Regulation and Firms: Evidence from Brazil

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Abstract

This paper examines how local regulatory reform shapes firm dynamics in a large developing economy. We study Brazil's *Lei da Liberdade Econômica* (LLE, Economic Freedom Law), a 2019 federal reform that authorized municipalities to exempt "low-risk" businesses from licensing requirements. We leverage the staggered adoption by municipalities as a unique setting to evaluate how changes in the regulatory environment influence local economic activity. Using an entropy-balanced stacked difference-in-differences framework, we find that LLE adoption increased firm openings and economic activity. The evidence is consistent with the creation of new economic activity rather than pure reallocation or formalization, though both are contributing factors. We further exploit administrative records covering the universe of applications to open a firm (approximately 4.8 million records), finding that reduced time costs is a key mechanism behind the main results.

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1 Introduction

Brazil has long been characterized by a burdensome business environment, marked by excessive bureaucracy, high compliance costs, and slow regulatory procedures. In one of the first systematic attempts to empirically assess regulatory barriers across countries, Djankov et al. (2002) document the number of procedures required for a start-up to operate formally across 85 countries. Brazil ranked near the bottom of their list: the authors report 15 required procedures, compared to a minimum of two in Canada and a maximum of 21 in the Dominican Republic. Such barriers have been linked to lower entrepreneurship, reduced formalization, and weaker productivity growth (Ulyssea, 2010; Rocha et al., 2018; OECD, 2020).

Motivated by the need to reduce these barriers, the Brazilian government enacted Law No. 13,874/2019, commonly known as the *Economic Freedom Law* (*Lei da Liberdade Econômica*, or LLE). The law was designed to simplify regulation and facilitate business formation and operation, notably by exempting low-risk activities from licensing and permit requirements. Importantly, implementation is decentralized: municipalities retain discretion over how and when to adopt and operationalize the law. As of December 2025, more than 40 percent of Brazil's 5,571 municipalities have adopted a local version of the LLE (Figure 1). In this paper, we examine whether local implementation of economic freedom laws reduced regulatory barriers and stimulated economic activity at the municipal level.

The LLE was not unanimously supported in Brazil. While many viewed it as a positive step toward reducing the state's role in the economy and encouraging entrepreneurship,[1] others remained skeptical. Critics raised a variety of concerns, including potential implications for labor protections and worker safety (Repórter Brasil, 2019). More relevant to this paper, even commentators broadly supportive of deregulation expressed concern that the law's ambiguous language and reliance on decentralized municipal adoption could

[1]For example, the Brazilian Confederation of Commercial and Business Associations (CACB) and the Support Service for Micro and Small Enterprises (SEBRAE) both publicly expressed support for the law (CACB, 2019; SEBRAE, 2019).

create legal uncertainty and uneven enforcement across locations, potentially limiting its effectiveness (Pinto, 2019; Sapper and Castillo, 2020).[2]

This paper assesses whether the LLE reduced bureaucratic barriers, encouraged entrepreneurship, and increased broader economic activity at the municipal level. Despite the attention the Economic Freedom Law has received in legal and policy circles, there is limited systematic empirical evidence on its effects. To our knowledge, only three empirical studies examine related questions at the municipal level. The first, (Pereira, 2025), shows broadly positive effects on economic outcomes but does not address challenges related to staggered adoption or selection into treatment. The other two focus primarily on formalization and labor market outcomes (Vechi et al., 2025; Barros and Rangel, 2026). We contribute to this literature by examining firm creation directly and, following Colonnelli and Prem (2022), investigating whether the reform generated broader economic effects consistent with new economic activity rather than the formalization of existing firms alone. We also explicitly examine potential spillovers across municipalities, an important consideration for place-based policies implemented at the local level. Finally, we examine the primary mechanism through which the reform is intended to operate by providing direct evidence on its effects on the business-registration process, specifically the time savings associated with LLE adoption.[3]

Our main estimation strategy combines entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013) with a stacked difference-in-differences design (Cengiz et al., 2019).[4] We compare changes in firm openings between municipalities that adopted the LLE and those that did not.[5] Using administrative data from the Federal Government's Business Panel (*Painel de Empresas*), we find that municipal adoption of the LLE in-

[2]For example, a professor at the Law School of the State University of Rio de Janeiro is quoted in Pinto (2019) as supporting the goal of reducing bureaucracy while criticizing the law's text.

[3]There is a complementary literature outside of Brazil that links economic freedom and regulatory burden to entrepreneurship and growth, often relying on composite indices for identification. See, for example, Gwartney et al. (2023, 1999); Dawson (2003); Hall and Lawson (2014); Lawson (2024); Sobel (2008); Garrett and Rhine (2010); Wiseman and Young (2013); Powell and Weber (2013); Bennett (2021); Grier and Grier (2021).

[4]We provide details concerning our estimation strategy in Section 3.

[5]We consider two treatment and control group definitions given Brazil's legal structure and the layered nature of business regulation; this is discussed in detail in Section 2.2.

creased firm openings by up to eight percent two years after adoption. We also exploit administrative records covering 4.8 million applications and use a combination of Mahalanobis Distance Matching and entropy balancing to study whether the law's stated objective of reducing bureaucratic barrier was achieved. By comparing matched applications in treated and control municipalities, we estimate an approximate 9 to 12 percent reduction in the time required to legally open a business in municipalities adopting a LLE. Together, these results indicate that the LLE reduced regulatory burdens and increased firm entry.

Two questions remain when interpreting these results in a broader economic context. The first concerns the presence and direction of spatial spillovers. The second is whether the observed increase in firm entry reflects meaningful changes in economic activity. We address each in turn.

An important concern with local economic policies is that they may generate spillovers beyond the treated jurisdiction, and these effects can operate in either direction.[6] A common concern in the place-based policy literature is that increases in local economic activity may reflect firm relocation rather than the creation of new activity (Kolko and Neumark, 2010; Neumark and Simpson, 2014; Bologna Pavlik, 2015; Ehrlich and Seidel, 2018). Attracting activity from other locations is sometimes cited as a goal of such policies (Ferrara, 1982; Ladd, 1994; Bondonio and Engberg, 2000). By contrast, a standard argument in *favor* of place-based policies emphasizes agglomeration effects, under which policy adoption in one municipality may generate positive spillovers to neighboring areas, offsetting any negative displacement effects.[7] While much of this literature focuses on narrowly defined enterprise zones, which are typically smaller in geographic scope and therefore more likely to experience localized spillovers, we directly assess these possibilities by examining outcomes in neighboring municipalities.

The second challenge is interpreting whether increased firm entry reflects meaningful

[6] See Siegloch et al. (2025) for a recent example of work analyzing spillovers with place-based policies.

[7] Glaeser and Gottlieb (2008) discuss agglomeration economies as a potential motivation for place-based policy, while remaining skeptical of their effectiveness. Other studies highlighting agglomeration as a potential benefit include Kline and Moretti (2014); Chaudhary and Potter (2019); Lu et al. (2019).

economic activity. The increase in firm openings is driven primarily by very small firms, which may have limited productive capacity. Moreover, if many of these firms previously operated informally (i.e., without a *Cadastro Nacional da Pessoa Jurídica* (CNPJ) number), their contribution to economic output may arise primarily through productivity gains associated with formalization rather than through the creation of entirely new activity. Firm entry may also provide an incomplete picture of the reform's effects because the policy can influence the performance of existing firms. While reduced regulatory burdens may benefit incumbents, increased competition from new entrants may offset some of these gains. These considerations raise an important question: does the observed increase in firm entry translate into a meaningful increase in overall economic activity?

We address the first challenge by examining the spatial dimension of LLE effects. Specifically, we test whether firm openings changed in never-treated municipalities that border treated municipalities. If the increase in firm openings reflects a reallocation of entrepreneurial activity from nearby areas, we would expect to observe a decline in firm entry in neighboring untreated municipalities. We find no evidence of such negative spillovers, and similar results hold when examining firm closings. We further examine whether the policy is more effective when implemented in isolation *or* within clusters of treated municipalities. If isolated adoption were more effective, this would suggest that municipalities are attracting entrepreneurial activity away from nearby areas. Instead, we find that the policy is less effective when implemented in isolation. This pattern is consistent with agglomeration forces and aligns with the literature emphasizing the benefits of spatial concentration.

To address the second challenge, we examine whether the increase in firm entry translates into broader measures of economic activity. We first draw on the framework developed by Ulyssea (2018), which models informality in Brazil along both extensive (firm registration) and intensive (informal labor) margins. In this setting, reductions in entry costs increase the number of active firms and induce some informal firms to formalize. While this generates substantial gains for firms that switch status, it also introduces gen-

eral equilibrium effects. Increased entry raises competitive pressure on incumbents, and because marginal entrants tend to be relatively low productivity, the expansion in firm mass may generate negative composition effects on aggregate productivity.

This framework implies that if reduced entry costs primarily induce formalization, we should not expect large or immediate increases in real economic activity. Instead, gains may arise gradually as previously size-constrained firms expand and improve productivity.[8] It also suggests that increases in firm entry may be sharp but transitory, reflecting reclassification rather than new production. Moreover, even if formalization increases along the extensive margin (i.e., firm registration), changes in formal employment may be muted if newly registered firms continue to rely on informal labor. This possibility is particularly relevant because labor regulations are determined primarily at the federal level.

Guided by these considerations, we examine several complementary measures of economic activity, beginning with total municipal GDP. GDP captures aggregate activity regardless of whether gains arise through formalization or new firm creation. Because the LLE reduces barriers to both entry and operation, GDP may increase even if some observed firm openings reflect formalization. We focus on total GDP, rather than per capita, to capture changes in aggregate activity and mitigate composition effects that may reduce productivity. Despite inherent noise in municipal GDP data[9] and a limited post-treatment window,[10] we find that LLE adoption increased total GDP by approximately 2%. We also find that population steadily increased throughout the post-treatment period, suggesting migration responses and stronger local demand.[11]

While these results suggest that economic activity is increasing, they do not by themselves distinguish between formalization and genuinely new economic activity. Thus, we

[8]That informal firms are intentionally small is well-established in the literature (De Paula and Scheinkman, 2007; La Porta and Shleifer, 2008; Ulyssea, 2010; La Porta and Shleifer, 2014).

[9]Municipal GDP in Brazil is constructed indirectly. It is imputed from the state level to municipalities using administrative proxies that rely heavily on formal sector activity (IBGE, 2016).

[10]Due to re-basing the series, the Instituto Brasileiro de Geografia e Estatística (IBGE) has not yet released local GDP estimates for 2024 or beyond.

[11]Brazil's 2020 Census was significantly delayed, requiring interpolation for 2023.

next examine treatment heterogeneity using pre-existing levels of informality. Using the 2010 Census, we split municipalities into "high" and "low" informality groups based on a measure of informal employment. If the effects are driven primarily by formalization, we would expect firm openings to increase sharply following adoption and then taper off, particularly in high-informality areas. By contrast, entry should be more gradual in low-informality areas, where there are fewer firms available to formalize. Our evidence is consistent with this pattern. Moreover, most measures of economic activity increase *more* in low-informality areas, where formalization is less likely to be the driving factor. This suggests that while formalization is an important component of the observed effects, LLE adoption also generates broader economic gains.

The remainder of the paper proceeds as follows. Section 2 describes the institutional details of the LLE, its implementation, and our treatment definition. Section 3 details the data. Section 4 outlines the empirical strategy and discusses the results. Section 5 concludes. Additional details and supplementary results are presented in Appendices A through D.

2 Background and Treatment Indicators

2.1 The Economic Freedom Law

The Economic Freedom Law (*Lei da Liberdade Econômica* – LLE) was first introduced at the federal level as Provisional Measure No. 881 in April 2019 and signed into law as Law No. 13,874 in September 2019. The law establishes a set of principles designed to reduce bureaucratic obstacles and protect individual liberty in economic life. It affirms the freedom to engage in economic activity without prior government approval, the right to legal certainty in public administration, and proportionality in regulatory enforcement. A full translation of the law is provided in Appendix B.

Its key provisions deal with historical problems in Brazil's bureaucratic and regulatory landscape. First, it exempts low-risk economic activities from permit and license

requirements. Second, it establishes tacit approval of applications when authorities fail to respond within a legal deadline. Third, it allows businesses to operate on any day of the year, including weekends and holidays. Fourth, in order to reduce paperwork and the necessity of stamps and physical copies, it recognizes electronic records and digital documents as valid. Finally, it limits the scope on public intervention in private contractual relations.

Since the federal law aims to provide general regulatory principles, these changes are broad and, at times, vague. They must also encompass all levels of government, and thus while all relevant to opening and operating a business, not all of them fall within the jurisdiction of municipalities. For instance, businesses must register with the *Junta Comercial* (commercial registry) and comply with fire safety regulations, both regulated at state-level. Among the changes listed above, only the first (low-risk exemptions) is managed and obtained at the municipal level. Nevertheless, as we discuss below, municipal reform can influence how entrepreneurs interact with higher levels of government. We discuss these issues at greater length in Section 3.1.2 below.

This layered system reflects Brazil's decentralized legal structure and the constitutional distribution of powers between federal, state, and municipal governments. Specifically, the Brazilian Constitution grants municipalities the ultimate authority over issues of sanitation, zoning, and public safety, as these are considered matters of "local interest."[12] Thus, while the federal law establishes an exemption for low-risk activities, it does so as a general "guiding principle" for Brazil's regulatory framework. Municipalities have the final word on whether to effectively adopt.

Though the federal LLE cannot directly change municipal law concerning activities that require licensing and permits, it plays a fundamental role. Without the federal law, a municipality would generally not have the authority to exempt businesses from licensing

[12]The simplest licenses typically establish the hours of operation and address zoning requirements. More complex cases may require separate licenses related to health, sanitation, environmental compliance, or other local regulations. For example, businesses that handle food, health, or personal care services may need distinct health surveillance licenses in addition to the general operating license. Requirements vary by municipality and by the specific activity of the business.

requirements, as federal legislation historically mandated that all businesses must be licensed and registered by municipalities.[13] By establishing the principle of exemption for low-risk activities at the federal level, the LLE provides a legal basis for municipalities to adopt their own local versions of the law, ensuring that such exemptions are consistent with federal norms and reducing the risk of legal challenges.

Municipalities can adopt LLE reforms in two ways. At the most formal level, a municipality can approve a law through its municipal council. However, the municipal mayor also has the ability to set a decree. In general, mayoral decrees tend to be used to operationalize existing municipal laws; however, most municipalities first adopted the LLE through mayoral decrees that acknowledged the federal law established a list of low-risk activities. This is likely because decrees are a quicker way to reform (as opposed to adopting a new municipal law) and the federal LLE provided general legal guidance. We treat the first instance of LLE adoption – whether it comes from decrees *or* through passage in the municipal council – as the municipality's first exposure to local LLE reform.

Lastly, we note that while municipal LLE reforms primarily focus on low-risk classifications and can only modify regulations within municipal jurisdiction, they may also complement reforms at higher levels of government. For example, the law in the city of Porto Alegre (RS) promotes the integration of procedures across municipal agencies and alignment with state-level systems, reducing duplication and simplifying compliance for firms. Taken together, we hypothesize that municipal LLE reforms could increase firm registration (openings) by lowering the costs of formal operation, particularly through reductions in licensing and permitting burdens, *and* by streamlining compliance with overlapping state and federal reform.

2.2 Treatment Indicators

As discussed above, even when a municipality adopts the LLE, differences in legal procedures and local provisions can lead to variation in its implementation. For example,

[13] See Art. 78, Law nº 5.172/1966, National Tax Code.

each municipality defines its own set of "low-risk" activities. Accordingly, we adopt an *intent-to-treat* framework in which LLE adoption defines treatment, while recognizing that the content and intensity of the law may vary across municipalities. We identify adopting municipalities using data from *Liberdade para Trabalhar*, an ongoing project by the Instituto Liberal de São Paulo (ILISP) that tracks the adoption of LLEs across the nation.[14] Based on this, we construct two treatment–control comparison groups, each summarized in Table 1.

Our primary treatment group includes all municipalities that adopted the law in or before 2022.[15] This baseline group contains 1,246 municipalities (see Table 2, *Panel A*). We compare this group to *never-treated* municipalities, defined as those that neither adopted the law at the municipal level nor are located in states that implemented (or partially implemented) equivalent state-level legislation.[16] This group contains 695 municipalities.

As noted above, the LLE operates across three layers of government—federal, state, and municipal—each of which can independently affect firm entry and operation. This first comparison is designed to capture the full effect of local LLE exposure by comparing treated municipalities to those not affected by any subnational reform beyond the federal law (i.e., neither state nor municipal adoption). By restricting the control group in this way, we avoid contamination from overlapping reforms and obtain a clean benchmark for the overall impact of regulatory liberalization. The importance of this comparison is illustrated by the case of Minas Gerais (MG), the state with the largest number of municipalities (853). MG adopted a state-level LLE and implemented an aggressive, state-led framework to promote and standardize municipal adoption. As a result, even municipalities that did not formally adopt the law are likely exposed to meaningful changes in the

[14]ILISP has two formal agreements with the states of Goiás and Paraná and has provided legal support and technical knowledge to multiple state and local agencies to help them implement an Economic Freedom Law in their jurisdictions. They have also collaborated with several state and local representatives through advocacy. Data is available at: https://liberdadeparatrabalhar.com.br

[15]We impose the 2022 cutoff to ensure at least three years of post-adoption data.

[16]We classify municipalities as never treated if they did not adopt through 2025. Because our outcome data end in 2024, this additional year reduces the likelihood of anticipation effects in the control group. For both comparison groups, we additionally include not-yet-LLE adopting municipalities as a robustness check. While this can improve the plausibility of the counterfactual, it may attenuate estimated treatment effects if municipalities or firms respond in anticipation of adoption. Results are largely unchanged.

regulatory environment. In such settings, within-state comparisons would capture only the marginal effect of municipal adoption relative to an already treated baseline, rather than the full effect of the reform.

Our second comparison group is designed to isolate municipal-level variation by netting out the effects of state-level LLE reforms. We compare LLE-adopting municipalities with *never*-adopting municipalities, both located within *ever*-adopting LLE states.[17] Because most municipal adoptions occur within LLE-adopting states, this definition yields 1,141 municipalities (see Table 2, *Panel B*). The corresponding control group includes 2,075 municipalities. Thus, this second comparison greatly expands our control group. These are also municipalities that are much geographically closer than the latter and allows for more testing of potential spillovers.[18]

Figure 1 shows the geographic distribution of municipalities adopting an LLE (top) and the distribution of states adopting a state-level LLE. The map is also color-coded by the timing of these reforms. As the maps show, adoption is highly clustered in the southeastern portion of the country, although many of these adoptions occurred after 2022 and therefore fall outside our evaluation period. Our evaluation samples include all municipalities adopting by 2022 for Group 1 comparisons and all adopting (again by 2022) municipalities within treated states for Group 2 comparisons.[19]

While Figure 1 highlights the geographic and temporal variation in adoption, there is also substantial heterogeneity in how the law is implemented across municipalities. One important dimension of this heterogeneity is the scope of activities classified as low-risk and thus exempt from licensing and permit requirements. Since 2025, the Brazilian government has encouraged municipalities to report the list of activities classified as low-risk and thus exempt from licensing and permit requirements. Although 2,361 municipalities

[17] We do not restrict the comparison based on the timing of state-level adoption. These reforms are typically implemented through legislation rather than decrees, and the legislative process is slow, likely generating substantial anticipation effects prior to formal adoption.

[18] For both comparison groups, we additionally include not-yet-LLE adopting municipalities under the same conditions as the controls. This allows a potentially more plausible counterfactual, but could mute the treatment effect through anticipation effects. Results are largely unchanged.

[19] See also Figure A1 for a map of the exact treated and control municipalities across groups.

have approved an LLE, only 678 have provided this information for the national report[20]. Within this sample, the average number of exempted activities is 740, though this varies substantially—from as few as 17 (1.3%) in Votuporanga (SP) to 1,129 (84.7%) of the 1,332 CNAEs in Pinhalzinho (SC). Notably, the average municipality has significantly expanded the scope of the federal law, which initially designated only 300 activities as low-risk.

3 Data

3.1 Main Outcome Variables

The goal of this paper is to estimate the effectiveness of local LLE adoption by exploiting variation in municipal implementation. Our first set of outcomes focuses on the most direct potential impacts of the policy: firm openings and the average number of hours required to start a business. We describe each in turn below.

3.1.1 Firm Openings

Our first municipal outcome is total firm openings, obtained from the federal government's *Painel de Empresas*. This database aggregates administrative information from individual firm tax numbers (CNPJ), derived primarily from the Brazilian Federal Revenue's (*Receita Federal*) registry. It provides annual data on newly registered firms by municipality, allowing us to construct a measure of firm entry over time, and is available through 2024.

The database also allows for disaggregation by firm size. Of particular importance for this analysis are firms classified as micro (with an annual revenue cap of R$ 360,000) and Individual Micro Entrepreneurs (with a cap of R$ 80,000), the latter commonly referred to as MEIs (*Micro Empreendedor Individual*). While these categories differ in their tax

[20] See https://www.gov.br/empresas-e-negocios/pt-br/drei/tabelas-de-dispensa-de-alvara/ranking-de-atividades-economicas-dispensadas-2o-trimestre-2025.pdf. Accessed October 24, 2025.

treatment,[21] the micro firm category in our data includes both traditional micro firms and MEIs. We therefore treat MEIs as a separate category, while interpreting the broader micro firm measure as encompassing both groups. Micro firms account for approximately 95% of all firm openings, while MEIs alone account for roughly 70% (see Figures A8 and A9 in Appendix A).

There are two challenges in studying firm openings in this context. First, extremely large municipalities, such as state capitals, were among the earliest adopters of the LLE. Second, firm entry is highly volatile from year to year.

As detailed below, our estimation strategy combines stacked difference-in-differences with entropy balancing to construct a plausible control group. While this approach requires only parallel trends in the pre-treatment period (as opposed to identical levels), the large differences in firm openings between treated and never-treated municipalities (see Figures A4 and A5 in Appendix A) raises concerns about common support. Even with pre-treatment matching, it is unlikely that a credible counterfactual can be constructed using the full sample. For the first comparison group, we therefore trim the sample by excluding municipalities above the 10th percentile of average (total) firm openings from 2014–2018 (the years prior to the federal LLE). This yields 931 treated and 677 never-treated municipalities. For the second comparison group, control municipalities are a bit larger and thus we only need to trim the top 1% of the sample. Doing so leaves us with 1,107 treatments and 2,074 controls. Focusing on smaller municipalities is also consistent with prior work in Brazil, where larger municipalities often operate under different institutional and monitoring environments.[22] We present results using the full sample in Appendix D Figure D1, but focus on the trimmed sample in the main text.

[21] MEIs are subject to a lump-sum tax of R$ 81 per month (approximately 15 USD), fixed at 5% of the monthly minimum wage, and face minimal bookkeeping requirements. However, if they exceed this revenue cap by more than 20%, they are automatically converted to a *Micro-Empresa* (micro firm) and become subject to the *Simples* tax system. Although still simplified and offering preferential tax rates (4% of gross revenue), *Simples* entails more extensive bookkeeping. See de Farias and Rocha (2025) for further discussion.

[22] For example, studies of the municipal anti-corruption audit program restrict attention to municipalities with populations below 200,000 (Ferraz and Finan, 2008, 2011; Avis et al., 2018, e.g.,). Indeed, only around 150 ($\approx 2.7\%$) of the 5,570 municipalities in Brazil have more than 200,000 inhabitants, and most of them are either state capitals or in their metro areas.

We must also account for the year-to-year volatility in firm openings when assessing parallel trends and pre-treatment matching. Figures A6 and A7 in Appendix A illustrates the raw pre-treatment variation for a random sample of 12 treated municipalities in each comparison group. Firm openings exhibit substantial short-run volatility, with pronounced peaks and troughs, yet most municipalities display a clear underlying trend. This suggests that matching on any single pre-treatment level or growth rate may fail to capture the underlying dynamics of firm entry. Accordingly, our matching covariates include *smoothed* measures of pre-treatment outcome growth.

Table 3 reports summary statistics for these outcomes. Even in the trimmed sample, firm openings are substantially higher in treated municipalities. Accordingly, we estimate our difference-in-differences specification in logs, which is more appropriate in this setting as it requires similar growth in percentage terms throughout the pre-treatment period. This approach is also consistent with prior work on firms in Brazil (see, e.g., Colonnelli and Prem (2022)).

3.1.2 Time to Start a Business

The second group of outcomes focuses on the time required to start a business, investigating a potential mechanism for greater firm openings. That is, we test whether LLE adoption reduced the number of hours required to open a business. We use rich administrative microdata on the universe of applications to register a business between 2019 and 2026, covering approximately 4.8 million observations. This data measures the number of "business hours" (i.e., hours excluding weekends and holidays) for each of the steps described in Figure B1. In addition, the data include detailed application-level information, including detailed timestamps, municipality, sector, and legal status (e.g., individual micro entrepreneur, individual entrepreneur, etc.), which we use for a matching strategy describe in 5.1. We briefly describe the procedure here and provide additional details in Appendix B.2.

The first step in the process is *viability*, during which the state commercial registry

(*Junta Comercial*) verifies that the proposed business name is available (i.e., does not duplicate an existing entity) and that the proposed activity, as defined by its CNAE industry code, is permitted at the proposed address under local regulations. Although name verification is generally completed quickly, address viability can create delays. Although operating licenses are technically a separate step that should occur *after* incorporation, many municipalities required documents and inspections upfront to asses risk on a case-by-case basis as a pre-condition for address viability.

LLE adoption streamlines this stage in several ways. First, in municipalities that have adopted an LLE, viability verification is conducted through an automated database query, often producing a result within minutes. Second, when a municipality lacks an automated system, the address-viability requirement may be waived altogether. Exemptions also apply to businesses that operate exclusively online. The LLE allowed low-risk firms to avoid these additional burdens altogether. Once the first step is completed, the commercial registry will issue an application document (*Documento Básico de Entrada*, or DBE).

The next stage is *validation*, during which state and local tax offices verify the business meets all legal requirements. At this stage, applicants must submit documents required for registration, including articles of incorporation and information on the firm's owners or partners.[23] LLE adoption substantially streamlines this process. For low-risk activities, many documentation requirements are eliminated altogether, while medium-risk firms are often permitted to submit documents electronically rather than through in-person procedures.

Finally, once the DBE is validated, the commercial registry archives the articles of incorporation and transmits the application to the Federal Revenue Service for the final *registration* stage. At this point, the firm is assigns a unique tax identification number, the CNPJ, which formally completes the incorporation process.[24]

[23]In some states, businesses can expedite this step by adopting pre-approved standardized articles incorporation for their chosen business structure (e.g. sole proprietorship, LLC, etc.), bypassing approval analysis.

[24]Since 2025, an additional federal reform created the *Módulo de Administração Tributária*, which

Because our measures to do not capture licensing times that occur after incorporation, they should be interpreted as lower bounds on the total time required to both open *and operate* a business in Brazil. In municipalities that adopt an LLE, low-risk businesses may begin operating immediately because they are exempt from licensing requirements. Medium-risk firms also benefit from the reform because of tacit approval provisions, whereby applications are automatically approved if the relevant authority does not issue a decision within a specified period, typically between 30 and 60 days.[25] FigureB1 summarizes the business registration process and highlights the channels through which LLE adoption reduces regulatory delays.

3.2 Broad Measures of Economic Activity

We also examine the LLE's broader effects on the municipalities. Specifically, we look at municipal GDP, population, and firm/establishment stocks.

Municipal GDP data comes from the Brazilian Institute of Geography and Statistics (IBGE) and is available through 2023.[26] It is measured in 2010 $R. Thus, our post-treatment period is shortened. Moreover, some researchers argue against using this measure as it is imprecise and only imputed from state-level aggregates (Colonnelli and Prem, 2022, e.g.,). Nevertheless, it provides a useful starting point in measuring total economic activity. We also use the IBGE's estimates of municipal population as a complementary indicator of activity. This series is available through 2024. However, there is a gap in this series in the year of 2023 due to delays in the 2020 Census and thus we interpolate this year for each municipalities using the 2022 and 2024 values.

allowed businesses to choose their tax regime (in particular, opting into *Simples*, a simplified regime) at this stage. If their application for the *Simples* regime is denied, their CNPJ will be issued regardless and they may start operating under the standard tax regimes (Receita Federal do Brasil, 2025). Before that, business wanting to opt into *Simples* would have to apply after their CNPJ is already issued, delaying the start of operations. See Paes (2014) and de Farias and Rocha (2025) on the *Simples* tax regime.

[25] State and local agencies can still inspect operations *ex post*. Non-compliant business may have their operation suspended at this point.

[26] The latest GDP estimates are delayed due to the re-basing of the national accounts system following the most recent Census. See IBGE documentation: https://biblioteca.ibge.gov.br/visualizacao/livros/liv102094.pdf.

Lastly, we use data on firm and establishment (individual locations) stocks from Central Register of Enterprises (CEMPRE). A central question is whether the LLE is increasing economic activity, and while firm openings are a part of that, firm closings could offset this increase.[27] Moreover, firm openings are calculated based on new tax numbers and could be driven by an increase in the number of a single firm's locations (as each location has a unique number) instead of entirely new firms. However, a shortcoming of this database, is that there were five major changes in reporting procedures that culminated in a series break in 2022 and beyond.[28] Therefore, for this dataset we can only study the first cohort (group of treated municipalities) as this is the only group with sufficient post-treatment years before this break.

3.3 Other Data

We also collect a set of indicators capturing additional municipal characteristics, some of which were introduced in the discussion of the matching procedure. These include value-added shares of agriculture, services, and industry, as well as the share of the population residing in urban areas. We use these variables to assess pre-treatment differences between treated and control municipalities (see Table 4) and as matching covariates in both robustness checks (Appendix D, Figures D4 and D4b) and our main matching results (Table 5). These data are constructed by the IBGE and obtained from the Institute of Applied Economic Research (IPEA).

Finally, using the most recent pre-treatment Census (2010), we construct a measure of informality by calculating the share of workers employed without a formal contract. This measure allows us to distinguish municipalities with relatively large informal sectors, where the potential for formalization effects is likely to be greatest.

[27] We did test this separately and find no evidence that firm closings increased. These results are available upon request.

[28] See here for details.

4 Empirical Strategy

We estimate the effect of the *Lei da Liberdade Econômica* (LLE) on firm openings and broader economic outcomes using an intent-to-treat framework implemented through a staggered difference-in-differences design combined with pre-treatment entropy balancing. We complement this analysis with disaggregated microdata on firm applications and a combined Mahalanobis distance matching and entropy balanced framework to examine whether the LLE reduced bureaucratic burdens. We begin by outlining our primary empirical strategy (a staggered difference-in-differences design) for estimating the effects of the LLE on firm openings and other economic outcomes. We then discuss our approach for identifying the law's effect on its primary mechanism: reducing bureaucratic burdens.

4.1 Stacked Difference-in-Differences

Because municipalities adopt the law at different times, our difference-in-differences design is staggered. A central concern in such settings with two-way fixed effects (TWFE) estimators is that already-treated units may be used as counterfactuals for later-treated units. This is often referred to as the "forbidden comparisons" problem (Roth et al., 2023; de Chaisemartin and D'Haultfœuille, 2023; Borusyak et al., 2024). When treatment effects are heterogeneous over time or across units, these comparisons can produce inconsistent estimates, potentially even of the wrong sign (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020).

To address this concern, we employ a stacked estimator similar to those used in Cengiz et al. (2019), Butters et al. (2022), and Grier et al. (2025). This approach organizes the data into a series of cohort-specific event studies centered on each adoption year. Treated municipalities are assigned to cohorts based on the year in which they *first* adopted the LLE (2019–2022), while never-treated municipalities over the same period serve as potential controls. The resulting cohort-specific datasets are then stacked, and a regression with appropriate fixed effects is used to estimate an average treatment effect

on the treated (ATET). By restricting the control group to never-treated municipalities, we avoid contamination from forbidden comparisons.

Our estimating equation is as follows:

$$y_{itc} \;=\; \mu_{ic} \;+\; \lambda_{tc} \;+\; \sum_{k \in \mathcal{K},\, k \neq -1} \beta_k \, \mathbf{1}\{t - T_i = k\} \;+\; \varepsilon_{itc}, \tag{1}$$

where μ_{ic} are cohort-specific municipality fixed effects, λ_{tc} are cohort-specific time fixed effects, $\mathbf{1}\{t - T_i = k\}_c$ are event-time indicators with $k = -1$ as the omitted period, and $\mathcal{K}$ is the chosen event window (e.g., $k \in [-5, +5]$). The coefficients β_k trace dynamic LLE effects; a static ATT can be obtained by replacing the event-time terms with a single post indicator $D_{itc} = \mathbf{1}\{t \geq T_i\}_c$. Standard errors are clustered at the municipality level.

4.1.1 Entropy Balancing

Even after addressing the forbidden comparisons problem, identification depends on the assumption that treated and control municipalities would have followed similar trends in the absence of treatment. Tables 3 and 4 document clear differences between treated and never-treated municipalities prior to adoption. In particular, treated municipalities have significantly more firm openings, are larger, more urban, and require more time to open a business in the pre-treatment period. While these differences are not necessarily problematic for identification, they suggest that a naïve DiD could be biased by pre-existing trends. Indeed, the unconditional stacked DiD estimates reflect this concern (see Appendix D Figures D5 and D5b).

To address these imbalances, we pre-weight the data using entropy balancing (Hainmueller, 2012; Hainmueller and Xu, 2013). This approach constructs weights such that, within each cohort, the mean of the covariates among never-treated municipalities matches that of the treated group in the pre-treatment period. Entropy balancing has been widely used in political economy applications (e.g., Angelucci et al., 2022; Bologna Pavlik et al., 2025; Rayamajhee and March, 2025) and is conceptually similar to other difference-in-differences approaches that combine weighting with regression (Sant'Anna and Zhao,

2020; Callaway and Sant'Anna, 2021). Our goal is not to eliminate all observed differences in Tables 3 and 4, but rather to construct a counterfactual with plausibly similar pre-treatment firm dynamics.[29]

We implement entropy balancing separately within each treatment cohort. For each cohort c, we construct weights so that never-treated municipalities resemble cohort-c municipalities throughout the pre-treatment period. Because never-treated municipalities appear in multiple cohorts, a given control municipality can receive different weights across cohorts. This allows the counterfactual comparison group to vary across cohorts, reflecting differences in the composition of treated municipalities over time.

Our preferred specification balances on pre-treatment firm-opening dynamics, which are the most relevant predictors of the untreated counterfactual path in a difference-in-differences framework. In this setting, the primary concern is that treated municipalities were already on different growth trajectories prior to adoption; balancing on pre-treatment outcome dynamics directly addresses this concern. We focus on two covariates: smoothed growth over the full pre-treatment period (T-5 to T-1) and the standard deviation of annual growth over the same interval. As discussed in the preceding section, firm openings are volatile, and these measures capture underlying dynamics without requiring matches on any single pre-treatment year. As shown in Appendix A Figures A2 through A3, our weighting strategy does not dramatically alter the control group.

For each firm-opening category (total, micro, or MEI), we match on dynamics specific to that group. For non–firm-opening outcomes, the weighting strategy is analogous, but with some important differences. For time-to-open variables, the limited pre-treatment data prevent matching on outcome-specific dynamics; instead, we match on total firm-opening dynamics (smoothed growth and its standard deviation). For population and GDP, which exhibit less volatility, we match only on smoothed growth over the same

[29]Matching on all covariates generates extreme weights, so we limit the set of matching covariates to balance comparability with weight stability. In Appendix D, we expand the covariate set using inverse propensity score weighting and obtain similar results. We also consider specifications that include not-yet-treated units as controls, which yield more comparable samples and again produce similar estimates. These additional results are available upon request.

pre-treatment period.

4.1.2 Aggregation & Window Trimming

Finally, we discuss aggregation and the interpretation of the resulting event-time estimates. Wing et al. (2024) highlight that stacked difference-in-differences estimators implicitly aggregate effects by assigning weights based on cohort shares. Specifically, treated units in cohort c receive weight proportional to the share of total treated observations in cohort c, while control units receive weights proportional to their share among controls. As a result, even within a cohort, treated and control units are not necessarily equally weighted, which can complicate interpretation. Moreover, if the sample is not trimmed such that each event-time observation is available for every cohort, compositional differences across event time can further bias estimates. To address these concerns, Wing et al. (2024) recommend a trimmed aggregate average treatment effect on the treated (ATET), where each cohort-specific ATET is weighted by its share of treated observations. We follow this approach.

Our entropy-balancing weights are normalized within each cohort so that their sum equals the number of treated observations in that cohort. Under this normalization, both treated and control units within a cohort receive the same total weight—equal to that cohort's share of total treated observations. This ensures that the resulting estimates can be interpreted as an average treatment effect on the treated, with each cohort contributing in proportion to its share of treated units. In addition, our estimates are not affected by compositional changes across event time, as we restrict attention to cohorts for which all pre-treatment and post-treatment periods are observed.

In Appendix C, we report cohort-specific estimates. Each cohort provides a standard two-group, dynamic difference-in-differences setting, without the need to pool across cohorts. This allows us to examine longer event windows within each cohort. In particular, for the 2019 cohort, we can extend the post-treatment window through 2024, while for later cohorts we can assess the plausibility of parallel trends over a longer pre-treatment

period. Importantly, the cohort-specific estimates are consistent with the aggregate results.

4.2 Matching

Our main results use aggregated municipal data and a staggered difference-in-differences design to estimate the effects of the LLE on firm openings and broader economic outcomes. This approach is well suited to studying aggregate impacts and allows us to assess the plausibility of the identifying assumptions through pre-treatment trends. However, it provides less direct evidence on the mechanism through which the law operates. Fortunately, we also have access to detailed microdata that track firm applications and allow us to examine the LLE's effects on bureaucratic processes more directly.

Two features of these data make our primary staggered difference-in-differences framework less attractive. First, the data are only available beginning in 2019, leaving a limited pre-treatment period. Second, aggregating the data to the municipal level would discard much of the rich application-level information. We therefore employ a two-step matching procedure that combines Mahalanobis distance matching (MDM) with entropy balancing.[30] Specifically, we impose exact matches on state, month, year, three-digit sector code (CNAE), and legal type, while additionally matching on municipal characteristics including population, GDP per capita, agricultural, and industrial value-added shares, and urban population.

Our two-step matching procedure is as follows. First, we use Mahalanobis distance matching to identify the $k \in \{1, 2, 3\}$ nearest neighbors based on population, GDP per capita, agricultural, and industrial value-added shares shares, while imposing exact matches on state, month, year, the three-digit sector code, and legal type. Second, we refine the resulting Mahalanobis neighbor grouped sample using entropy balancing

[30]MDM is commonly used in the political economy literature (Grier and Grier, 2021; Bastos and Pavlik, 2024; Bologna Pavlik et al., 2025). There is some evidence that it produces better matches than other matching procedures, such as Propensity Score Matching (PSM) (King and Nielsen, 2019). Using entropy balancing as a second step adjusts the MDM weights to ensure balance in the specified variables.

to achieve exact balance on the the targeted covariate moments.[31] We also report an specification that uses entropy balancing starting from equal weights (Jann, 2017). Because this design lacks the time variation that fixed effects would otherwise difference out, identification rests more directly on cross-sectional comparability between treated and control municipalities. We therefore prefer the combined MDM–entropy-balancing approach, which prioritizes locally close matches *before* imposing aggregate balance, over entropy balancing alone.[32]

The identifying assumptions differ with matching. Difference-in-differences relies on the assumption that treated and control units would have followed parallel trends in the absence of treatment, whereas matching relies on the assumption that treatment assignment is independent of potential outcomes conditional on the matched covariates. Given the richness of the matching procedure, particularly the exact matching within states, time, industry, and legal form, we focus on a single comparison group: firms located in adopting municipalities versus firms located in never-adopting municipalities. To the extent that municipal and state-level LLE reforms are complementary, these estimates should be interpreted as a lower bound of the municipal law's effect. While this approach relies on different identifying assumptions than our main specification, it provides a useful complement by offering direct evidence on the mechanism through which the LLE may affect economic activity.

4.3 Testing for Spillovers

A key concern with place-based policies is the potential for negative spillovers or relocation, resulting in zero-sum gains. That is, improvements in treated municipalities may come at the expense of neighboring areas. We examine this concern two ways.

First, we restrict the sample to never-treated municipalities and redefine treatment. A municipality is considered treated if any of its contiguous (bordering) neighbors adopt

[31] We use the first moment as our target.

[32] MDM also takes into account the correlation between covariates.

the LLE, with treatment timing defined by the first such adoption. If the policy increases firm openings by reallocating activity from nearby areas, we should observe a decline in outcomes among never-treated municipalities located near treated ones. Importantly, this treatment is defined prior to restricting the sample to our two comparison groups (treatments in any state versus treatments in LLE-states) and is equal to one if a never-treated municipality borders any treated municipality, regardless of state-level LLE adoption. As a result, never treated municipalities in the first comparison group are less likely to border treated municipalities than those in the second. For both, we recalculate the weights so that the never-treated controls without an LLE neighbor are matched to never-treated controls with an LLE neighbor.

Our second test examines heterogeneous treatment effects by allowing the impact of the LLE to vary with the share of neighboring municipalities that also adopt the policy. We restrict attention to the first comparison group (treatments in any state), as this provides the most meaningful variation in isolation by including municipalities outside LLE-adopting states. We construct an isolation measure equal to the share of contiguous municipalities that do not adopt the policy, so that higher values indicate greater isolation. If the observed effects reflect firm reallocation, we would expect larger effects in more isolated municipalities. Our entropy balancing weights are calculated here according to the treatment designation and not the isolation measure.

A caveat to this second test is that isolation may be correlated with state-level adoption. Our baseline focuses on the first comparison group, which includes treatments in both LLE-adopting and non-adopting states. If isolation is systematically higher in non-LLE states, the estimated effects may reflect broader state-level differences rather than isolation per se. To address this concern, we re-estimate the results separately by subsample. First, we restrict attention to municipalities within LLE-adopting states, reweighting the data to construct an appropriate counterfactual. We then repeat the analysis for municipalities in non-LLE states. If the results persist within LLE-adopting states, this suggests that the estimated effects are driven by isolation rather than broader

state-level factors.

4.4 Economic Activity

The final section of the paper examines broader measures of economic activity. Much of the empirical strategy has been described above, and we therefore apply the same framework here. The key extension is that we allow the treatment effect to vary across municipalities with differing levels of informality. Specifically, we compare outcomes between municipalities with relatively high and low pre-treatment informality. In high-informality areas, we expect the policy to generate more formalization, with potentially smaller increases in measured economic activity if these firms were already operating informally. In contrast, in low-informality areas, we expect larger increases in economic activity reflecting new firm creation.

5 Results

We begin by examining the effect of LLE adoption on firm openings. Figure 2 presents results for both comparison groups. Panel A shows Group 1 comparisons—never-treated municipalities in never-treated states versus municipal adopters in any state—while Panel B restricts the sample to treated and control municipalities within LLE-adopting states. We maintain this structure throughout the section.

Across both panels, we find no evidence of differential pre-trends. Firm openings increase across all three categories—total, micro, and individual micro-entrepreneurs (MEI)—beginning in the year of treatment, with effects that grow over time.

In terms of magnitude, these effects are economically meaningful. Two years after adoption, LLE implementation increases total firm openings by approximately 8 percent relative to the counterfactual path. Evaluated at the pre-treatment mean among treated municipalities of roughly 200 firms (see Table 4), this corresponds to an increase of about 16 additional firms. Because the counterfactual path itself exhibits modest positive

growth over this horizon, this calculation slightly understates the true increase in the number of firms.

The effects are smaller in Comparison Group 2 (Panel B), reaching approximately 5 percent after two years. This suggests that part of the overall effect operates through state-level reforms and/or complementarities between state and municipal policies.

5.1 Time to Open a Business

We next examine how LLE adoption affects the time required to start a business. We compare the total processing time for an individual application in a treated municipality (LLE adopter) to matched applications in non-adopting municipalities within the same state, month, year, sector (3-digit CNAE), and legal type (using IBGE's *Classificação de Natureza Jurídica*). Application-level characteristics are matched exactly, while municipal characteristics are matched using a Mahalanobis distance algorithm where the resulting sample is further refined using entropy balancing. We consider $k \in 1, 2, 3$ nearest neighbors, with results reported in Table 5. As with the main results, these estimates are reported for a trimmed sample that excludes municipalities above the 99^{th} percentile of the population distribution.

Panel A of Table 5 uses our preferred two-step matching procedure. First, we identify the k closest neighbors using the Mahalanobis distance metric. Second, using this subsample, we then refine the weights using entropy balancing to achieve exact balance on the first moments of the matching variables. With this approach, we find that LLE adoption significantly reduces the time required to open a business. The estimates range from 22.7 to 29 hours, corresponding to a 9.6 to 12.3 percent decline relative to the control-group mean.

Panel B of Table 5 uses entropy balancing alone. Here, the estimates are substantially larger, ranging from 43.9 to 44.9 hours, or approximately 17 percent relative to the control-group mean. Unlike Panel A, entropy balancing alone does not restrict the control group to similar observations at the individual level. Instead, it re-weights all available

control municipalities to achieve balance on the targeted moments. As a result, treated and control applications may differ considerably in their baseline processing times despite satisfying the aggregate balance conditions. This distinction is particularly important in the present setting because, unlike our difference-in-differences framework, identification relies on cross-sectional comparability rather than within-unit changes over time. We therefore prefer the combined MDM-entropy-balancing approach, which restricts comparisons to applications with similar observable characteristics before imposing aggregate balance.

As discussed in section 3.1.2, these records do not capture the time required to obtain licenses and permits after a business is formally registered. Consequently, we interpret them as a lower-bound estimate of the time costs to open *and operate* a business in Brazil.[33]

5.2 Spillover Effects

Next, we examine whether spillovers could explain our results. For total firms, we find no evidence that never-treated municipalities contiguous to treated municipalities experienced a *reduction* in firm openings or an *increase* in firm closings relative to never-treated municipalities without treated neighbors (Figure 3). These results use the same covariates and weighting procedure as the main specification and are nearly identical when running results using an unweighted counterfactual. There's some evidence that individual micro-entrepreneurs might be migrating within states from non-LLE adopters to LLE-adopters, but this effect is relatively small and decreasing through time.

We also examine whether the reform is more effective when implemented in isolation (Figure 4). In the full sample (Panel A), we find no evidence supporting this hypothesis; if anything, treatment effects are weaker for municipalities that adopt the reform in isolation, particularly two years after adoption. We next split the sample by treatment

[33]Moreover, they are likely attenuated by the nationwide adoption of streamlined registration systems, which substantially improved the speed of the process across all municipalities. Thus, it is notable that we still find significant effects associated with municipal adoption.

type—municipal adopters outside LLE-adopting states (Panel B) and those within LLE-adopting states (Panel C)—to separate isolation effects from broader state-level reforms. The aggregate effect appears to be driven primarily by municipalities adopting outside LLE states. In these areas, we find stronger evidence that isolation is associated with weaker treatment effects. By contrast, we find little evidence of isolation effects within LLE-adopting states, likely because municipalities in these states are rarely isolated.

5.3 Economic Activity & Formalization

Our final set of results returns to the question of whether LLE adoption increased true economic activity or primarily encouraged formalization. While formalization is likely beneficial in the long run, its short-run effects on measured economic activity may be limited.

We first examine whether LLE adoption increased total GDP (Figure 5), total population (Figure 6), and the total stock of firms and establishments (Figure 7). In all three cases, we find clear evidence of increases in economic activity. However, both GDP and firm counts rely on formal measures and may mechanically rise with increased formalization, making it difficult to interpret these changes as purely reflecting gains in underlying economic activity.

To address this, we split the sample by pre-treatment informality (measured in 2010) into high- and low-informality municipalities. We begin with firm openings (Figure 8). In high-informality municipalities—where the potential for formalization is greatest—we observe a sharp, immediate increase in firm openings following adoption, which then dissipates over time. In contrast, in low-informality municipalities, firm openings increase more gradually and continue to rise over time. This pattern holds across both comparison groups. We apply the same split to other measures of economic activity. We find that GDP, population, and the stock of firms and establishments increase more in low-informality municipalities (Figure 9).[34]

[34]We present these results for the second comparison group only, as the weights are more stable. In

Taken together, these results suggest that while formalization likely plays an important role, it does not fully explain the observed effects. Instead, the evidence is consistent with increases in underlying economic activity.

6 Conclusion

This paper examines the effects of Brazil's *Lei da Liberdade Econômica* (LLE), a major regulatory reform designed to reduce bureaucratic barriers to entrepreneurship. Exploiting staggered municipal adoption and an entropy-balanced stacked difference-in-differences framework, we provide causal evidence on how reductions in regulatory burden affect firm dynamics and local economic outcomes.

We find that LLE adoption leads to meaningful increases in firm formation. Two years after adoption, firm openings increase by approximately 8 percent relative to the counterfactual path, with effects driven almost entirely by micro and individual microentrepreneur firms. The reform also substantially reduced the time required to start a business. Using detailed microdata covering 4.8 million registration applications, we estimate that the time needed to open a business declined by between 9 to 12 percent. This indicates that the LLE meaningfully reduced bureaucratic delays.

Beyond firm dynamics, we find that LLE adoption increases GDP, population, and the stock of firms and establishments. While some of these gains may reflect increased formalization, heterogeneity by pre-treatment informality suggests that formalization is not the sole mechanism. In particular, municipalities with lower levels of informality exhibit sustained increases in firm openings and broader economic activity, consistent with genuine expansions in underlying economic activity rather than purely compositional shifts.

We also examine whether these effects reflect a reallocation of economic activity across municipalities rather than net gains. We find no evidence of negative spillovers to neigh-

the first comparison group, control municipalities are predominantly high-informality, limiting suitable matches for low-informality treated units.

boring municipalities and no indication that the reform is more effective in isolation. If anything, the LLE appears less effective when implemented in isolation, suggesting that clustering and agglomeration forces may play an important role in amplifying its impact.

Importantly, these positive effects persist throughout the COVID-19 period. This is notable given that early adopters tend to be more densely populated municipalities and were therefore among the most affected by the pandemic. Moreover, our analysis exploits only municipal-level variation in implementation. Because all municipalities were exposed to the federal provisions of the LLE and many were also affected by complementary state-level reforms, our estimates likely represent a lower bound on the law's overall economic effects.

Taken together, our results provide causal evidence that reducing bureaucratic barriers to entry can stimulate firm creation and improve local economic outcomes. More broadly, they suggest that even relatively targeted regulatory reforms can meaningfully improve the business environment in developing economies, particularly when supported by complementary policies and coordinated implementation across jurisdictions.

An important avenue for future research is to better understand the broader distributional and macroeconomic consequences of such reforms. The federal LLE generated substantial debate regarding its implications for labor-market outcomes, worker safety, and inequality. These effects are more likely to manifest at the national level and therefore lie beyond the scope of the present analysis. Evaluating these broader consequences remains an important task for future research.

Tables and Figures

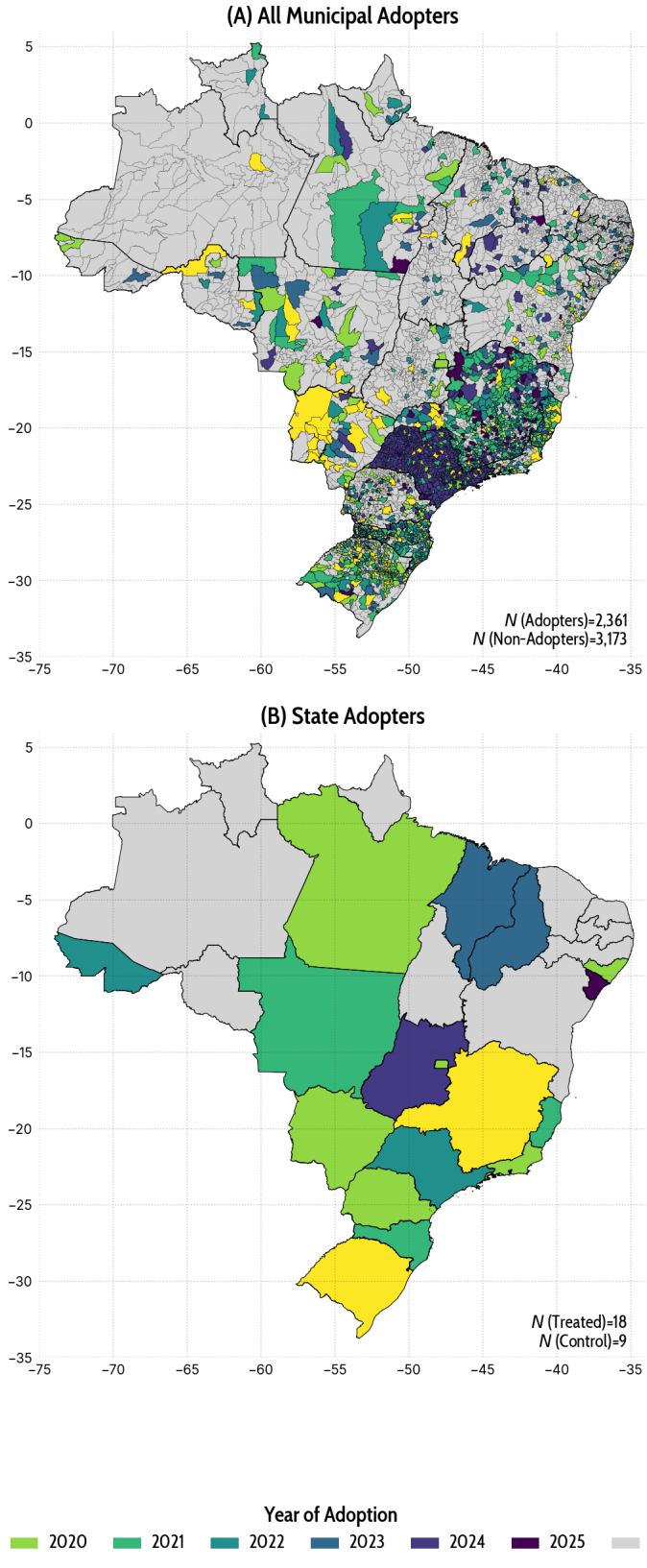


Figure 1: Adoption of Municipal LLEs

Notes: This figure plots the geographic distribution and timing of adoption of LLEs at both the state and municipal level. ***Panel A*** shows all municipalities adopting a LLE, ***Panel B*** does the same for state adoption. The different colors represent the different years of adoption. Data from the Instituto Liberal de São Paulo - *Liberdade para Trabalhar*.

Table 1: Municipal Treatment and Control Definitions

Definition		**N**
Group 1		
Treatment	LLE-adopting municipalities (in any state) that adopted in or before 2022	1,246
Control	Never-adopting municipalities in never-adopting states (through 2025)	695
Group 2		
Treatment	LLE-adopting municipalities (in or before 2022) in LLE-adopting states	1,141
Control	Never-adopting municipalities (through 2025) in LLE-adopting states	2,075

Table 2: Municipal Economic Freedom Law Adoption by Year

***Panel A**: LLE adopting vs. non-adopting municipalities (**Group 1**).*

	Panel A1: Full Sample		***Panel A2***: Trimmed Sample (Bottom 90%)	
Year	Adopt	Cumulative	Adopt	Cumulative
2019	166	166	94	94
2020	252	418	169	263
2021	466	884	366	629
2022	362	1,246	302	931
2023	265	1,511	231	1,162
2024	723	2,234	614	1,776
2025	127	2,361	121	1,897

***Panel B**: LLE adopting vs. non-adopting municipalities, both in LLE states (**Group 2**).*

	Panel B1: Full Sample LLE States Only		***Panel B2***: Trimmed Sample (Bottom 99%) LLE States Only	
Year	Adopt	Cumulative	Adopt	Cumulative
2019	148	148	138	138
2020	228	376	217	355
2021	427	803	419	774
2022	338	1,141	333	1,107
2023	233	1,374	232	1,339
2024	696	2,070	687	2,026
2025	122	2,192	121	2,147

Notes: This table includes all municipal LLE adoptions (either through mayoral decree or through municipal council) from 2019 through 2025. Our main analysis only considers adoptions from 2019–2022 given post-treatment data availability. The first trimmed sample (***Panel A2***) drops municipalities in the top 10% of average (total) firm openings from 2014–2018 and will constitute our main sample. **Panel B1** shows all municipal adoptions occurring within states that also adopted an LLE after dropping the top 1% of firm openings. These issues are discussed in detail in the text.

Table 3: Summary Statistics of Outcome Variables in the Year Prior to Treatment

	All Municipalities		Treatment Law		Control	
Variable	Mean	SD	Mean	SD	Mean	SD
Panel A: LLE adopting municipalities vs. non-adopting municipalities (Group 1).						
Firm Openings						
Total Firms	119.55	153.99	208.29	200.62	89.04	120.01
Micro Firms	114.09	146.70	197.90	190.44	85.28	114.97
MEI Firms	82.66	107.64	145.72	140.67	60.99	83.28
GDP & Population						
GDP	156,742	251,988	285,710	401,856	112,404	148,894
Population	15,622	15,960	16,968	15,796	15,160	15,993
Panel B: LLE adopting vs. non-adopting municipalities, both in LLE treated states (Group 2).						
Firm Openings						
Total Firms	209.93	578.89	725.27	1,315.30	141.13	329.98
Micro Firms	199.00	548.71	686.46	1,244.11	133.93	314.40
MEI Firms	144.71	410.18	505.55	922.12	96.53	239.96
GDP & Population						
GDP	275,312	741,044	916,771	1,819,285	189,676	344,492
Population	19,121	33,393	43,300	68,261	15,893	23,536

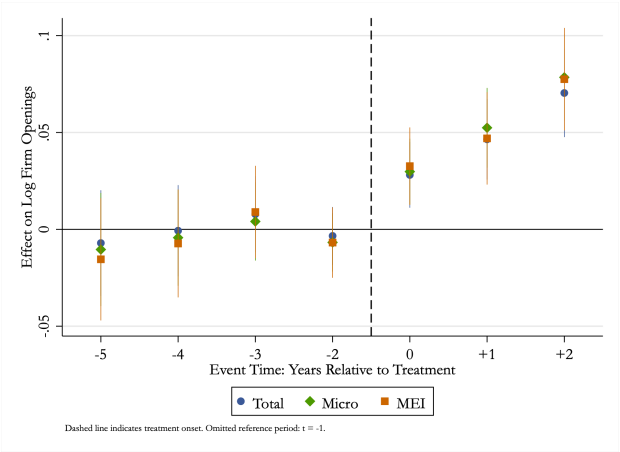
Notes: Summary statistics are measured at relative year −1 (the year prior to treatment) for treated municipalities. For control municipalities, using the stacked difference-in-difference design, relative year −1 is the year before treated municipalities within their cohort receive a treatment. ***Panel A*** restricts the control group to municipalities that are never treated, not located in a treated state, and not in a partially treated state. ***Panel A*** summary statistics are given only for the trimmed (bottom 90%) sample. ***Panel B*** restricts both treatment and control municipalities to those in states with an LLE at the state level and are given only for the trimmed (bottom 99%) sample. Firm openings and GDP per capita are logged in regressions. Given the prevalence of zeros in the days to start a business variables, we use their untransformed value but estimate the stacked model using a Poisson distribution. We provide similar results using alternative specifications in **Appendix B**.

Table 4: Baseline Summary of Municipal Characteristics

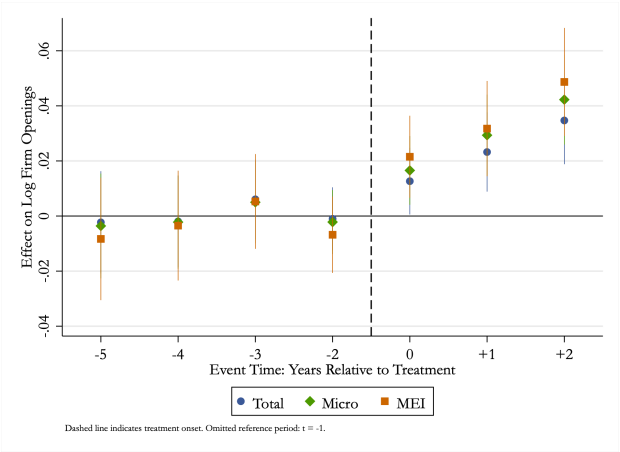
	All Municipalities		Treatment Law		Control	
Variable	Mean	SD	Mean	SD	Mean	SD
Panel A: LLE adopting vs. never-adopting municipalities in never treated states (Group 1).						
Pre-Treatment Characteristics						
Urban %	0.582	0.204	0.637	0.210	0.579	0.167
Agriculture VA %	0.221	0.155	0.209	0.149	0.151	0.124
Services VA %	0.664	0.169	0.627	0.154	0.763	0.147
Industry VA %	0.115	0.129	0.164	0.146	0.086	0.108
Observations						
N	4,046		931		677	
Panel B: LLE adopting vs. never-adopting municipalities in LLE treated states (Group 2).						
Pre-Treatment Characteristics						
Urban %	0.609	0.216	0.705	0.223	0.580	0.213
Agriculture VA %	0.205	0.156	0.178	0.154	0.249	0.162
Services VA %	0.669	0.167	0.632	0.150	0.641	0.173
Industry VA %	0.126	0.134	0.190	0.152	0.110	0.127
Observations						
N	4,473		1,107		2,073	

Notes: ***Panel A*** restricts the control group to municipalities that are never treated, not located in a treated state, and not in a partially treated state; it is trimmed by dropping the top 10% of average firm openings from 2014-2018. ***Panel B*** restricts both treatment and control municipalities to those in states with an Economic Freedom Law at the state level; it is trimmed by dropping the top 1% of average firm openings from 2014-2018. Observation numbers for the urban share category are slightly lower (by 5 at most) because this data comes from the 2010 Census and 5 municipalities have been created since then. Value added sector shares use averaged data from 2014–2018.

Figure 2: The Effect of LLE Adoption on Firm Openings by Firm Type



(a) Group 1 (Treatment in Any State)



(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

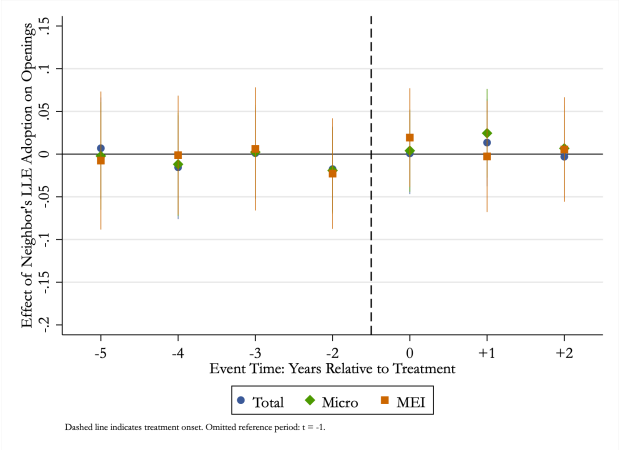
Table 5: Effect on Time to Open a Business: Application-Level Matching Results

Neighbors		Time to Open (in "business" hours)	Matched Sample Statistics Stat.	Treated	Control
Panel A: Trimmed Sample, Two-Step Matching					
k=1	ATT	-29.056***	Mean	206.247	235.302
	S.E.	(5.420)	N	501,933	215,822
k=2	ATT	-24.452***	Mean	207.837	232.289
	S.E.	(5.000)	N	454,807	295,123
k=3	ATT	-22.734***	Mean	208.627	231.361
	S.E.	(4.914)	N	419,707	337,157
		Total (Unmatched)	Mean	206.475	255.453
			N	572,675	980,893
Panel B: Trimmed Sample, Entropy Balance Matching					
k=1	ATT	-44.908***	Mean	206.247	251.155
	S.E.	(4.070)	N	501,933	215,822
k=2	ATT	-43.308***	Mean	207.837	251.145
	S.E.	(4.007)	N	454,807	295,123
k=3	ATT	-43.951***	Mean	208.627	252.578
	S.E.	(4.072)	N	419,707	337,157
		Total (Unmatched)	Mean	206.475	255.453
			N	572,675	980,893

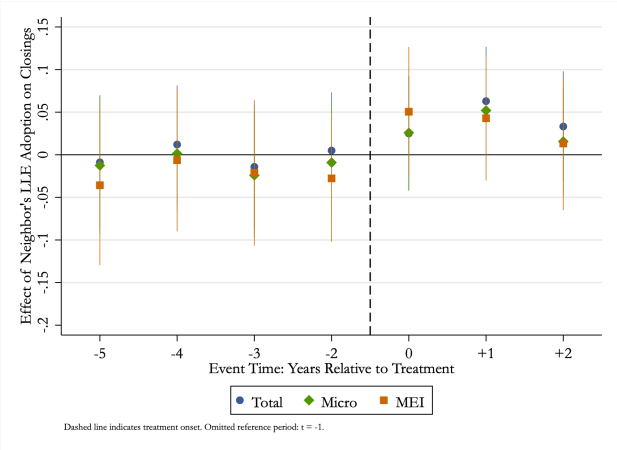
Notes: This table reports estimates of the average treatment effect of adopting an LLE on the total time (in "business" hours) to open a business, using application-level data. "Business" hours excludes weekends and holidays. We compare the total time for an individual application in a treated municipality (LLE-adopter) to matched applications in the same month, year, state, sector (3-digit CNAE), and legal type (using IBGE's *Classificação de Natureza Jurídica*), in a similar control municipality (non LLE-adopter). Application-level variables are exact matched, while municipality characteristics are matched using Mahalanobis Distance algorithm. Both panels exclude applications in municipalities at the top population percentile (> 481,749 people). ***Panel A*:** the Mahalanobis distance method reduces the covariate distance between treated units and potential control units to find a set of $k \in \{1, 2, 3\}$ nearest neighbors and matching weights are refine using entropy balance to achieve perfect covariate balance. ***Panel B*:** Proceeds directly to entropy balancing within observations in the common support of the data. For each sample, we consider $k \in \{1, 2, 3\}$ nearest neighbors. Standard errors clustered at the municipality-by-year level. ***, **, * denote significance at the 1%, 5%, and 10% levels. N is the number of treated and control observations in each sample. See Table A1 for additional summary statistics.

Figure 3: The Effect of a Contiguous Neighbor's LLE Adoption on Firm Openings and Closings in Never Treated Municipalities

Panel A: Group 1 (Treatment in Any State)

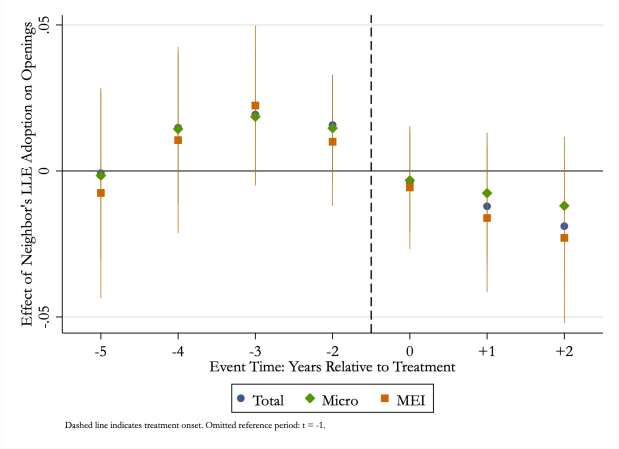


(a) Firm Openings

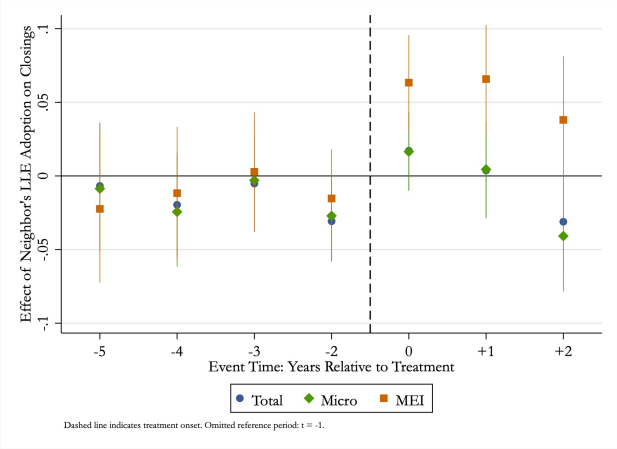


(b) Firm Closings

Panel B: Group 2 (LLE-Adopting States)



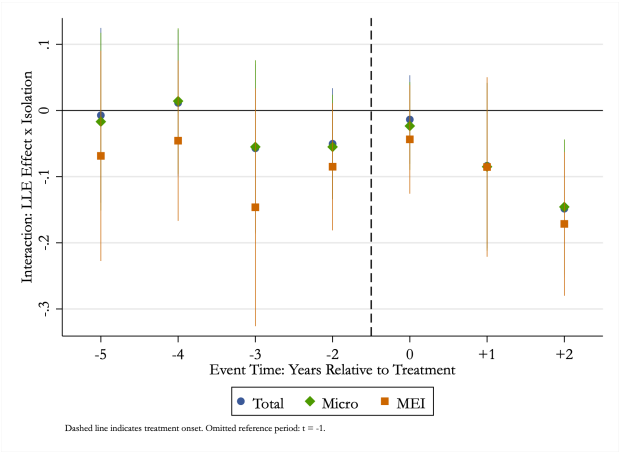
(c) Firm Openings



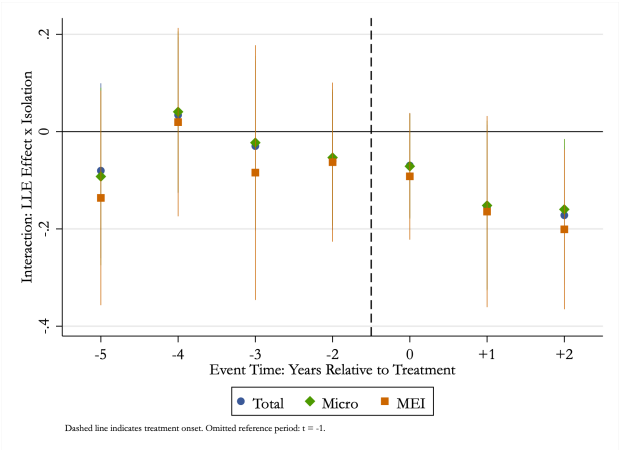
(d) Firm Closings

Notes: Each graph lots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period to create weights. This figure uses only municipalities without a municipal LLE law; the treatment defined by the first year in which a contiguous neighbor adopted an LLE law.

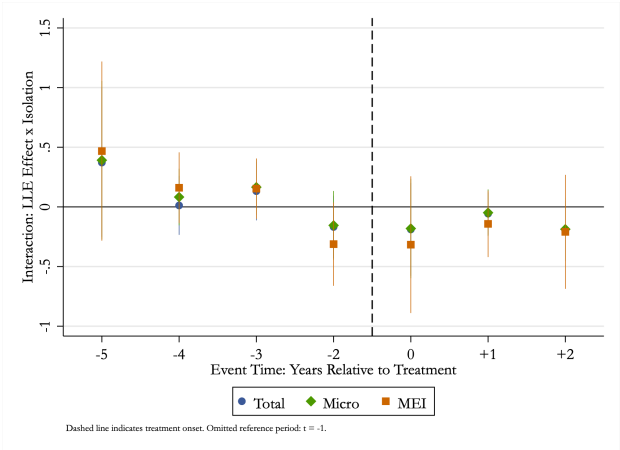
Figure 4: The Differential Effect of Adopting an LLE in Isolation



(a) Group 1 (Treatment in Any State)



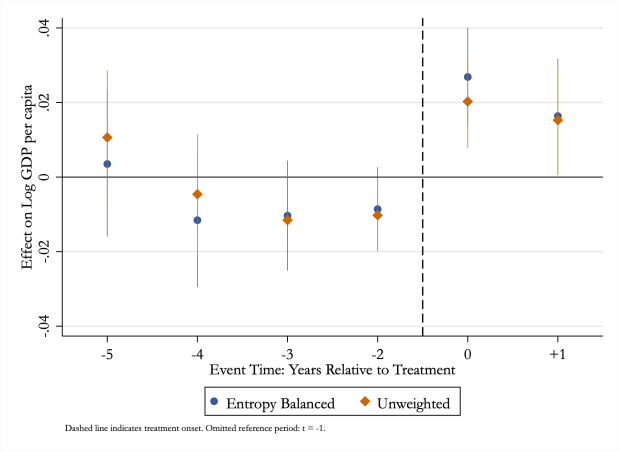
(b) Group 1 (Excluding Adopters in LLE-States)



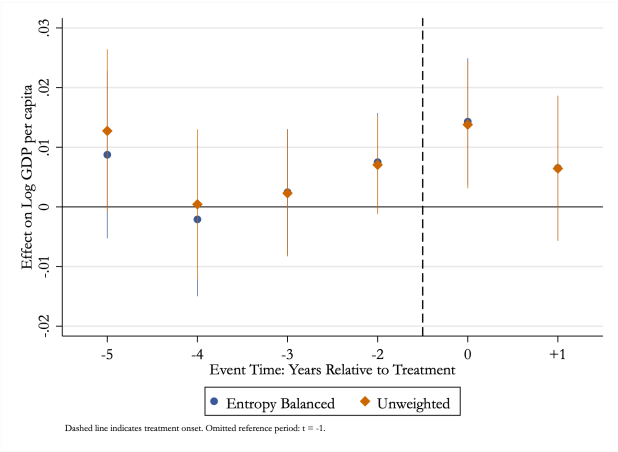
(c) Group 1 (Excluding Adopters Non-LLE-States)

Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period to create weights. This figure shows the estimated coefficient of an interaction between the treatment indicators and an isolation variable measured as the share of contiguous neighbors that are never treated.

Figure 5: The Effect of LLE Adoption on GDP



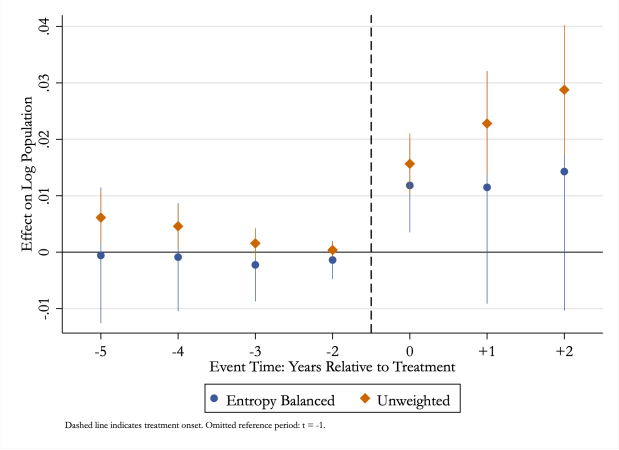
(a) Group 1 (Treatment in Any State)



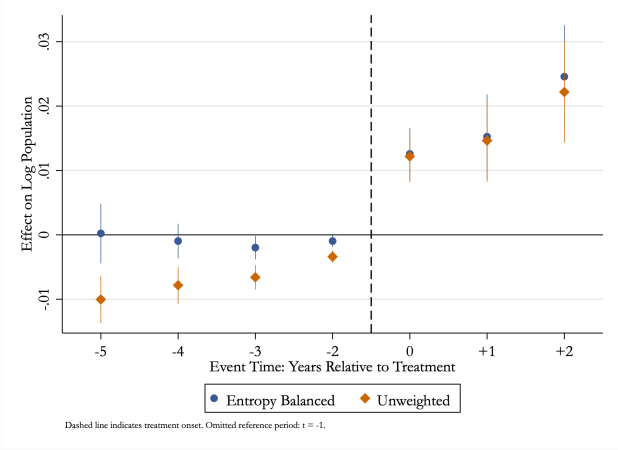
(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots two separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses smoothed GDP growth throughout the pre-treatment period. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

Figure 6: The Effect of LLE Adoption on Population



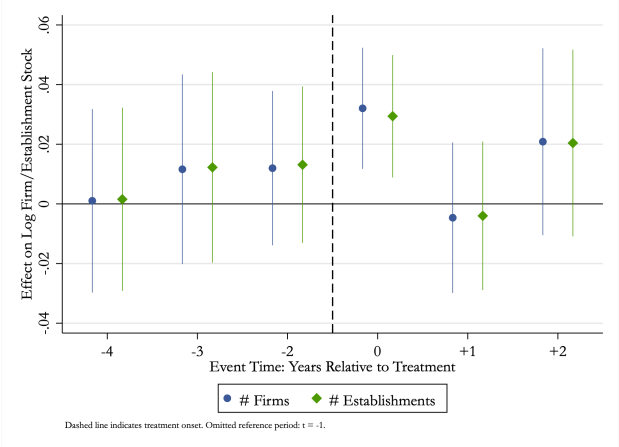
(a) Group 1 (Treatment in Any State)



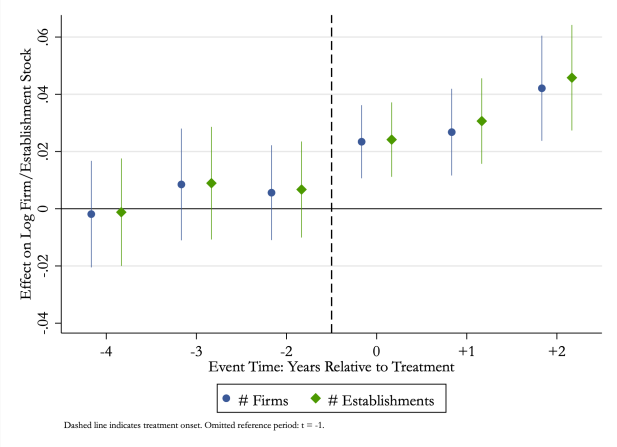
(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots two separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses smoothed GDP growth throughout the pre-treatment period. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

Figure 7: The Effect of LLE Adoption on Total Number of Firms and Establishments for the 2019 Cohort



(a) Group 1 (Treatment in Any State)

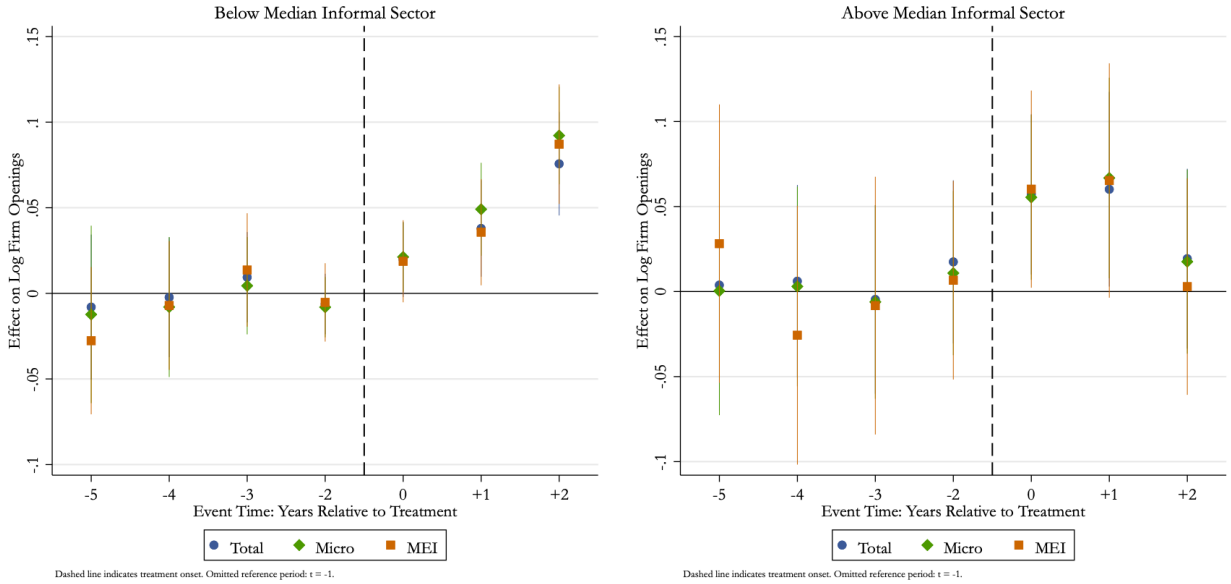


(b) Group 2 (LLE-Adopting States)

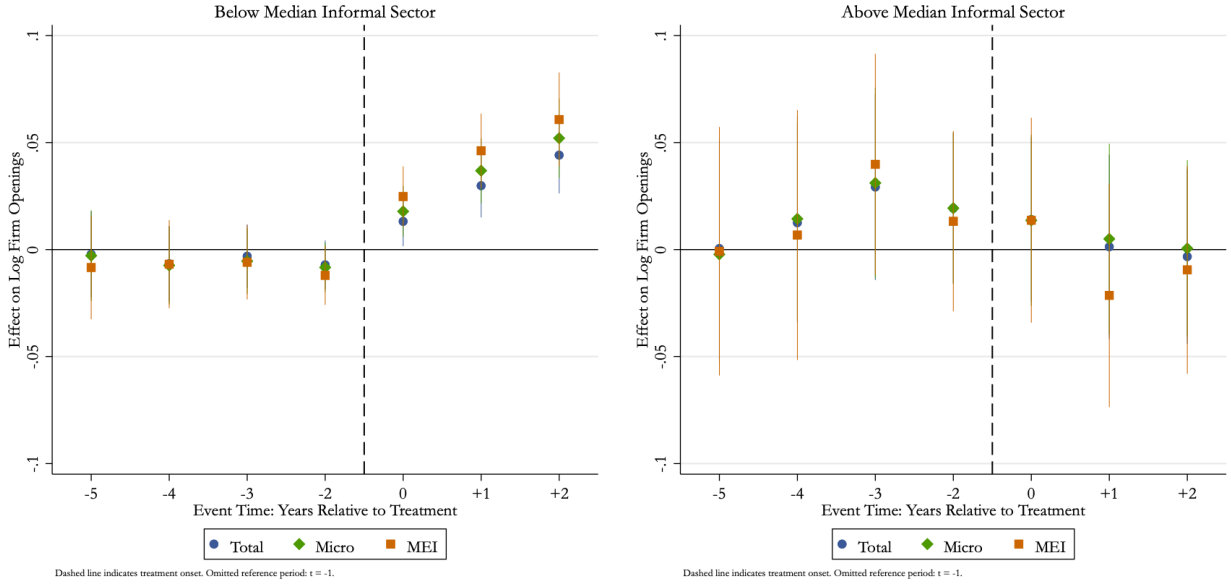
8

Notes: Due a major change in the methodology of CEMPRE in 2022 and beyond, we only evaluate LLE adoption for the 2019 cohort. Each graph plots two separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

Figure 8: The Heterogeneous Effect of LLE Adoption on Firm Openings Across Informal Size



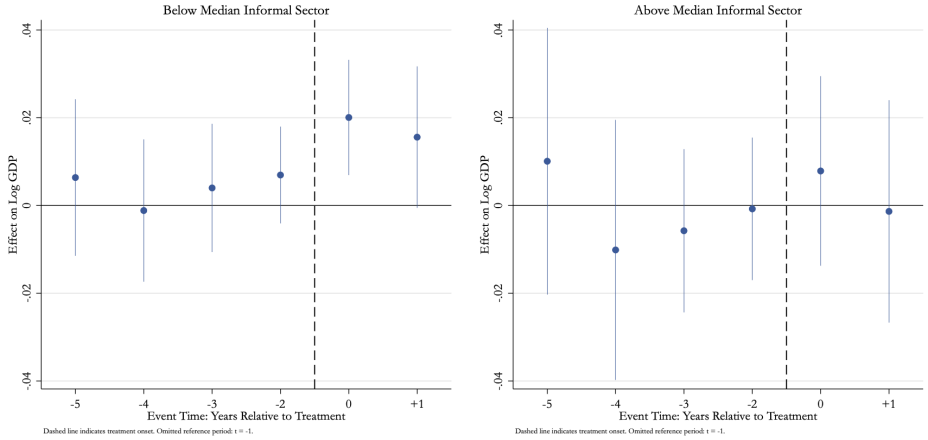
(a) Group 1 (Treatment in Any State)



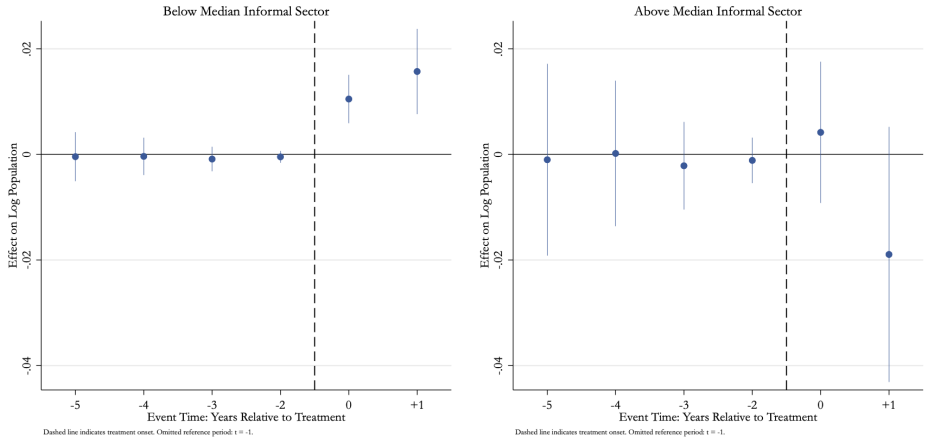
(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

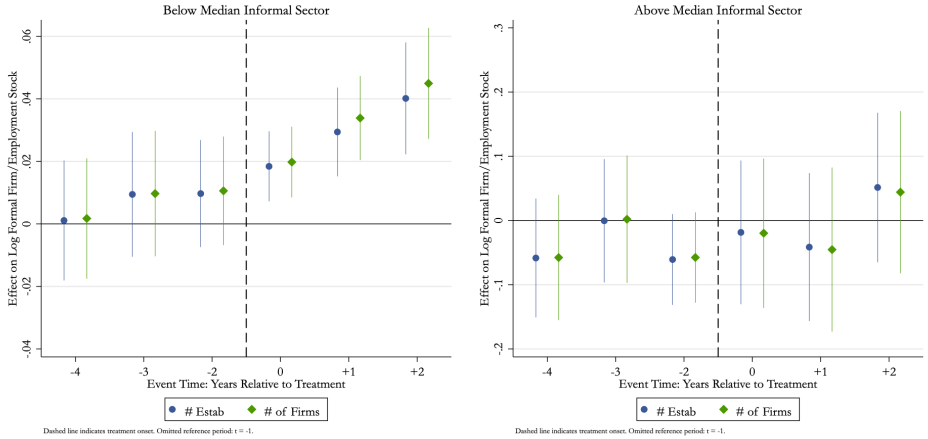
Figure 9: The Heterogeneous Effect of LLE Adoption on Economic Activity Across Informal Size for Group 2 Comparisons Only



(a) Total GDP



(b) Population



(c) Stock of Establishments and Firms

Notes: Each symbol/color represents a different set of stacked DID estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects; all estimates are entropy balanced using pre-treatment characteristics.

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Appendix

Contents

A Descriptive Statistics & Weights

A.1 Additional Maps

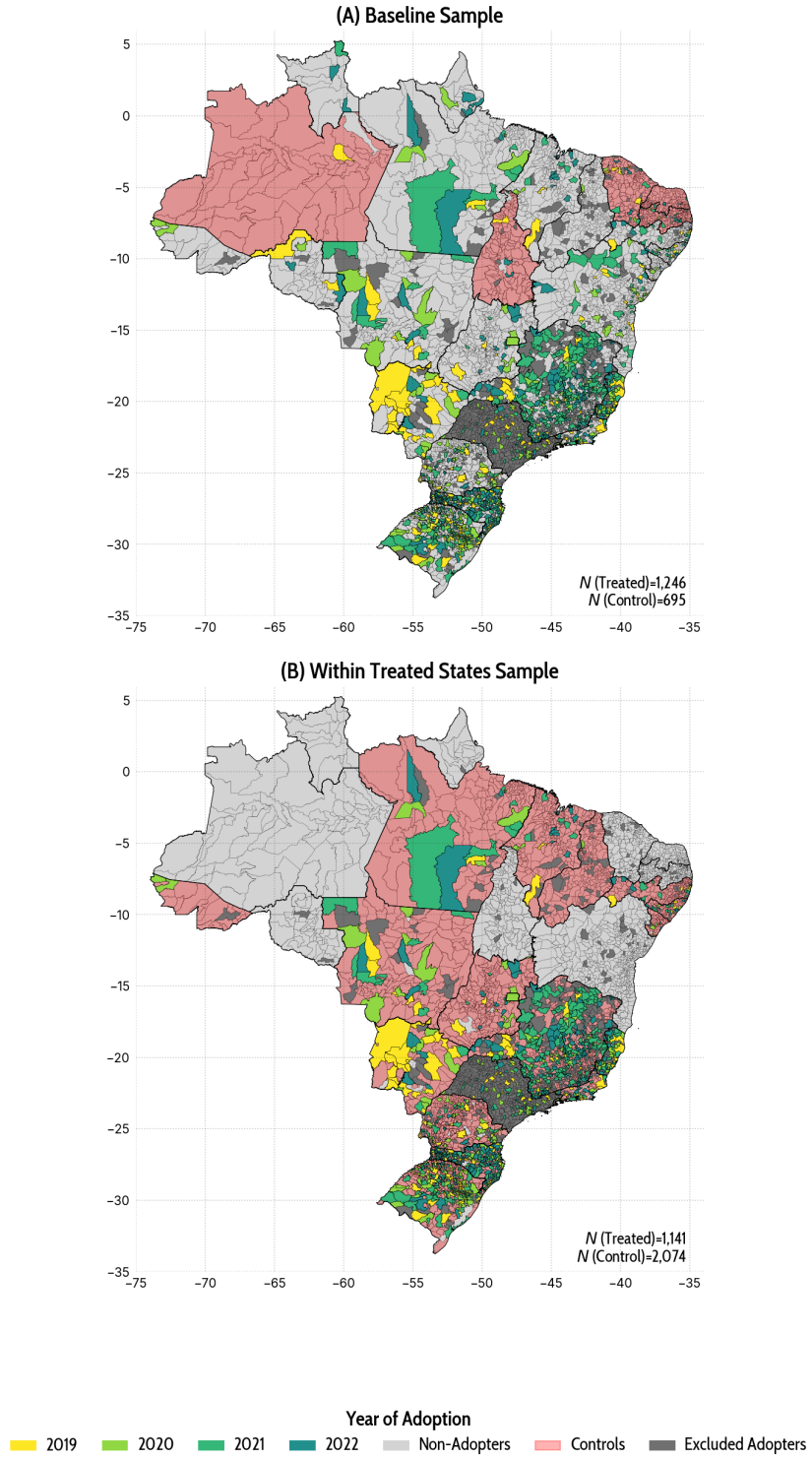
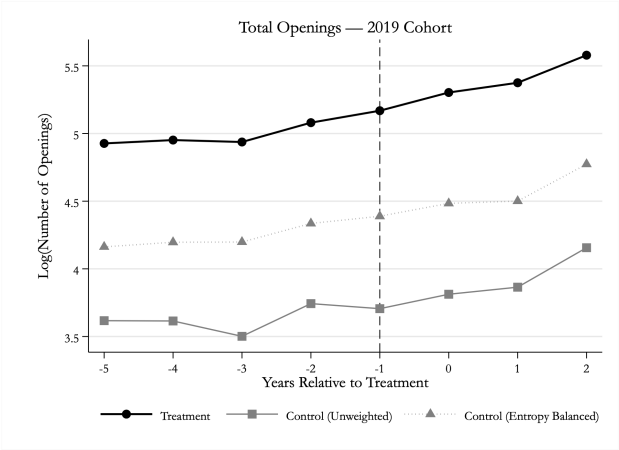


Figure A1: Maps Comparison Groups 1 & 2

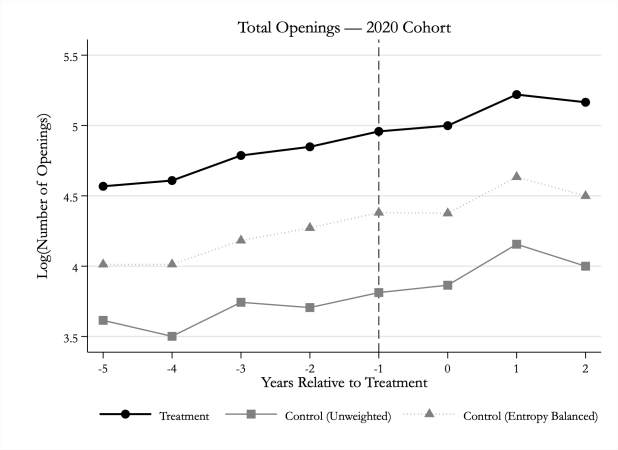
Notes: This figure plots the geographic distribution of our two comparison groups (see Table 1 for definitions). ***Panel A*** shows our first comparison group, ***Panel B*** does the same for the second. The different colors represent the different years of adoption; we exclude all adoptions occuring in 2023 or beyond. Data from the Instituto Liberal de São Paulo - *Liberdade para Trabalhar*.

A.2 Raw Data Graphs

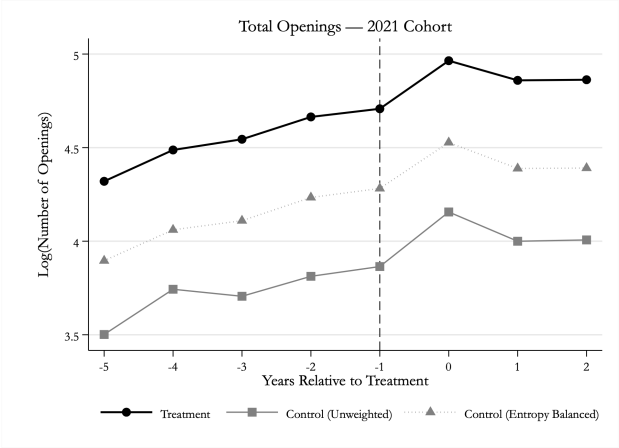
Figure A2: Total Firm Openings in Treatment and Control Groups by Cohort for Group 1 Comparisons



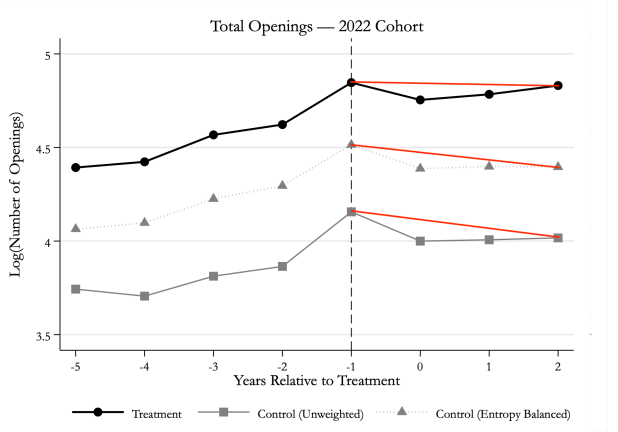
(a) 2019 Cohort



(b) 2020 Cohort



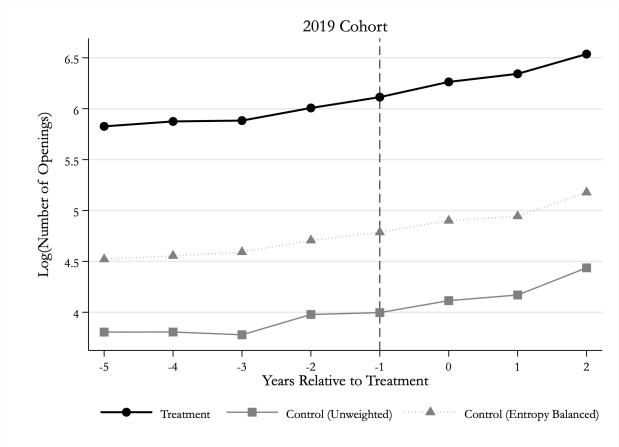
(c) 2021 Cohort



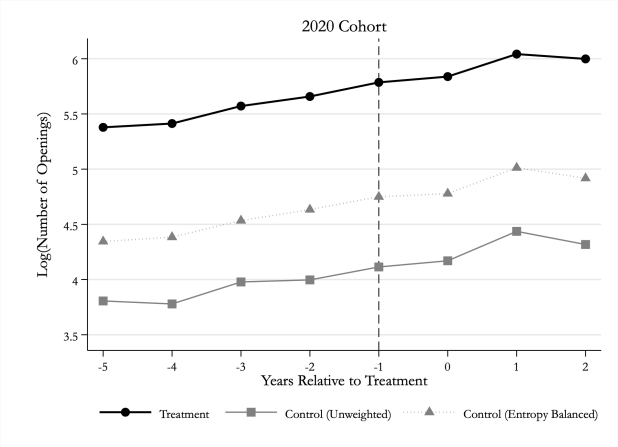
(d) 2022 Cohort

Notes: This figure compares the average number of (logged) firm openings in the treatment and control group through time for each cohort.

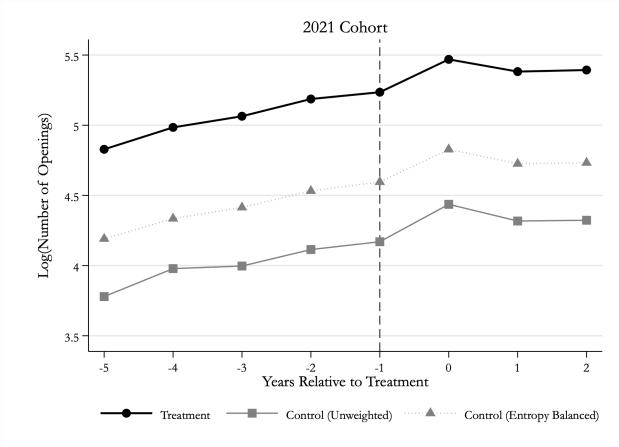
Figure A3: Total Firm Openings in Treatment and Control Groups by Cohort for Group 2 Comparisons



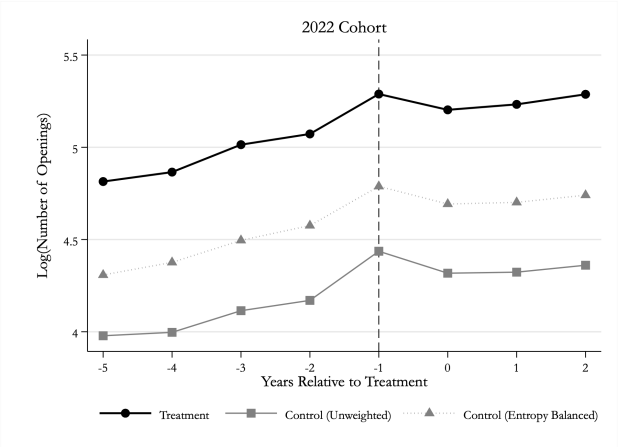
(a) 2019 Cohort



(b) 2020 Cohort



(c) 2021 Cohort

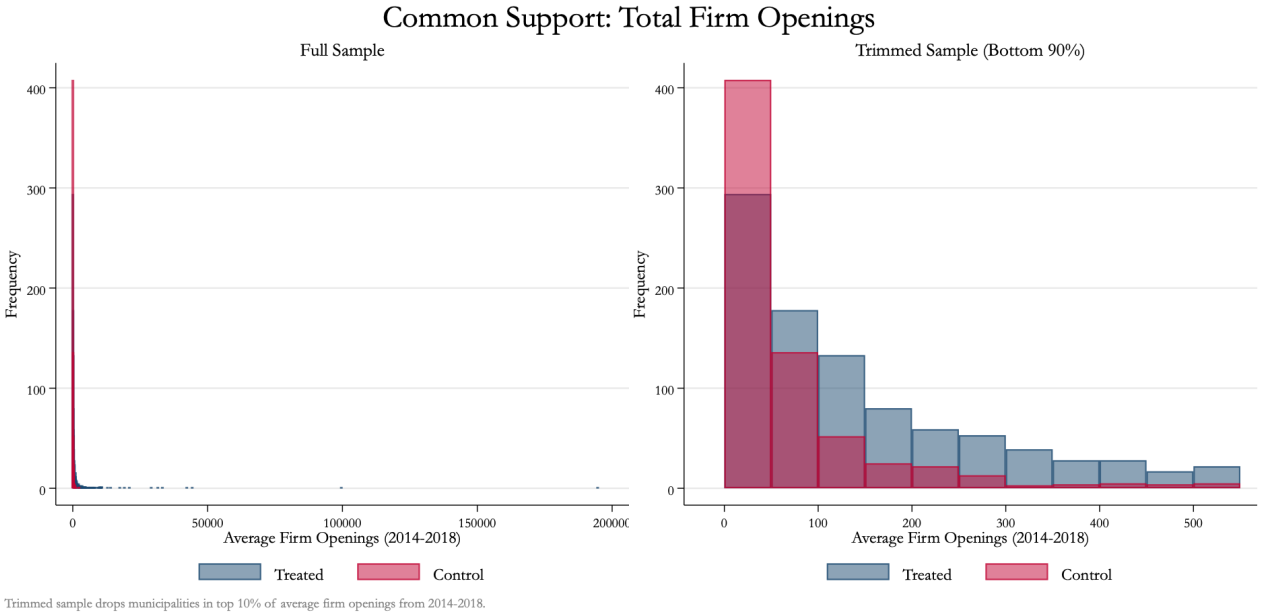


(d) 2022 Cohort

Notes: This figure compares the average number of (logged) firm openings in the treatment and control group through time for each cohort.

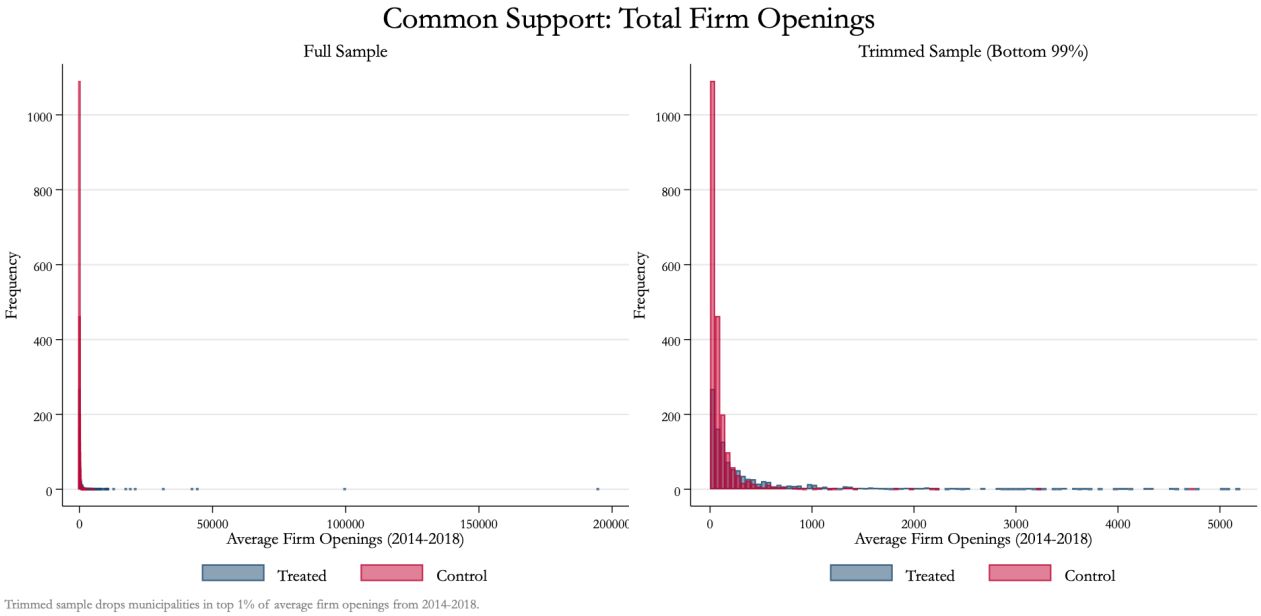
A.3 Additional Summary Statistics on Firm Openings

Figure A4: Total Firm Openings Overlap: Treated and Control for Comparison Group 1



Notes: The panel on the left shows the distribution of firm openings across the entire distribution of treated and control municipalities. The panel on the right shows the distribution after removing municipalities that had average firm openings in the top 10% of the overall (treatment and control) distribution from 2014-2018. Our primary results focus on this trimmed sample given the substantial overlap. Note that are only 695 unique controls, but each appear in the dataset four times for the four cohorts.

Figure A5: Total Firm Openings Overlap: Treated and Control for Comparison Group 2



Notes: The panel on the left shows the distribution of firm openings across the entire distribution of treated and control municipalities. The panel on the right shows the distribution after removing municipalities that had average firm openings in the top 1% of the overall (treatment and control) distribution from 2014-2018. Our primary results focus on this trimmed sample given the substantial overlap. Note that are only 2,075 unique controls, but each appear in the dataset four times for the four cohorts.

Figure A6: Pre-Treatment Variation Across Municipalities in the Treated Group for Comparison Group 1

Notes: This figure uses a random sample of 9 treated municipalities to highlight the high levels of variability in firm openings data through time.

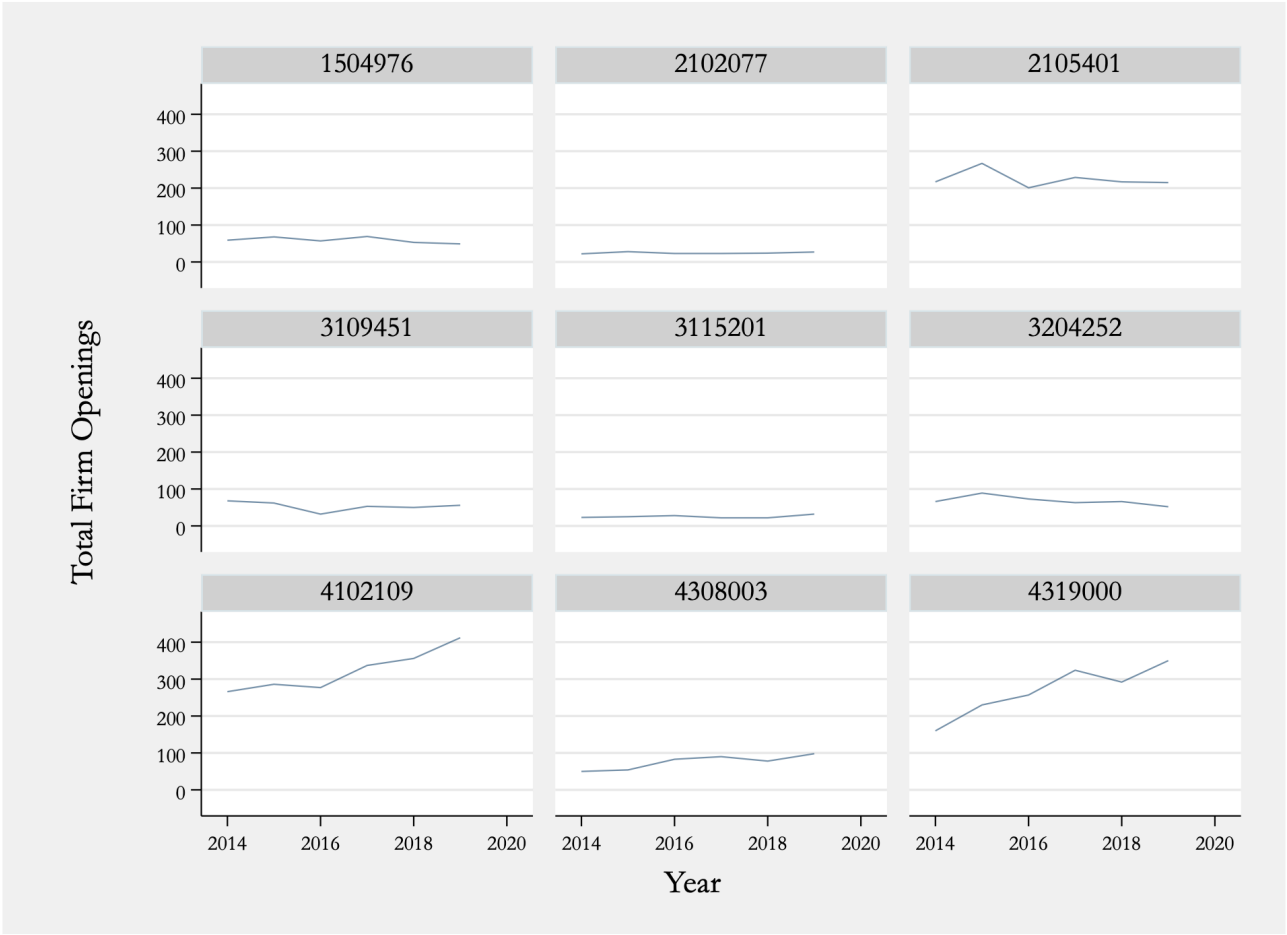


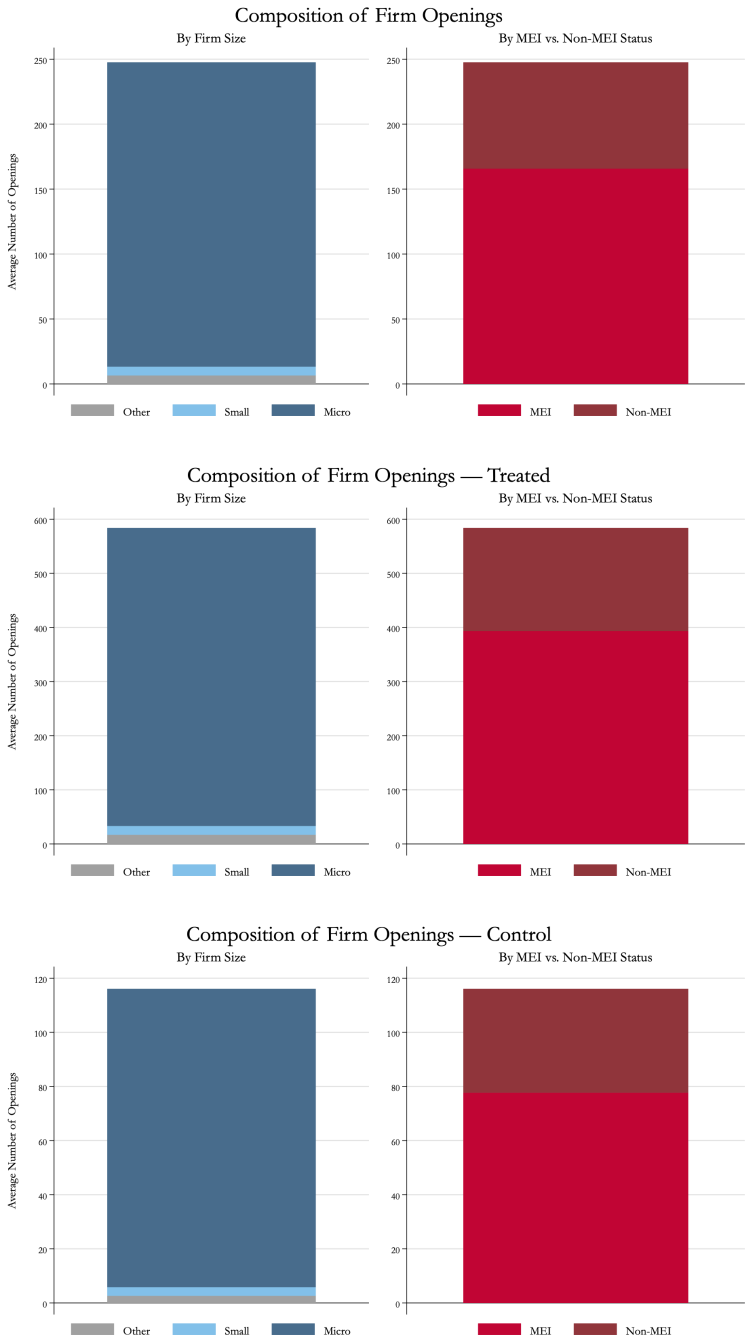
Figure A7: Pre-Treatment Variation Across Municipalities in the Treated Group for Comparison Group 2

Notes: This figure uses a random sample of 9 treated municipalities to highlight the high levels of variability in firm openings data through time.

Figure A8: Total Firm Openings in Treatment and Control Groups in 2018 for Comparison Group 1

Notes: This figure shows the composition of firms openings across firm type for comparison Group 1 (see Table 1).

Figure A9: Total Firm Openings in Treatment and Control Groups in 2018 for Comparison Group 2



Notes: This figure shows the composition of firms openings across firm type for comparison Group 2 (see Table 1).

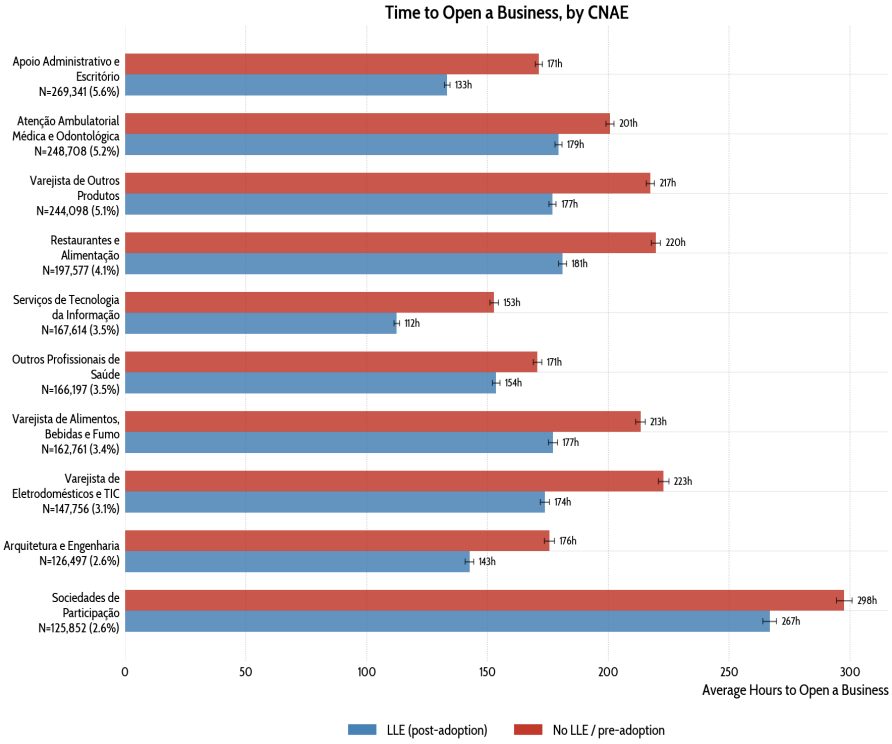
A.4 Additional Statistics on Time to Open a Business

Table A1: Summary of Time to Open Characteristics: Application-Level Data

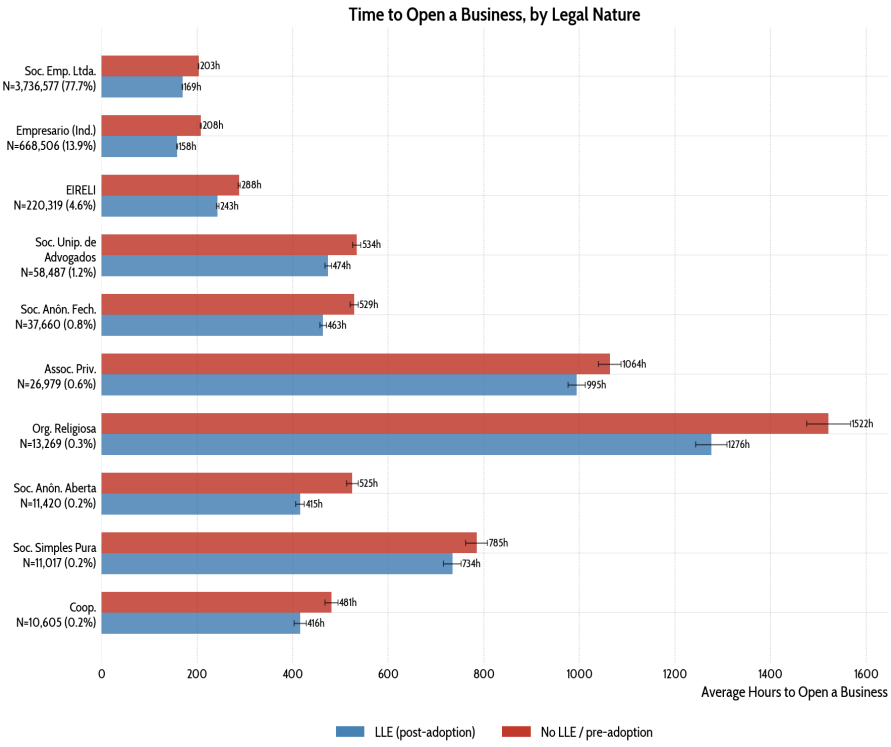
	Prior to Weighting				After Weighting ($k = 1$)			
	Treated		Control		Treated		Control	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Panel A: Trimmed Sample - Two-Step Matching								
Application-Level Characteristics								
Hours to Open a Business	206.475	303.903	255.453	358.502	206.247	301.827	235.302	393.172
Municipality-Level Characteristics								
Population	188,623	126,968	125,197	121,767	187,715	125,422	187,715	136,786
GDP per capita (R$)	27,844.7	27,681.4	21,736.9	19,577.6	28,155.3	27,948.1	28,155.3	25,699.9
Agriculture share	0.051	0.086	0.091	0.119	0.049	0.084	0.049	0.085
Industry share	0.251	0.126	0.216	0.133	0.253	0.125	0.253	0.127
Urban share	0.912	0.115	0.844	0.173	0.915	0.112	0.915	0.121
N	572,675		980,893		501,933		215,822	
Panel B: Trimmed Sample - Entropy Balance Matching								
Application-Level Characteristics								
Hours to Open a Business	206.475	303.903	255.453	358.502	206.247	301.827	251.155	386.989
Municipality-Level Characteristics								
Population	188,623	126,968	125,197	121,767	187,715	125,422	187,715	136,145
GDP per capita (R$)	27,844.7	27,681.4	21,736.9	19,577.6	28,155.3	27,948.1	28,155.3	25,651.7
Agriculture share	0.051	0.086	0.091	0.119	0.049	0.084	0.049	0.085
Industry share	0.251	0.126	0.216	0.133	0.253	0.125	0.253	0.127
Urban share	0.912	0.115	0.844	0.173	0.915	0.112	0.915	0.120
N	572,675		980,893		501,933		215,822	

Notes: This table reports summary statistics in application-level data on the time to open a business, before and after balance. All panels report means and standard deviations *prior* to matching in the "Prior to Weighting" columns, and for the $k = 1$ matched sample in the "After Weighting" columns. Both samples exclude applications in municipalities at the top population percentile ($>$ 481,749 people). ***Panel A*:** uses a two-step matching procedure, finding nearest neighbors with Mahalanobis distance matching and refining weights with entropy balance, as in Panel A of Table 5. ***Panel B*:** uses entropy balance alone. Value added sector shares use averaged data from 2014–2018.

Figure A10: Average Time to Open a Business, Most Common Categories



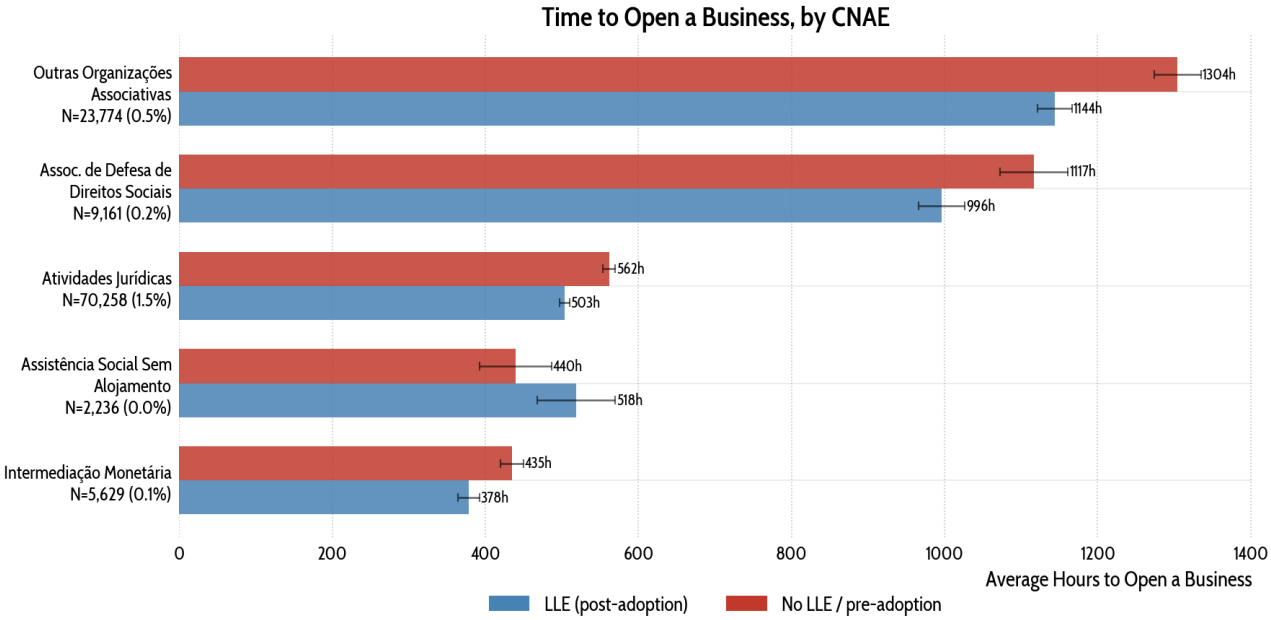
(a) Panel A: 10 most common industries



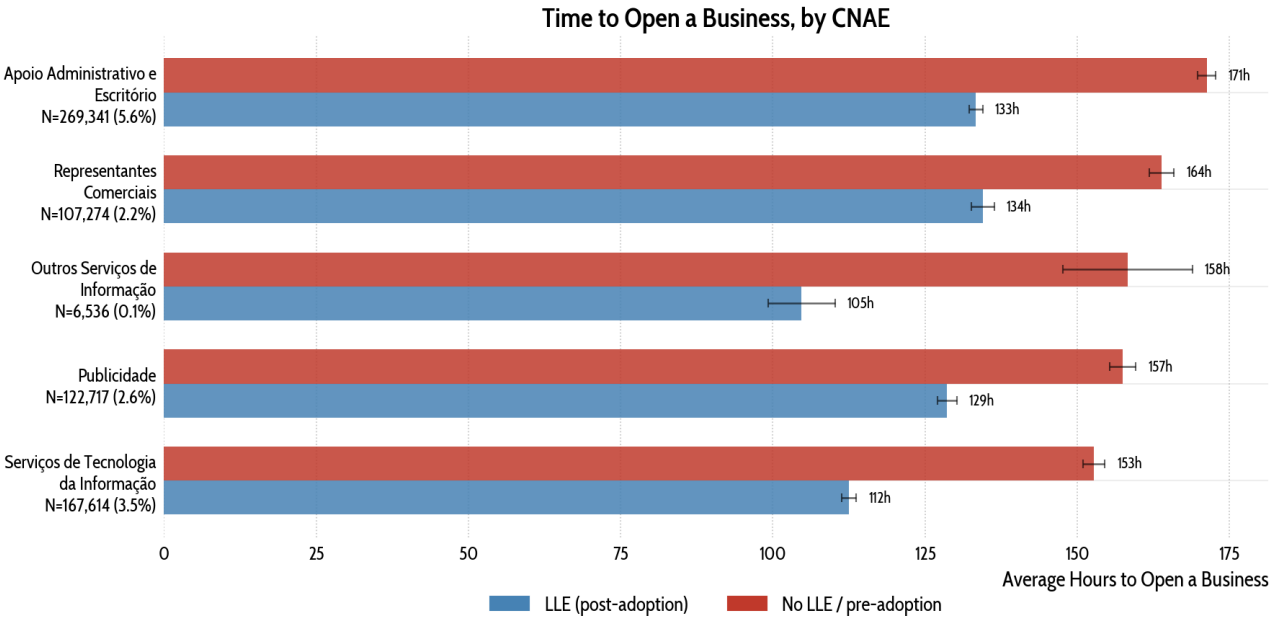
(b) Panel B: 10 most common legal types

Notes: This sample uses administrative microdata on the universe of 4,809,930 business opening applications between January 2019 and March 2026 (excluding micro-individual entrepreneurs, MEI). Treated: 2,289,056 application in post-adoption LLE; Control: 2,520,874 in never-treated or pre-LLE municipalities. Industries grouped at the 3-digit CNAE level.

Figure A11: Average Time to Open a Business, Top and Bottom Industries



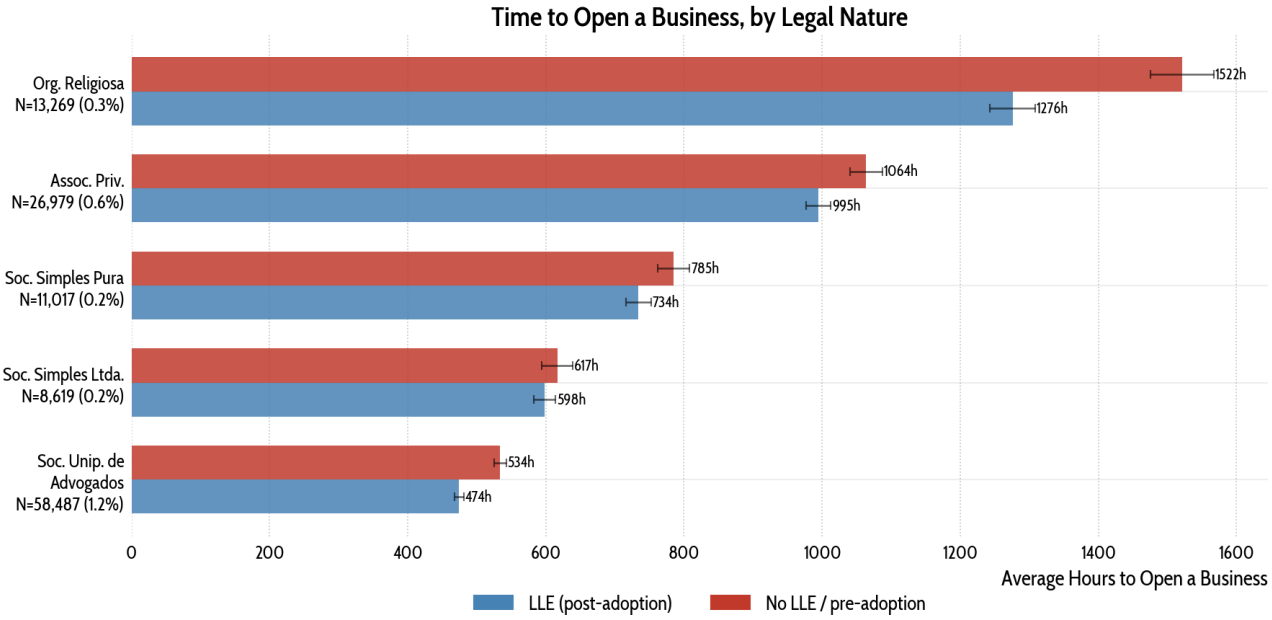
(a) Panel A: Top 5 industries (most time)



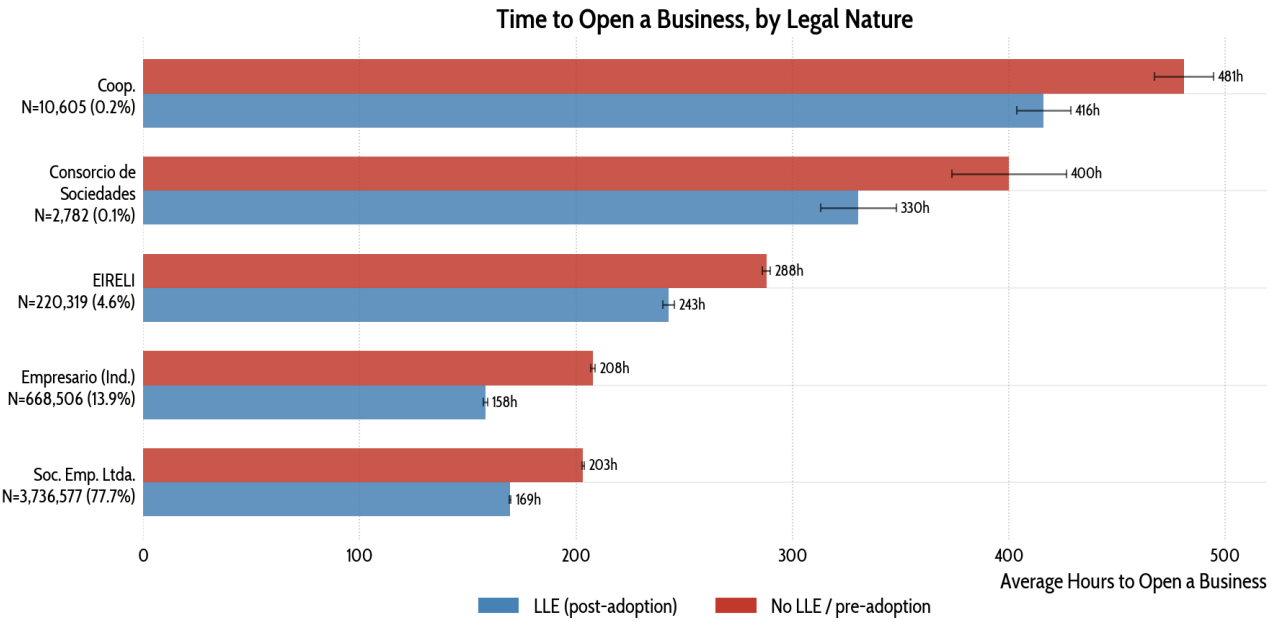
(b) Panel B: Bottom 5 industries (least time)

Notes: This sample uses administrative microdata on the universe of 4,809,930 business opening applications between January 2019 and March 2026 (excluding micro-individual entrepreneurs, MEI). Treated: 2,289,056 application in post-adoption LLE; Control: 2,520,874 in never-treated or pre-LLE municipalities. Industries grouped at the 3-digit CNAE level. Both panels exclude industries with less than 500 applications. **Most time:** "Other Associative Organizations", "Associations for Defense of Social Rights", "Legal Activities", "Social care without lodging", "Monetary Intermediation". **Least Time:** "Administrative and Office Support", "Commercial Representative", "Other Information Services", "Advertising", "IT Services".

Figure A12: Average Time to Open a Business, Top and Bottom Legal Types



(a) Panel A: Top 5 legal types (most time)



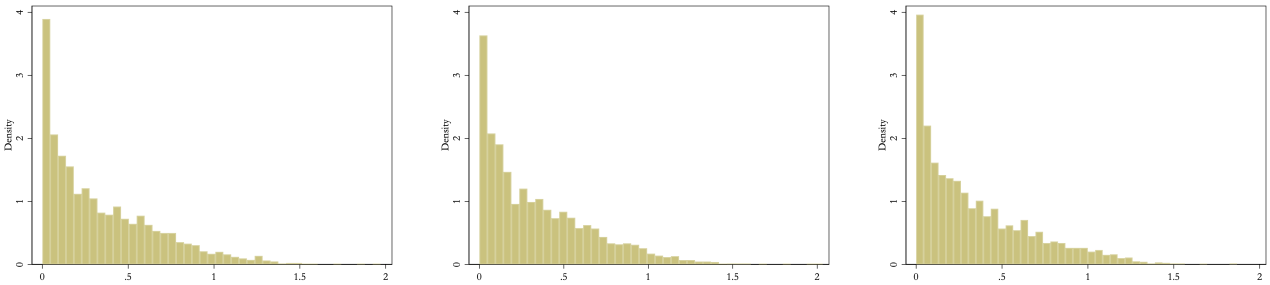
(b) Panel B: Bottom 5 legal types (least time)

Notes: This sample uses administrative microdata on the universe of 4,809,930 business opening applications between January 2019 and March 2026 (excluding micro-individual entrepreneurs, MEI). Treated: 2,289,056 application in post-adoption LLE; Control: 2,520,874 in never-treated or pre-LLE municipalities. Both panels exclude legal types (*natureza jurídica*) with less than 500 applications. **Most Time**: "Religious Organization", "Private Association", "Simple Pure Society", "Simple Limited Society", "Individual Lawyer Society". **Least Time:** "Cooperative", "Consortium of Societies", "Limited Responsibility Individual Enterprise", "Individual Entrepreneur", "Limited Enterprise Society".

A.5 Histogram of Weights for Figures 2 through 9

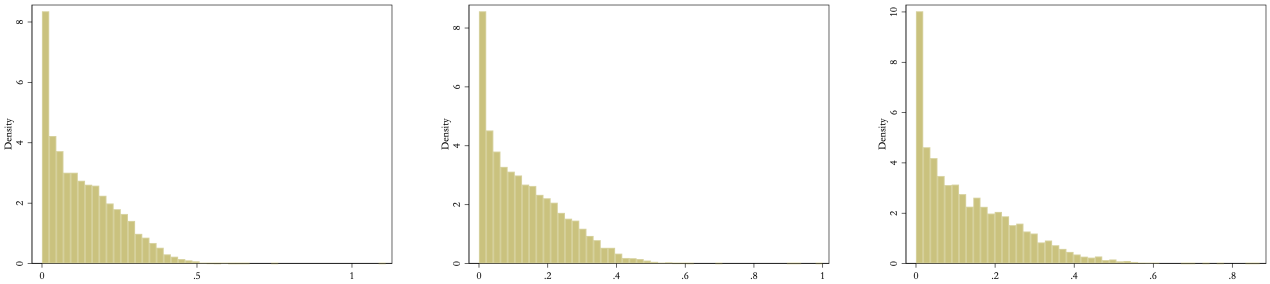
Figure A13: Histogram of Weights for Figure 2

Panel A: Group 1 Comparisons



(a) Total (b) Micro (c) MEI

Panel B: Group 2 Comparisons

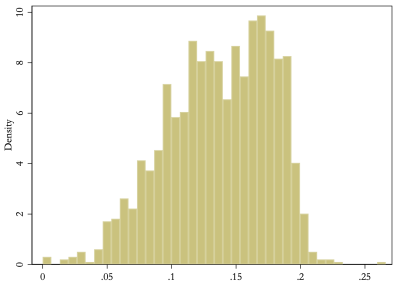


(d) Total (e) Micro (f) MEI

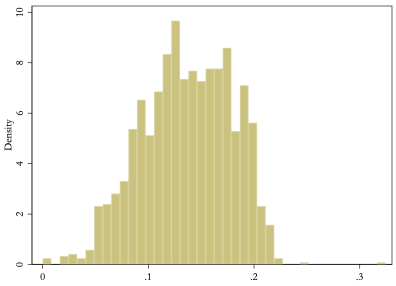
Notes: These weights are identical to the ones used in Figure 4.

Figure A14: Histogram of Weights for Firm Openings Part of Figure 3

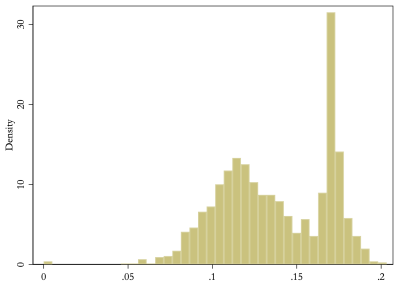
Panel A: Group 1 Comparisons



(a) Total

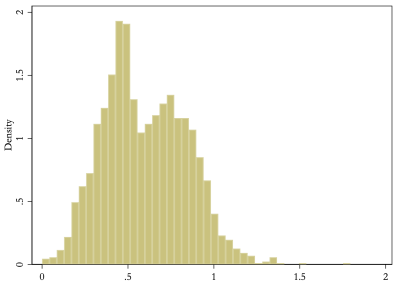


(b) Micro

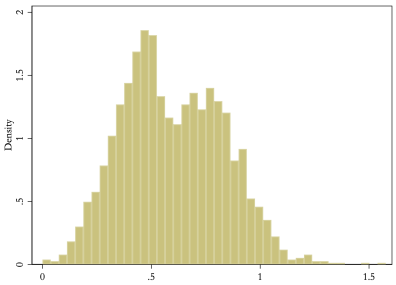


(c) MEI

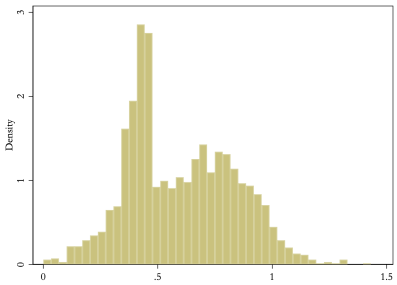
Panel B: Group 2 Comparisons



(d) Total



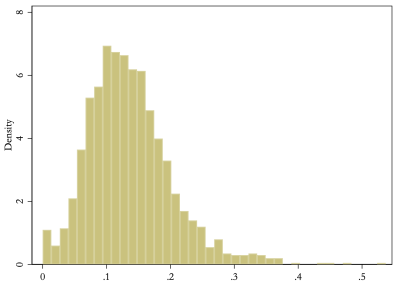
(e) Micro



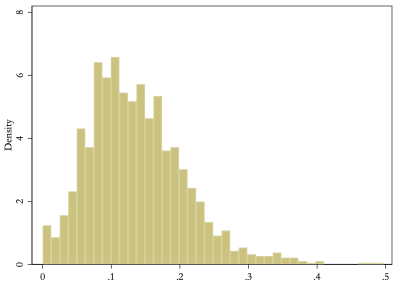
(f) MEI

Figure A15: Histogram of Weights for Firm Closings Part of Figure 3

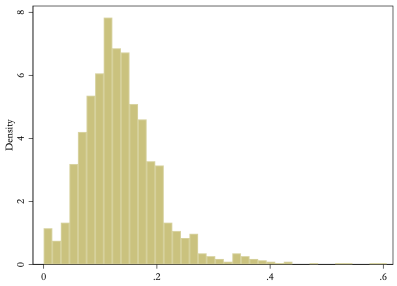
Panel A: Group 1 Comparisons



(a) Total

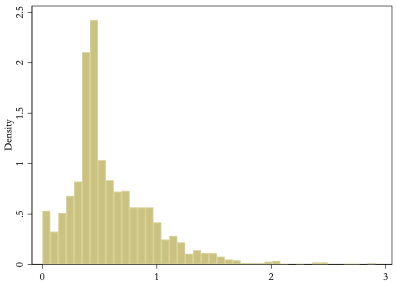


(b) Micro

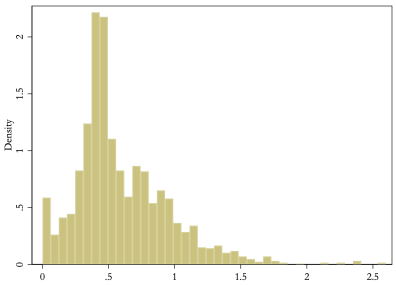


(c) MEI

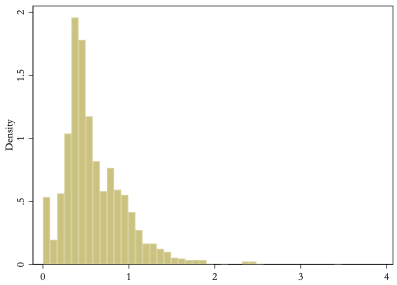
Panel B: Group 2 Comparisons



(d) Total



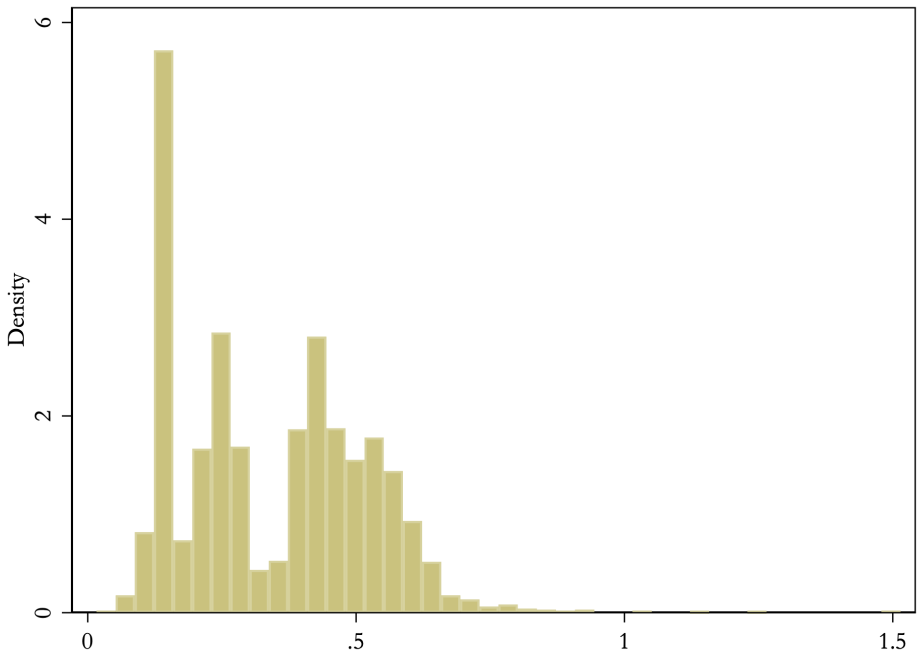
(e) Micro



(f) MEI

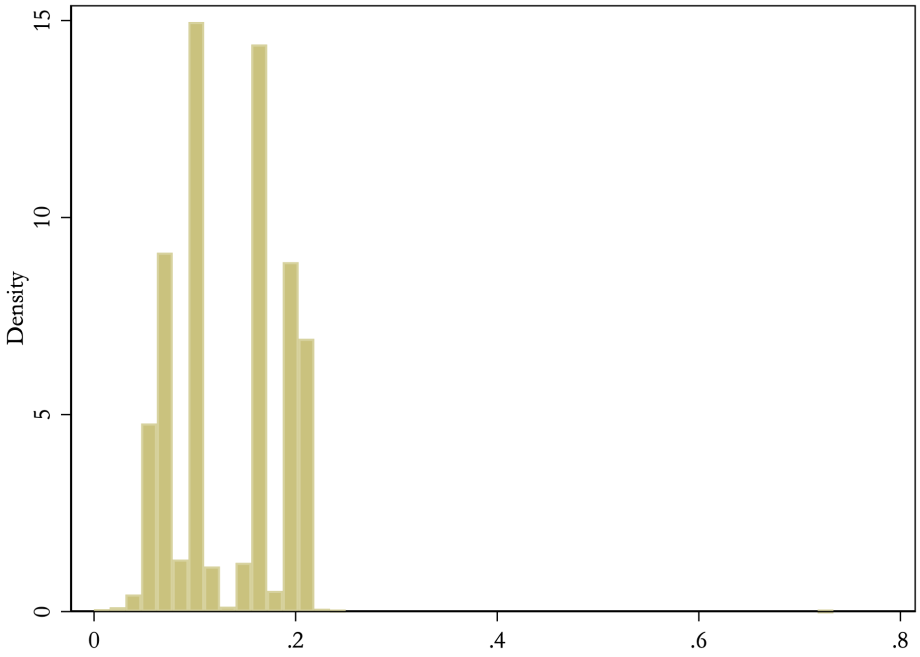
Figure A16: Histogram of Weights for Figure 5

Panel A: Group 1 Comparisons



(a) Total GDP

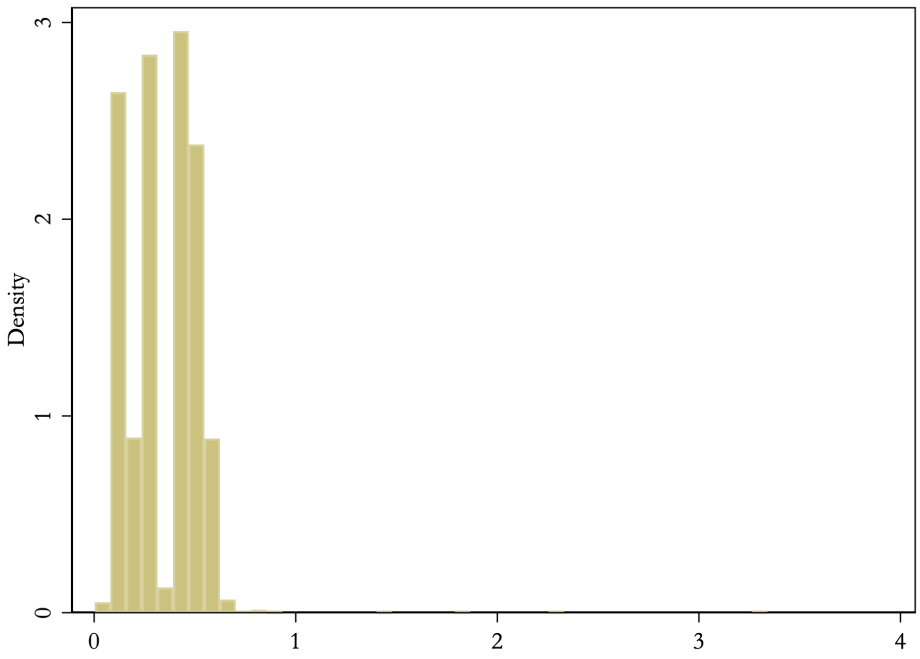
Panel B: Group 2 Comparisons



(b) Total GDP

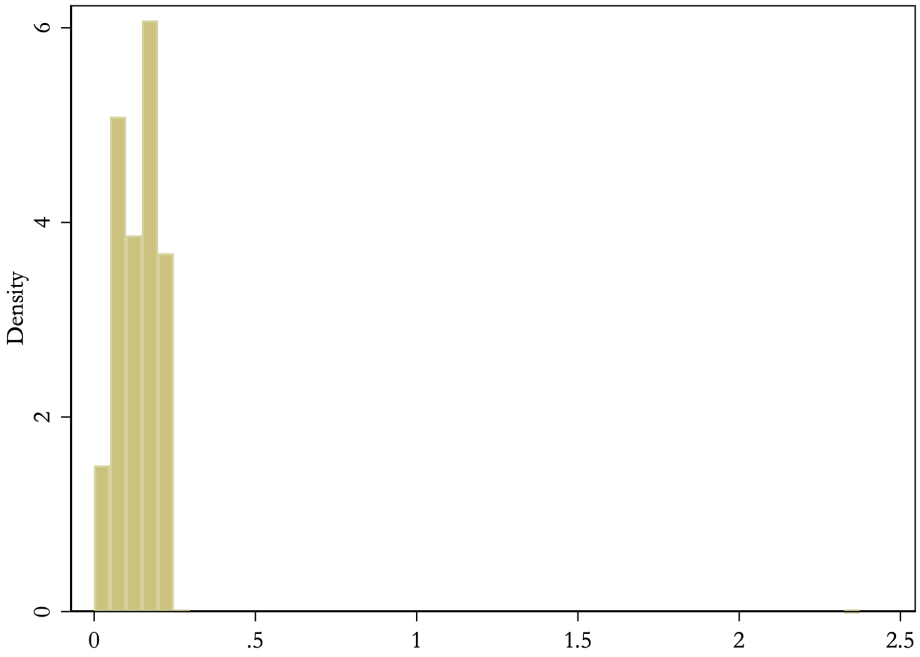
Figure A17: Histogram of Weights for Figure 6

Panel A: Group 1 Comparisons



(a) Total Population

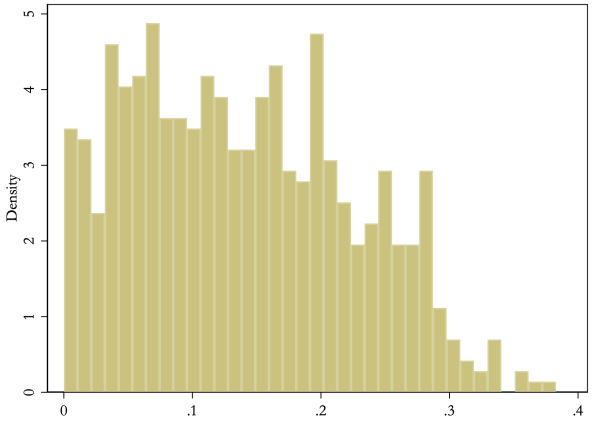
Panel B: Group 2 Comparisons



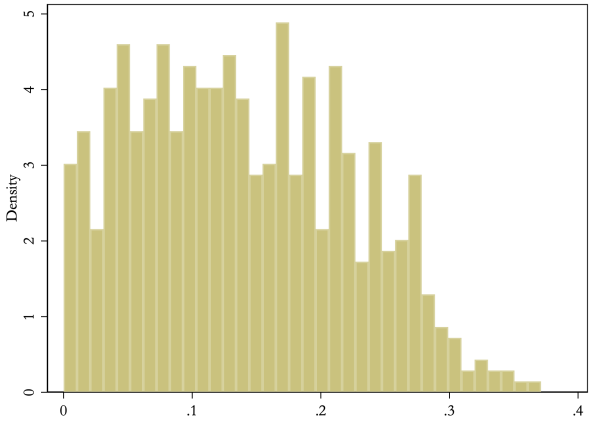
(b) Total Population

Figure A18: Histogram of Weights for Figure 7

Panel A: Group 1 Comparisons

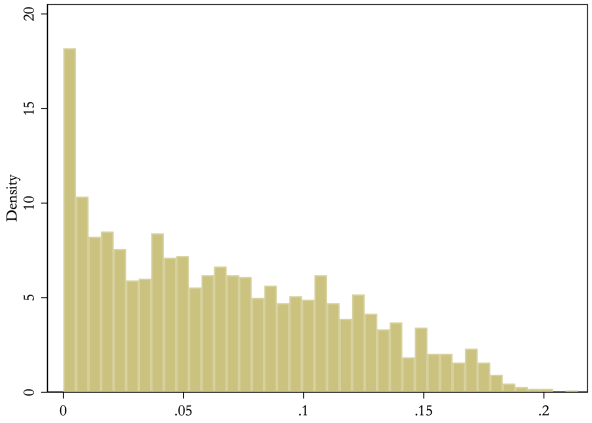


(a) Total Number of Establishments

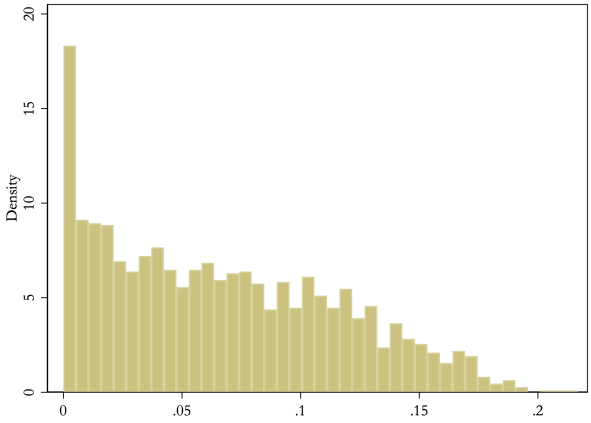


(b) Total Number of Firms

Panel B: Group 2 Comparisons



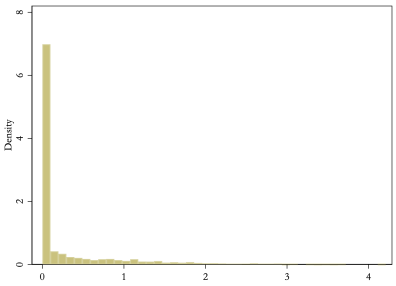
(c) Total Number of Establishments



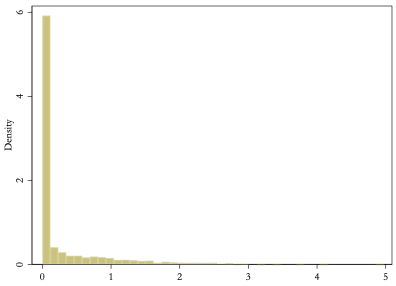
(d) Total Number of Firms

Figure A19: Histogram of Weights for Group 1 Comparisons of Figure 8

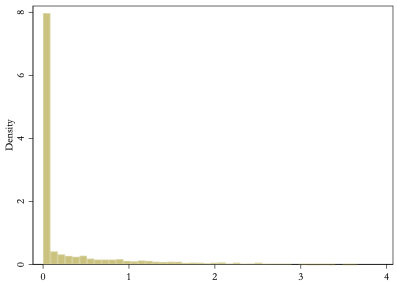
Panel A: Low Informality Group



(a) Total

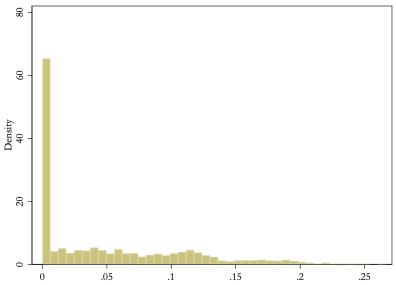


(b) Micro

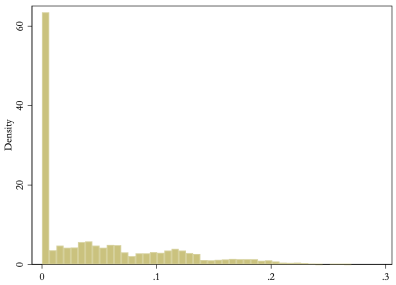


(c) MEI

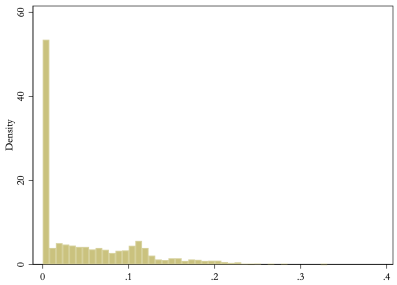
Panel B: High Informality Group



(d) Total



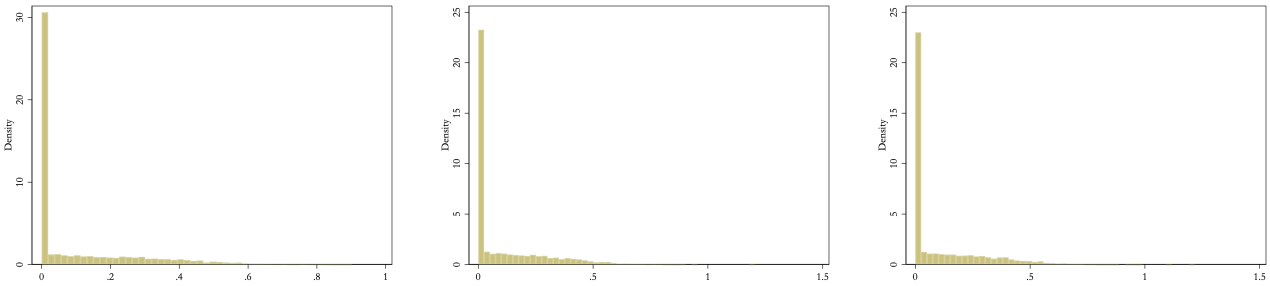
(e) Micro



(f) MEI

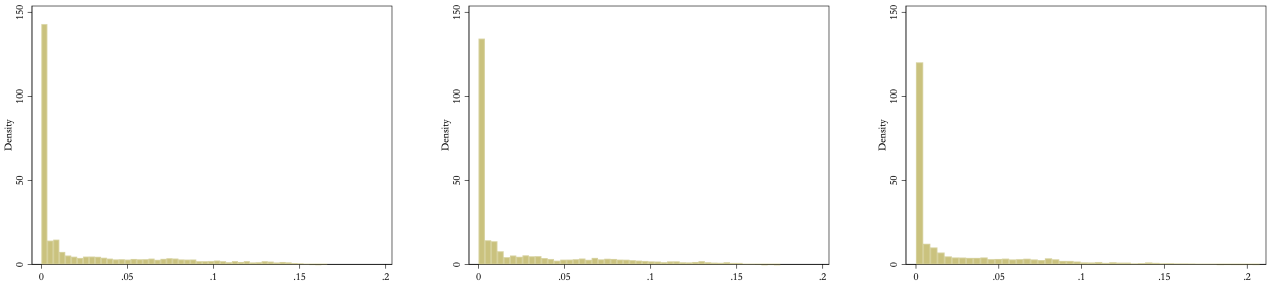
Figure A20: Histogram of Weights for Group 2 Comparisons of Figure 8

Panel A: Low Informality Group



(a) Total (b) Micro (c) MEI

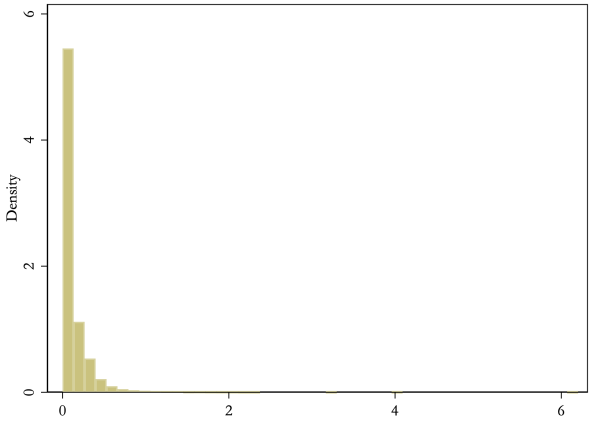
Panel B: High Informality Group



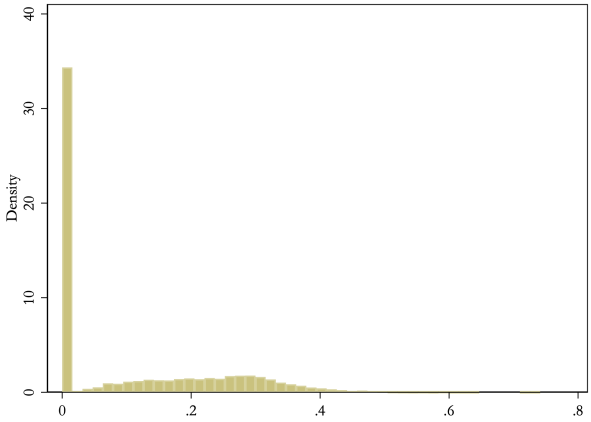
(d) Total (e) Micro (f) MEI

Figure A21: Histogram of Weights for GDP and Population Outcomes in Figure 9

Panel A: Low Informality Group

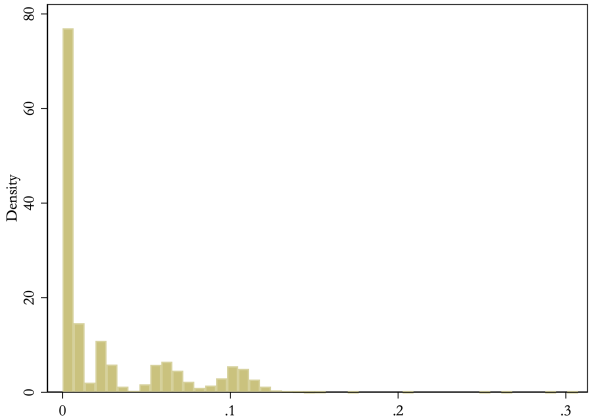


(a) Total GDP

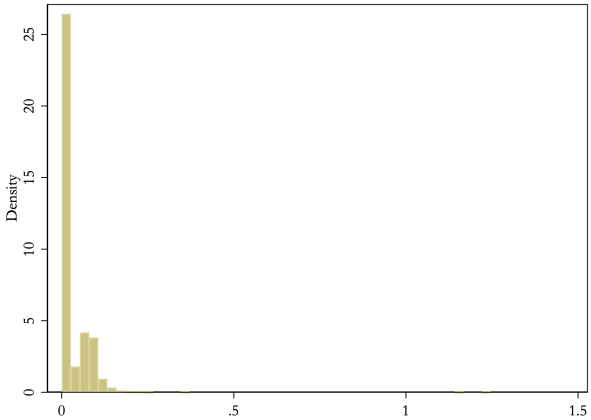


(b) Total Population

Panel B: High Informality Group



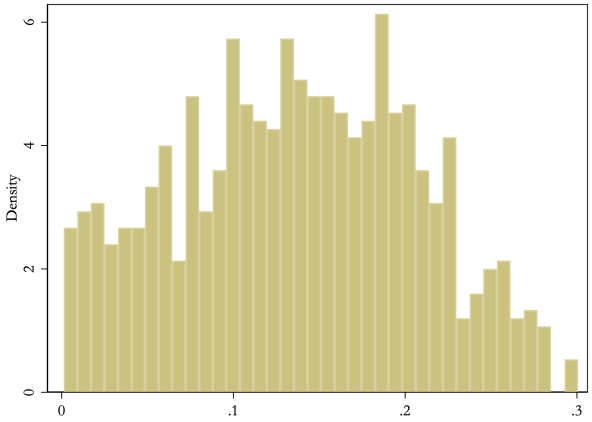
(c) Total GDP



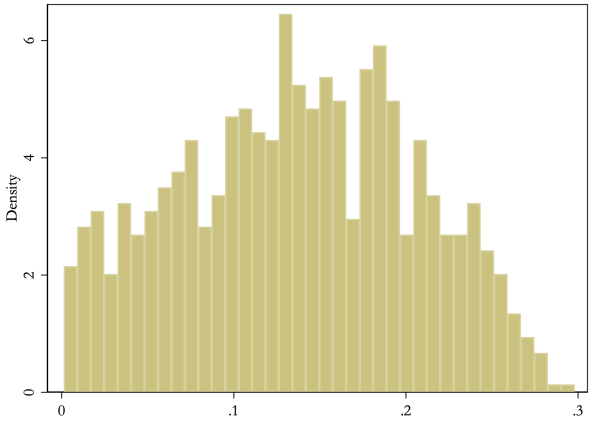
(d) Total Populations

Figure A22: Histogram of Weights for CEMPRE Outcomes in Figure 9

Panel A: Low Informality Group

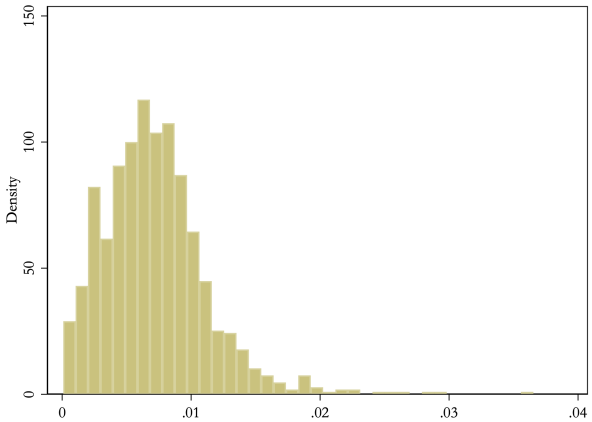


(a) Total Number of Establishments

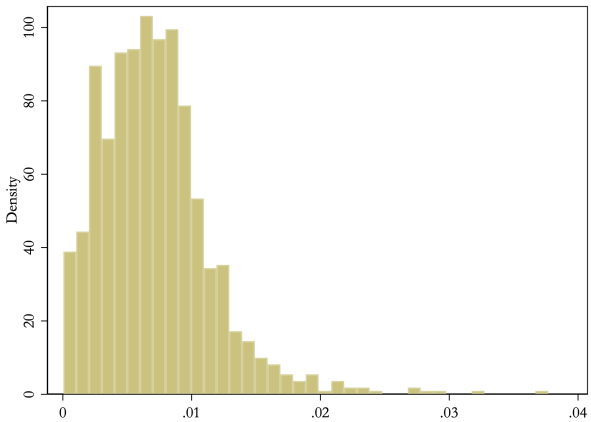


(b) Total Number of Firms

Panel B: High Informality Group



(c) Total Number of Establishments



(d) Total Number of Firms

B The Economic Freedom Law

Law No. 13,874, of September 20, 2019

Institutes the Declaration of Economic Freedom Rights; establishes free market guarantees; amends Laws Nos. 10,406, of January 10, 2002 (Civil Code), 6,404, of December 15, 1976, 11,598, of December 3, 2007, 12,682, of July 9, 2012, 6,015, of December 31, 1973, 10,522, of July 19, 2002, 8,934, of November 18, 1994, Decree-Law No. 9,760, of September 5, 1946, and the Consolidation of Labor Laws, approved by Decree-Law No. 5,452, of May 1, 1943; repeals Delegated Law No. 4, of September 26, 1962, Law No. 11,887, of December 24, 2008, and provisions of Decree-Law No. 73, of November 21, 1966; and provides for other matters.

THE PRESIDENT OF THE REPUBLIC, I hereby make it known that the National Congress decrees and I sanction the following Law:

Chapter I – General Provisions

Art. 1. The Declaration of Economic Freedom Rights is hereby established, setting norms for the protection of free enterprise and the free exercise of economic activity and provisions regarding the role of the State as a normative and regulating agent, pursuant to the terms of item IV of the *caput* of Art. 1, the sole paragraph of Art. 170, and the *caput* of Art. 174 of the Federal Constitution.

§1. The provisions of this Law shall be observed in the application and interpretation of civil, business, economic, urban, and labor law in legal relations within its scope and in public ordering, including the exercise of professions, commerce, commercial boards, public registries, transit, transport, and environmental protection.

§2. All public ordering norms regarding private economic activities shall be interpreted in favor of economic freedom, good faith, and respect for contracts, investments, and property.

§3. The provisions of this Chapter and Chapters II and III of this Law do not apply to tax law and financial law, except as provided in item X of the *caput* of Art. 3 of this Law.

§4. The provisions of Arts. 1, 2, 3, and 4 of this Law constitute a general norm of economic law, pursuant to item I of the *caput* and §§1, 2, 3, and 4 of Art. 24 of the Federal Constitution, and shall be observed for all public acts of release of economic activity performed by the States, the Federal District, and the Municipalities.

§5. The provisions of item IX of the *caput* of Art. 3 of this Law do not apply to States, the Federal District, and Municipalities, except if:

I - the public act of release of economic activity is derived or delegated by federal ordinary legislation; or

II - the federative entity or the body responsible for the act decides to bind itself to the provisions of item IX of the *caput* of Art. 3 of this Law through a valid and proper instrument.

§6. For the purposes of this Law, public acts of release are considered to be the license, authorization, concession, enrollment, permit, charter, registry, accreditation, study, plan, registration, and other acts required, under any denomination, by a body or entity of the public administration in the application of legislation, as a condition for the exercise of economic activity, including the beginning, continuation, and end for installation, construction, operation, production, functioning, use, exercise, or performance, in the public or private sphere, of activity, service, establishment, profession, installation, operation, product, equipment, vehicle, building, and others.

Art. 2. The following principles guide the provisions of this Law:

I - freedom as a guarantee in the exercise of economic activities;

II - good faith of the private individual before the public authority;

III - subsidiary and exceptional intervention of the State over the exercise of economic activities; and

IV - recognition of the vulnerability of the private individual before the State.

Sole Paragraph. Regulation shall provide for the assessment criteria for the dismissal of item IV of the *caput* of this article, limited to issues of bad faith, hyper-sufficiency, or recidivism.

Chapter II – Declaration of Economic Freedom Rights

Art. 3. Every natural or legal person has the following rights essential for the Country's economic development and growth, observed the provisions of the sole paragraph of Art. 170 of the Federal Constitution:

I - to develop low-risk economic activity, for which they exclusively use their own private property or that of consensual third parties, without the need for any public acts of release of economic activity;

II - to develop economic activity at any time or day of the week, including holidays, without being subject to additional charges or burdens, observed:

a) environmental protection norms, including those for the repression of noise pollution and disturbance of public peace;

b) restrictions arising from contracts, condominium regulations, or other legal transactions, as well as those arising from property law norms, including neighborhood rights; and

c) labor legislation;

III - to freely define, in unregulated markets, the price of products and services as a consequence of changes in supply and demand;

IV - to receive equal treatment from public administration bodies and entities regarding the exercise of acts of release of economic activity, in which case the act of release will be bound by the same interpretation criteria adopted in previous analogous administrative decisions;

V - to enjoy the presumption of good faith in acts practiced in the exercise of economic activity, for which doubts in the interpretation of civil, business, economic, and urban law shall be resolved in a way that preserves private autonomy, except if there is express legal provision to the contrary;

VI - to develop, execute, operate, or market new modalities of products and services when infra-legal norms become outdated due to internationally consolidated technological development;

VII - (VETOED);

VIII - to have the guarantee that parity business legal transactions will be subject to free stipulation by the pact works, applying all business law rules only subsidiarily to what was agreed, except for public order norms;

IX - to have the guarantee that, in requests for public acts of release of economic activity, once all necessary elements are presented, the private individual shall be expressly and immediately notified of the maximum term stipulated for the analysis of their request and that, once the fixed term has passed, the silence of the competent authority shall import tacit approval for all effects;

X - to archive any document by means of microfilm or digital media, pursuant to the technique and requirements established in regulation, in which case it shall be equated to a physical document for all legal effects;

XI - not to be required to perform an abusive compensatory or mitigatory measure or provision in the context of impact studies or other releases of economic activity in urban law, understood as one that:

b) requires a measure that was already planned for execution before the request by the private individual;

c) uses the private individual to perform executions that compensate for impacts that would exist regardless of the enterprise;

d) requires execution or provision of any type for areas or situations beyond those directly impacted by the economic activity; or

e) proves to be unreasonable or disproportionate;

XII - not to be required by the direct or indirect public administration to provide a certificate without express provision in law.

§1. For the purposes of item I: (I) a Federal Executive act shall provide for the classification of low-risk activities; (II) in the absence of such act, a CGSIM resolution shall apply; (III) States/Municipalities may edit specific norms.

§2. Inspection of the right in item I shall be carried out posteriorly, *ex officio* or following a complaint.

§3. Item III does not apply: (I) to situations where prices are used to reduce tax value/remit profits as costs abroad; (II) to antitrust law and consumer rights.

§6. Item IX does not apply when: (I) it concerns tax issues or trademark registration; (II) the decision involves a financial commitment from the public administration; (III) there is express objection in a treaty.

§7. Tacit approval does not apply when the applicant is a public agent (or relative up to 3rd degree) of the same body.

Chapter III – Guarantees of Free Initiative

Art. 4. It is the duty of the public administration and other entities bound by this Law, in the exercise of regulating public norms belonging to the legislation addressed herein, except in strict compliance with explicit legal provisions, to avoid the abuse of regulatory power so as to unduly:

I - create market reserves by favoring, in regulation, an economic or professional group to the detriment of other competitors;

II - draft statements that prevent the entry of new national or foreign competitors into the market;

III - demand technical specifications that are not necessary to achieve the desired end;

IV - draft statements that prevent or delay innovation and the adoption of new technologies, processes, or business models, except for situations considered high-risk in regulations;

V - increase transaction costs without demonstration of benefits;

VI - create artificial or compulsory demand for a product, service, or professional activity, including the use of notary offices, registries, or records;

VII - introduce limits to the free formation of business companies or economic activities;

VIII - restrict the use and exercise of advertising and publicity regarding an economic sector, except for cases expressly prohibited by federal law; and

IX - demand, under the pretext of tax registration, requirements of another nature in order to mitigate the effects of item I of the *caput* of Art. 3 of this Law.

Art. 4-A. (Included by Law 14,195/2021) It is the duty of the public administration, in applying public ordering to private economic activities:

I - to provide fair, predictable, and equal treatment between economic agents;

II - to issue notices of infraction or apply sanctions based on subjective terms only when properly regulated by clear and objective criteria;

III - to observe the "double visit" criterion for infractions in low or medium-risk activities.

Chapter IV – Regulatory Impact Analysis

Art. 5. Proposals for the enactment and amendment of normative acts of general interest to economic agents or users of the services provided, issued by a body or entity of the federal public administration, including autarchies and public foundations, shall be preceded by a regulatory impact analysis (RIA), which shall contain information and data on the possible effects of the normative act to verify the reasonableness of its economic impact.

Chapter V – Legislative Amendments and Final Provisions[35]

Art. 6: Extinguishes the Brazil Sovereign Fund (FSB).

Art. 7: Amends the Civil Code regarding legal personality, piercing of the corporate veil, contractual freedom, and the creation of a specific legal framework for investment funds.

Art. 8: Amends the Corporations Law regarding share subscription and public offerings.

Art. 9: Amends the National Network for the Simplification of Registration and Legalization of Companies and Businesses (Redesim) regarding low-risk activity classification.

Art. 10: Amends the law on document storage to authorize and regulate digital storage and the destruction of original physical documents.

Art. 11: Amends Decree-Law regarding the administration and coordination of Union property and related administrative appeals.

Art. 12: Amends the Public Records Law to allow for electronic bookkeeping and publication of records.

[35]Summarized

Art. 13: Amends tax administration law to streamline Federal Tax Administration (PGFN) litigation and the issuance of tax credits.

Art. 14: Amends the Public Registry of Mercantile Companies Law to automate registration processes and simplify company dissolution.

Art. 15: Amends the Consolidation of Labor Laws (CLT) to institute the digital work booklet and update workplace time-tracking requirements.

Art. 16: Replaces the eSocial system with a simplified digital bookkeeping system for labor and tax obligations.

Art. 17: Safeguards the validity of existing PGFN declaratory acts issued prior to this Law.

Art. 18. The effectiveness of item X of Art. 3 is conditioned on federal regulation.

Art. 19. Repeals Delegated Law No. 4/1962, Law No. 11,887/2008, and various articles of the Consolidation of Labor Laws (CLT) and Law 8,934/1994.

Art. 20. This Law enters into force on the date of its publication.

Brasília, September 20, 2019; 198th of Independence and 131st of the Republic.

B.1 State and Municipal-Level LLEs

In addition to the federal Economic Freedom Law, several Brazilian states enacted their own Economic Freedom legislation through state-level laws or executive decrees. These state initiatives typically sought to harmonize administrative procedures within the state, define state-level lists of low-risk activities, and integrate state regulatory agencies into the *RedeSim* business registration system.[36] In some cases, state governments also issued guidance encouraging municipalities to adopt local versions of the law.

Similar to the federal-municipal separation of powers discussed in the main text, state-level legislation may facilitate coordination and reduce legal uncertainty, but does not guarantee local enforcement. Again, the effective regulatory change experienced by firms occurs only when municipalities implement the law at the local level. Our empirical analysis therefore focuses on municipal-level enforcement rather than state-level approval, as this represents the point at which businesses actually become exempt from licensing requirements.

B.2 Time to Open a Business

Our main analysis uses firm-level microdata to study the effects of LLE adoption on the time required to open a business. Redesim also publishes municipal-level statistics and maps of average registration times.[37] However, these aggregated measures suffer from two important limitations.

[36] *RedeSim* (National Network for the Simplification of Business Registration and Legalization) is a federal integration system that connects federal, state, and municipal agencies involved in business registration, allowing administrative information to be shared electronically and reducing duplication in firm entry procedures.

[37] Available at https://estatistica.redesim.gov.br/tempos-abertura.

First, the municipality- and state-level panel reports simple averages across all applications and therefore does not account for compositional changes in the types of businesses being registered. Changes in the industry mix or legal structures of firms may affect registration times even in the absence of procedural reforms.

Second, the times reported in Redesim's maps and publications capture only procedures occurring strictly within the Redesim system. Specifically, the *Viability* and *Registration* stages listed in in Figure B1. As noted by DREI (2025), this measure "does not consider the time applicants take to submit documentation to government agencies between process steps" (DREI, 2025, p. 29, fn. 9). As a result, they substantially understate the true time required to open a business. More specifically, the procedures recorded within Redesim account for only about 20 percent of the total elapsed time.

The microdata used in our analysis addresses both limitations. It allows us to control for detailed firm characteristics, including industry and legal form, while also measuring the time required for applicants to gather documentation, submit materials to government agencies, and obtain validation approvals. Consequently, our data provide a much more comprehensive measure of the time required to open a business.

In Figure B1, we present a stylized diagram showing the procedure to open a business in Brazil. We note that although operating licenses are technically a separate step that should occur *after* incorporation, many municipalities required documents and inspections upfront to asses risk on a case-by-case basis as a pre-condition for address viability. This suggests that some of the gains described in the last "With LLE" note may take place at the Viability stage, during address checks.

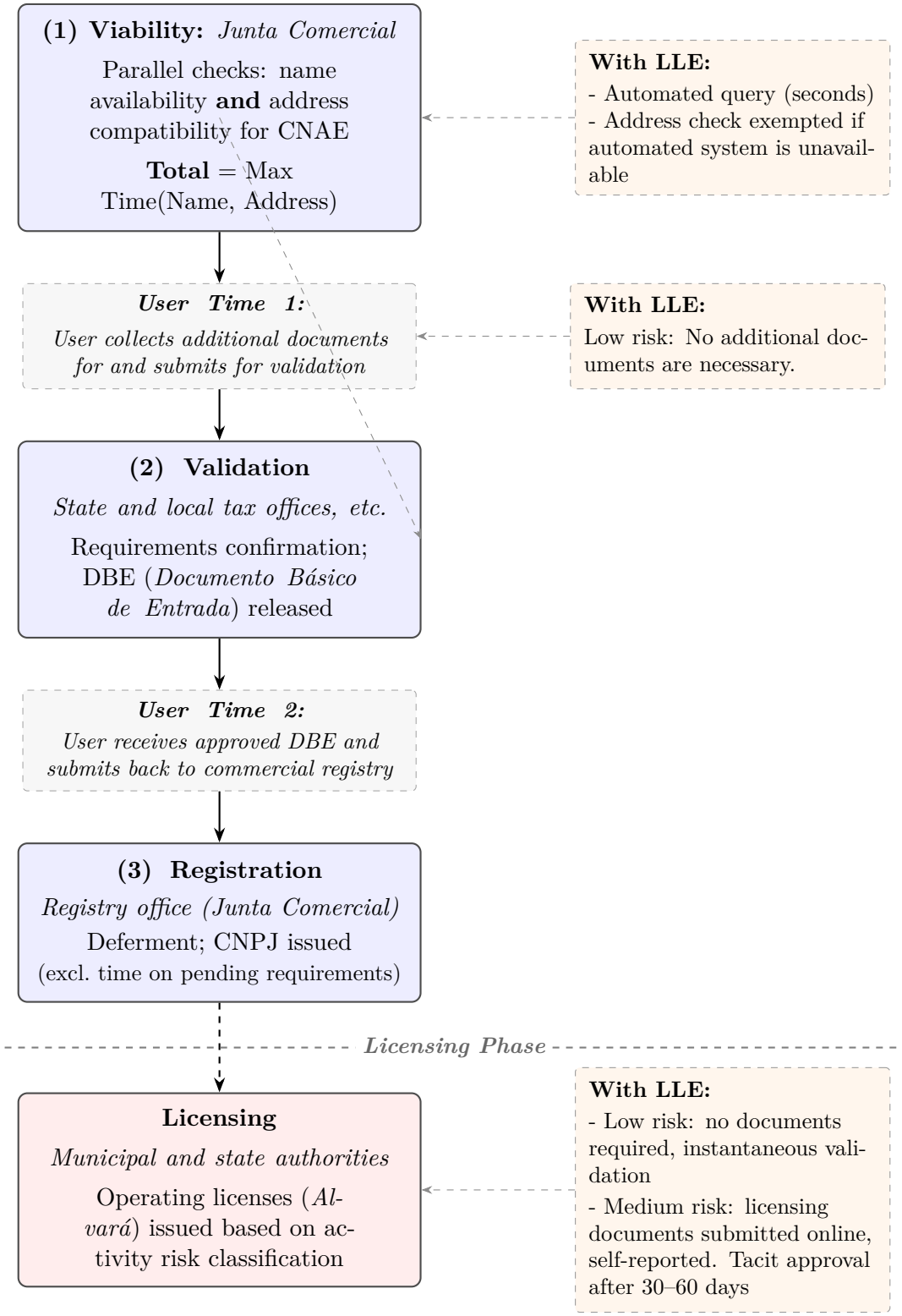
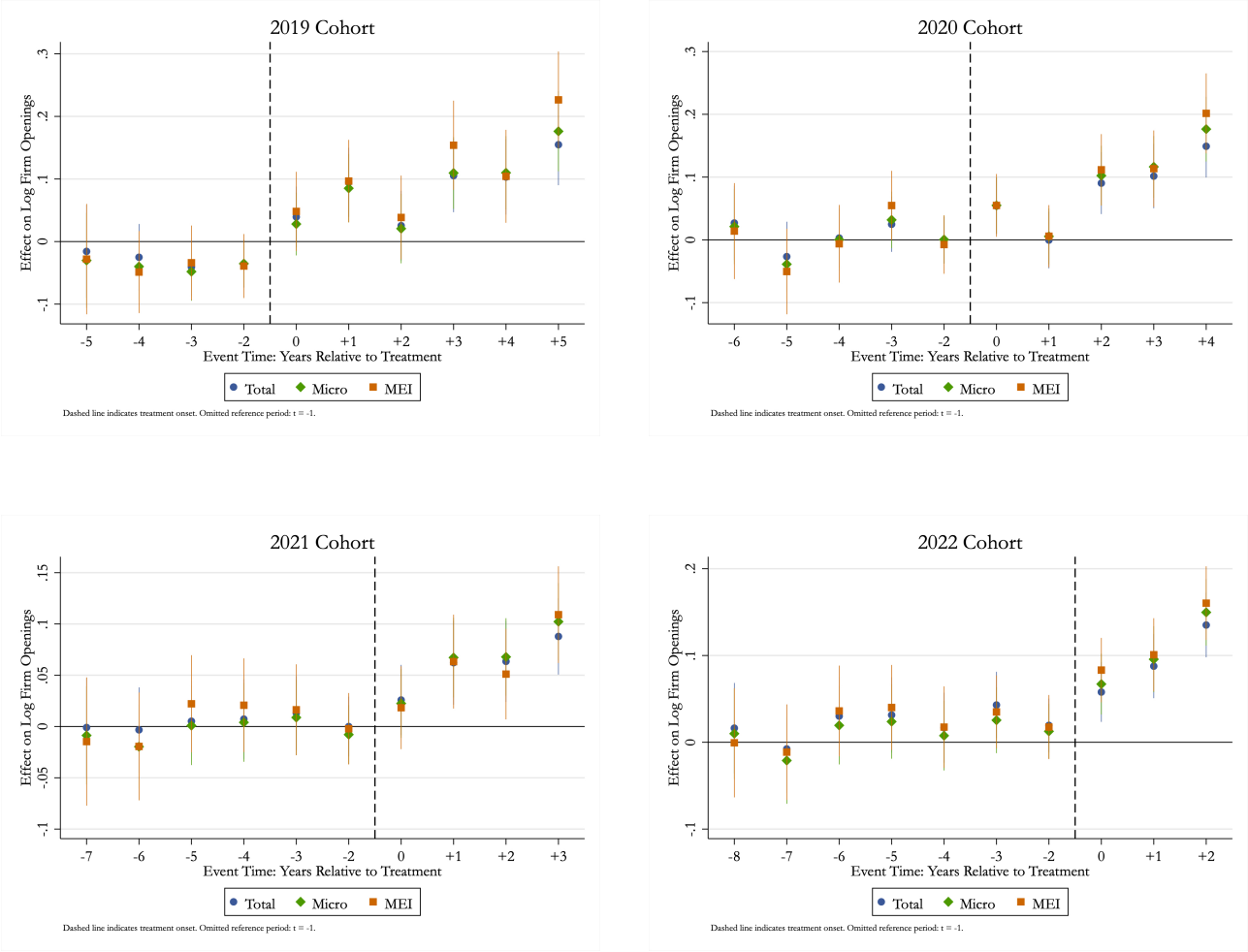


Figure B1: Business registration workflow in Brazil

Notes: This figure shows a simplified diagram of the procedures required to open a business. Gray dashed boxes indicate applicant-side time used for collecting documents. LLE = *Lei de Liberdade Econômica* (Economic Freedom Law).

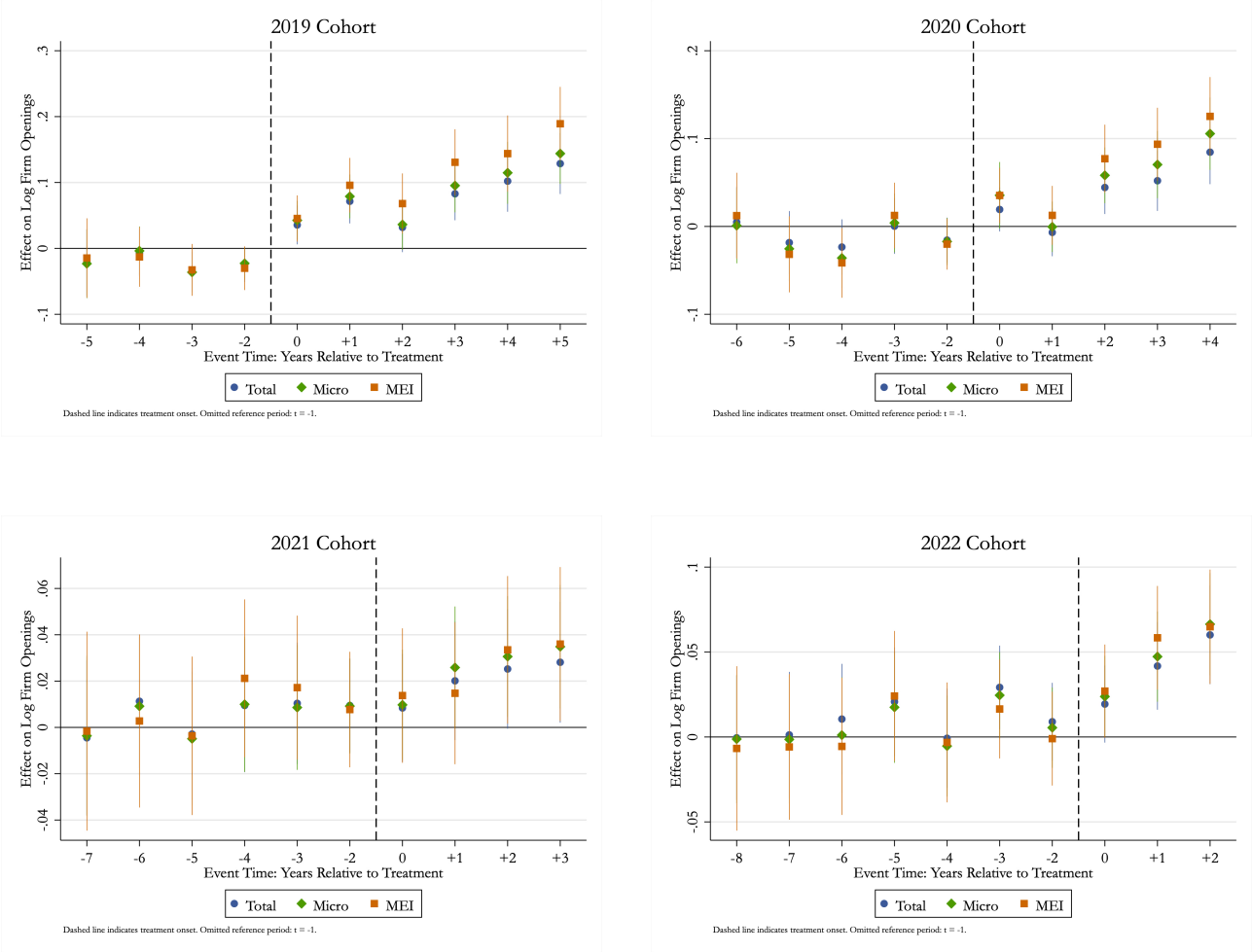
C Cohort by Cohort Effects

Figure C1: The Effect of LLE Adoption on Firm Openings, Cohort by Cohort Effects, Group 1 Comparisons



Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period to create weights.

Figure C2: The Effect of LLE Adoption on Firm Openings, Cohort by Cohort Effects, Group 2 Comparisons

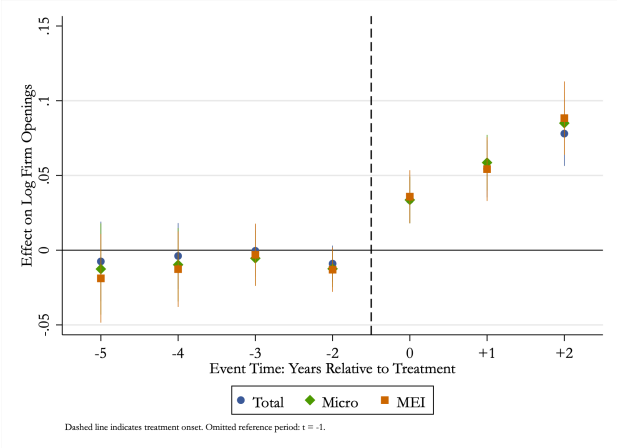


Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period to create weights.

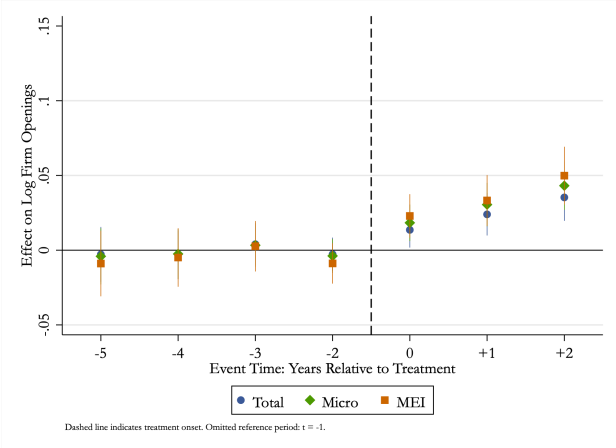
D Additional Results and Robustness Checks

D.1 Results Using the Full Sample

Figure D1: The Effect of LLE Adoption on Firm Openings by Firm Type for the Full Sample (Non-Trimmed)



(a) Group 1 (Treatment in Any State)

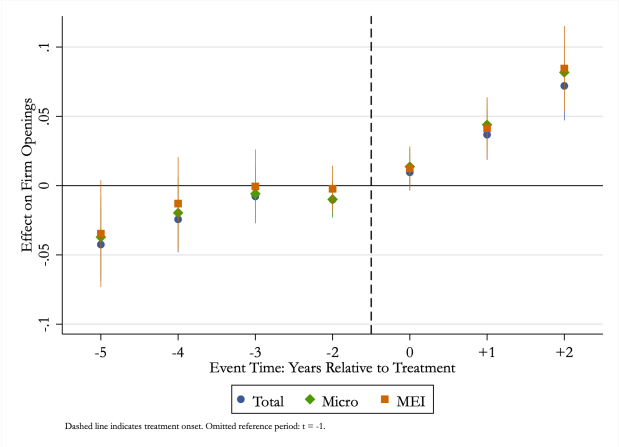


(b) Group 2 (LLE-Adopting States)

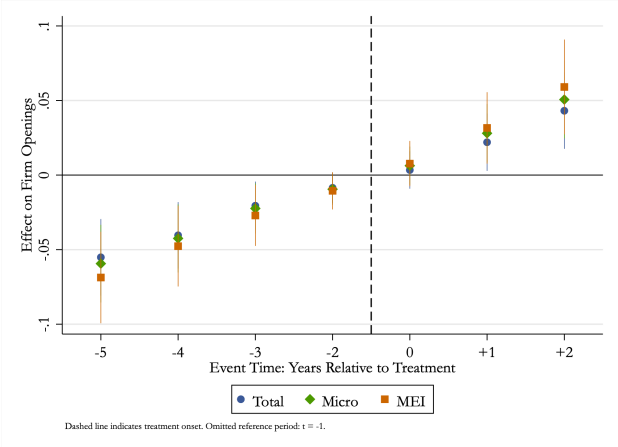
Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

D.2 Alternative Transformation Results

Figure D2: The Effect of LLE Adoption on Firm Openings by Firm Type using a Poisson Distribution



(a) Group 1 (Treatment in Any State)

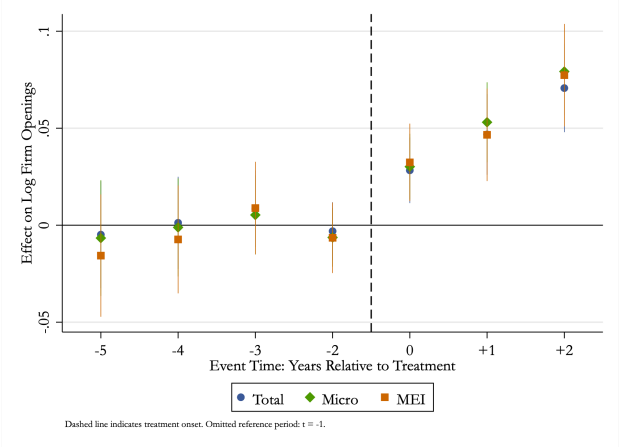


(b) Group 2 (LLE-Adopting States)

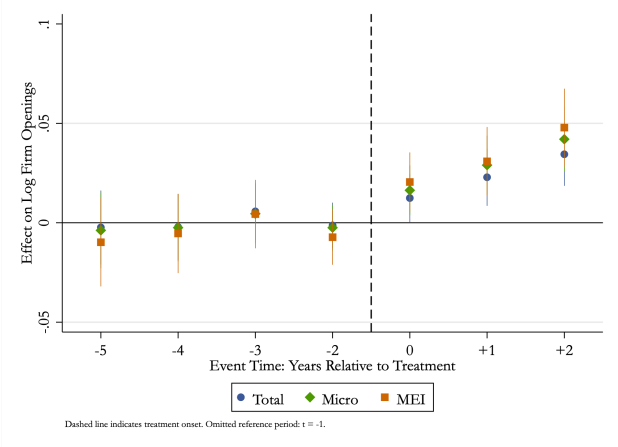
Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using level outcomes and a Poisson distribution, in a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

D.3 Results Using IPW Weights

Figure D3: The Effect of LLE Adoption on Firm Openings by Firm Type using IPW Weights with Baseline Covariates



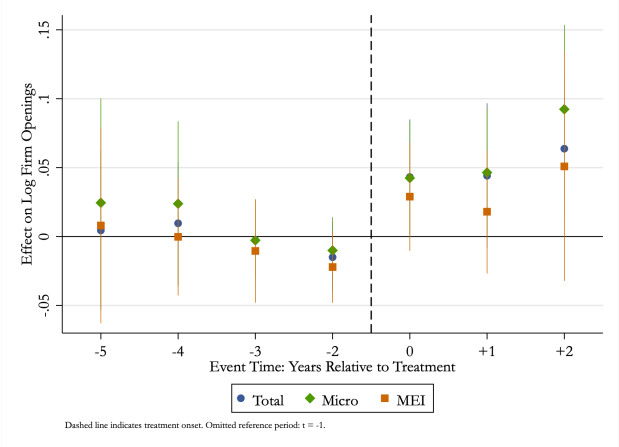
(a) Group 1 (Treatment in Any State)



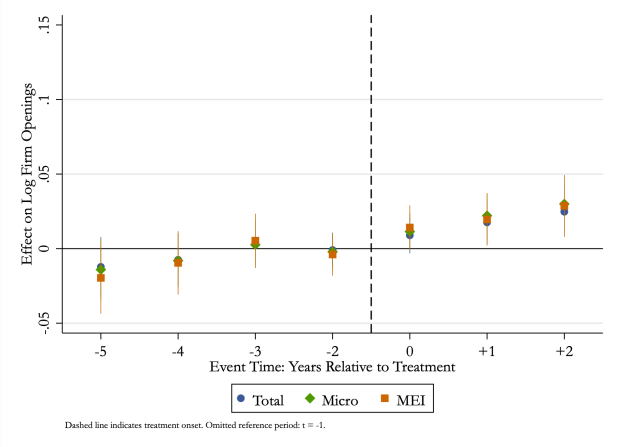
(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Inverse Propensity Score weighting uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

Figure D4: The Effect of LLE Adoption on Firm Openings by Firm Type using IPW Weights with Baseline + Additional Covariates



(a) Group 1 (Treatment in Any State)

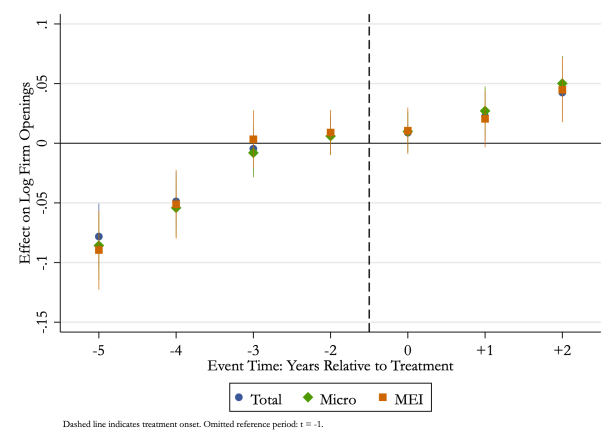


(b) Group 2 (LLE-Adopting States)

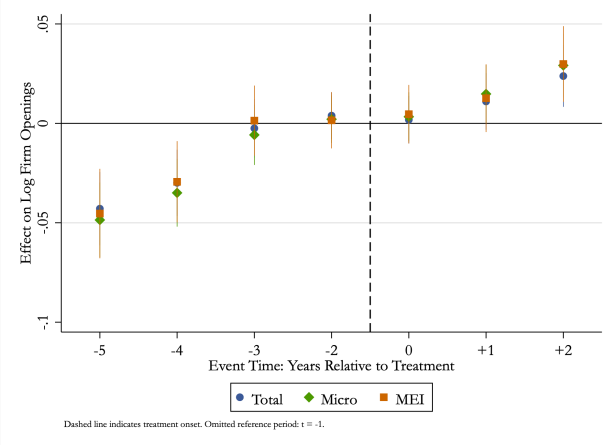
Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Inverse Propensity Score weighting uses a smoothed growth rate of the outcome variable throughout the pre-treatment period a standard deviation of year-to-year growth rates of the outcome over that same period, and lagged population, lagged per capita GDP, lagged agricultural value added, and lagged industry value added to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

D.4 Unconditional/Unweighted Stacked DiD

Figure D5: The Effect of LLE Adoption on Firm Openings by Firm Type



(a) Group 1 (Treatment in Any State)

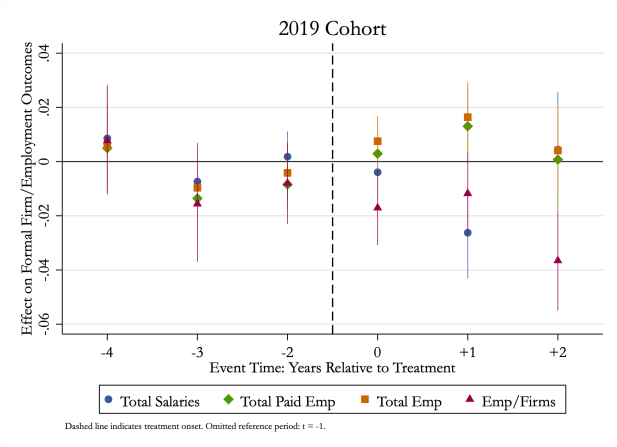


(b) Group 2 (LLE-Adopting States)

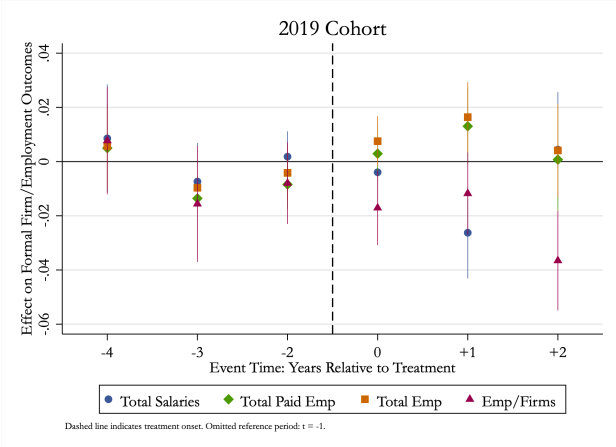
Notes: Each graph plots three separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using log transformed outcomes and a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Regressions are not weighted. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.

D.5 Additional Data

Figure D6: The Effect of LLE Adoption on Additional Firm Outcomes for the 2019 Cohort using CEMPRE Data



(a) Group 1 (Treatment in Any State)



(b) Group 2 (LLE-Adopting States)

Notes: Each graph plots four separate stacked difference-in-difference estimates; each symbol/color represents a different set of estimates. Lines show 95% confidence intervals. Standard errors are clustered at the municipality level. Regressions are estimated using level outcomes and a Poisson distribution, in a stacked design with cohort-by-year and cohort-by-municipality fixed effects. Entropy balancing uses a smoothed growth rate of the outcome variable throughout the pre-treatment period and a standard deviation of year-to-year growth rates of the outcome over that same period to create weights. Panel A uses Group 1 comparisons: ever-adopters vs. never-adopters, controls also in never-treated states. Panel B uses Group 2 comparisons: ever-adopters vs. never-adopters both in LLE-adopting states. See Table 1 for details.