (Instituto Brasileiro de Geografia e Estatística, IBGE henceforth) that allow comparability through time since Brazil has faced a large increase in the number of municipalities over time.

⁸<http://www.ipeadata.gov.br> ; 1991 is an interpolation of 1985 and 1996.

As for our dependent variables, we take three measures of entrepreneurship: employer status, self-employed status and a compound variable that we call entrepreneur, given by the sum of the two former. We proceed in this way because self-employed can often be regarded as small entrepreneurs, especially when allocated to industries that deal with outsourcing of a variety of services. We provide a detailed account of the major activities in which both self-employed and entrepreneurs are involved with in our sample in tables 4.2 and 4.1. The distribution of individuals by activity is remarkably similar among employers and self-employed individuals, supporting the claim that these are actually not very different.

Concerning investment, the only available measure is firm size. Additionally to being a very imperfect measure to assess model's predictions, since hiring an additional employee can hardly be seen as a specific investment to the supplier-entrepreneur relationship, and since we do not observe firms' capital stock, this variable can only be used categorically and is not available for 1970 and 1980. Appendix 6.5 describes in detail how these variables are built.

Table 4.1: Self-employed by activity

Activity	Percent	Cumulative
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	0.98	78.68
Horticulture and floriculture	0.95	79.63
Cleaning and building conservation services	0.95	80.58

We report below in tables 4.3, 4.4 and 4.5 a detailed description of the geographical distribution of TPCs and JECs as well as that of our measures of entrepreneurship and investment.

Table 4.2: Employer by activity

Activity	Percent	Cumulative
Other or undefined	13.31	13.31
Food catering services	8.83	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	0.99	75.04
Engineering and architecture services	0.98	76.02
Dentist services	0.97	76.99
Agriculture – other cultures	0.95	77.94
Commerce of furniture. tapestry and art	0.95	78.89
Commerce of office material	0.95	79.84
Cattle-raising	0.94	80.78

Table 4.3: Geographical distribution of TPCs and JECs

	TPC in 1991	JEC in 2000
RJ + SP	42.7%	81.4%
Rio de Janeiro	47.8%	70.0%
São Paulo	40.8%	85.5%

% of population living in an AMC with at least one TPC/JEC.

Table 4.4: Geographical and historical distribution of the outcomes of interest
- Whole sample

Whole Sample				
	1970	1980	1991	2000
Entrepreneur				
RJ + SP	21.5%	21.3%	24.1%	26.3%
Rio de Janeiro	18.7%	20.4%	24.5%	27.3%
São Paulo	22.9%	21.6%	24.0%	25.9%
Employer				
Brazil	2.5%	4.0%	5.2%	3.8%
Rio de Janeiro	2.3%	3.4%	4.2%	3.4%
São Paulo	2.6%	4.3%	5.6%	4.0%
Self-employed				
RJ + SP	19.1%	17.2%	18.9%	22.4%
Rio de Janeiro	16.5%	17.0%	20.3%	23.9%
São Paulo	20.4%	17.3%	18.4%	21.9%
Scale				
RJ + SP	-	-	1.94	1.60
Rio de Janeiro	-	-	1.94	1.61
São Paulo	-	-	1.94	1.60
Scale2				
RJ + SP	-	-	1.53	0.23
Rio de Janeiro	-	-	1.94	0.20
São Paulo	-	-	1.55	0.25

Table 4.5: Geographical and historical distribution of the outcomes of interest - Subsamples

Subsample: AMCs with a JEC in 2000		Subsample: AMCs without a JEC in 2000							
	1970	1980	1991	2000		1970	1980	1991	2000
					Entrepreneur				
RJ + SP	20.9%	20.8%	23.9%	26.4%	25.5%		23.9%	24.9%	25.9%
Rio de Janeiro	17.8%	19.4%	23.6%	27.1%	22.0%		23.4%	26.8%	27.8%
São Paulo	22.2%	21.2%	24.1%	26.1%	28.9%		24.3%	23.4%	24.4%
					Employer				
RJ + SP	2.6%	4.2%	5.4%	4.0%	2.0%		3.2%	4.4%	2.9%
Rio de Janeiro	2.4%	3.6%	4.3%	3.6%	1.8%		2.8%	3.8%	2.8%
São Paulo	2.6%	4.4%	5.7%	4.2%	2.3%		3.5%	4.8%	3.1%
					Self-employed				
RJ + SP	18.3%	16.6%	18.6%	22.3%	23.5%		20.7%	20.5%	22.9%
Rio de Janeiro	15.4%	15.8%	19.3%	23.5%	20.3%		20.6%	23.0%	25.0%
São Paulo	19.6%	16.9%	18.4%	22.0%	26.6%		20.8%	18.6%	21.4%
					Scale				
RJ + SP	-	-	1.95	1.61	-		-	1.91	1.54
Rio de Janeiro	-	-	1.95	1.63	-		-	1.92	1.54
São Paulo	-	-	1.94	1.61	-		-	1.91	1.54
					Scale2				
RJ + SP	-	-	1.55	0.25	-		-	1.47	0.18
Rio de Janeiro	-	-	1.53	0.22	-		-	1.40	0.15
São Paulo	-	-	1.55	0.26	-		-	1.51	0.19

It is interesting to notice in table 4.4 that the proportion of entrepreneurs and that of self-employed increase during the period of analysis, while that of employers fall in 2000 after a steady rise from 1970 to 1991. Our question of interest, though, concerns whether these outcomes increased more (or decreased less) in the AMCs where JECs were adopted, all other things equal. This is exactly what the econometric procedure is capable of delivering. Nonetheless, table 4.5 motivates through an unconditional analysis that entrepreneurship and self-employment have increased more in AMCs with a JEC in 2000, while employer status has decreased less in that group.

Investment variables are harder to interpret, but display a general decrease in firm size from 1991 to 2000. While it seems that our measure of investment conditional on employer status has decreased less in AMCs that had a JEC in 2000, there is no clear distinctive trend among AMCS with and without a JEC in what comes to our measure of investment conditional on entrepreneurial status. Again, this exercise is an unconditional one that only motivates the actual conditional analysis that we carry through the next subsections.

We explore the institutional change given by the creation of Special Civil Tribunals (JECs) and conversion of Small Claims Tribunals (TPCs) into the latter to assess the impact of a reduction in the costs of access to justice on entrepreneurship (measured by the probability of becoming an employer or self-employed) and investment (measured by the number of firm's employees⁹). Since we have data at the level of the individual before and after the institutional change has taken place - although we are not able to follow the same individual through time -, we can control for unobservable attributes of the municipality that are fixed throughout years. As such, we compare the variation in time of the probability of becoming an entrepreneur and the average size of the firm between those municipalities where a JEC has emerged and those where it did not, through a differences-in-differences approach.

Since treatment adoption is not random, there is an obvious concern that the treated units were following a different trend than the control ones. Whereas there is not a clear rule for the creation of JECs, what makes it unfeasible to implement an instrumental variable strategy, we perform a number of robustness procedures in order to show that the effects of interest can be indeed regarded as causal ones.

⁹We explore different possibilities as to regarding self-employed as a zero-employee firm.

The baseline specification is the following:

$$Y_{t,m,i} = \alpha_t + \beta_m + \sum_{k=1}^k \gamma_k * X_{k,t,m,i} + \delta * JEC_{t,m} + \epsilon_{t,m,i},$$

where indexes t , m and i stand for year, AMC and individual, respectively, $X_{t,m,i}$ is a vector of covariates and $JEC_{t,m}$ is a dummy variable that is 1 whenever there was an operating Special Civil Tribunal at this municipality at this year and 0 otherwise. $Y_{t,m,i}$ is either entrepreneur, employer or self-employed status (a binary variable) or one out of two definitions of firm's size.

In baseline specifications, vector $X_{t,m,i}$ includes a large set of individual controls – gender, age and its square, indicators of household wealth such as canalized water supply, canalized sewage outflow, electricity supply, car ownership and number of rooms – that are likely to condition the decision to become an entrepreneur not only because of wealth constraints but also because of heterogeneity in labor market outside options, whereas schooling and migrant status are added in a second set of regressions to control for potential entrepreneurial ability heterogeneity; finally, controls at the level of the AMC – % of households with access to the general network of water supply and that of sewage outflow, % of urban households, % of the work force allocated to the public sector, average per capita income, and average participation of manufacturing and services in municipal GDP – are included to account for market conditions and local economic development, which might definitely affect individuals' occupational choice.

Dependent variables are defined as follows:

$$entrepreneur = \begin{cases} 1, & \text{if } employer = 1 \text{ or } self-employed = 1 \\ 0, & \text{otherwise} \end{cases}$$

$$scale = \begin{cases} 1, & \text{if } employees = 1 \text{ or } 2 \\ 2, & \text{if } employees > 2 \end{cases}$$

$$scale2 = \begin{cases} 0, & \text{if } self-employed = 1 \\ 1, & \text{if } employees = 1 \text{ or } 2 \\ 2, & \text{if } employees > 2 \end{cases}$$

We cluster standard-errors at the AMC level in order to allow for general auto-correlation of individual outcomes at this level. The coefficient of interest, δ , reflects the causal effect of the creation of a JEC on entrepreneurship or

investment under the hypothesis that unobservable attributes not fixed in time $\epsilon_{t,m,i}$ correlated with $Y_{t,m,i}$ are not correlated with covariates.

There are a variety of reasons, though, why one might expect this correlation to be non-zero. First, the effect of JECs might be contingent on treatment duration, so that it may take some time until the new litigation technology transmits into increased incentives to entrepreneurship or investment. To control for heterogeneity of treatment, we create dummies for each age of JEC in the sample and test whether the estimated coefficient varies with age. Second, since the institutional change was ignited by laws at the state-level, it is possible that each state was following a different trend already before treatment, what could be confounded with the effects we are interested in estimating; in order to account for this possibility we estimate a specification with state-year fixed effects.

Third, since treatment adoption was not random, and in the absence of a clear rule of adoption that could render an IV identification strategy, one might wonder that JECs were implemented precisely where economic outcomes were worse-off. If there is mean reversion in what comes to entrepreneurship, for instance, such that markets which already have a large number of entrepreneurs present little opportunities for its rapid expansion, while markets with little entrepreneurship naturally present a faster growth in the number of new ventures – such that business opportunities are taken – then this would be confounded with a positive effect of JECs. Moreover, JECs were preceded by TPCs, such that it might be the case that the estimated effect for the former is confounded with that of the latter. To investigate whether this is indeed the case, we introduce a dummy for TPCs and a placebo that emulates treatment adoption in the previous period to the creation of the correspondent JEC (whenever this JEC emerged from a previous TPC, the placebo emulates the creation of the TPC in the previous period).

We further evaluate the net effect of JECs on the quintiles of the 1991 levels of the dependent variables to assess whether it could be that, if mean reversal is actually at stake, JECs could have had a negative effect on AMCs where entrepreneurship or investment were already high before treatment adoption. Fourth, the effects estimated on the baseline specifications might confound the actual effect of JECs by estimating an average effect instead of focusing on compliers – the marginal individuals –, when theory actually predicts a heterogeneous treatment effect. We explore more deeply model's predictions in what comes to the marginal individuals introducing quintile dummies for two measures of initial wealth.

4.4 Results

We present the results for the baseline specification previously introduced. Table 4.6 displays the results for entrepreneur as dependent variable. The first regression includes basically household controls besides some individual attributes – age, age² and gender –, the second one adds schooling and migrant status, whereas the third one includes a full set of controls at the level of the AMC. The JEC coefficient is positive and significant at 5% through all specifications.

Table 4.7 displays the results for employer and self-employed as dependent variables. Although the JEC coefficient is not significant for the employer regressions in the specifications that do not control for AMC attributes, it turns out that after accounting for the variation in these measures of local economic development the probability of becoming an employer or self-employed has increased more in the municipalities where a JEC has been established than that where it has not, and this difference is statistically significant at 1% and 5%, respectively.

Last, table 4.8 shows the results for our measures of investment as dependent variables. The coefficient of JEC is not significant at 10% in the full specification for neither measure, even though a positive effect seemed to be present in the specification that do not control for AMC attributes for firm size conditional on employer status.

Also, tables 4.6, 4.7 and 4.8 display interesting effects of individual and AMC level covariates on the outcomes of interest. For instance, the indicator of gender documents a statistically higher probability for men of becoming entrepreneur, employer or self-employed, as opposed to a theoretical literature that argues that groups that are discriminated against in the labor market should have higher incentives to self-employment.¹⁰

Interestingly, AMC characteristics don't seem to strongly affect occupational choice, except for the proportion of workers in the public sector: an increase in the proportion of occupied in this industry is associated with a very strong and significant decrease in entrepreneurship, and this effect is concentrated on self-employed. Moreover, market concentration seems to matter for the decision to start a new venture, but only in manufacturing; the effect of

¹⁰There is large evidence that women are discriminated against in the Brazilian labor market; see, for instance, Menezes Filho and Giuberti (2005). As an example of the aforementioned literature, see for example Moore (1983) and Sowell (1981). Conversely, Coate and Tennyson (1992) argue that since the group discriminated against in labor market will face a lower average return in self-employment – if entrepreneurial ability is evenly distributed across groups – then this group might also be discriminated against in markets complementary to self-employment, e.g.: credit markets, such that their overall incentives to self-employment might be actually smaller than that of other groups.

Table 4.6: Results – Baseline specification: Entrepreneur

	(1)	(2)	(3)
	entrepreneur	entrepreneur	entrepreneur
JEC	0.0229** (0.0111)	0.0233** (0.0110)	0.0133** (0.0052)
male	0.0961*** (0.0049)	0.0954*** (0.0049)	0.0988*** (0.0057)
age	0.0176*** (0.0006)	0.0179*** (0.0006)	0.0178*** (0.0006)
age ²	-0.000147*** (0.0000)	-0.000153*** (0.0000)	-0.000150*** (0.0000)
urban	0.0100* (0.0051)	0.0135*** (0.0051)	0.0338*** (0.0084)
water	0.00465 (0.0033)	0.00549* (0.0032)	0.00151 (0.0061)
sewage	-0.0016 (0.0036)	0.000129 (0.0032)	-0.00135 (0.0027)
electricity	-0.0385*** (0.0095)	-0.0385*** (0.0092)	-0.0234** (0.0106)
car	0.0835*** (0.0067)	0.0881*** (0.0064)	0.0821*** (0.0053)
rooms	0.0109*** (0.0014)	0.0117*** (0.0013)	0.0112*** (0.0014)
schooling		-0.00241*** (0.0003)	-0.00206*** (0.0004)
migrant		-0.0201*** (0.0047)	-0.0164*** (0.0054)
AMC water			-0.0496** (0.0244)
AMC sewage			-0.0008 (0.0097)
AMC urban			-0.0261 (0.0311)
public sector			-0.285** (0.1170)
pc income			-8.30e-06*** (0.0000)
manufacturing			-0.0719*** (0.0195)
services			-0.0349 (0.0214)
constant	-0.307*** (0.0160)	-0.305*** (0.0159)	-0.268*** (0.0269)
AMC fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes
Observations	5644835	5644835	4391729
Number of AMC	550	550	522
R ² overall	0.0472	0.0486	0.0524
R ² between	0.191	0.196	0.173
R ² within	0.0465	0.0473	0.0498

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

Table 4.7: Results – Baseline specification: Employer and self-employed

	(1) employer	(2) employer	(3) employer	(1) self-employed	(2) self-employed	(3) self-employed
JEC	0.00129 (0.0026)	0.000644 (0.0025)	0.00496*** (0.0018)	0.0218** (0.0091)	0.0228** (0.0091)	0.00883** (0.0044)
male	0.0329*** (0.0018)	0.0334*** (0.0016)	0.0351*** (0.0012)	0.0633*** (0.0051)	0.0620*** (0.0049)	0.0637*** (0.0060)
age	0.00421*** (0.0002)	0.00422*** (0.0002)	0.00457*** (0.0002)	0.0134*** (0.0005)	0.0137*** (0.0005)	0.0133*** (0.0006)
age ²	-4.03e-05*** (0.0000)	-3.75e-05*** (0.0000)	-4.06e-05*** (0.0000)	-0.000107*** (0.0000)	-0.000116*** (0.0000)	-0.000109*** (0.0000)
urban	0.0102*** (0.0015)	0.00792*** (0.0015)	0.0113*** (0.0015)	-0.000149 (0.0053)	0.0056 (0.0054)	0.0224*** (0.0076)
water	-0.00149 (0.0011)	-0.00302*** (0.0011)	-0.00494*** (0.0011)	0.00614* (0.0033)	0.00851** (0.0033)	0.00645 (0.0053)
sewage	0.00624*** (0.0008)	0.00374*** (0.0007)	0.00500*** (0.0010)	-0.00786** (0.0040)	-0.00363 (0.0037)	-0.00634* (0.0035)
electricity	-0.00836*** (0.0016)	-0.00836*** (0.0015)	-0.0120*** (0.0023)	-0.0302*** (0.0102)	-0.0301*** (0.0099)	-0.0115 (0.0118)
car	0.0465*** (0.0016)	0.0411*** (0.0018)	0.0388*** (0.0017)	0.0370*** (0.0063)	0.0470*** (0.0061)	0.0433*** (0.0059)
rooms	0.0107*** (0.0006)	0.00965*** (0.0005)	0.0101*** (0.0006)	0.000225 (0.0009)	0.00200** (0.0010)	0.00114 (0.0009)
schooling		0.00239*** (0.0002)	0.00243*** (0.0002)		-0.00479*** (0.0002)	-0.00449*** (0.0002)
migrant		0.00113 (0.0016)	0.000789 (0.0021)		-0.0213*** (0.0034)	-0.0172*** (0.0036)
AMC water			0.00294 (0.0056)			-0.0525** (0.0220)
AMC sewage			-0.0132*** (0.0033)			0.0122 (0.0094)
AMC urban			0.0185** (0.0072)			-0.0452 (0.0279)
public sector			-0.0196 (0.0251)			-0.277** (0.1180)
pc income			7.28E-08 (0.0000)			-8.42e-06*** (0.0000)
manufacturing			-0.0210*** (0.0073)			-0.0489*** (0.0164)
services			-0.00437 (0.0077)			-0.0279 (0.0173)
constant	-0.162*** (0.0062)	-0.165*** (0.0061)	-0.173*** (0.0091)	-0.146*** (0.0129)	-0.140*** (0.0131)	-0.0943*** (0.0216)
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5644835	5644835	4391729	5644835	5644835	4391729
Number of AMC	550	550	522	550	550	522
R ² overall	0.0553	0.0572	0.0576	0.0226	0.0257	0.0272
R ² between	0.218	0.235	0.246	0.419	0.327	0.154
R ² within	0.0538	0.0558	0.0567	0.0215	0.0239	0.025

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

Table 4.8: Results – Baseline specification: Investment

	(1) scale	(2) scale	(3) scale	(1) scale2	(2) scale2	(3) scale2
JEC	0.0432*** (0.0132)	0.0431*** (0.0131)	0.0193 (0.0123)	-0.0134 (0.0157)	-0.0157 (0.0163)	0.0272 (0.0167)
male	-0.00565*** (0.0011)	-0.00374*** (0.0011)	-0.00392*** (0.0011)	-0.0434*** (0.0072)	-0.0221*** (0.0076)	-0.0240*** (0.0085)
age	-0.00128*** (0.0004)	-0.00145*** (0.0004)	-0.00181*** (0.0005)	-0.0217*** (0.0016)	-0.0227*** (0.0017)	-0.0233*** (0.0018)
age ²	1.31e-05** (0.0000)	1.81e-05*** (0.0000)	2.22e-05*** (0.0000)	0.000198*** (0.0000)	0.000248*** (0.0000)	0.000254*** (0.0000)
urban	0.0755*** (0.0095)	0.0738*** (0.0093)	0.0671*** (0.0104)	-0.0014 (0.0153)	-0.0219 (0.0148)	-0.0236 (0.0189)
water	0.0295*** (0.0052)	0.0280*** (0.0054)	0.0346*** (0.0053)	0.0520*** (0.0086)	0.0356*** (0.0064)	0.0332*** (0.0094)
sewage	-0.000493 (0.0011)	-0.00277** (0.0012)	-0.00370*** (0.0010)	0.0335*** (0.0060)	0.00574 (0.0047)	0.00171 (0.0034)
electricity	0.0401*** (0.0064)	0.0381*** (0.0064)	0.0352*** (0.0072)	0.104*** (0.0219)	0.0871*** (0.0246)	0.116*** (0.0211)
car	-0.0143*** (0.0014)	-0.0193*** (0.0010)	-0.0189*** (0.0010)	0.00456 (0.0123)	-0.0528*** (0.0093)	-0.0546*** (0.0106)
rooms	0.00171*** (0.0002)	0.000741*** (0.0002)	0.000802*** (0.0002)	0.0225*** (0.0010)	0.00954*** (0.0011)	0.00950*** (0.0011)
schooling		0.00186*** (0.0002)	0.00176*** (0.0002)		0.0234*** (0.0008)	0.0226*** (0.0009)
migrant		0.00318** (0.0015)	0.00269 (0.0017)		0.0205*** (0.0050)	0.0188*** (0.0054)
AMC water			0.0878 (0.0907)			-0.113 (0.1220)
AMC sewage			-0.0401 (0.0432)			0.004 (0.0639)
AMC urban			-0.0836 (0.0689)			0.145 (0.0954)
public sector			-0.239* (0.1420)			0.0815 (0.2370)
pc income			0.000309*** (0.0001)			0.000277*** (0.0001)
manufacturing			0.0184 (0.0259)			0.292*** (0.0466)
services			-0.0315 (0.0228)			0.0931* (0.0544)
constant	1.799*** (0.0108)	1.798*** (0.0109)	1.774*** (0.0239)	1.719*** (0.0351)	1.680*** (0.0385)	1.460*** (0.0609)
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	837419	837419	731078	1342771	1342771	1165645
Number of AMC	550	550	520	550	550	522
R ² overall	0.103	0.104	0.101	0.358	0.367	0.372
R ² between	0.449	0.451	0.623	0.657	0.647	0.806
R ² within	0.0966	0.0972	0.0978	0.354	0.362	0.362

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

the participation of services in GDP is not statistically different from zero. In what comes to the investment outcomes, AMC attributes are by large irrelevant except for the share of public sector in total employment, also here associated with a large decline of firm size conditional on employer status.

4.5 Robustness

JEC age Heterogeneity due to treatment duration is a prime-order concern. Table 4.9 shows the geographic distribution of JECs by age in our sample.

Table 4.9: Geographic distribution of JECs by age

	RJ	SP
1 year	0.0%	11.5%
2 years	0.0%	74.0%
4 years	70.0%	0.0%

% of population living in an AMC which oldest JEC is of that age.

Notice that our sample does not have JECs created in 2000 (zero-year old JECs) nor three-year old ones. Moreover, all the one- and two-year old JECs are located in São Paulo, whereas all the four-year old ones are in Rio de Janeiro. Even though this feature of our sample leaves room for a potential confounding effect of state trends, we deal with it in the next subsection. As for now, we introduce dummies for each JEC age available in our sample to assess the potential heterogeneity of treatment effect, contingent on duration.

Results are present in table 4.10. There is strong evidence that, in fact, the intensity of JEC's effect on entrepreneurial status seems to be increasing in treatment duration. While one-year JECs are not reported to have statistically significant effects on none of the outcomes of interest, two-year ones have a positive and significant effect at 10% for employer and self-employed, and at the 5% level of significance for entrepreneur. On the fourth year of implementation, these effects are strongly augmented, different from zero at 1% level of significance for entrepreneur and employer and 5% for self-employed.

Moreover, punctual estimates for four-year JECs are approximately 2.4 that of two-year ones for entrepreneur, 3.6 for employer and 1.8 for self-employed. Results for the investment variables are seldom different from zero, and when they are it is at most at the 10% level of significance.

State-year fixed-effects One might worry that each of the states considered in the sample might have been following different trends, what could

Table 4.10: Results – Treatment duration

	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2
JEC 1y	0.011 (0.0090)	0.00303 (0.0023)	0.00865 (0.0082)	-0.00184 (0.0171)	0.0282 (0.0284)
JEC 2y	0.0101** (0.0049)	0.00328* (0.0017)	0.00729* (0.0043)	0.0242* (0.0135)	0.0263 (0.0174)
JEC 4y	0.0246*** (0.0066)	0.0116*** (0.0020)	0.0132** (0.0058)	0.0274 (0.0184)	0.0380* (0.0229)
Individual controls	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Observations	4391729	4391729	4391729	731078	1162729
Number of AMC	522	522	522	520	522
R^2 overall	0.0515	0.0575	0.0270	0.103	0.375
R^2 between	0.140	0.235	0.143	0.637	0.828
R^2 within	0.0498	0.0568	0.0249	0.0978	0.366

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

bias the estimated treatment effects. In order to control for this possibility, we introduce state-year fixed-effects.

Table 4.11 shows the results after controlling for state-year fixed effects. The effect of JEC is still significant and positive at 5% for entrepreneur, employer and self-employed. Also, these are not too different from the estimates in the baseline specification.

TPC and Placebo Still, treatment adoption might not have been random. If this is indeed the case, then treated units might be dynamically different from control ones, such that their potential outcomes are not the same, what would render unfeasible consistent estimation of treatment effects. In other words, as compared to observations in the control group, treated units might be simply following different trends (either a natural positive drift or mean reversal) so that the estimated effect of JEC on the outcomes of interest would not be a causal one.

Furthermore, state laws that implement JECs converted pre-existent TPCs in the former, such that what we have estimated so far may confound the effect of JECs on entrepreneurship and investment with that of small claim courts. To deal with these possibilities we estimate two specifications, including a TPC dummy in the first, along with a placebo indicator in the second, where

Table 4.11: Results – State-year fixed effects

	(1) entrepreneur	(1) employer	(1) self-employed	(1) scale	(1) scale2
JEC	0.0117** (0.0048)	0.00354** (0.0017)	0.00867** (0.0040)	0.0194 (0.0124)	0.0275* (0.0162)
Individual controls	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Observations	4391729	4391729	4391729	731078	1165645
Number of AMC	522	522	522	520	522
R^2 overall	0.0518	0.0576	0.0275	0.102	0.372
R^2 between	0.172	0.242	0.168	0.626	0.805
R^2 within	0.0499	0.0568	0.025	0.0978	0.362

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

$$placebo_{t,m} = \begin{cases} 1, & \text{if } t = 1991 \text{ and } JEC_{2000,m} = 1 \text{ and } TPC_{1991,m} = 0 \\ 1, & \text{if } t = 1980 \text{ and } JEC_{2000,m} = 1 \text{ and } TPC_{1991,m} = 1 \\ 0, & \text{otherwise} \end{cases}$$

Tables 4.12 and 4.13 show that previous results concerning the effects of JECs on entrepreneurship are preserved even after controlling for previous existence of TPCs. While the significance of the effect of JECs on employer status is increased in these specifications as compared to the baseline one, after accounting for potential previous tendency in treated units the effect on self-employment is no longer statistically different from zero.

Furthermore, we document no pre-existent trend of treated units concerning entrepreneurship or neither measures of investment, although there seems to be a positive trend in employment (only significant at 10%), counterbalanced by a negative one in self-employment (also only significant at the 10% level). We investigate whether these previous trends might imply a heterogeneous effect of JECs in the next subsection.

While this effect was absent in the baseline specification, controlling for previous existence of TPCs leads to a positive and significant estimated effect of JECs on firm size conditional on entrepreneur status at the 5% level. Nevertheless, JECs' impact on our measures of investment is not very robust through specifications.

What is startling is the systematic estimate of a negative impact of

Table 4.12: Results – Placebo

	(1)	(2)	(1)	(2)	(1)	(2)
	entrepreneur	entrepreneur	employer	employer	self-employed	self-employed
JEC	0.0121** (0.0052)	0.0108** (0.0052)	0.00362** (0.0015)	0.00447*** (0.0016)	0.00906** (0.0046)	0.0069 (0.0045)
TPC	-0.00504 (0.0032)	-0.00672* (0.0035)	-0.00593*** (0.0013)	-0.00482*** (0.0014)	0.000946 (0.0036)	-0.00182 (0.0039)
placebo		-0.00289 (0.0025)		0.00191* (0.0011)		-0.00476* (0.0029)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4391729	4391729	4391729	4391729	4391729	4391729
Number of AMC	522	522	522	522	522	522
R^2 overall	0.0523	0.0523	0.0576	0.0577	0.0272	0.0272
R^2 between	0.172	0.169	0.243	0.244	0.154	0.146
R^2 within	0.0498	0.0498	0.0568	0.0568	0.025	0.025

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

Table 4.13: Results – Placebo

	(1)	(2)	(1)	(2)
	scale	scale	scale2	scale2
JEC	0.00329 (0.0117)	0.00668 (0.0123)	0.0368** (0.0169)	0.0341** (0.0171)
TPC	-0.0536*** (0.0082)	-0.0512*** (0.0085)	0.0435*** (0.0116)	0.0416*** (0.0120)
placebo		0.01 (0.0088)		-0.00788 (0.0209)
Individual controls	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes
Observations	731078	731078	1165645	1165645
Number of AMC	520	520	522	522
R^2 overall	0.108	0.108	0.369	0.369
R^2 between	0.643	0.642	0.806	0.806
R^2 within	0.0981	0.0981	0.362	0.362

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

TPCs on entrepreneur and employer status, as well as on firm size conditional on being an employer. While the Law literature attributes a supposedly zero effects for small claim tribunals, it might have been the case that their interaction with informal legal systems might have disrupted preexisting relational contracts. Carneiro (2003)'s following passage backs-up the existence of such parallel justice, especially in slums:

"Concerning the existence of non-official justice, in the 1980's, professor Boaventura de Souza Santos conducted studies in Rio de Janeiro slums, 'where it was possible to detect and analyze the existence in these urban neighborhoods of an informal Law, non-official, non-professionalized, centered in the citizens association, that worked as a conflict resolution instance among neighbors(...)' (Introdução à sociologia da administração da justiça. *Direito e justiça*. Org. by José Eduardo Faria. São Paulo: Ática, 1994, p.54)", Carneiro (2003, p. 46, footnote 26).

Mean reversal Motivated by a sometimes significant effect of the placebo experiment in the previous subsection, one might wonder that treatment was adopted precisely in the AMCs with the worst institutional environment. If this is indeed the case and since mean reversal is perfectly plausible within this context, we now explore in further detail the possibility that treated units experienced mean reversal as well as estimate net effect of JECs under the presence of potential outcomes heterogeneity. The estimated equation is the following:

$$Y_{t,m,i} = \alpha_t + \beta_m + \sum_1^k \gamma_k * X_{k,t,m,i} + \delta * JEC_{t,m} + \psi * JEC_{t,m} * \bar{Y}_{t-1,m} + \epsilon_{t,m,i}$$

, where $\bar{Y}_{t-1,m}$ stands for the average level of the outcome of interest in the AMC in 1991, such that the net effect of JEC for a given initial level of a dependent variable is given by $\delta + \psi * \bar{Y}_{t-1,m}$.

Table 4.14 shows that the interaction of the JEC indicator with the average initial level of the outcomes of interest is generally negative (except for firm size conditional on employer status), suggesting that mean reversal has indeed taken place. As such, we compute in table 4.15 the net effects of JECs on the cut-points of 1991 level quintiles for each outcome of interest.

Table 4.15 is reassuring once it accounts for a positive and significant net effect of JECs for every cut-point; nonetheless, these effects are stronger for the first 1991 level quintiles. In fact, although always positive, JECs' effect on self-employed status is only significantly different from zero for the lower 1991

Table 4.14: Results – Mean reversal

	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2
JEC	0.0458***	0.0115***	0.0435***	-0.876**	1.126***
	-0.0116	-0.00272	-0.00965	-0.354	-0.0673
JEC*(level 1991)	-0.166***	-0.154***	-0.216***	0.461**	-0.720***
	-0.0476	-0.0446	-0.051	-0.184	-0.0444
Individual controls	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Observations	4003305	4003305	4003305	662803	1051748
Number of AMC	520	520	520	518	520
R^2 overall	0.051	0.0566	0.0265	0.102	0.374
R^2 between	0.17	0.219	0.144	0.639	0.804
R^2 within	0.0492	0.0559	0.0251	0.096	0.364

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

Table 4.15: Results – Effects on 1991 level quintiles

	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2
net effect at	0.01234	0.00559	0.00988	0.01405	0.03208
1st cut-point	(0.0035)	(0.0012)	(0.0136)	(0.2722)	(0.0315)
net effect at	0.00970	0.00464	0.00789	0.02536	-0.0045
2nd cut-point	(0.0168)	(0.0056)	(0.0444)	(0.0753)	(0.7557)
net effect at	0.0089	0.00340	0.00554	0.02637	-0.02701664
3rd cut-point	(0.0269)	(0.0423)	(0.1562)	(0.0680)	(0.0840)
net effect at	0.00797	0.00340	0.00351	0.0265	-0.0330
4th cut-point	(0.0475)	(0.0423)	(0.3738)	(0.0666)	(0.0362)

P-values in parentheses;

1st quintile: entrepreneur ≤ 0.2015 ; 2nd quintile: entrepreneur > 0.2015 and ≤ 0.2174 ; 3rd quintile: entrepreneur > 0.2174 and ≤ 0.2221 ; 4th quintile: entrepreneur > 0.2221 and ≤ 0.2278 ; 5th quintile: entrepreneur > 0.2278 ;

1st quintile: employer ≤ 0.0383 ; 2nd quintile: employer > 0.0383 and ≤ 0.0445 ; 3rd quintile: employer > 0.0445 and ≤ 0.0525 ; 4th and 5th quintiles: employer > 0.052597 ;

1st quintile: self-employed ≤ 0.1556 ; 2nd quintile: self-employed > 0.1556 and ≤ 0.1648 ; 3rd quintile: self-employed > 0.1648 and ≤ 0.1757 ; 4th quintile: self-employed > 0.1757 and ≤ 0.1851 ; 5th quintile: self-employed > 0.1851 ;

1st quintile: scale ≤ 1.9307 ; 2nd quintile: scale > 1.9307 and ≤ 1.9552 ; 3rd quintile: scale > 1.9552 and ≤ 1.9574 ; 4th quintile: scale > 1.9574 and ≤ 1.9578 ; 5th quintile: scale > 1.9578 ;

1st quintile: scale2 ≤ 1.5193 ; 2nd quintile: scale2 > 1.5193 and ≤ 1.5702 ; 3rd quintile: scale2 > 1.5702 and ≤ 1.6014 ; 4th quintile: scale2 > 1.6014 and ≤ 1.6098 ; 5th quintile: scale2 > 1.6098 .

level quintiles. For firm size conditional on employer status, the opposite is true: only AMCs that had the higher levels of initial investment are documented to have had a positive effect of JECs on this outcome. Net effects are the most heterogeneous for firm size conditional on entrepreneurial status: while AMCs with low initial levels faced a positive and significant effect, those with higher initial levels were negatively affected by JECs. It is hard to make sense of these effects, since for these subset of AMCs employers were documented to have increased firm size by the most were under the presence of JECs, while self-employment has not responded in a statistically significant way to Special Civil Tribunals.

Marginal individual So far, we have not taken account of a strong prediction of our theoretical model: not only should a decrease in litigation costs increase entrepreneurship and investment, it should specifically relax the wealth constraint for the marginal individual. By estimating an average effect, we may be foregoing a heterogeneous effect of JECs – conditional on initial wealth – as well as underestimating its effect on the outcomes of interest since considering in the sample a whole subset of never-takers as opposed to estimating its effect on compliers, so as to adopt the terminology from the potential outcomes literature.

In order to try to disentangle the effects on the marginal individual we stratify our sample according to two measures of initial wealth. The first one, that we name household wealth, is the first auto-vector of the principal component decomposition of urban, water, sewage, electricity, car, rooms and schooling ¹¹, while the second is schooling itself, since parents educational decisions should be correlated with initial wealth if individuals are potentially wealth-constrained.

The empirical strategy consists of including quintile dummies for each of these measures in separate regressions in order to assess whether it is the case that the effects of JECs are concentrated on the upper or lower quintiles. If they are stronger in lower quintiles or if, conversely, they are positive in upper quintiles but zero in the lower ones, model's predictions concerning the effects on the marginal entrepreneur are backed-up, although the first would indicate that this marginal individual belongs to the lower tail of the distribution of wealth, whereas the second would indicate that he belongs to the upper tail.

¹¹This component responds for 40% of their total variability.

Table 4.16: Results – Marginal individual (Household Wealth)

	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2
JEC 1st q	0.00282 (0.00667)	0.00163 (0.00387)	0.00163 (0.00591)	-0.000174 (0.02960)	-0.0482* (0.02510)
JEC 2nd q	0.0111 (0.00778)	0.0138*** (0.00255)	-0.00234 (0.00748)	-0.0439 (0.03650)	-0.0982*** (0.02420)
JEC 3rd q	-0.00126 (0.00601)	0.00892*** (0.00207)	-0.00975** (0.00481)	-0.133*** (0.02040)	-0.110*** (0.02250)
JEC 4th q	0.00765 (0.00530)	-0.00453*** (0.00187)	0.0126*** (0.00463)	-0.114*** (0.01290)	-0.0737*** (0.01900)
JEC 5th q	0.0288*** (0.00675)	0.00818*** (0.00291)	0.0210*** (0.00569)	0.0533*** (0.01320)	0.173*** (0.01720)
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Observations	4391729	4391729	4391729	731078	1165645
Number of AMC	522	522	522	520	522
R^2 overall	0.0526	0.0578	0.0274	0.104	0.376
R^2 between	0.167	0.247	0.144	0.633	0.818
R^2 within	0.05	0.057	0.0251	0.101	0.366

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

Table 4.16 pinpoints that, after controlling for AMC covariates, only individuals of the upper tail of the initial wealth distribution were affected by treatment when it comes to entrepreneurship. Accordingly, the estimated effect for the 5th quintile is of much higher magnitude than the average one estimated in the baseline specification. Breaking down these effects for employer and self-employed status. It is also the case that treatment effects are concentrated on upper quintiles, although not so clearly as for entrepreneurship.

Table 4.16 also investigates whether heterogeneity is at stake when it comes to investment. It is somewhat puzzling that although positive and significant effects are concentrated on the upper quintile, we estimate a negative and significant effect for the mid and lower quintiles (for firm size conditional on entrepreneurial status). These effects could be explained, though, based upon the entry of new startups with smaller scale than previously operating ventures.

We now turn to the schooling decomposition. First quintile denotes individuals with 2 or less years of schooling, second quintile, those with 3 to 4 years of schooling, third quintile, those with 5 to 6 years of schooling, fourth quintile, those with 7 to 11, whereas fifth quintile denotes the ones with 12 or more years of schooling.

Table 4.17: Results – Marginal individual (Schooling)

	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2
JEC 1st q	-0.0283*** (0.00715)	0.00489* (0.00272)	-0.0328*** (0.00521)	-0.0817*** (0.01930)	-0.00273 (0.02310)
JEC 2nd q	0.00134 (0.00521)	-0.00271 (0.00198)	0.00449 (0.00431)	-0.0926*** (0.01410)	-0.0225 (0.01920)
JEC 3rd q	0.0205*** (0.00532)	0.00126 (0.00188)	0.0197*** (0.00474)	-0.103*** (0.01790)	-0.0692*** (0.01910)
JEC 4th q	0.0210*** (0.00570)	0.00469** (0.00183)	0.0167*** (0.00475)	8.74E-05 (0.01250)	0.0165 (0.01730)
JEC 5th q	0.0282*** (0.00628)	0.0183*** (0.00417)	0.0103* (0.00561)	0.0846*** (0.01270)	0.201*** (0.01690)
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes
Schooling/Migrant	Yes	Yes	Yes	Yes	Yes
AMC controls	Yes	Yes	Yes	Yes	Yes
AMC fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
AMC clustering	Yes	Yes	Yes	Yes	Yes
Observations	4391729	4391729	4391729	731078	1165645
Number of AMC	522	522	522	520	522
R^2 overall	0.0527	0.0577	0.0275	0.109	0.374
R^2 between	0.171	0.242	0.151	0.647	0.813
R^2 within	0.0501	0.057	0.0252	0.1	0.364

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

Table 4.17 shows that also by taking schooling as reference for the sample decomposition the treatment effect on entrepreneurship is concentrated upon the upper quintiles. What is novel is a negative and significant effect on the lower quintile, what probably reflects a combination of low entrepreneurial ability and labor market general equilibrium effects of increased entrepreneurship in the upper quintiles.

Moreover, the general patterns of the previous results is preserved. The only feature worth remarking is that the negative effect on the lower quintile is concentrated on self-employment, rather than on employer status. This is due to the fact that low-skilled individuals occupational choice is probably degenerated into labor supply or self-employment, whereas only higher-skilled ones actually hire employees in order to expand the firm.

Last, table 4.17 reproduces the previous results of a positive and significant effect on firm size for the upper quintiles in contrast to a negative and significant effect for mid and lower quintiles (for firm size conditional on employer status). Once more, it is reasonable that new startups - so far constrained - have a lower number of employees than previously settled business, resulting in an estimated decrease in average firms' scale following the installation of JECs.

Finally, we assess the economic significance of the estimated effects in table 4.18, where we divide the estimated coefficients in several previous specifications for each outcome by the sample average of this variable in 1991. Although the baseline effect generally overestimates a heterogeneous net effect of JECs conditional on the average initial level of that outcome, what is remarkable is the increase in the size of the effect once we restrict attention to marginal individuals.

Table 4.18: Results – Economic significance of estimated effects

	(1)	(1)	(1)	(1)	(1)	(1)
	entrepreneur	employer	self-employed	scale	scale2	
net effect 1st cut-point / average 1991	5.6%	11.8%	5.7%	0.7%	2.1%	
net effect 2nd cut-point / average 1991	4.4%	9.8%	4.5%	1.3%	-0.3%	
net effect 3rd cut-point / average 1991	4.0%	7.2%	3.2%	1.4%	-1.7%	
net effect 4th cut-point / average 1991	3.6%	7.2%	2.0%	1.4%	-2.1%	
effect ¹ / average 1991	6.0%	10.4%	5.1%	1.0%	1.7%	
5th q (wealth) effect / average 1991	13.0%	17.2%	12.0%	2.7%	11.1%	
5th q (schooling) effect / average 1991	12.7%	38.5%	5.9%	4.4%	12.9%	

¹ baseline specification

As a matter of fact, not only is the effect estimated for the 5th quintile of either measure of initial wealth always larger than the average one but also the economic significance of this effect sometimes drastically changes: for instance, for entrepreneur, this effects more than doubles to approximately 13%, and for employer, while the average effect is of 10.4%, the effect on the upper tail of the distribution ranges from 17.2% to 38.5%, what is a very sizeable effect, depending on the choice of variable for initial wealth.