Place-Based Industrial Policy, Skill Scarcity and Labor Reallocation

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Abstract

Industrial policies are increasingly used to attract investment and create employment. Yet, these policies have often failed to deliver employment growth: beneficiary firms expand, but these gains do not necessarily translate into aggregate employment gains. In this paper, I provide novel evidence on this divergence using Colombia's Free Economic Zones (FEZs), a place-based industrial policy that provides tax incentives to firms operating in designated areas. Combining the universe of zones with firm-, worker-, and municipality-level data, I use a difference-in-differences design that exploits the staggered opening of new zones. The absence of aggregate employment gains masks a reallocation of workers across firms and toward informality, which increases by roughly five percentage points. The policy increased production and profits among beneficiary firms while raising their demand for high-skilled workers. To meet this growing demand, large firms and FEZ beneficiaries offered higher wages to attract skilled workers already employed by other formal firms, consistent with a limited local supply of skilled labor. This upward wage pressure spread across local labor markets: smaller and less productive formal firms faced rising labor costs and became more likely to exit. I provide suggestive evidence that low-skilled workers displaced by these firm exits moved into informal employment. These results highlight skill scarcity as a mechanism behind jobless industrialization: labor can be abundant in aggregate but scarce at the margin demanded by industrial policy.

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1 Introduction

Industrial policy is intended to transform economies by expanding productive firms and creating modern jobs (Rodrik, 2013; Harrison and Rodríguez-Clare, 2010; Juhász and Lane, 2024; Fernandes and Reed, 2026). Governments around the world subsidize manufacturing, strategic industries, and place-based investment in pursuit of these goals. Yet industrialization in developing economies has increasingly failed to deliver employment, a pattern often described as jobless industrialization: large industrial firms exhibit strong productivity performance while generating relatively few jobs, whereas smaller firms continue to absorb labor despite limited productivity gains (Diao et al., 2024; Eslava et al., 2024; Alfaro et al., 2023).

The absence of employment gains from industrial policy may reflect weak labor demand from the firms it subsidizes or limits on the labor supply available to them. A prevailing view emphasizes demand, arguing that modern firms in developing economies adopt capital-intensive technologies, experience declines in manufacturing prices, and face distortions that hold hiring down (Diao et al., 2024; Acemoglu and Restrepo, 2022; Rodrik, 2015; Hsieh and Klenow, 2009). Labor supply, by comparison, is rarely seen as a constraint: in economies with high unemployment and pervasive informality, workers are presumed to be readily available for an expanding modern sector (Lewis, 1954; Harris and Todaro, 1970). Yet the firms industrial policy targets are among the most skill-intensive employers in these economies (Hjort et al., 2022; Gottlieb et al., 2025), while informal employment is concentrated among low-skilled workers (La Porta and Shleifer, 2014; Ulyssea, 2020; OECD, 2024).[1] Labor may therefore be abundant in aggregate yet scarce at the margin industrial policy demands. How industrial policy adjusts to this constraint remains largely unknown.

In this paper, I study how local labor markets accommodate the expansion of subsidized firms using Colombia's Free Economic Zones (FEZs), which grant tax incentives to selected firms in designated locations and are among the most widely used place-based industrial policy instruments in developing economies. Using survey and administrative microdata at the municipality, firm, and worker levels, I show that participating firms expand substantially, yet employment does not. Contrary to the conventional view that expanding the formal sector draws workers out of informality (McCaig and Pavcnik, 2018), I find that industrial policy can instead push less-educated workers into it: expanding firms draw on a limited pool of skilled workers already employed elsewhere, while the less-educated workers displaced from contracting firms cannot access the new formal jobs.

I focus on Colombia, where Law 1004 of 2005 fundamentally redesigned the country's Free Economic Zone (FEZ, or *zonas francas*) regime.[2] The reform replaced five publicly administered industrial parks with a system that allowed privately promoted zones to be established throughout the country. To promote investment and job creation, participating firms received

[1] Globally, 94% of workers with no education and 85% of those with only primary education are employed informally; nearly 45% of informal workers have at most a primary education, compared with 7% of formal workers (OECD, 2024).

[2] Colombia also provides a natural setting because roughly half of its workforce is employed informally, so the labor reserve industrial policy is presumed to draw upon is substantial.

exemptions from corporate income taxes and import tariffs together with simplified administrative procedures.[3] Firms receiving these incentives operate exclusively within the zone, so industrial expansion remains localized and expanding firms compete with incumbent employers for the same workers. In the decade and a half following the creation of the new regime, the number of zones grew to nearly 120, roughly 22 percent of all FEZs in Latin America and the Caribbean and the largest share in the region—yet Colombia ranked only seventh in zone-related employment (UNACTD, 2018; Cámara de Usuarios de Zonas Francas, ANDI, 2019).

To assess the impact of FEZs, I assemble a novel georeferenced panel linking administrative and survey microdata at the municipality, firm, and worker levels, including household surveys covering formal and informal employment, manufacturing censuses, firm financial statements, business registries, and matched employer–employee records covering the universe of formal firms and workers. Using machine-learning techniques applied to unstructured administrative reports, I recover the exact geographic coordinates and opening dates of every zone established between 2001 and 2018 and link them to these data. This allows me to follow the adjustment to industrial expansion throughout the local economy, observing the firms operating inside zones, the incumbent establishments competing with them, and the workers moving between them.

Identifying the causal effects of FEZ establishment is nonetheless challenging because FEZ locations are not randomly assigned. Indeed, municipalities receiving a zone are systematically larger and economically different than those that do not, and my identification strategy does not assume otherwise. Instead, identification comes from the staggered timing at which the first zone becomes operational following the 2005 reform. Approval is only the first step in a lengthy process: after designation, infrastructure must be built and firms must establish operations, with opening often occurring several years later. Because approval does not imply immediate operation, I argue that, conditional on municipality fixed effects and common aggregate shocks, the timing of operation is plausibly unrelated to contemporaneous local labor-market trends. I identify operational start dates using two independent sources—the entry of the first firm into the zone and satellite imagery documenting site development—allowing treatment to be measured when economic activity actually begins rather than when approval is granted. I estimate dynamic treatment effects using the estimator of Callaway and Sant'Anna (2021), comparing municipalities receiving a zone with never- and later-treated municipalities. The results are robust to allowing for differential trends based on pre-treatment municipal characteristics, restricting the analysis to municipalities for which labor-market outcomes are measured more precisely, redefining local labor markets using commuting-based functional urban areas, excluding nearby municipalities from the comparison group, and assigning placebo treatment dates and locations.

I organize the analysis around a simple conceptual framework that rationalizes how the expansion of subsidized firms can affect aggregate employment and informality. The framework builds on Ulyssea (2018), where heterogeneous firms sort between formal and informal

[3]The regime itself embeds a trade-off between investment and employment: larger investment commitments are accompanied by lower employment requirements.

production according to their productivity. I extend this setting by introducing FEZ firms, which operate formally but benefit from lower corporate taxation and face higher fixed costs, and by allowing firms to differ in their demand for workers of different skill levels. FEZ firms are more skill-intensive than ordinary formal firms, which are in turn more skill-intensive than informal firms. FEZ incentives induce beneficiary firms to expand, but the aggregate employment effect depends on how the resulting labor demand is accommodated. When the skills demanded by FEZ firms are in limited supply, wages rise and workers are reallocated across firms. Informality increases when displacement from ordinary formal firms exceeds the employment created by FEZ firms.

I first examine the aggregate labor-market response. At the municipality level, total employment does not change following FEZ establishment, despite persistent increases in fiscal revenues, nighttime luminosity, and tax collections. Instead, the local informality rate rises by approximately five percentage points relative to baseline, while unemployment and labor-force participation remain unchanged. Sectoral decompositions reveal only modest employment gains concentrated in manufacturing and no detectable effects in services. Industrial policy therefore reshapes the composition of local employment rather than expanding it.

I next examine whether this absence of employment growth reflects a failure of beneficiary firms to respond to the policy. Using administrative financial statements, I find that FEZ firms substantially expand investment, capital, inventories, and wage bills following entry. Wage bills increase by roughly 78 percent, accompanied by large increases in long-term assets and inventories, while net income before taxes rises by about 46 percent and operating income remains broadly unchanged, indicating that the gains are driven primarily by preferential tax treatment rather than by improvements in operating performance. Beneficiary firms therefore expand substantially, deepening rather than resolving the puzzle: because they account for a small share of local employment, even large proportional increases in their hiring leave municipal totals unchanged.

To understand how local labor markets accommodate this expansion, I turn to worker-level data, where the aggregate null masks pronounced heterogeneity across skill groups. Medium- and high-skilled workers become 1.9 and 1.6 percentage points less likely to work informally and receive wage premia of up to 4 percent relative to low-skilled workers. Low-skilled workers, by contrast, become approximately 6 percentage points more likely to work informally, are increasingly absorbed into informal occupations with stagnant earnings, and show no change in unemployment. Because low-skilled workers make up the majority of the local labor force, their transition into informality accounts for most of the municipality-level increase in the informality rate. The expansion of modern firms is thus accompanied by reallocation across labor-market segments rather than by broad-based employment creation.

I then examine how incumbent firms adjust to these pressures. Using the universe of formal manufacturing establishments, I find that surviving firms in treated municipalities raise wages per worker by approximately 4 percent, expand employment by about 4.8 percent, and become more productive. At the same time, formal-firm exit rates rise by 7–8 percent, concentrated among younger, smaller, and less productive establishments operating near

the productivity cutoff. Displacement therefore operates through exit rather than through contraction at surviving firms: competition for scarce skilled labor raises the productivity threshold for survival, reallocating employment toward larger and more productive firms while pushing the workers of exiting firms into informality.

These results describe a coherent adjustment process. FEZs expand modern, capital- and skill-intensive firms, yet aggregate employment remains unchanged because the workers these firms need were already employed in the formal sector. Beneficiary firms expand by bidding skilled workers away from incumbent employers. Incumbents raise wages in response, and those unable to sustain higher labor costs exit. Lower-skilled workers cannot fill the skilled vacancies created by expanding FEZ firms, and the jobs they held disappear with the firms that employed them; informality absorbs them. Skill scarcity therefore determines what industrial policy can achieve: where the marginal worker must be drawn from another formal employer, policy-induced firm expansion reallocates labor rather than creates employment. Two features of the FEZ regime sharpen this constraint. Its benefits accrue to a narrow subset of firms, concentrating the increase in labor demand, while the requirements and fixed costs associated with operating under the regime tend to favor larger, more capital-intensive firms. As these firms are also relatively skill-intensive, the policy directs labor demand toward precisely the skills that are locally scarce. Industrialization is jobless when the workers modern firms need are already employed.

Related Literature I contribute to three strands of literature.

First, my paper speaks to the macro-development literature on the relationship between industrial policy, informality, and productivity. A central insight is that many developing economies experience "jobless" or "premature" industrialization, in which increases in productivity and capital intensity are not accompanied by proportional employment growth (Rodrik, 2015; McMillan and Zeufack, 2022; Alfaro et al., 2023). Existing explanations locate the constraint on the demand side, emphasizing capital-intensive technology adoption, declining manufacturing prices, and firm-level distortions (Diao et al., 2024; Acemoglu and Restrepo, 2022; Rodrik, 2015; Hsieh and Klenow, 2009). Closest to this paper, Amodio et al. (2026) show for Ethiopia that increases in industrial employment raise employment at small informal firms through local demand linkages, tracing how modern-sector expansion propagates through the local labor market. I examine the same propagation on the hiring margin: where the workers that expanding firms employ are drawn from, and what happens to the employers they leave. FEZs raise productivity and wages among capital- and skill-intensive firms but absorb few additional workers, because the workers these firms require are already formally employed. Jobless industrialization, in this setting, reflects the composition of available labor rather than the weakness of labor demand.

Second, my paper relates to the literature on the effects of place-based industrial policies. Most existing studies focus on developed economies, where adjustment is interpreted through commuting, migration, and reallocation across firms (Neumark and Kolko, 2010; Gobillon et al., 2012; Kline and Moretti, 2013; Mayer et al., 2015; Criscuolo et al., 2019; Freedman et al.,

2023). In developing economies, where informality accounts for a large share of employment, adjustment extends beyond movements across firms and locations to include movements across the formal–informal boundary (La Porta and Shleifer, 2014; Meghir et al., 2015; Dix-Carneiro and Kovak, 2017; Ulyssea, 2018), and evidence on these programs remains limited and mixed. China's Special Economic Zones increased capital investment, employment, output, productivity, and wages within treated regions (Alder et al., 2016), with limited spillovers to surrounding areas (Wang, 2013; Lu et al., 2019). McCaig et al. (2024) show that industrial zones in Vietnam expanded formal employment and firm creation, especially in manufacturing. Evidence from India points in conflicting directions: Gallé et al. (2022) find that SEZs boosted informal employment and expanded non-agricultural jobs, whereas Abeberese and Chaurey (2019) document a decline in firm counts driven by the exit of informal entities or their transition to formality. Across these settings, the labor supply facing beneficiary firms is treated as unconstrained, so employment effects are read as evidence about how much firms expand rather than about where their workers come from. Whether expanding firms draw workers from unemployment, informal employment, or incumbent formal employers changes how industrial policy affects the modern sector, the distribution of gains across workers, and welfare. I show that this margin is where the action lies: the resulting general equilibrium adjustment—wage pressure, a higher productivity threshold for survival, and exit among marginal formal firms—accounts for the absence of aggregate employment gains.[4]

Third, my paper contributes to the development literature on informality and firm dynamics. A large body of work identifies determinants of informality such as entry costs, labor and tax regulation, corruption, and weak enforcement (see Ulyssea, 2020, for a review). Interventions that lower the costs of formalization have shown positive but often short-lived effects on firms' registration decisions (Djankov et al., 2010; Ulyssea, 2018; Rocha et al., 2018), while value-added taxes can propagate informality along supply chains (De Paula and Scheinkman, 2010). A related literature finds that expansions of formal employment draw workers out of informality: McCaig and Pavcnik (2018) study a labor-intensive export boom in Vietnam that expanded formal employment and reduced informal microenterprise work, Zárate (2022) shows that improved urban mobility reallocates workers from informal to formal establishments, and Imbert and Ulyssea (2026) find that rural–urban migration reduces informality in Brazilian cities over a decade, though the shock there operates through labor supply rather than labor demand. I document the opposite direction. As beneficiaries bid up local wages, marginal formal firms exit and the workers they employed reallocate into informal work. The two sets of findings are reconciled by the skill content of induced demand: expansions concentrated in skill-intensive activities recruit from a pool the informal sector does not supply, so the traditional sector adjusts through displacement rather than absorption. Spatially concentrated policies can therefore reinforce existing labor-market distortions even when aggregate employment is unchanged.

[4]Displacement of incumbent formal employers within the treated labor market is precisely the object those designs net out; my setting allows me to recover it.

2 Institutional Context and Data

This section describes the institutional framework governing Free Economic Zones in Colombia, their expansion following the 2005 reform, and the data used in the empirical analysis. I first describe how zones are established and operated, how firms become beneficiaries, and the tax and customs incentives they receive. I then document the spatial and temporal expansion of FEZs and describe the data used to measure their effects on local labor markets, firms, and workers.

2.1 Free Economic Zones

Special Economic Zones (SEZs) are among the most widely used place-based industrial policy instruments in developing countries. They aim to attract investment, promote industrial development and competitiveness, and generate employment by offering firms preferential economic conditions within designated areas. Their use has expanded rapidly: globally, the number of SEZs increased from around 500 in 1995 to 5,400 in 2018 (Facility for Investment Climate Advisory Services, 2008; World Bank Group, 2017; UNCTAD, 2019). Although their institutional design varies across countries, SEZs share a common feature: geographically delimited areas in which eligible firms receive preferential treatment through reduced taxes and tariffs, customs advantages, or streamlined administrative procedures (Akinci et al., 2008). Colombia's *Zonas Francas* belong to this broader family of policies.[5]

Colombia has operated *Zonas Francas* since the 1950s, initially as publicly established and managed industrial areas. The institutional framework changed substantially with Act 1004 of 2005 and its subsequent regulation, which expanded private participation and introduced a new set of tax and customs benefits. Under this framework, FEZs are geographically delimited areas in which qualified firms operate under a special tax, customs, and foreign-trade regime. The Act established four objectives: generating employment and investment, promoting regional development and competitiveness, fostering productive industrial processes and economies of scale, and simplifying international trade procedures.

Access to the FEZ regime involves two stages: the declaration of the zone and, in multi-user zones, the subsequent qualification of firms operating within it. To establish an FEZ, an interested legal entity applies for a specific site and submits a *Plan Maestro de Desarrollo General* (Master Development Plan), together with technical, economic, financial, and market feasibility studies. The Plan specifies the proposed location, activities, investment, employment, and implementation schedule, while the site must satisfy land-use, environmental, infrastructure, and other legal requirements. Permanent FEZs (ZFPs) are multi-user zones and must generally comprise at least 20 contiguous hectares, host at least five qualified users, and undertake approximately USD 12 million in new investment within five years. Special Permanent FEZs (ZFPEs), by contrast, are established around a single industrial or service project and face

[5]Throughout the paper, I refer to the Colombian program as Free Economic Zones (FEZs), while using SEZs to refer to the broader international policy framework.

sector-specific investment and employment requirements (Appendix Table 1).[6]

Applications undergo several stages of national review. The Technical Secretariat of the Intersectoral Commission of Free Economic Zones reviews the Plan Maestro and supporting documentation, after which the Intersectoral Commission assesses the project and decides whether to approve the Plan. Following approval, DIAN verifies the remaining legal requirements and issues the administrative resolution declaring or rejecting the proposed area as an FEZ. The statutory process could be completed in approximately four months for a complete application, although requests for additional documentation, technical assessments, or verification could suspend the applicable decision periods.

Once declared, an FEZ remains privately owned and operated. Permanent FEZs are administered by an authorized *Usuario Operador* (zone operator), a legally separate entity responsible for administering the zone, qualifying firms, monitoring compliance, and overseeing movements of goods into, out of, and within the FEZ. Operators must satisfy financial and operational requirements and remain independent from firms operating within the zone.[7]

In Permanent FEZs, locating within the designated area does not automatically make a firm a beneficiary. Firms apply to the *Usuario Operador*, which formally qualifies eligible firms as industrial goods, industrial services, or commercial users. Qualification entails administrative requirements and, for industrial users, investment and employment commitments that increase with firm assets, although there is no universal minimum threshold (Appendix Table 1, Panel C). In Special Permanent FEZs, the applicant is itself the sole industrial user, and these commitments are assessed as part of the declaration process (Panel B).

Qualified firms receive a combination of tax and customs benefits. Act 1004 initially established a preferential corporate income tax rate of 15 percent, increased to 20 percent in 2017 but remaining below the ordinary rate. Foreign goods, intermediate inputs, and capital equipment can enter and remain within the FEZ without import tariffs or import VAT, while eligible goods entering from the Colombian customs territory receive preferential VAT treatment. Importantly, these benefits are not conditional on exporting: industrial users may sell domestically, although goods entering the national customs territory are treated as imports and subject to the corresponding duties and taxes.

Access to these benefits is conditional on maintaining qualified-user status. A central condition for industrial users is the *principle of exclusivity*: their authorized industrial activities must be carried out within the FEZ's designated boundaries. This does not prevent the same corporate group from operating elsewhere in Colombia, but the goods or services covered by the FEZ qualification cannot be produced or provided through branches or agencies outside the zone. Firms must also meet their investment and employment commitments and comply with

[6]Statutory monetary thresholds are expressed in units of the Colombian monthly minimum wage (SMMLV). I report approximate U.S. dollar equivalents using the 2010 monthly minimum wage of COP 515,000 and an exchange rate of COP 2,000 per U.S. dollar.

[7]Declaration does not necessarily coincide with the start of operations. Following declaration, the developer must implement the infrastructure and commitments specified in the Master Development Plan, including fully enclosing the site before operations can begin. The time between declaration and operation therefore varies across projects; I document this in the next section.

the customs and inventory-control requirements of the regime. DIAN administers and enforces these provisions, while the zone operator performs monitoring and control functions within the FEZ.

The principle of exclusivity gives the FEZ boundary substantive economic and legal meaning. Each zone occupies a precisely defined and physically enclosed area, and movements of goods across its boundaries are subject to customs control. Crucially, this boundary does not coincide with the municipality: an FEZ generally occupies only a fraction of municipal territory, while firms elsewhere in the municipality remain under the ordinary tax and customs regime. FEZ designation does not create a separate local jurisdiction, however, and the site remains subject to municipal land-use requirements and embedded in the same local economy. Thus, beneficiary and non-beneficiary firms can operate within the same municipality while facing different national tax and customs regimes.

Following the 2005 reform, the number of FEZs expanded rapidly (Figure 1). Colombia had more than 100 zones by 2015 and 124 by 2024, of which 42 were Permanent and 82 Special Permanent FEZs. The zones span industry, services, and agroindustry and are concentrated around Bogotá, Valle del Cauca, and the Caribbean coast (Cámara de Usuarios de Zonas Francas, ANDI, 2019). During the period studied in this paper, 109 FEZs were established, of which 34 were Permanent and 75 Special Permanent FEZs. Despite their large number, their aggregate contribution remains modest relative to several other Latin American FEZ regimes: in 2022, FEZ exports represented 3.8% of Colombian exports and FEZ activity accounted for 0.8% of GDP.[8]

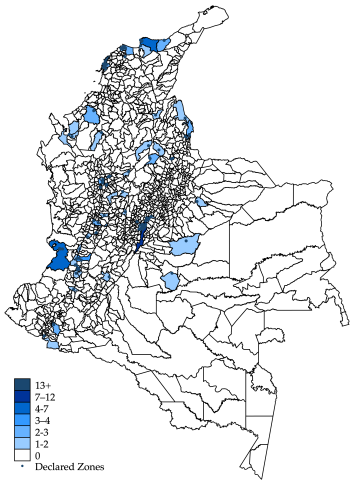
Colombian FEZs differ in important ways from the larger place-based programs commonly studied in the literature. They are relatively small, fenced industrial sites, rather than the municipality-sized SEZs found in China or the broader neighborhoods and census tracts targeted by U.S. Empowerment Zones. Moreover, the Colombian regime is primarily oriented toward new economic activity: proposed activities generally cannot already be taking place at the designated site, and investment commitments are defined in terms of new investment following declaration.[9] Thus, rather than extending preferential treatment over a broad existing economic area, Colombian FEZs concentrate new investment and beneficiary firms within relatively small, physically delimited sites embedded in broader local economies.

This distinction between the FEZ boundary and the surrounding local economy is important for the empirical analysis. The boundary determines which firms receive the policy's direct benefits, but does not isolate them from firms and workers outside the zone. In particular, firms inside and outside the FEZ may draw workers from the same local labor market despite facing different policy regimes. The effects of an FEZ may therefore extend beyond beneficiary firms, inducing adjustments among firms and workers elsewhere in the municipality.

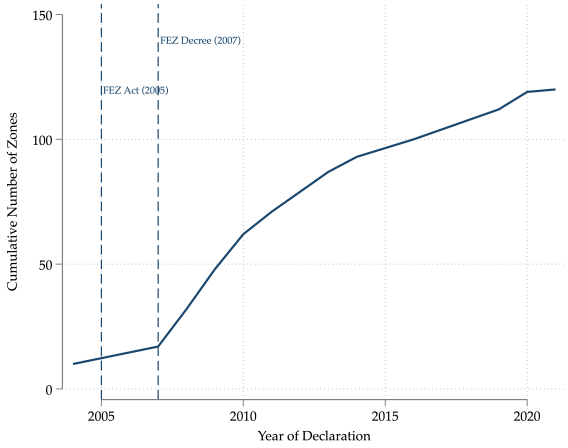
[8] Appendix B.1 provides comparative evidence on FEZ design, activity, and statutory incentives across countries.

[9] The regime includes specific provisions for Special Permanent FEZs involving pre-existing investments; see Appendix A.1.

Figure 1: Free Economic Zones in Colombia



(a) Spatial distribution of FEZs



(b) Cumulative number of FEZs

Notes: Panel (a) displays the spatial distribution of declared FEZs in Colombia. Panel (b) shows the cumulative number of FEZs with known establishment dates. Source: Ministry of Commerce, Industry and Tourism and location data collected by the author.

2.2 Data

I focus on the period 2000–2018 and combine multiple data sources, including household surveys, structural sectoral surveys, administrative data on workers and firms, and hand-collected information on FEZs.

Free Economic Zones. I compile administrative records on the declaration and operation of Free Economic Zones (FEZs) from the Ministry of Industry and Commerce of Colombia for the period 2005–2023. These records include the declaration year, type, and legal status of each zone. Using machine-learning algorithms, I match the administrative names of FEZs to their locations in Google Maps and georeference them at the municipality level. All coordinates are manually verified against official documentation and local reports. Unlike studies that approximate SEZs using circular areas, I hand-collect information on the exact geographic boundaries and sizes of

FEZs, which allows me to measure their actual spatial footprint. Each FEZ is contained within a single municipality and occupies a delimited area within it, rather than extending across municipal boundaries. The georeferencing further allows me to compute distances to seaports, airports, and highways, which I use as control variables in the empirical analysis.

Administrative declaration does not necessarily coincide with the start of operations. I therefore use Google Earth's historical imagery and firm-level qualification records to identify when each FEZ became active. I use this operational date, rather than the declaration date, to define the onset of treatment. To focus on zones with potential spatial and lasting effects, I restrict the sample to operating Permanent and Special Permanent FEZs, excluding offshore and transitory zones as well as those declared but never operational. The final dataset includes all operating FEZs in Colombia, with detailed information on their location, geographic boundaries, area, type, declaration date, and operational date. These data allow me to construct measures of exposure and treatment intensity based on zone size and to link FEZ locations to municipality-level labor and firm data. Descriptive statistics on the number, area, and sectoral and regional composition of FEZs are presented in Appendix B.1.

Local economic and labor-market outcomes. I use municipality-level data compiled by the Center for Studies on Economic Development (CEDE, Universidad de los Andes) to characterize local economic conditions. This dataset provides a comprehensive panel of Colombian municipalities from the 1990s onward, constructed primarily from administrative records. It includes time-varying measures such as local tax revenues, public investment, population, and urban–rural composition, as well as time-invariant geographic characteristics, including distance to Bogotá, distance to major cities, and land area.[10] To proxy for local economic activity, I additionally use nighttime light intensity aggregated at the municipality level, combining data from the Defense Meteorological Satellite Program (DMSP) and the Visible Infrared Imaging Radiometer Suite (VIIRS). These data are harmonized into a consistent panel by Li et al. (2020).

To measure local labor-market outcomes, I use the *Gran Encuesta Integrada de Hogares* (GEIH), the labor force survey administered by the National Statistics Office of Colombia (DANE). The GEIH is a monthly cross-sectional survey covering approximately 240,000 households per year and provides information on employment, unemployment, sector of work, earnings, and social security coverage, together with demographic and socioeconomic characteristics such as sex, age, marital status, and education. I identify informality using workers' self-reported contributions to the social security system for health care and pensions. I restrict the sample to individuals aged 18–64 and, for the wage analysis, to workers with positive labor income.

The GEIH is not designed to be representative at the municipality level for all Colombian municipalities. Its sampling design provides representative estimates for the municipalities comprising the 23 main cities and metropolitan areas, as well as at the department level, so municipality-level estimates outside these domains are measured less precisely, particularly in smaller and more remote municipalities. Because sampling error in the dependent vari-

[10] Monetary variables, including tax revenues and public investment, are deflated to constant 2010 pesos using the national Consumer Price Index (CPI).

able of a difference-in-differences specification inflates standard errors rather than biasing the coefficient, the concern is primarily one of statistical power, and I address it in three ways. First, I re-estimate the main specifications on the geographic domains for which the survey is representative—the 23 main cities and metropolitan areas, and departments—and report the minimum detectable effect alongside each estimate, so that results that are precisely estimated null can be distinguished from results that are simply imprecise. Second, I aggregate municipalities into functional urban areas, which pool the survey observations underlying each cell and thereby raises precision, and which correspond more closely to the commuting zone within which a local labor market clears. Third, as an alternative measure of informality that does not depend on the GEIH sampling design, I use the *Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales* (SISBEN), an administrative registry covering low-income and vulnerable households throughout the country, and construct the share of the municipal population registered in it. [11]

Firms. To examine how firms adjust to FEZ entry, I combine three complementary sources of firm-level data. First, I use administrative balance-sheet and income-statement data from the Superintendencia de Sociedades to compare firms that enter FEZs with firms operating outside the zones. During the period of analysis, reporting to this agency was mandatory for firms with annual sales or assets exceeding 20,000 times the legal monthly minimum wage (approximately USD 4.1 million), together with firms subject to specific monitoring or supervision requirements. The dataset is therefore concentrated among larger formal firms. Although these firms are a small fraction of registered businesses, they account for the large majority of formal corporate assets and revenues in Colombia, and this concentration is particularly relevant in the present setting because firms entering FEZs are drawn from the same part of the firm-size distribution. The data provide detailed information on sales, assets, liabilities, equity, profits, wage bills, and investment, allowing me to examine how firms adjust after entering the FEZ regime. Of the 486 firms entering FEZs during the period of analysis, 282 (58 percent) can be matched to the Superintendencia data. Matched entrants are large even relative to this already-truncated universe: they sit at the 77th percentile of its asset distribution, and 47 percent fall in its top decile. [12]

Second, I use the *Encuesta Anual Manufacturera* (EAM), the annual manufacturing census collected by the National Statistics Office (DANE), to examine adjustment among manufacturing establishments. The EAM covers the universe of manufacturing establishments belonging to firms with at least one plant employing ten or more workers or whose annual production value exceeds a threshold close to USD 100,000. The census spans 2001–2022 and includes between 4,000 and 5,000 establishments per year, accounting for over 90 percent of Colombia's formal manufacturing production. Each establishment is assigned a unique identifier that allows it to be followed over time independently of ownership changes. The data report detailed information

[11] Because SISBEN eligibility is based on socioeconomic vulnerability rather than employment status, this measure does not identify informal workers directly; it is also affected by the timing of national recertification campaigns, which shift enrollment for administrative reasons. I therefore treat it as corroborating rather than primary evidence.

[12] The balance-sheet results should accordingly be read as describing adjustment among the larger beneficiaries rather than among FEZ entrants as a whole.

on output, sales, employment, wages, investment, and input use, allowing me to examine employment and production responses among incumbent manufacturing establishments.

Finally, to study firm entry and exit across the broader formal sector, I use microdata from the *Registro Único Empresarial y Social* (RUES), maintained by the Colombian Confederation of Chambers of Commerce (Confecámaras). By law, all firms must obtain a commercial license (*matrícula mercantil*) within one month of beginning operations and renew it annually by March 31. RUES combines the commercial registries maintained by Colombia's 57 local chambers of commerce and records each firm's original registration date, including for firms established before the registries were centralized nationally in 2011. I use this information to recover firm entry dates, including for firms established prior to 2011, while annual registration and renewal records allow me to identify whether firms remain active and when they exit. The registry also provides precise geographic identifiers, allowing me to link firms to municipalities and FEZ exposure. RUES covers the universe of registered formal firms and therefore complements the other firm-level sources by allowing me to examine entry and exit across the firm-size distribution, including smaller firms that are largely absent from the Superintendencia data.[13]

Matched employer–employee records. I complement these data with the *Planilla Integrada de Liquidación de Aportes* (PILA), an administrative dataset from the Colombian social security system managed by the Ministry of Health and Social Protection, covering 2008–2016. PILA records the universe of formal workers in tax-registered firms, together with formal self-employed contributors. Each observation corresponds to a worker–employer match in a given month and reports the worker's earnings base, sex, age, municipality, employment type, and anonymized worker and firm identifiers. These identifiers allow me to follow workers and firms over time, observe movements of workers across formal employers, and identify firm entry and exit from formal employment. I use these data to examine how employment is reallocated across firms and workers following FEZ entry.[14]

Descriptive Facts. The descriptive evidence characterizes the local economies in which FEZs expand, focusing on the composition of local labor markets and the firms that participate in the regime. Municipalities that subsequently host an FEZ are larger and more economically active than those that do not: before the expansion of the regime, they are more densely populated, have higher fiscal revenues and average wages, employ a larger share of high-skilled workers, and are better connected to major cities and departmental capitals (Appendix Table A.5). FEZs therefore tend to locate in relatively large, economically active, and well-connected municipalities.

[13] Renewal fees range from approximately USD 13 to USD 638 depending on assets, and non-compliance can result in sanctions or closure (Guarin et al., 2023). Colombia's mandatory annual renewal system is unusual in Latin America (Salazar et al., 2017), and holding an active license is commonly used as an indicator of formality (Cárdenas and M, 2007; Galiani et al., 2016; Echeverry, 2010).

[14] Firm identities are anonymized, so I cannot identify which employers are FEZ beneficiaries. Similarly, workers are only observed while contributing to the social security system, so when they leave formal employment I cannot distinguish whether they move into informal employment, unemployment, or inactivity. PILA therefore allows me to characterize adjustment within the formal sector, including worker reallocation and firm exit, complementing the broader labor-market outcomes observed in the GEIH.

Despite these differences, informality remains an important feature of the labor markets in which FEZs operate, but it is concentrated in particular segments of those markets. Formality rises sharply with worker education: 6.4 percent of employed workers with no formal education hold formal jobs, compared with 41.4 percent of high school graduates and 69.0 percent of workers with post-secondary education (Appendix Table A.6). It also rises sharply with employer size, from 8.5 percent among the self-employed to 93.5 percent in the largest establishments identified in the survey. Establishments with fifty-one workers or more account for 62.4 percent of formal employment, while the self-employed and workers in the smallest establishments account for only 21.6 percent, despite representing 58 percent of employment overall (Appendix Table A.7). Thus, although these municipalities contain a large pool of workers outside formal employment, that pool is concentrated among less-educated workers and workers attached to very small establishments, rather than among workers resembling those employed by large formal firms.

FEZ beneficiaries are drawn precisely from this latter segment. The Superintendencia de Sociedades panel itself contains virtually no small firms: under the asset bands defined by Ley 905 de 2004, 99 percent of firm-years are classified as medium or large, reflecting the reporting threshold rather than sampling. Within this already selected universe, firms that subsequently enter the FEZ regime are larger still. In the year immediately preceding entry, they report sales roughly seven times, total assets roughly five times, and net fixed capital roughly five times those of non-participating firms observed in the same calendar years. Moreover, 91 percent of their firm-years fall in the largest size class, compared with 70 percent among other firms (Appendix Tables A.9). The firms targeted by the regime are therefore drawn from the upper tail of the formal firm distribution, while the municipality's large informal workforce is concentrated at the opposite end of the worker and firm distributions. High informality may consequently coexist with limited labor availability at the margin relevant for the firms induced to expand by the policy.

3 Conceptual Framework

This section develops a simple equilibrium framework to characterize how the introduction of Free Economic Zones (FEZs) affects firms and workers in a local labor market with informality. The framework builds on Ulyssea (2018), in which heterogeneous firms sort between formal and informal production, and extends it to incorporate worker skills and FEZ participation as a third firm status. Although FEZs are a place-based policy, I deliberately abstract from their spatial dimension and model participation as a firm status rather than a location. The place-based dimension enters through the local labor market in which FEZ and non-FEZ firms compete for workers. The framework therefore isolates the labor-market adjustment to FEZ entry, abstracting from agglomeration economies, land-market effects, and firm relocation across municipalities. Appendix C provides the derivations.

3.1 Environment

There is a single homogeneous final good whose price is normalized to one. Product and labor markets are competitive, and firms of all statuses face the same output price.

Workers. There are two types of workers, high skill ($k = 1$) and low skill ($k = 2$), supplied in fixed quantities,

$$L_k^s = \bar{L}_k, \qquad k = 1, 2. \tag{1}$$

Workers cannot change skill type over the horizon considered, and the local labor market is closed. Conditional on skill, workers earn a common equilibrium wage w_k across firms. I allow low-skill labor to be abundant relative to high-skill labor, $\bar{L}_2 \gg \bar{L}_1$.

Firms. Firms operate in one of three statuses, $s \in \{i, f, z\}$: *informal* firms are not registered; *ordinary formal* firms are registered and comply with tax and labor legislation; and *FEZ* firms are registered firms that operate under the preferential regime. FEZ firms are formal in every respect that matters for the labor market: they hire in the same market, pay the same wages, and remit the same payroll contributions. A firm with productivity θ in status s produces

$$y = \theta\, \ell_s^{\alpha}, \qquad \ell_s = \left(\eta_s l_1^{\rho} + (1 - \eta_s) l_2^{\rho}\right)^{1/\rho}, \qquad \alpha < 1, \tag{2}$$

where $\sigma \equiv 1/(1 - \rho)$ is the elasticity of substitution between skill types. Decreasing returns bound firm size.

Every period there is a large mass M of potential entrants. Each observes a noisy signal of its effective productivity, $\nu \sim G$. To enter, a firm must pay a sunk cost, with $E_z > E_f > E_i$: the difference $E_f - E_i$ reflects the costs of registration, and $E_z - E_f$ the investment commitments, infrastructure requirements, and administrative costs of the FEZ regime. After entry the firm draws its actual productivity from $F(\theta \mid \nu)$, which is the same in all statuses, increasing in θ and decreasing in ν, so that a better signal implies a stochastically better draw.[15] Once realized, θ remains constant; firms exit exogenously at rate κ_s, and immediately if $\pi_s(\theta, \mathbf{w}) < 0$, so firm values are $V_s(\theta, \mathbf{w}) = \max\{0, \pi_s/\kappa_s\}$.

Registration is a decision taken at entry, whereas FEZ status is adopted by established registered firms. I therefore treat FEZ adoption as a post-entry decision made by registered firms once θ is known, so that their continuation value is

$$\widetilde{V}_f(\theta, \mathbf{w}) = \max\left\{0,\ \frac{\pi_f(\theta, \mathbf{w})}{\kappa_f},\ \frac{\pi_z(\theta, \mathbf{w})}{\kappa_z} - (E_z - E_f)\right\}. \tag{3}$$

Letting $V_s^e(\nu, \mathbf{w})$ denote the expected value of entry into sector s, entry into the formal sector

[15] Because status is chosen before productivity is realized, the model generates overlapping productivity distributions across statuses, as observed in the data, rather than the perfect sorting implied by fully static models.

occurs if $V_f^e - E_f \geq \max\{V_i^e - E_i, 0\}$, so that when both sectors receive entrants

$$V_i^e(\bar{\nu}_i, \mathbf{w}) = E_i, \qquad V_f^e(\bar{\nu}_f, \mathbf{w}) = V_i^e(\bar{\nu}_f, \mathbf{w}) + (E_f - E_i). \tag{4}$$

Since the advantage of registering is increasing in θ (see Appendix C.4) and $F(\theta \mid \nu)$ is ordered by first-order stochastic dominance, $V_f^e - V_i^e$ is increasing in ν: firms with better signals register, and less promising firms enter informally.

The three statuses also differ in the composition of labor they employ, which will be central to the equilibrium response to FEZ adoption.

Assumption 1 (Skill intensity). $\eta_z \geq \eta_f > \eta_i$.

The second inequality follows Ulyssea (2018) and captures the greater concentration of low-skill workers in informal firms. The first allows FEZ firms to be at least as skill-intensive as ordinary formal firms, consistent with their observed skill composition in the data. Together with the fixed endowments in (1), this separates labor abundance from skill availability: low-skill labor may be plentiful, $\bar{L}_2 \gg \bar{L}_1$, while the firms attracted by the regime are intensive in the relatively scarce factor.[16]

The government levies a payroll tax τ_w and a profit tax t_s on registered firms. Registered firms also face a status-specific operating wedge τ_s, capturing administrative and procedural costs associated with each regime. Registration and labor legislation are imperfectly enforced, and net tax revenue is rebated to the household as a lump-sum transfer.[17] Letting $\tilde{w}_k \equiv (1 + \tau_w)w_k$ denote the effective wage paid by registered firms, gross profits are

$$\Pi_i(\theta, \mathbf{w}) = \max_{l_1, l_2} \left\{ \theta \ell_i^\alpha - \tau_i(\ell_i)(w_1 l_1 + w_2 l_2) \right\}, \tag{5}$$

$$\Pi_s(\theta, \mathbf{w}) = \max_{l_1, l_2} \left\{ (1 - \tau_s)\theta \ell_s^\alpha - \tilde{w}_1 l_1 - \tilde{w}_2 l_2 \right\}, \qquad s = f, z, \tag{6}$$

where informal firms avoid taxes and regulation but face an expected detection cost, $\tau_i(\ell_i)$, that is increasing and convex in firm size ($\tau_i' > 0$, $\tau_i'' > 0$), reflecting the greater visibility of larger firms to enforcement authorities. The size dependence of this cost generates sorting between formal and informal production: informality is relatively cheaper for small, low-productivity firms, while its cost rises with firm size, increasing the relative advantage of formal operation.[18] All firms also incur a status-specific fixed operating cost $\bar{c}_s$. Net profits are therefore

$$\pi_s(\theta, \mathbf{w}) = (1 - t_s)\Pi_s(\theta, \mathbf{w}) - \bar{c}_s. \tag{7}$$

FEZ firms face the same payroll costs as ordinary formal firms, but benefit from $t_z < t_f$

[16] With fixed endowments, this scarcity is accommodated through wages and the reallocation of workers across firms and sectors, rather than through changes in aggregate labor supply.

[17] As in Ulyssea (2018), imperfect enforcement allows firms to operate informally. The expected costs associated with informality represent resource losses rather than government revenue.

[18] Without a size-dependent cost, the formal–informal sorting margin would degenerate. The convex detection cost generates the concentration of informality among small, low-productivity firms documented across developing economies (La Porta and Shleifer, 2014; Ulyssea, 2018), a pattern also observed in this paper.

and $\tau_z < \tau_f$. The first captures the preferential corporate income tax rate, while the second captures the simplified administrative and customs procedures associated with FEZ status. These advantages operate through different margins. Under full deductibility, the preferential profit tax increases the value of FEZ status without affecting conditional input choices. The lower operating wedge instead raises the effective return to production and therefore affects firm scale. The model thus separates the incentives governing FEZ adoption from those governing expansion.

Equilibrium. I focus on stationary equilibria. With $M_i = [G(\bar{v}_f) - G(\bar{v}_i)]M$ and $M_f = [1 - G(\bar{v}_f)]M$ denoting the measures of entrants into each sector, and μ_s the mass of active firms in status s, stationarity requires that successful entry offsets firm exit,

$$\mu_s = \frac{1 - F_{\theta_s}(\bar{\theta}_s)}{\kappa_s} M_s. \tag{8}$$

> **Definition.** A stationary equilibrium is a wage vector $\mathbf{w} = (w_1, w_2)$, cutoffs $(\bar{v}_i, \bar{v}_f, \bar{\theta}_i, \bar{\theta}_f, \theta_z)$, masses (μ_i, μ_f, μ_z), and a transfer T such that: (i) firms choose inputs and status optimally, given (5)–(7) and (3); (ii) $\pi_s(\bar{\theta}_s, \mathbf{w}) = 0$ in every status; (iii) the free-entry conditions (4) hold, with equality whenever $M_s > 0$; (iv) each sector's size is constant, as in (8); (v) both labor markets clear, $\sum_s \int l_k^s \, \mathrm{d}\mu_s = \bar{L}_k$ for $k = 1, 2$; and (vi) the government budget balances.

The equilibrium features positive selection across both margins. Firms with better ex-ante productivity signals are more likely to register formally, while among registered firms, sufficiently productive firms adopt the FEZ regime according to the productivity cutoff θ_z.

3.2 Firm decisions

Input choice and profits. For formal firms, homotheticity of the labor aggregate allows the input problem to be solved in two steps. Minimizing $\tilde{w}_1 l_1 + \tilde{w}_2 l_2$ subject to $\ell_s = 1$ gives the unit labor cost index and conditional factor demands (Appendix C.1),

$$c_s(\mathbf{w}) = \left[\eta_s^\sigma \tilde{w}_1^{1-\sigma} + (1 - \eta_s)^\sigma \tilde{w}_2^{1-\sigma}\right]^{\frac{1}{1-\sigma}}, \qquad l_k = \ell_s \, \varsigma_{ks} \left(\frac{c_s}{\tilde{w}_k}\right)^\sigma, \qquad s = f, z, \tag{9}$$

where $\varsigma_{1s} = \eta_s^\sigma$ and $\varsigma_{2s} = (1 - \eta_s)^\sigma$. The associated labor cost shares are symmetric, $s_k^s = \varsigma_{ks} \tilde{w}_k^{1-\sigma} c_s^{\sigma-1}$ for $k = 1, 2$, and sum to one by the definition of c_s; their ratio, $s_1^s / s_2^s = [\eta_s / (1 - \eta_s)]^\sigma (\tilde{w}_1 / \tilde{w}_2)^{1-\sigma}$, is free of c_s and increasing in η_s, so more skill-intensive statuses devote a larger share of costs to skilled labor.

Given $c_s(\mathbf{w})$, the firm chooses scale by solving $\max_\ell \{(1 - \tau_s)\theta \ell^\alpha - c_s \ell\}$, which yields

$$\ell_s^* = \left[\frac{\alpha(1 - \tau_s)\theta}{c_s}\right]^{\frac{1}{1-\alpha}}$$

and operating profits

$$\Pi_s(\theta, \mathbf{w}) = \kappa_0 \left[(1-\tau_s)\theta\right]^{\frac{1}{1-\alpha}} c_s(\mathbf{w})^{-\frac{\alpha}{1-\alpha}}, \qquad \kappa_0 = (1-\alpha)\alpha^{\frac{\alpha}{1-\alpha}}, \qquad s = f, z. \tag{10}$$

Because the expected cost of informality $\tau_i(\ell_i)$ varies with firm size, the informal problem does not admit the same closed-form solution. Following Ulyssea (2018), I require only that the relative advantage of formal operation, $\Pi_f - \Pi_i$, increases with productivity. This generates sorting along the informal–formal margin: as firms become larger and more productive, the expected cost of remaining unregistered increases relative to the cost of formal operation.

Selection into the regime. The productivity term $\theta^{1/(1-\alpha)}$ is common to both registered statuses. Differencing (7) across ordinary formal and FEZs gives

$$\pi_z(\theta, \mathbf{w}) - \pi_f(\theta, \mathbf{w}) = \Gamma\, \theta^{\frac{1}{1-\alpha}} - \Delta\bar{c}, \qquad \Delta\bar{c} \equiv \bar{c}_z - \bar{c}_f, \tag{11}$$

where

$$\Gamma = \kappa_0 \left[(1-t_z)(1-\tau_z)^{\frac{1}{1-\alpha}} c_z^{-\frac{\alpha}{1-\alpha}} - (1-t_f)(1-\tau_f)^{\frac{1}{1-\alpha}} c_f^{-\frac{\alpha}{1-\alpha}}\right]. \tag{12}$$

The FEZ operating advantage can be summarized by the proportional premium

$$\Psi \equiv \frac{1-t_z}{1-t_f}\left(\frac{1-\tau_z}{1-\tau_f}\right)^{\frac{1}{1-\alpha}}\left(\frac{c_f}{c_z}\right)^{\frac{\alpha}{1-\alpha}}, \qquad \Gamma\theta^{\frac{1}{1-\alpha}} = (\Psi - 1)(1-t_f)\Pi_f(\theta, \mathbf{w}). \tag{13}$$

Thus, Ψ measures the proportional gain from operating under FEZ rather than ordinary formal status. Conditional on wages, it is independent of productivity, although the gain in levels increases with θ. The premium can be decomposed as

$$\ln \Psi = \underbrace{\ln \frac{1-t_z}{1-t_f}}_{\text{profit-tax gain}} + \underbrace{\frac{1}{1-\alpha} \ln \frac{1-\tau_z}{1-\tau_f}}_{\text{operating-cost gain}} - \underbrace{\frac{\alpha}{1-\alpha} \ln \frac{c_z}{c_f}}_{\text{skill-cost term}}. \tag{14}$$

The first two terms capture the advantages of the FEZ regime, while the last reflects differences in unit labor costs and vanishes when $\eta_z = \eta_f$. I assume $\Psi > 1$, so that FEZ status offers an operating advantage over ordinary formal status.[19] This operating advantage has two implications for firm behavior. It determines which registered firms find it profitable to adopt the regime and, conditional on adoption, how their scale responds.

Proposition 1 (Selection). *Suppose $\Psi > 1$ and $\kappa_f = \kappa_z = \kappa$. Then $\pi_z - \pi_f$ is strictly increasing in θ, and there is a unique adoption cutoff*

$$\theta_z = \left[\frac{\kappa(E_z - E_f) + \Delta\bar{c}}{\Gamma}\right]^{1-\alpha}, \tag{15}$$

such that a registered firm adopts FEZ status if and only if $\theta \geq \theta_z$. Equivalently, adoption occurs when

[19] This condition is also consistent with observed participation in the regime. Since FEZ adoption entails an additional cost, $E_z > E_f$, a firm would choose to participate only if the operating gain were positive. In particular, observed adoption requires $\Gamma\theta^{1/(1-\alpha)} \geq \kappa(E_z - E_f) + \Delta\bar{c} > 0$, which implies $\Gamma > 0$ and therefore $\Psi > 1$.

the proportional operating gain covers the annuitized incremental cost,

$$(\Psi - 1)(1 - t_f)\Pi_f(\theta) \geq \kappa(E_z - E_f) + \Delta\bar{c}.$$

Moreover,

$$\frac{\mathrm{d}\ln\theta_z}{\mathrm{d}\ln(1-\tau_z)} = -\frac{\Psi}{\Psi - 1} < 0, \qquad \frac{\mathrm{d}\ln\theta_z}{\mathrm{d}\ln w_1} = \alpha\left[s_1^f + (s_1^z - s_1^f)\frac{\Psi}{\Psi - 1}\right] > 0,$$

and θ_z is increasing in $E_z - E_f$ and decreasing in $t_f - t_z$.

The selection mechanism follows the same logic as the informal–formal margin, now applied to FEZ participation. Because the regime combines a proportional operating gain with an additional fixed cost, participation becomes profitable only above a productivity threshold, generating positive selection of larger, more productive firms into FEZ status.

The response of the adoption cutoff to skilled wages introduces an equilibrium feedback. A higher w_1 reduces registered firms' profits and, when FEZ production is more skill-intensive ($s_1^z > s_1^f$), raises FEZ unit labor costs by more than those of ordinary formal firms, reducing Ψ. Skilled-wage pressure therefore raises the adoption threshold and partially erodes the initial advantage of the regime.

Proposition 2 (Expansion)**.** *Conditional on wages, a firm that adopts the FEZ regime changes its total labor input by*

$$\Delta\ln\ell = \frac{1}{1-\alpha}\left[\ln\frac{1-\tau_z}{1-\tau_f} - \ln\frac{c_z}{c_f}\right],$$

so it expands if and only if the operating advantage outweighs the difference in unit labor costs, $(1-\tau_z)/c_z > (1-\tau_f)/c_f$, and relative skill demand within the firm rises by $\ln(\lambda_z/\lambda_f) \geq 0$. When $\eta_z = \eta_f$, adoption raises employment of both skill types by $\frac{1}{1-\alpha}\ln\frac{1-\tau_z}{1-\tau_f} > 0$ and leaves the skill composition of employment unchanged.

Adoption changes both the wedge and the technology. The lower operating wedge raises the return to production and pushes the firm to expand, while the more skill-intensive technology can raise the unit cost of the labor bundle and work against it. The profit tax affects neither margin, so a firm may find adoption profitable without expanding: observed expansion of beneficiaries is evidence of the operating advantage of the regime, not of its tax gain alone.[20] Under Assumption 1, adoption therefore raises the skill intensity of aggregate labor demand through two reinforcing margins: the reallocation of activity toward more skill-intensive firms and, when $\eta_z > \eta_f$, the shift toward skilled workers within adopting firms. How this additional labor demand is accommodated—through wages, contraction or exit of other firms, or reallocation between formal and informal employment—is determined in equilibrium.

[20] Under full deductibility, the profit tax scales after-tax profits without changing the inputs that maximize them, so $\Psi > 1$ does not imply $\Delta\ln\ell > 0$. Because θ cancels from the comparison, all adopters change their scale by the same proportion.

3.3 Equilibrium adjustment

Throughout this subsection, I assume $\sigma < 1/(1-\alpha)$, so that high- and low-skill labor are gross complements in firm-level demand: an increase in the wage of either skill type reduces demand for both (see Appendix C.6).[21]

Labor-market clearing. From (9), relative skill demand within a firm is

$$\frac{l_1}{l_2} = \lambda_s \left(\frac{w_2}{w_1}\right)^{\sigma}, \qquad \lambda_s \equiv \left(\frac{\eta_s}{1-\eta_s}\right)^{\sigma}, \tag{16}$$

where the common payroll-tax factor cancels. Relative skill demand varies across statuses only through λ_s, and Assumption 1 implies $\lambda_z \geq \lambda_f > \lambda_i$. Aggregating across firms gives

$$\frac{L_1}{L_2} = \bar{\lambda} \left(\frac{w_2}{w_1}\right)^{\sigma}, \qquad \bar{\lambda} \equiv \sum_s \omega_s \lambda_s, \tag{17}$$

where ω_s denotes status s's share of low-skill employment. Since the supplies of both skill types are fixed, labor-market clearing requires

$$\bar{\lambda} \left(\frac{w_2}{w_1}\right)^{\sigma} = \frac{\bar{L}_1}{\bar{L}_2}. \tag{18}$$

Thus, changes in the composition of employment across firm types must be accommodated by changes in relative wages.

Relative wages. The clearing condition holds both before and after the reform, so the change in the skill premium is pinned down exactly:

$$\Delta \ln \frac{w_1}{w_2} = \frac{\Delta \ln \bar{\lambda}}{\sigma}. \tag{19}$$

With fixed relative labor supply, any shift in the skill composition of labor demand translates directly into a change in relative wages.

Proposition 3 (Skilled-wage pressure)**.** *The FEZ regime raises the skill premium if and only if it increases the skill intensity of aggregate labor demand:*

$$\Delta\bar{\lambda} = \sum_s \lambda_s \, \Delta\omega_s > 0.$$

Equivalently, since $\sum_s \Delta\omega_s = 0$, this condition can be written as

$$(\lambda_z - \lambda_f)\Delta\omega_z > (\lambda_f - \lambda_i)\Delta\omega_i.$$

Given $\lambda_z \geq \lambda_f > \lambda_i$, the skill premium rises when the reallocation of low-skill employment toward FEZ firms dominates, in λ-weighted terms, the reallocation toward informal firms.

[21] This condition is consistent with the estimates in Ulyssea (2018).

The proposition captures the two opposing compositional forces at the center of the framework. The expansion of FEZ firms shifts employment toward a more skill-intensive status: each low-skill worker reallocated from an ordinary formal firm to an FEZ firm raises the skill intensity of aggregate labor demand by $\lambda_z - \lambda_f$. At the same time, the equilibrium adjustment may push low-skill employment out of ordinary formal firms and toward informality, working in the opposite direction: each low-skill worker reallocated from ordinary formal to informal employment lowers aggregate skill intensity by $\lambda_f - \lambda_i$.[22] With fixed skill supplies, which of these two forces dominates determines the direction of the relative wage response.

Corollary 3.1 (Aggregate labor abundance)**.** *Conditional on a given shift in the skill composition of labor demand, $\Delta \ln \bar{\lambda}$, the relative-wage response depends on the substitutability between skill types, σ, but not directly on the level of informality or the size of the low-skill labor reserve. As $\sigma \to \infty$, $\Delta \ln(w_1/w_2) \to 0$, and the wage-mediated reallocation channel vanishes.*

Corollary 3.1 highlights the distinction between aggregate labor abundance and the availability of the skills demanded by expanding firms. A large reserve of low-skill workers does not by itself relieve pressure on skilled wages: it does so only to the extent that low- and high-skill workers are substitutable in production. When substitution is limited, the expansion of skill-intensive firms can therefore generate skilled-wage pressure even in a labor market with substantial aggregate slack.

Incumbent response. With inelastic labor supply, the expansion of beneficiaries must be accommodated through wages and the reallocation of existing workers: there are no additional workers to hire.

Proposition 4 (Contraction and exit)**.** *Suppose the reform shifts relative labor demand toward skill, $\Delta\bar{\lambda} > 0$. Then, in the new equilibrium:*

(i) the skilled wage and the unit labor cost of ordinary formal incumbents rise, $\mathrm{d}\ln w_1 > 0$ and $\mathrm{d}\ln c_f = s_1^f \mathrm{d}\ln w_1 + s_2^f \mathrm{d}\ln w_2 > 0$ (Lemma 1, Appendix C.7); no additional condition is required;

(ii) every ordinary formal incumbent's operating profit falls, $\mathrm{d}\ln \Pi_f = -\frac{\alpha}{1-\alpha}\mathrm{d}\ln c_f < 0$;

(iii) holding w_2 fixed, incumbents reduce demand for both skill types: $\partial \ln l_2^f / \partial \ln w_1 = s_1^f[\sigma - \frac{1}{1-\alpha}] < 0$ (Appendix C.6 reports the full differentials);

(iv) the zero-profit cutoff rises, $\mathrm{d}\ln \bar{\theta}_f = \alpha\, \mathrm{d}\ln c_f > 0$, so firms with $\theta \in [\bar{\theta}_f^0, \bar{\theta}_f^1)$ exit. Since θ maps monotonically into firm size, exit is concentrated among the smallest formal firms and, by (8), the mass of active formal firms falls for any given flow of entrants.

[22] Reallocation toward FEZ firms depends on the change in scale and skill composition of adopting firms upon switching status (Proposition 2) and on the mass of registered firms above the adoption cutoff θ_z. Reallocation toward informality is determined endogenously by the response of incumbent firms and workers, characterized in Propositions 4 and 5. The condition is therefore stated in terms of equilibrium employment shares rather than primitives.

Part (i) is where fixed labor supply does the key work. If incumbents' labor costs did not rise, aggregate low-skill labor demand would weakly increase: surviving incumbents would demand more as their costs fell, adopters expand by Proposition 2, informal firms respond to the lower low-skill wage, and higher entry values encourage entry. This is inconsistent with the fixed supply $\bar{L}_2$, so equilibrium adjustment must raise incumbents' unit labor costs (Lemma 1). Part (iii) shows that the resulting contraction is not confined to skilled labor. A rise in the skilled wage also reduces incumbents' demand for low-skill workers because, under $\sigma < 1/(1-\alpha)$, the contraction in scale dominates substitution toward low-skill labor. Displacement can therefore extend beyond the workers whose wage rises.

Proposition 5 (Informality and reallocation)**.** *Suppose $\Delta\bar{\lambda} > 0$. With $\bar{L}_2$ fixed, the change in informal low-skill employment is exactly the mirror image of the change in registered low-skill employment, $\Delta L_2^i = -\Delta\left(L_2^z + L_2^f\right)$. Decomposing by firm group, the share of low-skill workers employed informally rises if and only if*

$$\underbrace{\Delta L_2^z}_{\text{adopters, net}} + \underbrace{\Delta L_2^{f,\text{intensive}}}_{<0} + \underbrace{\Delta L_2^{f,\text{exit}}}_{<0} < 0, \tag{20}$$

where ΔL_2^z is the change in low-skill employment at adopting firms net of their pre-reform employment as ordinary formal firms, positive when adopters expand (Proposition 2); $\Delta L_2^{f,\text{intensive}}$ is the change at firms remaining ordinary formal, negative by Proposition 4(iii); and $\Delta L_2^{f,\text{exit}}$ is the employment lost through exit, negative by Proposition 4(iv).[23] *If (20) holds, the low-skill wage falls, $\mathrm{d}\ln w_2 < 0$ (Appendix C.7). Thus, an increase in low-skill informality is accompanied by a deterioration in the relative price of low-skill labor.*

Condition (20) makes clear that an increase in informality is possible but not necessary. FEZ firms absorb low-skill workers when their expansion outweighs the shift toward skill within the regime (Proposition 2); informality rises when this net absorption fails to offset the contraction and exit of ordinary formal firms. The direction of the effect therefore depends on the strength of the equilibrium reallocation. Informality is more likely to rise when substitution between skill types is limited, FEZ production is more skill intensive relative to the activity it displaces—so that adopters re-absorb few of the low-skill workers released—and a larger mass of ordinary formal firms lies close to the zero-profit cutoff. Whether these forces are strong enough for (20) to hold is ultimately a quantitative question, which I leave to the empirical analysis.

Sorting margins. The equilibrium adjustment also changes the selection of firms across statuses. While the increase in c_f raises the survival threshold of ordinary formal firms, the fall in the low-skill wage can improve the relative position of informal firms. Despite the size-dependent cost of informality, an informal firm's profitability depends on wages through its unit labor cost index c_i (Appendix C.8), so the relevant wage effect is summarized by

$$\mathrm{d}\ln c_i = s_1^i \mathrm{d}\ln w_1 + s_2^i \mathrm{d}\ln w_2.$$

[23] The intensive-margin sign holds to first order: the rise in c_f contracts low-skill demand under gross complements, while the induced fall in w_2 partially offsets it. The exit term also includes the reduction in entry into formal status.

Proposition 6 (Firm sorting). *Suppose* $\Delta\bar{\lambda} > 0$ *and condition* (20) *holds, so that* $\mathrm{d}\ln w_1 > 0 > \mathrm{d}\ln w_2$ *(Propositions 4–5), and that informal production is sufficiently low-skill intensive for its unit labor cost to fall,*

$$s_1^i \mathrm{d}\ln w_1 + s_2^i \mathrm{d}\ln w_2 < 0.$$

Then:

(i) the informal survival cutoff falls, $\mathrm{d}\ln \bar{\theta}_i < 0$*, while the ordinary formal survival cutoff rises by Proposition 4;*

(ii) the value of informal entry increases, $V_i^e(\nu, \mathbf{w})$ *rises for every* ν*, making informal entry more attractive;*

(iii) these survival and entry responses reinforce the reallocation of activity toward the informal sector.

The firm response is therefore asymmetric across the two sectors. Higher labor costs reduce the profitability of ordinary formal firms and push marginal firms out, while lower informal labor costs improve the survival and entry incentives of informal firms. The same wage adjustment that reallocates low-skill workers toward informality can therefore also reallocate firms toward the informal sector. This mechanism is strongest among firms close to the formal–informal margin, while the most productive registered firms remain those most likely to benefit from FEZ participation.

Scope of the framework. The framework identifies the conditions governing the direction of the equilibrium response rather than imposing it. The policy induces sufficiently productive registered firms to adopt and expand; whether this expansion contracts ordinary formal firms and pushes low-skill employment toward informality, or instead draws workers out of informality, depends on the skill intensity of FEZ production, the substitutability between skill types, and the distribution of incumbent firms around the survival margin. The empirical analysis that follows examines these margins and determines which of these forces dominates in the Colombian setting.

Four limitations are worth noting. First, the local labor market is closed, and labor supply is fixed. Allowing workers, particularly skilled workers, to migrate into treated areas would make labor supply more elastic and attenuate the wage and reallocation responses (Appendix C.10). Second, the model abstracts from unemployment, non-participation, and search frictions. Informality is therefore the only employment margin outside the registered sector, and total employment is fixed by construction. Third, I abstract from the intensive margin of informality—registered firms hiring workers informally—which is central in Ulyssea (2018). Allowing for this margin would provide an additional channel through which low-skill workers can move into informal employment (Appendix C.11). Finally, the empirical analysis observes firm expansion, incumbent adjustment, and worker reallocation in different datasets and at different levels of aggregation. The framework shows how these outcomes can arise jointly in equilibrium, but it does not imply that individual transitions between firms or employment statuses are directly observed.

4 Research Design

This section describes the empirical strategy used to examine the margins highlighted by the conceptual framework. I exploit the staggered rollout of Free Economic Zones (FEZs) in a difference-in-differences framework. The identifying logic is common across the different exercises, while the treatment and corresponding estimand vary depending on whether I study local FEZ exposure or firms that directly receive FEZ benefits.

4.1 Treatment and Econometric Framework

The empirical analysis considers two levels of treatment. For local economic outcomes, as well as for workers and firms operating in hosting municipalities, treatment is defined at the municipality level and begins in the year in which the first FEZ becomes operational. This captures the effect of local exposure to the policy—analogous to an intention-to-treat effect—since only a subset of firms within a hosting municipality directly receives FEZ benefits. For firms that can be directly identified as FEZ beneficiaries, treatment is instead defined at the firm level and begins in the year in which the firm obtains FEZ status. I define the two treatments as

$$FEZ_{mt} = \mathbf{1}\{t \geq T_m\}, \qquad FEZ_{ft} = \mathbf{1}\{t \geq T_f\}, \tag{21}$$

where T_m denotes the year in which the first FEZ in municipality m becomes operational, and T_f denotes the year in which firm f obtains FEZ status and becomes eligible for the associated tax and customs benefits.

Given the staggered adoption of these treatments, all empirical exercises rely on a difference-in-differences framework. The exact specification varies with the unit of analysis and the structure of each dataset, but the underlying comparison is the same: changes in outcomes around FEZ exposure across units treated at different points in time. For municipality-level treatment, municipalities that have not yet received a FEZ serve as controls until treatment, together with municipalities that never receive one during the sample period. The same treatment variation underlies the municipality-, worker-, and local firm-level analyses. For direct beneficiaries, the comparison is instead between firms that obtain FEZ status at different points in time and firms that remain outside the regime.

4.2 Identification

The main empirical challenge is selection into FEZ treatment. FEZs are not randomly allocated across municipalities, and firms that obtain FEZ status are not randomly selected. Municipalities that host FEZs may differ systematically from those that do not in characteristics that also shape economic outcomes, such as market access, infrastructure, land availability, and pre-existing economic activity. Similarly, beneficiary firms may differ from non-beneficiary firms even before entering the regime. Identification therefore relies on the assumption that, absent FEZ treatment, treated and comparison units would have followed parallel trends in the outcomes of interest,

conditional on the fixed effects and pre-treatment characteristics included in each specification. This assumption allows treated and comparison units to differ in levels before treatment, but requires that these differences do not generate differential changes in outcomes around FEZ exposure.

I implement the difference-in-differences design using the doubly robust estimator of Callaway and Sant'Anna (2021). The estimator constructs group-time average treatment effects by comparing each treatment cohort with units that are untreated at that point in time, allowing treatment effects to vary across cohorts and over time. I aggregate these estimates to report both average treatment effects and event-study coefficients around FEZ exposure. This avoids the contamination that can arise in conventional two-way fixed-effects estimators when already-treated units serve as controls and treatment effects are heterogeneous (Sun and Abraham, 2021).

For municipality-level exposure, selection may operate through both FEZ placement and the timing of operation. I examine these two margins directly. First, I test whether predetermined geographic, demographic, and economic characteristics predict whether a municipality ever receives a FEZ. Second, among municipalities that eventually receive a zone, I examine whether these characteristics predict when the first FEZ becomes operational. While placement and timing are related to characteristics such as population size and geographic location, I find little evidence that pre-policy economic and labor-market conditions systematically predict either margin of treatment (see Table B.12).

The institutional process also creates a distinction between FEZ placement and operation. Establishing a zone involves application, regulatory approval, investment, construction, and implementation, and declaration does not imply immediate operation. The time between declaration and effective operation varies substantially across zones (see Appendix B.9). The empirical design therefore exploits variation in the timing of operation rather than relying on cross-sectional differences between municipalities that do and do not host FEZs. I do not assume that operational timing is random; identification requires that, conditional on the controls included in the specification, it is not systematically related to counterfactual changes in local outcomes.

I account for remaining differences across municipalities using municipality fixed effects, which absorb permanent differences across locations, and department-by-year fixed effects, which absorb time-varying shocks common to municipalities within the same department. I further allow outcomes to evolve differently according to predetermined municipal characteristics associated with FEZ placement and timing, including distance to major cities and rurality. I assess the identifying assumption using event-study estimates and placebo tests to examine whether treated and comparison municipalities were already following differential trends before FEZ operation. Because the worker and local-firm analyses use the same municipality-level treatment, they rely on the same identifying variation and assumptions; the use of individual- and firm-level data changes the estimand, but not the source of treatment variation.

Two additional concerns arise with municipality-level exposure. First, local economic activity may adjust after a zone is announced or declared but before it becomes operational. I assess

such anticipation directly from the event-study coefficients in the years preceding operation. Second, FEZs may affect neighboring municipalities through commuting, supplier linkages, or local demand, contaminating the comparison group. I address this in two complementary ways. I first test for spatial spillovers directly, estimating effects on untreated municipalities by distance band from the nearest treated municipality, and re-estimate the main specifications excluding municipalities within alternative distances of treated areas, so that the comparison group contains only municipalities plausibly unaffected by the regime. I then redefine local labor markets as functional urban areas, which group municipalities according to observed commuting patterns and therefore internalize adjustment that crosses municipal boundaries.

For direct beneficiaries, the same identifying logic applies at the firm level, but treatment varies with the timing of FEZ certification. Firms that obtain FEZ status may differ systematically from firms that do not, so identification does not rely on random selection into the regime. Instead, it requires that, absent FEZ certification, beneficiary and comparison firms would have followed similar trends in the outcomes of interest. I compare beneficiary and non-beneficiary firms operating in the same municipality and detailed sector in a given year, include firm fixed effects and pre-treatment firm characteristics interacted with year indicators, and assess pre-treatment dynamics using firm-level event studies.

4.2.1 Local Labor Markets

The first set of exercises estimates the aggregate effect of local FEZ exposure on economic activity and labor-market outcomes. I consider outcomes including economic activity, employment, wages, informality, and fiscal revenues. I estimate group-time average treatment effects using the doubly robust difference-in-differences estimator of Callaway and Sant'Anna (2021), comparing municipalities according to the year in which their first FEZ becomes operational. I aggregate these effects by event time to trace the dynamic response before and after FEZ operation, using the year prior to treatment as the reference period.

For comparison, I also estimate the following two-way fixed-effects event-study specification:

$$Y_{mt} = \sum_{k \neq -1} \beta_k \mathbb{1}\{t - T_m = k\} + \delta_m + \lambda_t + \lambda_{dt} + \mathbf{X}_m' \theta_t + \varepsilon_{mt}, \tag{22}$$

where Y_{mt} is the outcome of municipality m in year t, δ_m and λ_t are municipality and year fixed effects, and λ_{dt} are department-by-year fixed effects. The vector $\mathbf{X}_m$ contains predetermined, pre-policy municipality characteristics interacted with year indicators, allowing outcomes to evolve differently with baseline conditions. The coefficients β_k trace the dynamic response around the year in which the first FEZ becomes operational. The corresponding static specification replaces the event-time indicators with FEZ_{mt}. Standard errors are clustered at the municipality level.

The resulting estimand captures the aggregate effect of local FEZ exposure. It combines the direct activity of firms operating under the regime with the response of workers and firms elsewhere in the local economy.

4.2.2 Workers

The second set of exercises examines how local FEZ exposure affects workers' labor-market outcomes and whether these effects differ across skill groups. I use the GEIH labor force survey and restrict the sample to urban workers aged 18–64. Treatment remains defined at the municipality level, so workers become exposed when the first FEZ in their municipality becomes operational.

The GEIH is a repeated cross-section, so I cannot follow the same individuals over time or observe transitions between firms or employment statuses. The analysis instead compares the outcomes of workers observed in treated and comparison municipalities before and after FEZ operation. I estimate

$$Y_{imt} = \beta FEZ_{mt} + \mathbf{X}'_{imt}\theta + \delta_m + \lambda_t + \varepsilon_{imt}, \tag{23}$$

where Y_{imt} denotes the outcome of individual i in municipality m and year t, including employment status, formality, and earnings. The vector $\mathbf{X}_{imt}$ contains individual characteristics, including gender, age, education, and marital status, while δ_m and λ_t are municipality and year fixed effects. I exclude characteristics that may themselves respond to treatment, such as sector of employment or migration status. Standard errors are clustered at the municipality level. Treatment varies at the municipality level; accordingly, β captures the effect of local FEZ exposure on workers rather than the effect of employment by a beneficiary firm.

To examine whether these effects differ across skill groups, I interact FEZ exposure with workers' educational attainment:

$$Y_{imt} = \beta FEZ_{mt} + \sum_s \gamma_s \left(FEZ_{mt} \times Skill_{is}\right) + \mathbf{X}'_{imt}\theta + \delta_m + \lambda_t + \varepsilon_{imt}, \tag{24}$$

where $Skill_{is}$ indicates medium- and high-skilled workers, with low-skilled workers as the omitted category. The coefficients γ_s capture the differential effect of FEZ exposure for each skill group relative to low-skilled workers. This specification follows the regional-shocks literature (Dix-Carneiro and Kovak, 2017; Kovak and Morrow, 2023) and allows me to examine how the incidence of local FEZ exposure varies across skill groups. Since the GEIH does not follow individuals over time, these estimates do not identify individual transitions between formal and informal employment or across firms.

4.2.3 Firms

The third set of exercises examines the effect of local FEZ exposure on firms operating in hosting municipalities. I use firm-level data from the *Encuesta Anual Manufacturera* (EAM, Annual Manufacturing Survey), covering 2001–2018, and restrict the estimation sample to firms already operating before 2005. The EAM does not identify whether a firm is an FEZ beneficiary. The estimated effect therefore combines the response of beneficiary firms included in the data with that of non-beneficiary firms operating in the same municipalities. I interpret these estimates

as the effect of local FEZ exposure on manufacturing firms, rather than as an indirect effect on non-beneficiary firms.

Formally, I estimate

$$Y_{fmt} = \beta FEZ_{mt} + \mathbf{X}_f'\theta_t + \chi_f + \lambda_{st} + \varepsilon_{fmt}, \tag{25}$$

where Y_{fmt} denotes the outcome of firm f in municipality m and year t, including employment, output, and exports; χ_f are firm fixed effects; and λ_{st} are 4-digit-sector-by-year fixed effects, which absorb shocks common to firms operating in the same detailed industry in a given year. The vector $\mathbf{X}_f$ contains predetermined firm characteristics, such as initial size, age, and capital intensity, interacted with year indicators. Standard errors are clustered at the municipality level, where treatment varies. The coefficient β captures the effect of local FEZ exposure on manufacturing firms in hosting municipalities.

To examine whether adjustment differs across firms, I allow the effect of FEZ exposure to vary with predetermined firm size and age. This heterogeneity is particularly relevant to the conceptual framework, in which the response to beneficiary expansion depends on firms' position relative to the contraction and exit margin. I complement the EAM with the RUES business registry, which covers the universe of registered firms and allows me to measure firm exit, and with PILA, which provides administrative information on formal employment and affiliations at the firm level over 2008–2016. Together, these data allow me to examine adjustment along both the intensive and extensive margins following local FEZ exposure.

FEZ-Adopting Firms The final set of exercises examines firms that directly obtain FEZ status. In this case, treatment is observed at the firm level, allowing me to estimate the response of firms that receive the policy benefits rather than the broader effect of local FEZ exposure. I focus on outcomes including investment and assets, inventories, sales, and labor expenditures.

I use annual financial reports from the *Superintendencia de Sociedades* (Super-Sociedades), which provide detailed balance sheets and income statements for medium and large corporations, and match them to the official registry of FEZ-certified firms compiled by the Ministry of Commerce. The matched data identify beneficiary firms and the year in which they obtain FEZ status. The resulting panel spans 2001–2015.[24]

I estimate

$$Y_{fmt} = \beta FEZ_{ft} + \mathbf{X}_f'\theta_t + \gamma_f + \lambda_{smt} + \varepsilon_{fmt}, \tag{26}$$

where Y_{fmt} denotes the outcome of firm f in municipality m and year t; γ_f are firm fixed effects; and λ_{smt} are 4-digit-sector-by-municipality-by-year fixed effects. The latter absorb shocks common to firms operating in the same detailed sector and municipality in a given year. The vector $\mathbf{X}_f$ contains predetermined firm characteristics interacted with year indicators.

[24]The sample ends in 2015 because, from 2016 onward, firms transitioned from the national PUC accounting system to international NIIF standards, introducing inconsistencies across reports. Further details on reporting thresholds and sample construction are provided in Section 2.2.

Identification therefore comes from changes in beneficiary firms around FEZ adoption relative to non-beneficiary firms operating in the same municipality and sector at the same point in time. Standard errors are clustered at the firm level.

The coefficient β captures the effect of obtaining FEZ status on beneficiary firms. I complement the baseline specification with firm-level event studies to examine the dynamics around adoption and pre-treatment trends. I also test whether FEZ adoption predicts exit from the Super-Sociedades reporting panel and re-estimate the main specifications on a balanced panel (Appendix Table C.15). Finally, I examine heterogeneity by firms' predetermined size and age.

5 The Effects of FEZs on Local Economies

This section presents the effects of FEZs on local economies, workers, and firms, following the margins highlighted by the conceptual framework. Section 5.1 examines whether FEZ exposure generates local economic and employment growth. Section 5.2 studies how the labor-market response differs across skill groups and along the formal–informal margin, while Section 5.3 examines the expansion of beneficiary firms and the response of other local firms. Section 5.4 presents robustness checks and alternative mechanisms.

5.1 Main Results: Effects of FEZs on Municipalities

Local Labor Market. I begin by examining whether FEZ-induced expansion translates into employment growth in the local labor market. This is the central margin of interest: industrial policies such as FEZs are designed to expand productive firms and, through their growth, generate modern employment. As I show below, FEZ operation generates a sustained expansion of local economic activity; the question is whether that expansion creates jobs. I therefore first estimate the effect of FEZ exposure on total municipal employment.

Figure 3 shows no statistically significant effect of FEZ exposure on total employment. The estimated coefficients remain close to zero following FEZ operation, with no evidence of a sustained increase in employment. The pre-treatment coefficients are also small, providing no indication of differential employment trends before treatment. Thus, despite targeting firm expansion, FEZs generate little detectable net employment growth at the local level.[25] This result is unchanged when the sample is restricted to the geographic domains for which the GEIH is designed to be representative, and when municipalities are aggregated into functional urban areas (Section F.2).

The aggregate employment effect may nevertheless mask differences in labor demand across sectors and skill groups. I therefore decompose the municipality-level response along these

[25] Appendix results using the employment-to-population ratio, based on both administrative and survey data, corroborate this finding: there is no evidence of increased labor market engagement following FEZ implementation (Figure C.12). This result is unchanged when the sample is restricted to the geographic domains for which the GEIH is designed to be representative, and when municipalities are aggregated into functional urban areas (Section **??**). The result is robust to deviations from parallel pre-trends and remains stable under randomization inference with placebo treatment timing (Rambachan and Roth, 2023; Roth, 2022).

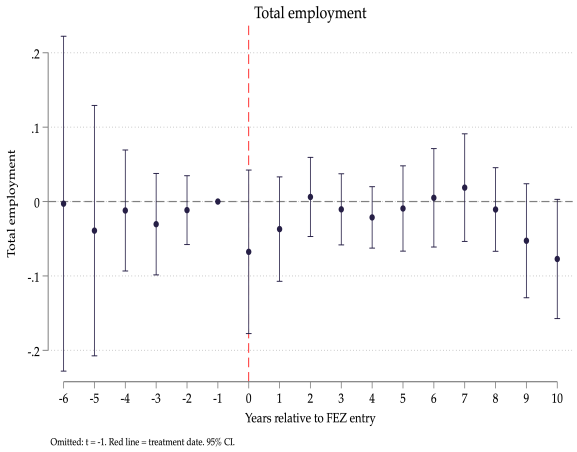
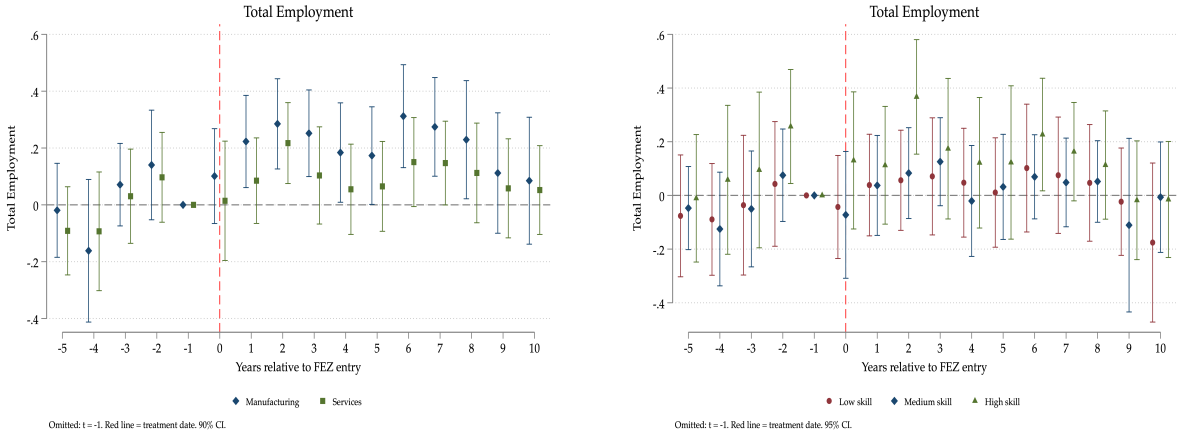


Figure 3: Event-study estimates of total employment following FEZ implementation.

Notes: Figure 3 displays event-study coefficients and 95% confidence intervals from specification (1). The outcome is the logarithm of total municipal employment. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using LFS (GEIH) and FEZ information.



(a) Employment effects by sector

(b) Employment effects by skill level

Figure 4: Anatomy of the FEZ effects on employment, by sector and skill level.

Notes: Panels 4a and 4b display event-study coefficients and 95% confidence intervals from specification (1). Panel 4a shows the effect of FEZ openings on the logarithm of total employment by sector, and Panel 4b shows the effect on the logarithm of total employment by skill level. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using GEIH (LFS) and FEZ information.

two dimensions. Figure 4a shows that employment gains are concentrated in manufacturing, while the response in services is limited. More importantly, Figure 4b shows that high-skilled employment rises only transitorily—a positive effect in the first year after operation that subsequently vanishes—with no evidence of employment growth among low- and middle-skilled workers. The absence of a persistent quantity response for skilled labor is consistent with the framework, in which the local supply of skills is fixed: a sustained increase in the demand for skilled workers cannot be met by additional employment and must instead be reflected in relative wages, which I examine in Section 5.2.

I next estimate the effect of FEZs on the informality rate, defined as the ratio of total informal employment to total employment, to assess how FEZs affect job composition along the formal–informal margin. This margin is particularly relevant in developing economies, where informality constitutes a large share of total employment and represents an important margin of labor-market adjustment. Figure 5 shows that the informality rate rises steadily following FEZ implementation, reaching an increase of approximately five percentage points—about 8 percent of the baseline informality rate of 63 percent in comparison municipalities.[26] Thus, while FEZs generate little aggregate employment growth, they are associated with a substantial change in the formal–informal composition of local employment.[27]

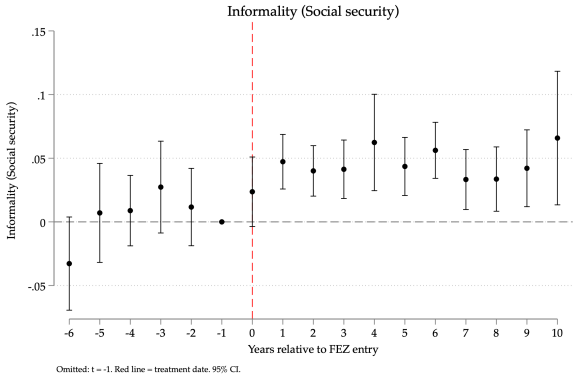


Figure 5: Event-study estimates of the overall informality rate following FEZ implementation.

Notes: Figure 5 displays event-study coefficients and 95% confidence intervals from specification (1). The outcome is the informality rate, defined as the share of employed workers without social security contributions over total employment. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated).
Source: Authors' calculations using GEIH (LFS) and FEZ information.

The increase in informality is evident across multiple sectors, with Figure 6a showing the largest rise in manufacturing and more moderate increases in services.[28] The response is more heterogeneous across skill groups. Figure 6b shows that the increase in informality is

[26] The same pattern holds using an administrative proxy that does not depend on the GEIH sampling design: the share of the municipal population registered in SISBEN, the registry used to target subsidized social protection (Appendix Figure C.14).

[27] These results should be interpreted relative to the broader decline in informality in Colombia over the sample period. The estimates indicate that, following FEZ implementation, treated municipalities experienced a slower decline—or a relative increase—in informality compared with control municipalities.

[28] The agricultural sector is excluded because FEZs are primarily located in urban or peri-urban municipalities and have minimal overlap with agricultural employment, which follows distinct seasonal and regulatory dynamics unrelated to the FEZ program.

concentrated among low-skilled workers, while the effects for middle- and high-skilled workers are considerably smaller. These estimates show that the aggregate labor-market response masks substantial differences across skill groups: employment gains are concentrated among more-skilled workers, while the increase in informality is concentrated among less-skilled workers.

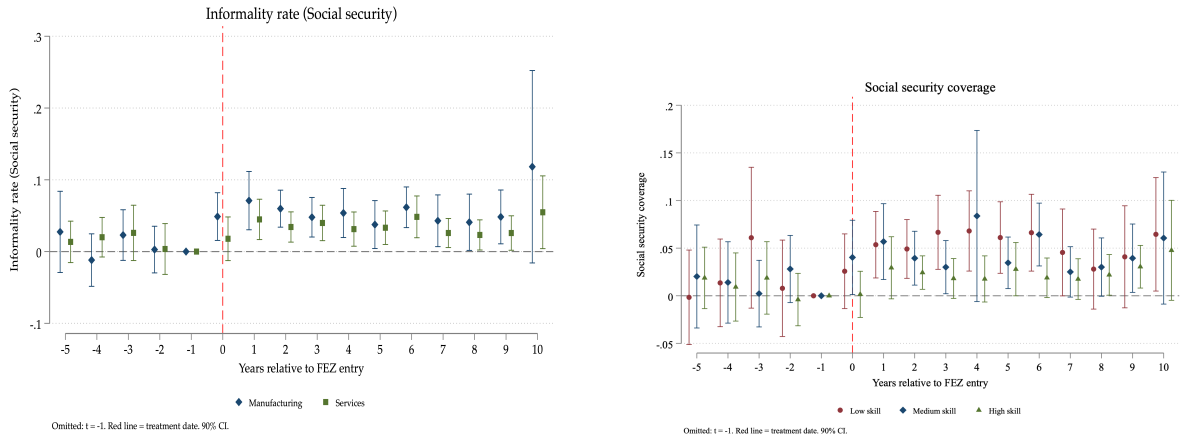
The response also varies with the initial skill composition of the local labor market. Figure C.13a splits municipalities according to their pre-policy share of low-skilled workers. The increase in informality is substantially larger in municipalities with a higher initial concentration of low-skilled labor, while the response is considerably smaller in municipalities with a lower low-skilled share. This gradient follows directly from the mechanism in the conceptual framework: where skilled labor is locally scarce, incumbent firms operate with many low-skilled workers per skilled worker, so the reallocation of skilled labor toward FEZ firms releases more low-skilled workers than the more skill-intensive zones re-absorb. The rise in informality is therefore strongest precisely where low-skilled labor is most abundant, consistent with the hypothesis that labor is abundant in aggregate yet scarce at the margin demanded by the firms the policy targets.

Consistent with this reallocation mechanism, I find no significant effects on unemployment or labor force participation, indicating that workers displaced from the formal sector are absorbed into informal jobs rather than exiting the labor force (Appendix E.1). This pattern aligns with the documented rise in informality, suggesting that labor market adjustments occur primarily through shifts between formal and informal employment rather than through changes in overall employment levels.[29]

Despite these substantial compositional changes, average wages at the municipal level remain largely unchanged following FEZ implementation, with only modest increases detected within the manufacturing sector. This muted aggregate response may mask heterogeneous wage adjustments across workers and firms: FEZs may generate upward wage pressures for specific skill groups or sectors that are not reflected in average municipal wages. In this sense, the worker-level analysis, examining differences in earnings across skill groups, provides a more disaggregated view of how FEZ exposure affects earnings. These results suggest that FEZs alter the composition of local employment and may generate heterogeneous wage pressures without significantly affecting overall wage levels.

Economic Development. I next examine the broader local development effects of Free Economic Zones (FEZs) on hosting municipalities. Figure 7a reports the estimated effects on nightlight intensity, a widely used proxy for local economic activity. Treated municipalities experience a marked and persistent rise in luminosity following FEZ operation, reaching approximately 1–1.5 standard deviations above pre-treatment levels within four years. Although the confidence intervals remain wide, the overall pattern indicates a sustained increase in local economic activity following the establishment of the zones. Importantly, this result shows that

[29] Although short-run transitions in and out of unemployment may occur at higher frequencies—such as month-to-month or quarterly—the use of annual data smooths over these temporary fluctuations.



(a) By sector

(b) By skill level

Figure 6: Anatomy of the FEZ effects on informality, by sector and skill level.

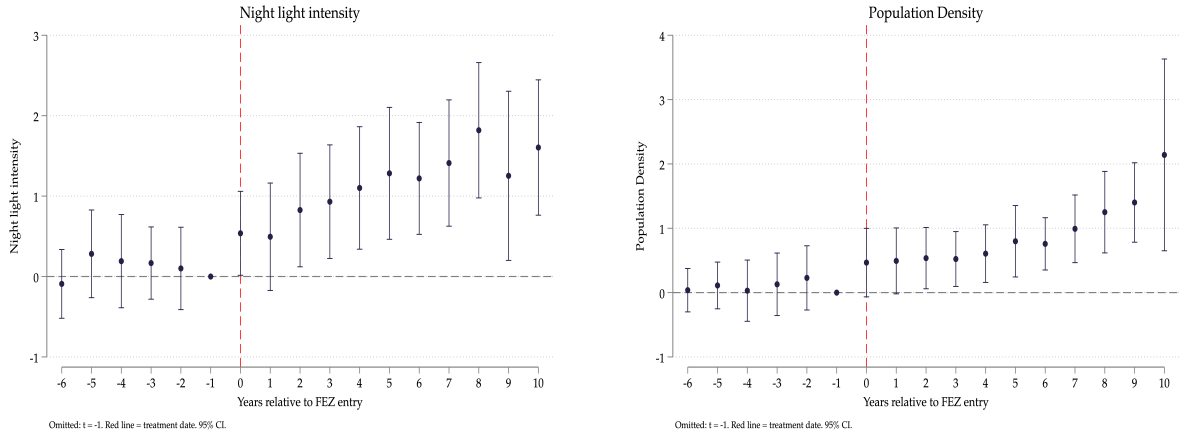
Notes: Panels 6a and 6b display event-study coefficients and 95% confidence intervals from specification (1). The outcome variable is the informality rate, defined as the share of employed workers without social security contributions over total employment. Coefficients are expressed in percentage points (pp). Panel 6a shows effects by sector, while Panel 6b shows effects by skill level. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using GEIH (LFS) and FEZ information.

the limited aggregate employment response documented above does not reflect an absence of local economic expansion.[30]

Figure 7b shows the estimated effect of FEZ implementation on log population density. Population density increases gradually following FEZ operation, although treated municipalities also exhibit mild positive trends prior to treatment. Moreover, annual population is measured using DANE population projections rather than directly observed population counts, limiting the extent to which these estimates can be interpreted as evidence of migration responses to FEZ implementation. I therefore view the post-treatment increase as suggestive of greater population concentration in hosting municipalities, rather than as causal evidence that FEZs attract population.

I next examine the local fiscal response. Municipal tax revenues provide a complementary measure of local economic activity and fiscal capacity, capturing whether the effects of FEZs extend beyond the firms directly receiving the policy benefits. Figure 8a shows that municipal tax revenues increase steadily following FEZ operation, reaching approximately 15 percent by year five, with no evidence of differential pre-trends between treated and control municipalities. Municipal revenues are primarily drawn from three local sources: the Industry and Commerce Tax (ICA), the Unified Property Tax, and the fuel surtax. Figure C.10 shows that the increase is particularly pronounced for property tax revenues, which may reflect an expansion of the tax base or higher assessed property values.

[30] These results also motivate a broader research agenda on the spatial effects of FEZs, including whether zones generate agglomeration economies by attracting firms and economic activity to their surroundings. Examining this margin would require geolocating firms and tracing changes in their spatial concentration around FEZs, which I leave for future research.

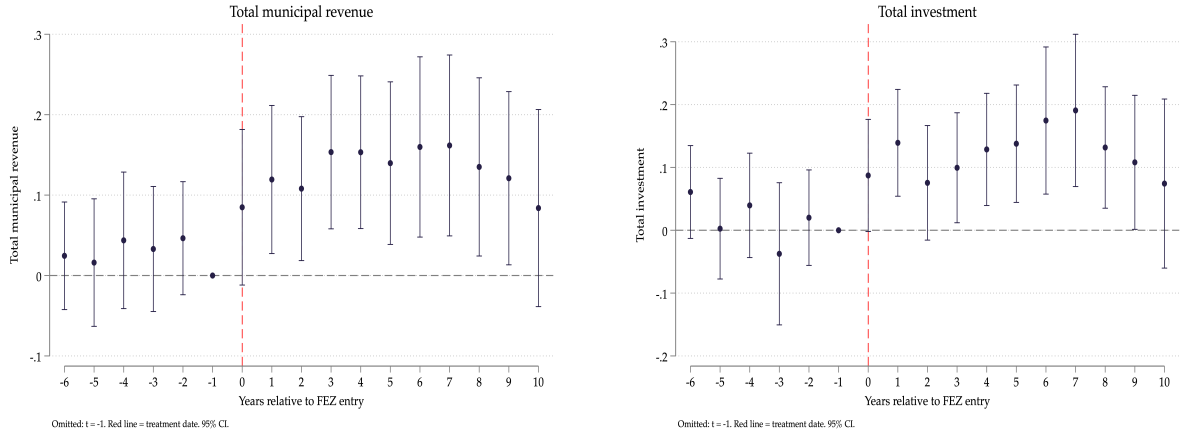


(a) Night-light intensity

(b) Population density

Figure 7: Event-study estimates of FEZ effects on local development outcomes.

Notes: Panels 7a and 7b display event-study coefficients and 95% confidence intervals from specification (1). Panel 7a shows the effect of FEZ openings on night-light intensity, measured in standard-deviation units, and Panel 7b shows the effect on the logarithm of population density. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Author's calculations using harmonized DMSP–VIIRS nighttime lights (Li et al., 2020), the CEDE municipal panel, and FEZ information.



(a) Municipal tax revenues

(b) Municipal investment

Figure 8: Event-study estimates of FEZ effects on local fiscal outcomes.

Notes: Panels 8b and 8a display event-study coefficients and 95% confidence intervals from specification (1). Panel 8b shows the effect of FEZ openings on the logarithm of municipal investment, and Panel 8a shows the effect on the logarithm of municipal tax revenues. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level and all regressions include municipality and time fixed effects.
Source: Authors' calculations using CEDE panel and FEZ information.

Public investment constitutes a key channel through which local governments can complement or amplify the effects of FEZs. By upgrading infrastructure and public services, municipalities can facilitate firm operations and support broader local economic activity. Figure 8b shows that municipal investment increases gradually following FEZ operation and remains elevated for several years, reaching approximately 12 percent before returning toward pre-treatment levels around the tenth year. The persistence of this response is inconsistent with a purely short-lived increase in expenditures associated with the initial implementation of the zones. Instead, FEZ operation is accompanied by a broader and sustained increase in municipal investment. Together with the increase in municipal revenues documented above, this pattern shows that the local fiscal response extends well beyond the initial establishment of the zones.

In summary, FEZs stimulate local economic activity and expand municipal fiscal capacity, as reflected in sustained increases in night-light intensity, tax revenues, and municipal investment. Yet these gains do not translate into broad-based employment growth. Aggregate employment remains largely unchanged, while informality increases and employment responses differ across sectors and skill groups. These results reveal a disconnect between local economic expansion and employment creation, and point to important changes in the composition of local employment. Understanding how workers and firms adjust to this expansion is therefore central to explaining the aggregate labor-market response. I turn to these margins next.

5.2 Workers

Having established that FEZs generate little aggregate employment growth but substantial changes in the composition of local employment, I next examine how these effects differ across workers. While the municipality-level results show how the composition of employment changes following FEZ exposure, the individual-level analysis identifies which groups of workers experience these changes and how their earnings respond. Controlling for individual characteristics also allows me to assess whether these patterns reflect differential responses across workers rather than changes in the observable composition of the local workforce.

I begin by examining changes in employment, informality, and unemployment status. Table 1 reports individual-level difference-in-differences estimates using the estimator proposed by Callaway and Sant'Anna (2021). Column (1) shows no statistically significant average effect of FEZ exposure on the probability of employment, consistent with the aggregate null result documented above. Once I allow the response to vary by skill, however, a clear pattern emerges. Column (2) shows that the aggregate null masks a 1.5 percentage point decline in employment among low-skilled workers, while medium- and high-skilled workers experience relative gains of 1.9 and 1.6 percentage points, respectively. The decline among low-skilled workers is consistent with the contraction of incumbent formal firms highlighted in Proposition 4, a margin I examine directly in Section 5.3. These estimates are robust to controlling for predetermined individual characteristics, including age, gender, education, and marital status, and remain similar when I additionally control for potentially endogenous characteristics such as migration status and sector of employment. This suggests that the heterogeneity by skill is not explained by observable changes in the demographic or sectoral composition of workers

across municipalities.

Table 1: Regression Results: Employment, Informality, and Unemployment

	Employment		Informality		Unemployment	
	(1)	(2)	(3)	(4)	(5)	(6)
FEZ	-0.00137	-0.0149***	0.0204***	0.0628***	-0.00444***	0.00183*
	(-1.16)	(-10.66)	(11.46)	(29.39)	(-5.72)	(1.99)
Medium-skilled		0.00664***		-0.0219***		0.0202***
		(9.80)		(-20.86)		(44.12)
High-skilled		0.0944***		-0.190***		0.0363***
		(108.59)		(-152.41)		(62.75)
FEZ×Medium-skilled		0.0186***		-0.0617***		-0.00739***
		(17.24)		(-36.57)		(-10.36)
FEZ×High-skilled		0.0162***		-0.0420***		-0.0105***
		(12.77)		(-22.65)		(-12.51)
Observations	3,574,586	3,577,772	1,908,735	1,910,463	3,302,001	3,304,351

Notes: Columns (1), (3), and (5) report Callaway and Sant'Anna aggregate estimates; columns (2), (4), and (6) report TWFE estimates with skill interactions. *t*-statistics in parentheses. Controls include age, marital status, children, education, gender, and year and municipality fixed effects. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Turning to informality, Table 1 shows that FEZ exposure significantly increases the likelihood of informal employment. Once I allow the effect to vary by skill (Column 4), the increase is concentrated among low-skilled workers, whose probability of informal employment rises by approximately 6.3 percentage points. The interaction terms are negative for medium- and high-skilled workers, implying considerably smaller total effects of approximately 0.1 and 2 percentage points, respectively. Thus, the aggregate increase in informality is driven primarily by low-skilled workers, while more-skilled workers are substantially less exposed to this margin of adjustment. Together with the employment results above, this reveals a sharp asymmetry across skill groups: low-skilled workers experience both lower employment and greater informality, whereas medium- and high-skilled workers experience relative employment gains and little or substantially smaller increases in informality. Through the lens of the conceptual framework,, this pattern is consistent with the displacement regime in Proposition 5, in which the contraction of low-skill employment in ordinary formal firms more than offsets its expansion among FEZ firms.[31]

For unemployment, the baseline results in Table 1 show that FEZs modestly reduce the likelihood of being unemployed, with an average decline of about 0.44 percentage points. This finding is consistent with the earlier evidence that workers affected by FEZ-driven reallocation are not leaving the labor force but are instead absorbed elsewhere—primarily into informal employment. The disaggregated results reinforce this interpretation: unemployment falls among medium- and high-skilled workers by 0.74 and 1.05 percentage points relative to low-skilled workers, respectively, while for low-skilled workers unemployment is, if anything, marginally higher. These heterogeneous effects mirror the patterns observed for employment and informality. The stock of skilled workers in formal employment expands, while the adjustment for less-skilled workers appears in informality rather than unemployment, consistent with the

[31] Appendix Tables C.13 and C.14 examine the margins underlying the increase in informality. The results are consistent across alternative dimensions of informal work and show that the response among low-skilled workers is accompanied by lower social security contributions and business registration and a higher incidence of unpaid work.

aggregate participation results.[32]

Table 2: Regression Results: Wages

	Wages	
	(1)	(2)
FEZ	0.0201***	-0.000266
	(11.14)	(-0.12)
Medium-skilled		0.0756***
		(70.56)
High-skilled		0.140***
		(188.24)
FEZ×Medium-skilled		0.00496*
		(2.26)
FEZ×High-skilled		0.0328***
		(23.03)
Observations	864,597	864,597

Notes: Column (1) reports Callaway and Sant'Anna aggregate estimates; column (2) reports TWFE estimates with skill interactions. *t*-statistics in parentheses. Controls include age, marital status, children, education, gender, and year and municipality fixed effects. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Finally, I turn to wages. Table 2 shows that FEZs have a positive average effect on wages, but this effect is highly heterogeneous across skill levels. Column (1) presents the aggregate estimate, indicating that workers in municipalities hosting FEZs earn about 2 percent higher wages on average compared to those in untreated areas. This individual-level gain is compatible with the muted municipal-level averages documented above: the worker-level specification holds observable composition fixed, so it captures wage changes for comparable workers rather than shifts in the average composition of employment. Column (2) shows a clear pattern of skill heterogeneity. The direct FEZ effect for low-skilled workers is essentially zero. Under the displacement regime, Proposition 5 implies downward pressure on the low-skill wage; with a binding minimum wage, this pressure appears in quantities—the reallocation into informality documented above—rather than in formal wages. The interaction terms reveal that medium-skilled workers experience a wage increase of roughly 0.5 percent, while high-skilled workers earn about 3.3 percent more relative to low-skilled workers in FEZ municipalities.

Through the lens of the framework, the rise in the skill premium is the diagnostic result: it establishes that FEZ expansion shifted aggregate labor demand toward skill—the reallocation of low-skill employment toward the more skill-intensive regime dominated, in λ-weighted terms, the offsetting movement toward informality (Proposition 3). Given this shift, the framework predicts the contraction and exit of incumbent formal firms, which the next section documents. In other words, FEZs generate both a zone premium—higher average wages at the local level—and a skill premium, with disproportionate gains for more educated or technically qualified workers. Together, these patterns suggest that FEZs stimulate localized wage growth by favoring skill-intensive production and competition for qualified labor, while leaving low-skilled workers' formal wages largely unaffected. This pattern aligns with the broader

[32]The estimated effects capture within-market adjustments rather than shifts in observable workforce composition, since the specifications control for predetermined individual characteristics; results are unchanged when additionally controlling for migration status.

reallocation mechanism: FEZs increase productivity and labor demand at the upper end of the skill distribution, reinforcing inequality across workers and municipalities.

Overall, these results indicate that FEZs shift stable, better-paid employment toward skilled workers, but at the cost of pushing less-skilled workers into informality. This redistribution of labor market opportunities is consistent with a mechanism in which FEZs attract high-productivity firms that demand skilled labor, while simultaneously displacing low-skilled workers into more precarious forms of work.[33] This pattern echoes the broader phenomenon of jobless industrialization in late-industrializing economies, where large, high-productivity firms generate disproportionate value added but relatively little employment. Structural distortions—such as high payroll taxes, binding minimum wages, and capital–labor cost imbalances—further limit these firms' capacity to absorb labor, pushing displaced workers into informality or self-employment (Alfaro et al., 2023).

In this sense, FEZs can be viewed as a micro-level manifestation of these forces: they enhance productivity and competitiveness at the upper end of the firm and skill distribution, but their limited labor absorption reinforces regional inequality. Within treated municipalities, wage gains accrue primarily to medium- and high-skilled workers, while informal and low-skilled workers experience stagnant earnings. These dynamics widen both the formal–informal divide and wage dispersion within local labor markets. To understand the channels through which these heterogeneous effects emerge, the next section turns to firms—the direct beneficiaries of FEZ incentives—whose responses in investment, hiring, and production provide the mechanisms linking FEZ expansion to local labor market adjustment.

5.3 Firm-level Results: Firms as a Channel of Adjustment

5.3.1 Direct Effects on FEZ-Adopting Firms

Before turning to treatment effects, it is worth noting that firms that obtain FEZ status differ from non-adopters before certification: they are larger, more capital intensive, and more productive (Table **??**). Rather than a threat to the analysis, this pattern is itself a prediction of the framework: because the regime combines a proportional operating gain with an additional fixed cost, only sufficiently productive registered firms find adoption profitable (Proposition 1). What the research design treats as selection into treatment, the model predicts as sorting into the regime.

I estimate Equation 26, which identifies the causal effect of FEZ designation on firm-level outcomes such as investment, production, and profitability. The analysis focuses on firms that existed both before and after FEZ implementation, ensuring that the estimates capture within-firm adjustments rather than entry or exit effects. I present difference-in-differences estimates using the methodology developed by Callaway and Sant'Anna (2021).[34]

Table 3 reports the effects of FEZ designation on firm performance outcomes. The results

[33] Anecdotal evidence supports this view: for example, business associations in Dominican Free Zones report persistent difficulties in hiring qualified staff, underscoring that these zones actively seek skilled workers but struggle to absorb the less skilled (Diario Libre, 2024).

[34] Detailed estimates and event-study dynamics are reported in Appendix E.3.1.

Table 3: Regression Results: Firm Performance

	Operating Income	Net Income (Before Tax)	Non-Operating Income
FEZ	0.518	0.458***	0.607***
	(0.342)	(0.167)	(0.160)
Observations	204,302	157,502	182,669
Mean of Dep. Variable	14.30	12.25	10.63
Controls	X	X	X
Firm FE	X	X	X
Year FE	X	X	X
Sector x Year FE	X	X	X

Notes: t-statistics in parentheses. All variables are deflated using industry-specific yearly Producer Price Index and expressed in logarithms. The control group are non-FEZ firms. Specifications control for pre-policy assets $\times$ year, firm age, and ownership type, as well as firm, year, and sector fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

show no statistically significant effects on operating income, indicating stable operating margins. By contrast, profitability gains are concentrated in non-operating components: net income before tax rises by roughly 46 percent, and non-operating income increases by approximately 61 percent. This divergence between operating and net income is consistent with the structure of the incentives in the framework (Equation 14): the preferential profit tax raises firm value without affecting conditional input choices or operating margins, while the operating advantages of the regime raise scale. The results below show precisely this pattern—stable operating margins alongside a pronounced expansion of scale—indicating that FEZs improve firms' financial outcomes through fiscal channels while expanding production capacity, rather than through higher productivity. This pattern has implications for the incidence of FEZ benefits. Gains accrue disproportionately to firms that were already productive or capital intensive, consistent with the positive selection documented above, while the absence of corresponding improvements in operating income suggests limited efficiency spillovers to workers or local suppliers.

I then examine labor costs and intensities. Table 4 shows that FEZ designation substantially increases wage bills by about 78 percent, with salary expenditures rising sharply while labor shares in total costs and revenues remain stable. Stable labor shares alongside a rising wage bill indicate that labor costs grow in proportion to firm scale: adopting firms expand rather than substitute away from labor. Although I do not observe the number of workers directly, the results are consistent with expansion in both compensation and headcount. Figure C.17 shows that these effects are particularly pronounced among manufacturing and medium-sized firms.

Additionally, I examine balance sheets and production capacity. FEZ designation triggers a pronounced expansion in firm balance sheets (Table C.16). Total assets and liabilities rise substantially, with growth concentrated in long-term capital and inventories, consistent with large-scale investment and scaling-up behavior. Firms appear to use FEZ incentives to finance expansion rather than reduce costs: liabilities increase more than equity, suggesting that debt is leveraged to fund equipment and infrastructure.

FEZ designation also reshapes firms' production profiles (Table C.17). Net PPE increases by 63%, indicating greater reliance on physical capital and production facilities. Inventories expand markedly, with raw materials rising by 68% and finished goods by 136%, consistent

Table 4: Regression Results: Labor Costs and Intensity

	Salaries	Labor-to-Cost Ratio	Labor-to-Revenue Ratio	Labor Intensity
FEZ	0.787***	0.0310	0.0525	-0.0837
	(0.232)	(0.118)	(0.0988)	(0.153)
Observations	194,797	149,887	167,376	166,437
R^2	0.839	0.718	0.724	0.814
Mean of Dep. Variable	9.297	4.608	4.668	2.467
Controls	X	X	X	X
Firm FE	X	X	X	X
Sector x Year FE	X	X	X	X

Notes: t-statistics in parentheses. All variables are deflated using industry-specific yearly Producer Price Index and expressed in logarithms. The control group are non-FEZ firms. Specifications control for pre-policy assets $\times$ year, firm age, and ownership type, as well as firm, year, and sector fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with intensified production processes and higher output volumes. This expansion of scale is the second prediction of the framework: conditional on adoption, the operating advantage of the regime raises firm size (Proposition 2). These patterns also align with program requirements that condition FEZ eligibility on large upfront investment commitments, reflected in the higher entry cost of the regime. By contrast, land holdings and real estate for sale remain largely unaffected, reflecting the fact that firms do not need to acquire land directly but instead pay fees to the zone developer, indicating that expansion is not driven by property speculation. The ratio of net PPE to labor costs also increases, pointing to a shift toward more capital-intensive operations; the framework abstracts from capital, so this margin should be read as complementary to, rather than driving, the labor-market mechanism. Finally, reported financial investments do not grow significantly, reinforcing the view that FEZs channel resources into production capacity rather than portfolio accumulation.

Overall, the firm-level evidence confirms the first two predictions of the framework: FEZ status is adopted by firms at the top of the productivity distribution, and adoption induces substantial expansion of scale, investment, and labor costs, with financial gains concentrated in the fiscal channels the model assigns to the profit-tax advantage. Beneficiary firms therefore did expand—the absence of aggregate employment gains cannot reflect a weak response by the firms the policy targets. The question is how this expansion was accommodated by the rest of the local economy. The next section broadens the analysis to all manufacturing firms in hosting municipalities to capture these wider equilibrium adjustments.

5.3.2 Firms in Hosting Municipalities

I next estimate the effects of local FEZ exposure on manufacturing firms using Equation 25. Because the manufacturing census does not identify FEZ beneficiaries, these estimates combine the response of beneficiary firms included in the data with that of non-beneficiary incumbents operating in the same municipalities (Section 4.2.3). The coefficients reported in Tables 5–6 represent difference-in-differences estimates based on the staggered adoption estimator of Callaway and Sant'Anna (2021), which accounts for heterogeneous treatment timing across municipalities.

Table 5 shows that FEZ exposure increases employment in surviving formal manufacturing firms by 4.8%, with no effect on unpaid employment. The wage bill per worker rises by 4.0%, indicating that FEZ exposure increases labor compensation at these firms. These patterns are consistent with firms scaling up production and competing more intensely for skilled workers, and they mirror the manufacturing employment gains in the municipal sector decomposition of Section 5.1.

A positive average effect on surviving manufacturing firms is not at odds with the contraction of incumbents predicted by Proposition 4, for three reasons. First, the estimate combines the expansion of beneficiary firms present in the census with the response of non-beneficiary incumbents, so beneficiary growth can dominate the average. Second, the sample conditions on survival: the framework predicts that contraction is concentrated among the smallest formal firms, which exit and therefore drop out of the panel, biasing surviving-firm estimates upward. Third, the municipal results show that manufacturing gains coexist with a flat aggregate, so the offsetting adjustment operates outside surviving manufacturing firms—in other sectors and in informality. The sharp test of the incumbent margin is therefore not the surviving-firm average but the heterogeneity of the response across firm size and the entry and exit margins, which I examine below.

Table 5: Regression Results: Log Coefficients with Firm FE

Variable	Employment	Employment (Unpaid)	Total Wages	Total Wages Direct
FEZ	0.0480***	0.00183	0.0401***	0.0382***
	(6.39)	(0.13)	(4.62)	(4.18)
Year FE	X	X	X	X
Municipality FE	X	X	X	X
Firm FE	X	X	X	X
Observations	121,801	26,243	121,034	118,958

Notes: t-statistics in parentheses. Wage outcomes are measured as the total wage bill per worker. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

In terms of operational spending, Table 6 reports that FEZs lead to a 3.5% increase in firm total expenses, reflecting higher variable costs associated with expanded production capacity. Outsourcing production rises by 8.4%, suggesting that firms increasingly rely on subcontractors as output scales. Unlike purchasing standard inputs, outsourcing shifts segments of production to external entities that are more likely to operate outside formal labor regulations. This strategy provides flexibility and cost savings, but also creates an indirect channel through which FEZ activity may stimulate informal production—the intensive margin of informality from which the framework abstracts (Appendix C.11).

Turning to production outcomes, FEZ-exposed firms exhibit meaningful scale gains. Value added increases by 5.4%, while both the quantity of goods produced and the use of intermediates rise in tandem by around 4.1%. These results indicate that FEZs stimulate manufacturing firms to mobilize more inputs and expand output, consistent with the capital-deepening dynamics observed in their balance sheets.

The firm-level evidence points to an amplification mechanism. FEZ activity increases de-

Table 6: Regression Results: Production, Value Added, Costs, and Outsourcing

	Value Added	Quantity Produced	Intermediates	Total Expenses	Outsourcing Production
FEZ	0.0540***	0.0411*	0.0406*	0.0350**	0.0843*
	(4.04)	(2.57)	(2.47)	(2.92)	(2.50)
Year FE	X	X	X	X	X
Municipality FE	X	X	X	X	X
Firm FE	X	X	X	X	X
Observations	121,384	111,149	119,777	121,833	39,372

Notes: t-statistics in parentheses. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

mand for medium- and high-skilled labor, pushing up local wages. Smaller and less productive firms—even those outside the zone—are compelled to raise compensation to retain workers, but can only do so for certain occupations. This generates asymmetric benefits: formal employment and wage gains accrue to skilled workers, while low-skilled workers are increasingly displaced into informal jobs. Importantly, the absence of any effect on unemployment suggests that displaced workers are absorbed by the informal sector rather than exiting the labor force. Two features of the policy help explain why FEZs do not absorb the entire local workforce. First, skill scarcity limits firms' ability to hire additional workers even as output expands—the margin at the center of the framework. Second, eligibility rules require large upfront investment commitments, encouraging firms to adopt more capital-intensive production processes rather than labor-absorbing expansion. These forces generate heterogeneous adjustment pressures across firms within treated municipalities. If FEZs are causing differential pressures across firms, what happens to the structure of the formal firm population? To address this question, I next examine how FEZs affect the dynamics of formal firms, focusing on entry and exit margins.

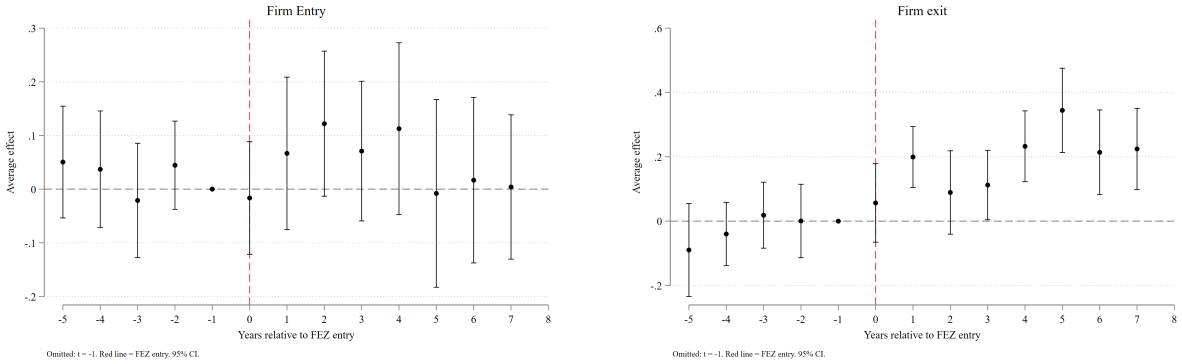
5.3.3 Dynamics of Formal Firms

I estimate Equation 22, with formal firm entry and exit rates at the municipality level as outcomes. The specification compares treated and untreated municipalities over time, allowing me to identify how FEZs reshape the composition of formal activity through changes in firm turnover.

Figure 9a shows no statistically significant change in formal firm entry rates around FEZ creation. This null effect is notable given the explicit policy objective of promoting investment and business creation, but it is what the framework predicts: higher formal labor costs reduce the value of formal entry, and the sorting margin shifts entry incentives toward informality rather than toward new formal firms (Proposition 6).[35] By contrast, Figure 9b reveals a sharp and persistent increase in firm exit rates after FEZ establishment. The treatment effect grows steadily over time, reaching 0.3–0.5 percentage points (about a 7–8% increase relative to baseline). This is the exit margin predicted by Proposition 4: the rise in incumbents' unit labor costs raises the zero-profit threshold for survival, pushing marginal formal firms out.

To better understand the nature of firm exits, Figure 10a decomposes exit dynamics by firm

[35]This result is not at odds with the decline in business registration documented in Section 5.2. The entry rate measures new firm registrations relative to the stock of active formal firms, while the GEIH registration margin captures whether workers' employers or own businesses operate as registered—the sorting margin along which existing marginal businesses adjust.

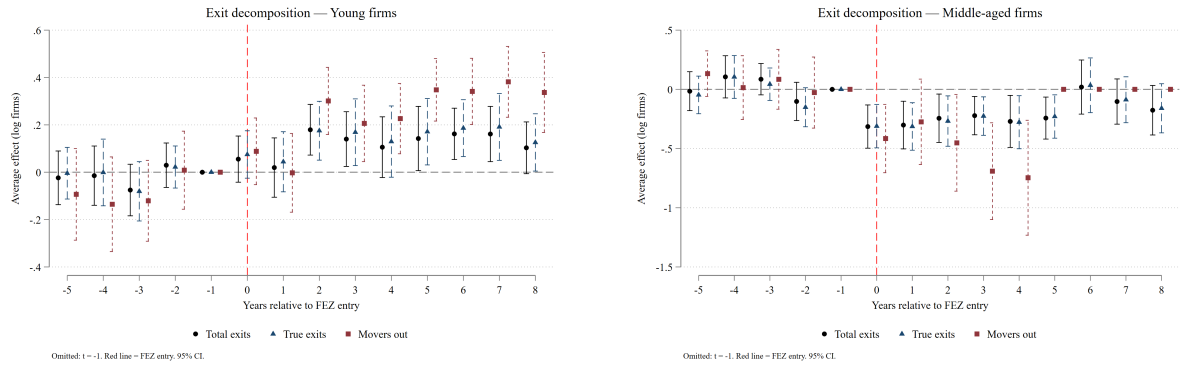


(a) Entry rate (b) Exit rate

Notes: Panels 9a and 9b display event-study coefficients and 95% confidence intervals from specification (22). The outcome variables are the formal firm entry and exit rates, defined respectively as the share of new or exiting formal firms over the total number of active formal firms in a municipality. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Author's calculations using RUES and FEZ information.

age and exit type. I distinguish between total exits, true exits (firms that cease operations), and firms that relocate out of the municipality and subsequently exit in the next year. The increase in exit rates is concentrated among young firms, which tend to be less productive and more vulnerable to competitive pressures (Hopenhayn, 1992). The post-treatment effects for this group are positive and increase over time, consistent with a tightening of the survival threshold. The decomposition shows that this increase reflects both true exits and relocations followed by exit, indicating that FEZ exposure affects firm dynamics along multiple margins. By contrast, exit rates among middle-aged firms decline following FEZ implementation. This asymmetry is itself consistent with the framework, in which the operating gain from the regime rises with productivity: established, more productive firms—the pool from which adopters are drawn—see their profitability improve, while marginal firms face rising costs. The results point to a reallocation process in which less productive, typically younger firms exit through both shutdown and relocation channels, while more established firms are relatively insulated from exit. Combined with the absence of new formal entry, these patterns are consistent with a shift in the composition of firms toward more productive incumbents rather than an expansion in the number of firms.

Social Security Data. The analysis above relies on the RUES registry, which covers the universe of firms that register and renew their *matrícula mercantil* and is well-suited to measure the entry and exit of formal firms, but it does not allow tracking worker outcomes. As a complementary exercise, I use administrative records from the social security system (PILA), which provide firm–worker level information on wages, hours, worker age, and sector of activity, and therefore allow me to examine which types of firms exit (using firm size as a proxy for productivity) and what happens to their workers—whether they reallocate to new formal employers or drop out of formal employment altogether. I define firm exit as the disappearance of a firm from the registry (no contributions for at least two consecutive years) and worker exit as leaving a formal job



(a) Young firms (b) Middle-aged firms

Notes: Panels 10a and 10b report event-study coefficients and 95% confidence intervals from specification (22), separately by firm age group. The outcomes decompose total exits into (i) true exits (firms that cease operations) and (ii) relocations out of the municipality that subsequently result in exit. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone. Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects. *Source:* Author's calculations using RUES and FEZ information.

without reappearing in another registered firm.[36] To mitigate concerns that exits are driven by retirement, I restrict attention to workers younger than 60 years old. The PILA data cover 2008–2016; to ensure consistent pre- and post-treatment coverage, I exclude municipalities treated before 2008 and after 2015. I estimate the same event-study specification as in Equation 22, replacing the outcomes with firm and worker exit rates computed from PILA, which ensures comparability with the RUES-based analysis while exploiting the richer worker-level dimension of social security records.

Figure C.18 confirms that FEZs increase firm exit rates, with Figure C.19 showing that this effect is concentrated among smaller, less productive firms, consistent with the tightening of the survival cutoff in Proposition 4 (Melitz, 2003; Ulyssea, 2018). Following firm exit, affected workers are considerably more likely to leave formal employment than to transition to a new formal employer. Because PILA only records formal jobs, it cannot distinguish between unemployment, labor force exit, or transitions into informal employment. However, combined with survey evidence showing no systematic changes in unemployment or labor force participation, these patterns suggest that displaced workers predominantly reallocate outside the formal sector. This reinforces the interpretation that FEZs operate through selection on productivity rather than by expanding formal labor demand. The firm dynamics results provide strong empirical support for the selection mechanism at the center of the framework: FEZs raise the profitability of high-productivity formal firms, allowing them to expand, invest, and bid up wages—particularly for medium- and high-skill occupations. This intensifies competition in local labor markets and raises the survival threshold, pushing out less productive formal firms, especially younger and smaller ones. Skill scarcity limits expanding firms' ability to absorb additional workers even as output grows; the eligibility rules' large upfront investment requirements, which encourage capital-intensive production, reinforce this limit. The absence of new formal entry completes the picture: reallocation occurs through the expansion of productive incumbents and the exit

[36] Some individuals may leave the labor force for reasons unrelated to firm dynamics, so these results should be interpreted with caution.

of marginal firms rather than through net firm creation. These dynamics explain the earlier finding of no aggregate employment effects in treated municipalities. Employment gains in expanding firms are offset by job losses from closures, and displaced workers are more likely to leave formal employment, contributing to the rise in informality documented in Sections 5.1 and 5.2. Taken together, the evidence points to a cascade in which FEZs raise productivity cutoffs and generate local wage pressures that reshape the composition of both firms and jobs without increasing total employment.

5.3.4 Wage Structure and Firm Heterogeneity

To assess the role of firm productivity in shaping the wage effects of FEZs, I use matched employer–employee data from PILA to estimate an Abowd–Kramarz–Margolis (AKM) model and perform a Blinder–Oaxaca decomposition. These exercises allow me to separate the contribution of worker characteristics from firm-specific wage-setting premia. Appendix E.5 provides full details. The AKM results in Table C.21 show that firm fixed effects explain a substantial share of wage variation in the formal sector, and that workers in FEZ municipalities are disproportionately employed in firms with higher wage premia. The Oaxaca decomposition in Table C.22 confirms that differences in firm effects dominate observable worker characteristics in explaining FEZ–non-FEZ wage gaps. This does not imply that skills are unimportant; rather, conditional on worker characteristics, much of the wage differential reflects both sorting into more productive, high-premium firms and higher wage-setting within those firms due to increased competition for skilled labor. These findings reinforce the selection mechanism documented above. FEZs raise the productivity cutoff for survival, reallocating employment toward firms that are more capital- and skill-intensive and that offer higher wage premia. Less productive firms exit and do not re-enter, while displaced workers are less likely to remain in formal employment. Together, this helps explain why aggregate formal employment does not increase despite the appearance of high-wage jobs within FEZs. By highlighting that wage premia are driven primarily by firm heterogeneity rather than worker composition, these results support the reallocation mechanism at the center of the conceptual framework, in which activity shifts toward high-productivity, high-premium firms that bid up skilled wages while marginal firms exit.

5.4 Robustness and Alternative Explanations

The baseline results show that FEZs raise local wages and increase informality by pushing low-productivity formal firms out of the market. This section examines whether other forces could generate the same patterns. Migration inflows, changes in labor force participation, sectoral reallocation, land market pressures, and product-market crowding-out could all, in principle, shift the composition of employment toward informality without operating through firm-level selection. Each of these mechanisms carries a distinct empirical signature, and none matches the data. I close by summarizing a battery of robustness exercises addressing measurement, treatment definition, identification, and inference; details are reported in the Appendix.

A first concern is that the rise in informality reflects compositional shifts in the local labor force rather than firm selection. Migration inflows could increase informality if new arrivals disproportionately take informal jobs; however, controlling for migrant status leaves the FEZ estimates unchanged, and interacting FEZ exposure with migrant status shows that the increase in informality is concentrated among local residents.[37] Moreover, since aggregate employment does not change, a migration-based explanation would require one-for-one displacement of local workers, which is not supported by the data. FEZs could instead draw nonparticipants into the labor force, mechanically raising informality among marginal entrants; yet municipality-level regressions of labor force participation and unemployment show no systematic changes around FEZ entry. Finally, workers could be moving into sectors with structurally higher informality, such as services or commerce; disaggregating employment by sector shows instead that informality rises within sectors, not merely across them. Compositional changes in migration, participation, or sectoral structure therefore cannot account for the rise in informality.

FEZ entry could also raise local land values, through infrastructure investments, speculative activity, or clustering, pushing out less productive firms unable to afford higher rents. To explore this mechanism, I split municipalities into urban (denser) and rural (sparser) areas and re-estimate the FEZ effects on property tax revenues and firm exit. If land pressures were the main driver, exit effects should be stronger in urban municipalities; instead, the evidence points to similar patterns across space, suggesting that land competition is not the primary channel.

A final alternative is that FEZ firms capture market share in imperfectly competitive product markets, so that incumbents exit not because they face higher labor costs but because they lose customers to FEZ firms supplying similar products at lower prices. The two mechanisms make different predictions about who exits. Product-market crowding-out should push out direct competitors of any size, including medium-sized, large, and relatively productive non-FEZ firms; the labor-cost mechanism of the framework predicts exit concentrated among small, low-productivity firms closest to the survival margin (Proposition 4). The data support the second pattern: exit is disproportionately concentrated among small, low-productivity firms, while medium- and large-sized—and more productive—firms remain active and, in many cases, expand. If product-market crowding-out were the dominant channel, exit would extend up the productivity distribution. The persistence and growth of high-productivity firms is instead consistent with a rising wage bill that only less productive firms cannot sustain.

The main estimates are robust to a broad set of measurement, spatial, and identification checks. First, the results are not driven by the unequal precision with which the GEIH measures municipality-level outcomes. Restricting the analysis to the 23 main cities and metropolitan areas for which the survey is designed to provide representative estimates, or to the largest quartile of municipalities, produces similar point estimates for employment and informality (Appendix **??**). The estimates are also virtually unchanged when I allow municipalities to follow

[37] Colombia and Venezuela share a long border with historically high bidirectional migration. Colombian migration to Venezuela rose during the oil boom of the 1950s and Colombia's internal conflict, while conflict-induced displacement raised baseline informality in urban centers. Both trends predate the FEZ program. The Venezuelan exodus accelerated only after 2013 and peaked in 2021, and conflict-induced displacement was declining in the mid-2000s. Neither aligns with the timing of FEZ treatment.

different trends according to baseline population, nightlights, employment, fiscal capacity, and distance to major cities (Appendix F.1). Redefining local labor markets using functional urban areas yields similar point estimates, although with wider confidence intervals due to the smaller number of geographic units (Appendix F.3). Finally, donut specifications that exclude municipalities near operational FEZs show that contamination of the comparison group by spatial spillovers does not drive the results (Appendix F.4).

A complementary set of placebo exercises assesses whether the results could arise from spurious variation in the timing or location of FEZ entry. Shifting FEZ entry backward by one, two, and three years produces placebo estimates close to zero, while randomly reallocating municipalities across the observed treatment cohorts generates placebo distributions centered around zero. The actual increase in informality does not emerge under either falsified assignment (Appendix F.5). Leave-one-cohort-out and leave-one-municipality-out exercises further confirm that no single cohort or large metropolitan area drives the estimates (Appendix F.6). The results are also robust to alternative measures of FEZ exposure and informality.

Finally, to address concerns about staggered adoption and comparison-group composition, I define the relevant labor market as a two-digit industry $\times$ municipality cell and exploit the fact that only certain subsectors within broad sectors are represented in FEZs. Comparing FEZ-exposed and non-exposed subsectors within the same municipality and broad sector absorbs local and sector-specific shocks, and yields estimates similar in magnitude and precision to the baseline. Together with the heterogeneity of the informality response across municipal skill composition documented in Section 5.1—which matches the gradient the mechanism predicts—these exercises indicate that the results reflect labor market reallocation induced by FEZ activity rather than pre-existing trends, measurement error, or spatial spillovers.

6 Discussion, Policy Implications, and Limitations

The findings in this paper have implications for the design of place-based industrial policies in economies with pervasive informality. FEZs succeed at attracting investment and fostering expansion among high-productivity, capital-intensive firms, but their benefits are unevenly distributed. The wage pressures they generate raise the local survival threshold, accelerating the exit of marginal formal firms and pushing displaced workers into informal employment. Absent complementary reforms, these dynamics risk reinforcing economic dualism rather than alleviating it.

The results reveal clear distributional trade-offs. High-productivity firms and skilled workers benefit from FEZ incentives through higher profits and wages, while low-productivity firms and low-skill workers face rising costs and displacement. These are, however, relative outcomes: without an aggregate general equilibrium framework, it is not possible to quantify the overall impact on the Colombian economy. Even so, the evidence suggests that aggregate productivity (TFP) could initially rise as the most efficient firms expand, although these gains may be partially offset by productivity losses associated with higher informality and firm exit. The net welfare effect thus remains uncertain and depends on the balance between efficiency

gains and reallocation costs.

Policy implications. Two design features of the FEZ program help explain these distributional patterns. First, eligibility requirements based on large upfront investment commitments favor firms with access to capital, encouraging production processes that are more capital- and skill-intensive. As these firms expand, they compete for a limited pool of qualified workers, generating local skill scarcity and upward pressure on wages. Second, weak enforcement of labor regulations outside the zones makes informality an elastic margin of adjustment. When wage pressures intensify, low-skill workers are absorbed informally rather than into expanding formal firms.

These dynamics suggest several policy considerations. Training and upskilling programs could ease skill bottlenecks, allowing more workers to access the high-wage jobs created by FEZs. In parallel, lowering administrative barriers to entry into the zones could broaden access to program benefits and enable a wider range of firms to participate, reducing the concentration of gains among a narrow set of capital- and skill-intensive producers.

Limitations. While the analysis provides new evidence on how FEZs reshape local labor markets and firm dynamics, several limitations remain. First, the paper focuses on local outcomes and does not capture upstream or downstream linkages. FEZs may generate indirect benefits through supplier networks, logistics improvements, technology diffusion, or learning-by-exporting that are not observable in the data used here. These potential spillovers could offset some of the distributional costs identified in this paper, but their magnitude and persistence remain an open question. Second, although I leverage up to a decade after FEZ implementation and document persistent medium-run effects, the paper is less informative about short-run adjustment dynamics. Disentangling temporary disruptions from longer-term reallocations would clarify the timing and incidence of adjustment costs. Third, the study cannot fully evaluate the trade channel. Export and import promotion is a central objective of FEZ policy, yet the available data do not allow me to quantify how FEZs reshape export varieties, sourcing patterns, or global market access. Understanding the extent to which these trade margins offset local adjustment costs remains an important direction for future work.

Fourth, the conceptual framework that organizes the analysis is deliberately simple and is not estimated. It abstracts from margins that the evidence itself suggests are active: capital, which the firm-level results show expanding alongside labor; migration and participation responses, which the population results hint at; unemployment and search frictions; the intensive margin of informality, for which the outsourcing results provide suggestive evidence; and imperfections in labor markets, even though the AKM results indicate that firm-specific wage premia matter for wage setting. Because the framework is not estimated, it signs the mechanisms but cannot quantify them. It establishes the conditions under which informality rises, and the data select that regime, but it cannot decompose the informality increase into its margins—incumbent contraction, firm exit, and entry—nor assess whether the estimated magnitudes of beneficiary expansion, incumbent exit, and informal absorption are mutually

consistent as a single equilibrium. Its key elasticities, most importantly the substitution between skill types, are taken from estimates for other settings rather than estimated on Colombian data.

These limitations point to a natural agenda. Estimating the framework—extended with capital–skill complementarity, elastic labor supply and migration, and a spatial dimension with commuting and firm relocation—would allow the reduced-form estimates to discipline counterfactual analysis. Because the model separates the incentives governing adoption (the profit-tax advantage) from those governing expansion (the operating advantage), an estimated version could ask which combination of instruments generates firm expansion with less displacement, and how the answer varies with local skill composition. It would also permit welfare analysis, including the fiscal cost of displacement: forgone revenues per formal job created, net of the payroll and income tax losses from workers displaced into informality. Despite these limitations, the evidence underscores that distributional channels are central to evaluating place-based policies in settings where informality remains pervasive. Future research combining microdata on supply chains, short-run dynamics, and trade outcomes with structural welfare analysis would deepen our understanding of the net benefits and trade-offs of spatial development strategies.

7 Conclusion

Place-based industrial policies are often justified by their potential to stimulate growth and employment in economically lagging areas. Yet industrialization in developing economies has increasingly failed to deliver employment: subsidized firms expand while jobs do not follow. This paper argues that part of the explanation lies in a constraint that is easy to overlook in economies with high unemployment and pervasive informality—labor may be abundant in aggregate yet scarce at the margin industrial policy demands. Colombia's Free Economic Zones (FEZs) illustrate the consequences: rather than producing broad-based development, they reshape local economies through the reallocation of resources between formal and informal firms and workers operating side by side.

Using rich administrative and survey data, I find that FEZs effectively benefit high-performing formal firms, which expand their scale and pay higher wages. However, these localized gains come at a cost. Expanding firms compete for a limited pool of skilled workers, raising the skill premium; as labor costs rise, less productive formal firms exit, and displaced workers are absorbed into the informal sector. The increase in informality does not arise from shifts in regional specialization or labor supply, but from the reorganization of production and employment within municipalities—and it is strongest precisely where the local workforce is most heavily low-skilled. Aggregate employment remains largely unchanged, masking deep compositional shifts that redistribute rents, labor, and capital within local markets.

The results highlight that the incidence of FEZ incentives is uneven: fiscal benefits and wage gains accrue to larger, more capital-intensive firms and skilled workers, while smaller firms and low-skilled labor face displacement pressures. FEZs thus enhance productivity and competitiveness among the top performers but reinforce inequality within local economies by

widening the gap between formal and informal sectors.

This paper offers a new perspective on the costs of place-based policies in developing countries. Prevailing explanations of jobless industrialization emphasize weak labor demand—capital-intensive technologies and distortions that hold hiring down; the evidence here points instead to the supply side, and to the skill composition of the workers that informality holds in reserve. Misallocation need not stem solely from geographic distortions: even when activity remains local, distortions can intensify if policies shift the balance between formal and informal sectors without addressing underlying frictions. Evaluating these interventions therefore requires attention to how they reallocate resources—not only across space, but among firms and workers within places. Future research should explore how complementary enforcement, training, and inclusion measures can offset these distributional effects and enhance the inclusiveness of spatial development strategies in economies where informality remains pervasive.

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Supplementary Material

A Free Economic Zones

A.1 Additional Institutional Details on Free Economic Zones

This appendix provides additional details on the regulatory requirements governing the declaration and operation of Free Economic Zones (FEZs) and the qualification of beneficiary firms.

Requirements for FEZ declaration. Beyond the requirements described in Section 2, applicants must document their legal right to use the proposed site and its compatibility with municipal land-use plans, environmental regulations, and public-service provision. Applications include property records and topographic information, as well as technical, economic, financial, and market feasibility studies supporting the *Plan Maestro de Desarrollo General*. The Plan Maestro establishes the investment, employment, infrastructure, and implementation commitments subsequently used to assess compliance with the approved project. Permanent FEZs must also be suitable for industrial, commercial, or service activities and fully enclosed before operations begin.

The investment and employment requirements differ across zone types and activities, as summarized in Table 1. Special Permanent FEZs (ZFPEs) generally must complete their new investment and employment commitments within three years of declaration. Industrial-goods projects initially require approximately USD 38.6 million in new investment and 150 direct formal jobs; each additional USD 5.9 million in investment reduces the employment requirement by 15 jobs, to a minimum of 50. Agroindustrial projects can qualify through either USD 19.3 million in investment or at least 500 workers. Service projects face a graduated schedule ranging from 500 direct jobs for investments of approximately USD 2.6–11.8 million to 150 jobs for investments above USD 23.7 million. Port projects require approximately USD 38.6 million in new investment, 20 direct formal jobs, and 50 associated jobs.[38]

Requirements for zone operators. Applicants for *Usuario Operador* status must be legally established in Colombia and have the operation of FEZs as their corporate purpose. They must demonstrate financial and operational capacity, provide evidence of experience in foreign trade and customs, submit financial statements and operating procedures, and establish the required bank or insurance guarantee. They must also have no outstanding enforceable obligations with DIAN, except those covered by a valid payment agreement, and are subject to external auditing once authorized. Operators of Permanent FEZs must hold liquid net worth of approximately USD 5.9 million; the corresponding requirement for operators of Special Permanent FEZs is

[38] Special provisions apply to projects involving pre-existing investments. These projects must have liquid net worth above approximately USD 38.6 million and undertake more than USD 178.2 million in new investment within five years, together with additional taxable-income requirements.

approximately USD 0.6 million.

In Permanent FEZs, the operator must remain institutionally independent from industrial and commercial users: it cannot simultaneously operate as an industrial user or be economically or corporately affiliated with firms established within the zone. Beyond administering the site and qualifying users, the operator is responsible for implementing the Master Development Plan and maintaining the systems used to monitor movements of goods and compliance with the customs regime.

Qualification of firms in Permanent FEZs. Firms seeking beneficiary status apply directly to the *Usuario Operador*. In addition to demonstrating the economic and financial viability of the proposed project, applicants must provide information on the ownership and origin of the capital associated with the project and obtain any environmental or sector-specific authorizations required for their activities. Qualified firms must also obtain certification of the procedures, infrastructure, and technology associated with their activities within two years of qualification.

Industrial users face investment and employment requirements that depend on their total assets (Table 1). Firms with assets up to approximately USD 0.13 million face no minimum investment or employment requirement; those with assets between USD 0.13 and 1.3 million must create at least 20 direct formal jobs; firms with assets between USD 1.3 and 7.7 million must undertake approximately USD 1.3 million in new investment and create 30 jobs; and firms with assets above USD 7.7 million must undertake approximately USD 3 million in new investment and create 50 jobs. These commitments are adjusted if the firm's assets change in the year following qualification.

Customs control and compliance. Qualified industrial and commercial users must maintain computerized permanent or continuous inventory systems integrated with their accounting records, allowing the zone operator and DIAN to monitor movements of goods into, out of, and within the FEZ. The physical infrastructure of Permanent FEZs must similarly accommodate customs supervision, including designated control and inspection areas. These requirements complement the physical enclosure and exclusivity provisions described in the main text by allowing the preferential regime to be administered at the level of the designated FEZ site.

Some activities are subject to specific exceptions. In service ZFPEs that do not involve cargo movements, for example, the declaration may cover several geographically delimited areas with approval from the Intersectoral Commission. Under specific conditions, employees may also perform activities outside the declared area through authorized telework arrangements.

Table 1: Statutory Requirements for FEZ Declaration and Firm Qualification

Panel A. Declaration Requirements: Permanent FEZs (ZFPs)

Requirement	Minimum requirement
Contiguous area	20 hectares
Industrial users	5 qualified users within five years of declaration
New investment	USD 11.8 million within five years of declaration, undertaken jointly by the zone operator and industrial users
Physical enclosure	Fully completed before operations begin
Operator liquid net worth	USD 5.9 million

Panel B. Declaration Requirements: Special Permanent FEZs (ZFPEs)

Project type	Minimum new investment	Minimum employment
Industrial goods	USD 38.6 million	150 direct formal jobs[a]
Agroindustry	USD 19.3 million	500 workers (alternative to investment)[b]
Services	USD 2.6–11.8 million	500 direct jobs
	USD 11.8–23.7 million	350 direct jobs
	Above USD 23.7 million	150 direct jobs
Ports	USD 38.6 million	20 direct formal jobs and 50 associated jobs
Pre-existing projects	USD 178.2 million within five years	Additional requirements apply[c]

Panel C. Firm Qualification Requirements within Permanent FEZs (ZFPs)

Firm assets	Minimum new investment	Minimum direct formal jobs
Up to USD 0.13 million	None	None
USD 0.13–1.3 million	None	20
USD 1.3–7.7 million	USD 1.3 million	30
Above USD 7.7 million	USD 3.0 million	50

Notes: The table summarizes the main statutory requirements for FEZ declaration and, in multi-user Permanent FEZs, the qualification of industrial users. Monetary thresholds are approximate U.S. dollar equivalents of statutory requirements expressed in units of the Colombian monthly minimum wage (SMMLV), using the 2010 monthly minimum wage of COP 515,000 and an exchange rate of COP 2,000 per U.S. dollar. Requirements may vary for specific activities and projects.

[a] Industrial-goods ZFPEs initially require 150 direct formal jobs. For each additional USD 5.9 million invested above the minimum threshold, the employment requirement is reduced by 15 jobs, to a minimum of 50. From the second year of operation onward, at least 90% of the required employment must be maintained.

[b] Agroindustrial projects may qualify either by investing at least USD 19.3 million or by employing at least 500 workers within three years of declaration.

[c] Pre-existing projects must have liquid net worth above approximately USD 38.6 million, undertake at least USD 178.2 million in new investment within five years, and satisfy additional taxable-income requirements.

A.2 International Comparison

Figure 1: Physical Design of Free Economic Zones in Colombia



(a) Delimited parcels within fenced perimeter



(b) Industrial park layout with controlled access

Notes: These images illustrate the physical structure of Free Economic Zones (FEZs) in Colombia. FEZs are spatially delimited areas with controlled entry points and clearly defined boundaries. Firms operate within fenced perimeters and are allocated specific parcels of land inside the zone. This enclosed design reflects the regulatory framework governing FEZs, where economic activity is geographically restricted and monitored. *Source:* Zona Franca La Cayena (https://zonafrancalacayena.com/); Analdex (https://analdex.org/tag/zona-franca-del-pacifico/).

Table A.2 provides a cross-country comparison of the economic importance of Free Economic Zones (FEZs) across Latin American and Caribbean countries. It shows that in several Central American economies, FEZs account for a substantial share of total exports and GDP, highlighting their role as key export platforms. In contrast, FEZs in Colombia represent a relatively small share of aggregate exports and economic activity.

Table A.2: Exports and GDP of AZFA Member Countries (2022)

Country	*Macroeconomic aggregates*			*FEZ contribution*		
	Population (thousands)	GDP (million USD)	Exports (million USD)	FEZ exports (million USD)	Share of exports (%)	Share of GDP (%)
Nicaragua	6,948	15,711	7,870	3,616	45.9	23.0
Honduras	10,433	31,710	13,394	5,000	37.3	15.8
El Salvador	6,336	32,189	10,164	3,052	30.0	9.4
Dominican Republic	11,229	113,621	25,169	7,108	28.5	6.3
Costa Rica	5,181	70,477	29,104	8,271	28.4	11.7
Uruguay	3,423	71,305	22,461	5,322	23.7	7.5
Colombia	**51,874**	**344,587**	**78,237**	**2,777**	**3.8**	**0.8**
Guatemala	17,944	95,648	11,184	580	5.2	0.6
Ecuador	18,001	115,049	35,943	277	0.8	0.2
Panama	4,109	76,523	35,021	143	0.4	0.2
Brazil	215,314	1,923,365	389,610	1,453	0.4	0.1
Peru	34,050	251,142	71,129	84	0.1	0.0

Notes: This table reports macroeconomic aggregates and the contribution of Free Economic Zones (FEZs) to exports and GDP across selected countries in 2022. Shares are expressed as percentages of total exports and GDP.
Source: Authors' calculations based on AZFA reports.

Table A.3 complements this evidence by comparing the fiscal incentives offered to firms operating inside and outside FEZs. While most countries provide generous tax exemptions within FEZs, Colombia combines a relatively large corporate tax differential with exemptions on VAT and tariffs. This contrast suggests that, despite strong fiscal incentives, the aggregate footprint of FEZs in Colombia remains limited relative to other countries in the region.

Table A.3: Tax Incentives in Free Economic Zones (FEZs)

	Corporate tax		*VAT*		*Tariffs*		*Dividends*	
Country	FEZ	Outside	FEZ	Outside	FEZ	Outside	FEZ	Outside
Argentina	35%	35%	0%	21%	0%	13.1%	—	—
Brazil	34%	34%	0%	16%	0%	8.6%	—	—
Colombia	**20%**	**35%**	**0%**	**19%**	**0%**	**6%**	**0%**	**0%**
Costa Rica	15%	30%	0%	13%	0%	5.6%	0%	15%
Cuba	0%	35%	0%	10%	0%	12.0%	0%	10%
Ecuador	10%	25%	0%	12%	0%	10.2%	0%	10%
El Salvador	0%	30%	0%	13%	0%	5%	0%	25%

Notes: This table compares tax treatment for firms operating inside and outside Free Economic Zones (FEZs). FEZ regimes typically include exemptions from VAT and import tariffs, along with reduced corporate income taxes.
Source: Authors' compilation based on AZFA reports.

A.3 Source Documents for Data Construction of Free Economic Zones

Figure A.3: Administrative Documents: Declaration of Free Economic Zones

NIT	NOMBRE ZONA FRANCA	CLASE DE ZONA FRANCA	TIPO DE USUARIO	DEPARTAMENTO	MUNICIPIO	Resolución de declaratoria	Resolución de prorroga	CIIU
900136638	Zona Franca Permanente Especial Cervecería del Valle S.A.	Permanente Especial	Usuario Operador	Valle del Cauca	Yumbo	Res. 4767 de 30 de Abril de 2007	Vacía	1103
900117087	Zona Franca Permanente Especial Biocombustibles Sostenibles del Caribe S.A.	Permanente Especial	Bienes y Servicios	Magdalena	Santa Marta	Res. 12771 de 30 de octubre de 2007	Res. 236 de 9 de febrero de 2022	1030
900104517	Zona Franca Permanente Especial Bio D Facatativá	Permanente Especial	Bienes y Servicios	Cundinamarca	Facatativá	Res. 13930 de 22 de noviembre de 2007	Res. 235 de 9 de febrero de 2023	2029
900150942	Zona Franca Permanente Especial Agroindustrias del Cauca	Permanente Especial	Bienes y Servicios	Cauca	Guachené	Res. 15748 de 20 de diciembre de 2007	Res. 1963 de 23 de diciembre de 2022	1071
900164755	Zona Franca Permanente Especial Argos	Permanente Especial	Bienes y Servicios	Bolívar	Cartagena	Res. 181 de 9 de enero de 2008	Res. 1065 del 21 de Julio de 2022	2394
900147693	Zona Franca Permanente Especial Ecodiesel Colombia	Permanente Especial	Bienes y Servicios	Santander	Barrancabermeja	Res. 5686 de 27 de Junio de 2008	Res. 1052 de 18 de julio de 2022	2029
900189181	Zona Franca Permanente Especial PEPSICO Alimentos Z.F.	Permanente Especial	Bienes y Servicios	Cundinamarca	Funza	Res. 6748 de 28 de Julio de 2008	Res. 745 de 23 de junio de 2023	1081
800084048	Zona Franca Permanente Especial de Servicios Marinilla	Permanente Especial	Servicios	Valle del Cauca	Buenaventura	Res. 10409 de 28 de Octubre de 2008	Vacía	5222
830036921	Zona Franca Permanente Especial Vidrio Andino	Permanente Especial	Bienes y Servicios	Cundinamarca	Soacha	Res. 121 de 11 de Noviembre de 2008	Res. 1338 de 16 de noviembre de 2023	2310
900112515	Zona Franca Permanente Especial Refinería de Cartagena	Permanente Especial	Bienes y Servicios	Bolívar	Cartagena	Res. 1397 de 10 de Diciembre de 2008	Res. 12304 del 16 de diciembre de 2015	1921
800187234	Zona Franca Permanente Especial de Servicios Sociedad Portuaria Regional de Santa Marta	Permanente Especial	Servicios	Magdalena	Santa Marta	Res. 1886 de 19 de Diciembre de 2008	Vacía	5222
800116164	Zona Franca Permanente Especial de Servicios CONTECAR	Permanente Especial	Servicios	Bolívar	Cartagena	Res. 2704 de 13 de Marzo de 2009	Vacía	5222
817000705	Zona Franca Permanente Especial Colombina del Cauca	Permanente Especial	Bienes y Servicios	Cauca	Santander de Quilichao	Res. 3285 de 31 de marzo de 2009	Res. 674 de 20 de mayo de 2022	1081
830113630	Zona Franca Permanente Especial Prime Termoflores S.A.S. E.S.P.	Permanente Especial	Bienes y Servicios	Atlántico	Barranquilla	Res. 3681 de 13 de abril de 2009	Res. 1107 de 27 de julio de 2022	3511
900260749	Zona Franca Permanente Especial TELEMARK SPAIN S.L. Sucursal Colombia	Permanente Especial	Servicios	Risaralda	Pereira	Res. 6387 de 17 de Junio de 2009	Res. 1346 de 16 de noviembre de 2023	8220
900261353	Zona Franca Permanente Especial Hospital San Vicente de Paul Rionegro	Permanente Especial	Servicios	Antioquia	Rionegro	Res. 7849 de 27 de Julio de 2009	Res. 981 de 9 de agosto de 2024	8610
900208445	Zona Franca Permanente Especial Cerámica San Lorenzo Industrial de Colombia	Permanente Especial	Bienes y Servicios	Cundinamarca	Sopo	Res. 12663 de 20 de noviembre de 2009	Res. 1607 de 12 de diciembre de 2024	2392
900248882	Zona Franca Permanente Especial Clínica Portoazul	Permanente Especial	Servicios	Atlántico	Puerto Colombia	Res. 13085 de 2 de diciembre de 2009	Res. 673 de 20 de mayo de 2022	8610
900251415	Zona Franca Permanente Especial SOFTYS Gachancipá	Permanente Especial	Bienes y Servicios	Cundinamarca	Gachancipá	Res. 13273 de 4 de diciembre de 2009	Res. 1567 de 6 de diciembre de 2024	1701
900304632	Zona Franca Permanente Especial ECONTACT	Permanente Especial	Servicios	Caldas	Manizales	Res. 14072 de 24 de diciembre de 2009	Res. 1615 de 12 de diciembre de 2024	8220

Notes: This figure shows examples of official documents declaring the creation of Free Economic Zones (FEZs) (*declaratoria*). These documents provide the administrative designation date of each zone.
Source: Ministry of Industry, Commerce, and Tourism of Colombia and regional development agencies.

Figure A.4: Administrative Documents: Firm Entry into Free Economic Zones

Zona Franca	Tipo de usuario	Ciiu	Fecha acto de calificación
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	21/01/2014
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5210 - Almacenamiento y depósito	14/04/2015
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND BIENES Y SERVICIOS	5229 - Otras actividades complementarias al transporte	30/03/2015
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND BIENES Y SERVICIOS	5224 - Manipulación de carga	29/02/2016
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	4/11/2016
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND BIENES Y SERVICIOS	5229 - Otras actividades complementarias al transporte	26/12/2016
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	24/01/2018
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	3099 - Fabricación de otros tipos de equipo de transporte ncp	14/08/2019
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	COMERCIAL	4663 - Comercio al por mayor de materiales de construcción, artículos de ferretería, pinturas, productos de vidrio, equipo y	12/06/2020
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	31/05/2021
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	30/10/2021
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5229 - Otras actividades complementarias al transporte	30/11/2021
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	24/03/2023
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	14/11/2023
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	12/03/2024
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5224 - Manipulación de carga	9/08/2024
CENTRO LOGISTICO DEL PACIFICO SA USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5229 - Otras actividades complementarias al transporte	6/12/2024
CENTRO LOGISTICO INDUSTRIAL DEL PACIFICO CLIP SAS USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	4112 - Construcción de edificios no residenciales	12/03/2020
CENTRO LOGISTICO INDUSTRIAL DEL PACIFICO CLIP SAS USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	4520 - Mantenimiento y reparación de vehículos automotores	29/03/2021
CENTRO LOGISTICO INDUSTRIAL DEL PACIFICO CLIP SAS USUARIO OPERADOR DE ZONA FRANCA	IND BIENES	2011 - Fabricación de sustancias y productos químicos básicos	19/04/2022
CENTRO LOGISTICO INDUSTRIAL DEL PACIFICO CLIP SAS USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	4290 - Construcción de otras obras de ingeniería civil	7/12/2023
CENTRO LOGISTICO INDUSTRIAL DEL PACIFICO CLIP SAS USUARIO OPERADOR DE ZONA FRANCA	IND SERVICIOS	5229 - Otras actividades complementarias al transporte	26/07/2024

Notes: This figure presents examples of documents recording the entry or allotment of firms within Free Economic Zones (FEZs). These records allow us to identify the timing of the first firm establishment in each zone. In the empirical analysis, we use the date of first firm entry as the treatment timing, as it more accurately captures the onset of economic activity relative to the administrative declaration. *Source:* Ministry of Industry, Commerce, and Tourism of Colombia and regional development agencies.

B Additional Descriptive Evidence

This appendix provides additional descriptive evidence on the FEZ setting, the municipalities and workers exposed to the expansion of the regime, and the firms operating in the formal sector.

B.1 Free Economic Zones

Figure A.5 documents the spatial expansion of the FEZ regime between 2005 and 2018. While zones were initially concentrated around a small number of economic centers, their geographic coverage expanded substantially following the 2005 reform. Figure A.6 shows the distribution of zones across departments, distinguishing between Permanent FEZs, which host multiple firms within industrial parks, and Special Permanent FEZs, which are designated for individual firms.

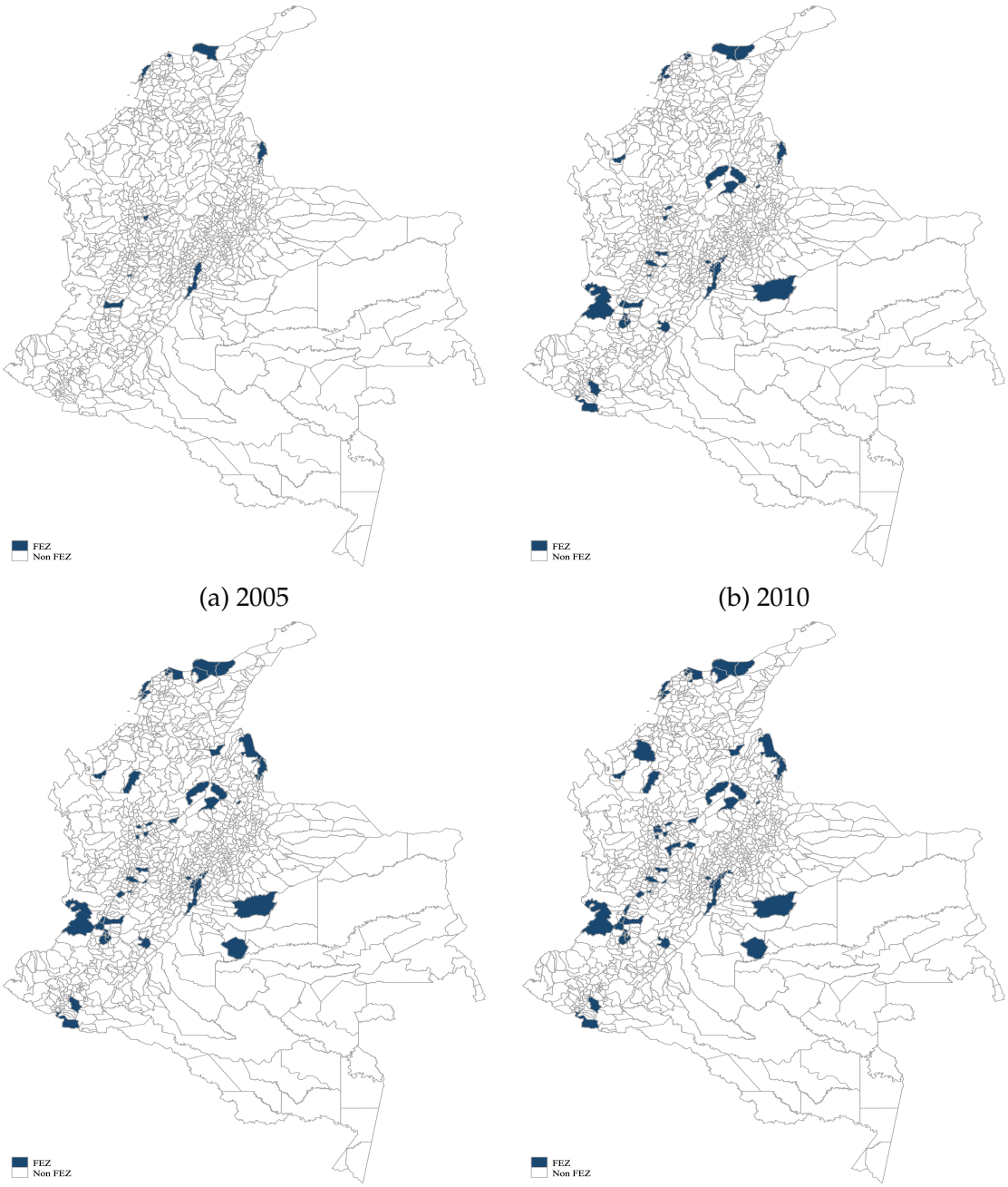
Table A.4 describes the 109 FEZs established during the period of analysis. Permanent zones host approximately 30 qualified firms on average, whereas Special zones are single-firm projects by construction. The two types also differ in the timing of firm entry: in Permanent zones, the first firm enters approximately 2.7 years after declaration on average, while entry in Special zones generally coincides with declaration. Both types are located relatively close to departmental capitals and national highways, although Permanent zones tend to be closer to both. Industry and services account for most qualified firms under either regime, with relatively limited participation in agroindustry.

Zone size varies substantially across FEZs, particularly among Special zones. Figure A.7 shows the distribution of FEZ area based on the hand-collected geographic boundaries. The average zone covers approximately 77 hectares, although the distribution is highly skewed because of a small number of considerably larger zones.

B.2 Municipalities and Local Labor Markets

Characteristics of FEZ municipalities. Table A.5 compares baseline economic and labor-market characteristics of municipalities that subsequently host a Free Economic Zone (FEZ) with those that do not, measured in 2005, the year of the FEZ reform. FEZ municipalities are larger and more economically active at baseline. They have higher population density and fiscal revenues, lower poverty rates, and are located closer to departmental capitals and major cities. Their labor markets also differ markedly: average wages are higher, employment is more concentrated in manufacturing and services, and the share of high-skilled workers is substantially larger (47.4% compared with 30.2%).

Two features of the baseline comparison are particularly relevant for the analysis. Informality is already higher in municipalities that subsequently host an FEZ (50.7% compared with 45.6%), as is unemployment (14.3% compared with 10.0%), while employment and participation rates are relatively similar. FEZs therefore tend to locate in distinct local economies, characterized by



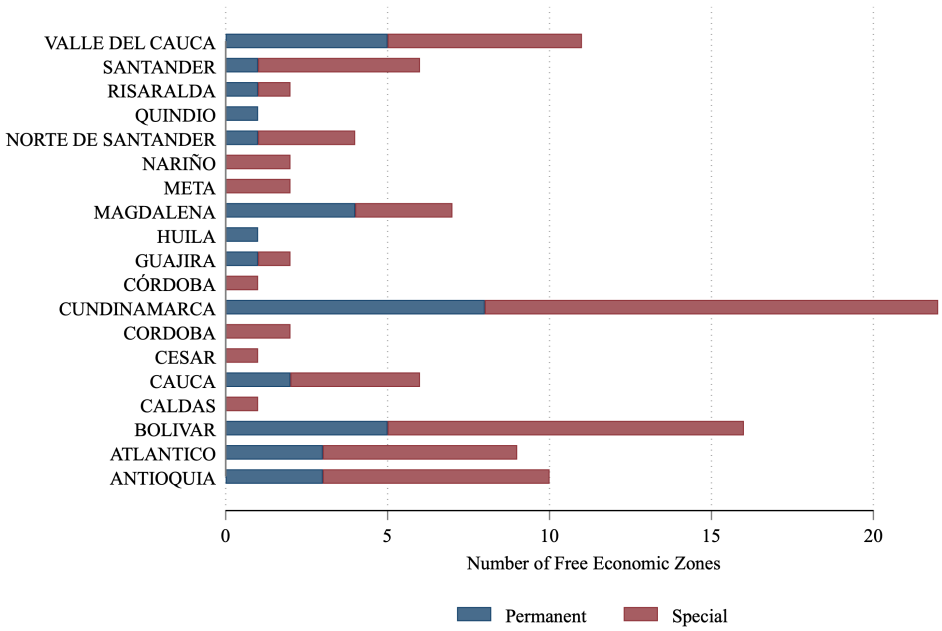
(a) 2005 (b) 2010

(c) 2015 (d) 2018

Figure A.5: Evolution of the Spatial Distribution of Free Economic Zones

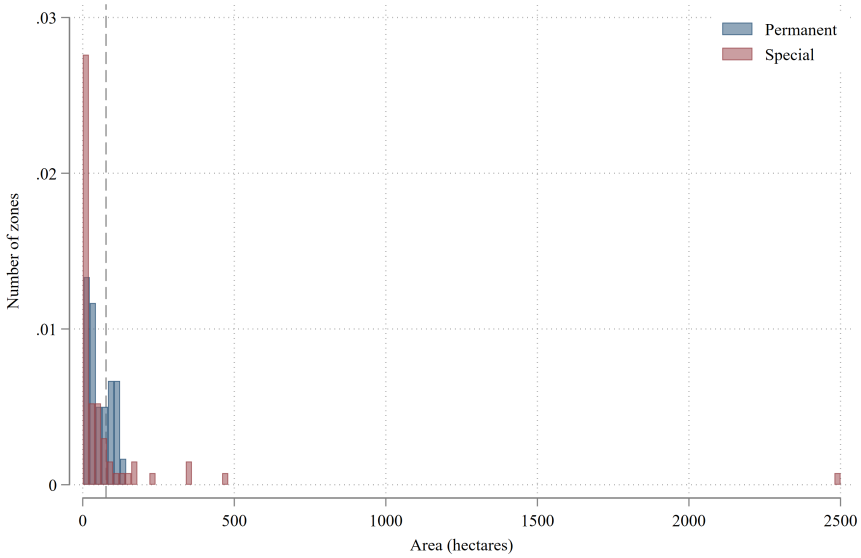
Notes: The figure shows the evolution of the spatial distribution of Free Economic Zones (FEZs) across Colombian municipalities between 2005 and 2018. *Source:* Ministry of Industry, Commerce, and Tourism of Colombia and location data collected by the author.

Figure A.6: Distribution of Free Economic Zones by Department



Notes: The figure shows the number of FEZs established between 2005 and 2018 by department, distinguishing between Permanent zones, which host multiple firms within industrial parks, and Special Permanent zones, which are designated for individual firms. Departments are sorted by the total number of zones. *Source:* Ministry of Industry, Commerce, and Tourism of Colombia.

Figure A.7: Distribution of Free Economic Zone Size



Notes: The figure shows the distribution of FEZ area, in hectares, for operating zones established between 2005 and 2018 for which geographic boundaries are available ($N = 97$). Area is computed from hand-collected zone boundary polygons using Colombia's official projected coordinate system (EPSG:9377). The distribution is highly skewed, with areas ranging from 0.1 to 2,496.3 hectares. Values above the 99th percentile are winsorized for visual clarity. The dashed vertical line indicates the overall mean. *Source:* Author's calculations based on hand-collected zone boundary data and administrative records from the Ministry of Industry, Commerce, and Tourism of Colombia.

Table A.4: Descriptive Statistics on Free Economic Zones

	Mean (Standard Deviation)		
	Permanent	Special	All Zones
Zone characteristics			
Area (hectares)	55.35	86.49	76.86
	(38.93)	(311.51)	(259.58)
Number of qualified firms	29.79	1.00	9.98
	(35.94)	(0.00)	(23.97)
Years from declaration to first firm entry	2.68	0.00	0.83
	(2.58)	(0.00)	(1.89)
Geographic location			
Distance to Bogotá (km)	394.39	398.37	397.14
	(273.97)	(248.07)	(254.92)
Distance to department capital (km)	31.34	41.83	38.59
	(25.62)	(34.16)	(32.01)
Distance to national highway (km)	1.21	3.90	3.07
	(1.26)	(6.76)	(5.78)
Sector composition of qualified firms (share)			
Industry	0.45	0.47	0.46
	(0.26)	(0.50)	(0.44)
Services	0.49	0.52	0.51
	(0.25)	(0.50)	(0.44)
Agroindustry	0.06	0.01	0.03
	(0.20)	(0.12)	(0.15)
Observations	34	75	109

Notes: Descriptive statistics for FEZs established in Colombia between 2005 and 2018. Means are reported with standard deviations in parentheses. *Permanent* zones host multiple firms within industrial parks; *Special* zones are designated for single-firm operations. Sector composition reports the share of qualified firms in each sector. Geographic distances are straight-line distances computed from zone centroids using Colombia's official projected coordinate system (EPSG:9377). *Source:* Author's calculations based on administrative records from the Ministry of Industry, Commerce, and Tourism of Colombia.

both substantial informality and a more skill-intensive workforce. These systematic pre-existing differences also make clear that FEZ location is not random; the empirical strategy therefore relies on variation in the timing of FEZ exposure rather than cross-sectional differences between FEZ and non-FEZ municipalities.

Table A.5: Baseline Economic and Labor Market Characteristics by FEZ Status (2005)

	Non-FEZ	FEZ	Total
Panel A. Economic Characteristics			
Population density (pop / ha)	1.064	7.999	1.392
	(5.264)	(14.305)	(6.170)
Total revenue (log)	8.586	10.500	8.677
	(0.775)	(1.637)	(0.929)
Expenditure per capita	252216.756	326309.727	255910.749
	(59550.335)	(83253.092)	(62998.496)
Poverty rate	0.517	0.430	0.513
	(0.104)	(0.125)	(0.106)
Distance to departmental capital (km)	82.309	61.754	81.338
	(60.130)	(64.235)	(60.458)
Distance to main city (km)	132.837	67.653	129.755
	(112.190)	(68.912)	(111.369)
Guerrilla attacks	0.350	0.480	0.356
	(1.042)	(0.875)	(1.035)
Panel B. Labor Market Composition and Outcomes			
Employment rate	0.532	0.509	0.528
	(0.086)	(0.035)	(0.080)
Unemployment rate	0.100	0.143	0.107
	(0.057)	(0.025)	(0.055)
Participation rate	0.590	0.594	0.591
	(0.080)	(0.044)	(0.076)
Informality rate (DANE)	0.456	0.507	0.464
	(0.130)	(0.096)	(0.126)
Average wage (log)	12.803	13.155	12.859
	(0.463)	(0.239)	(0.454)
Share of high-skill workers	0.302	0.474	0.330
	(0.148)	(0.139)	(0.159)
Share of low-skill workers	0.548	0.344	0.516
	(0.178)	(0.145)	(0.189)
Share of manufacturing employment	0.173	0.235	0.183
	(0.118)	(0.079)	(0.114)
Share of services employment	0.259	0.363	0.276
	(0.114)	(0.078)	(0.115)

Notes: Means and standard deviations by FEZ status. Sample restricted to baseline year (2005). Column 0 = Non-FEZ municipalities; Column 1 = FEZ municipalities.

Skill composition of formal and informal employment. Informality nevertheless remains an important feature of the local labor markets in which FEZs operate, and it is stratified along two dimensions that matter for what follows: worker education and employer size. Table A.6 reports formality rates across levels of educational attainment among employed workers in the GEIH. Informal work is disproportionately concentrated at lower levels of education: only 6.4 percent of workers with no formal education and 17.1 percent of those who completed only elementary school hold formal jobs, against 41.4 percent of high school graduates and 69.0 percent of workers with post-secondary education. A worker with post-secondary education is

therefore more than ten times as likely to be formally employed as a worker with none.

Table A.6: Formality rate by educational attainment

	Formality rate	95% CI
Preschool	0.120	[0.023, 0.217]
Elementary	0.171	[0.169, 0.173]
Middle	0.214	[0.212, 0.216]
High school	0.414	[0.412, 0.416]
Post-secondary	0.690	[0.689, 0.692]
Total	0.414	

Notes: GEIH pooled cross-sections, employed workers (N = 3,157,852). Weighted means using survey expansion factors. Formality defined as contributing to both pension and health social security. The preschool category contains 227 observations; its estimate is imprecise.

The same stratification appears along the employer dimension. Table A.7 shows that the formality rate rises from 8.5 percent among the self-employed and 20.7 percent in establishments of fewer than ten workers to 71.3 percent in establishments of eleven to fifty workers and 93.5 percent in the largest category the survey identifies. Formal employment is correspondingly concentrated at the top of the size distribution: establishments of fifty-one workers or more account for 62.4 percent of all formal employment, while the self-employed and the smallest establishments together account for 21.6 percent despite employing 58 percent of all workers.

Table A.7: Formality and firm size

	Formality rate	Share of formal employment (%)	N
Self-employed	0.085	6.99	1,200,000
	(0.0005)		
Micro	0.207	14.62	870,000
	(0.0007)		
Small	0.713	15.96	260,000
	(0.0015)		
Medium	0.935	62.44	830,000
	(0.0005)		
Total	0.414	100.00	3,158,343

Notes: GEIH pooled cross-sections, employed workers. Column 1 reports the weighted share of workers in each firm-size category who are formal (pension and health contributions); standard errors in parentheses. Column 2 reports the distribution of formal employment across firm-size categories. All statistics use survey expansion factors (fex_c_2011).

Earnings follow the same gradient. Table A.8 reports log earnings differences across establishment sizes within the formal sector; workers in the largest establishments earn 0.47 log points—roughly 60 percent—more than formal self-employed workers, and the gap is essentially unchanged when education, age and year are held constant, so it does not simply reflect the sorting of more educated workers into larger employers.

Table A.8: Firm-size wage premium among formal workers

	(1) Raw	(2) Conditional
Micro	0.263***	0.310***
	(0.020)	(0.018)
Small	0.317***	0.348***
	(0.019)	(0.022)
Medium	0.529***	0.470***
	(0.021)	(0.021)
Education controls		✓
Age and age squared		✓
Year fixed effects		✓
Observations	1,117,033	1,116,975
R^2	0.048	0.255

Notes: OLS estimates on the sample of formal workers. Dependent variable is the log of real monthly labor income (base January 2008). Omitted category is self-employed. Regressions weighted with survey expansion factors; standard errors clustered at the department level (24 clusters) in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

These patterns show that the large pool of workers outside formal employment differs substantially from the formal workforce, in skill composition and in the kind of employer that hires it. Informality implies substantial labor availability in the aggregate, but that availability is concentrated among the less educated and among the self-employed and the smallest establishments—precisely the segments in which formal employment is rare and earnings are lowest.

B.3 Firms

FEZ beneficiaries occupy a narrow and unusually large segment of the formal firm-size distribution. Two features of the Superintendencia de Sociedades data shape what can be shown. First, reporting is mandatory only above an asset or revenue threshold, so the panel is not a sample of Colombian firms but a census of large ones: Table A.9 shows that 99.4 percent of firm-years fall in the medium or large categories of Ley 905 de 2004 and that micro firms are absent entirely. Second, the data record balance-sheet and income-statement items but not employment, so firm size is measured by assets throughout and labor cost per worker cannot be computed.

Table A.10 compares firms that subsequently enter an FEZ with other firms in the same data, using characteristics measured in the year immediately preceding entry and differences conditional on calendar year. Entrants are substantially larger on every measure of scale: sales are roughly seven times, total assets roughly five times, and net property, plant and equipment roughly five times those of non-participating firms. Within the size distribution of Table A.9, 91 percent of their firm-years fall in the largest class, against 70 percent among other firms. They

Table A.9: Distribution of Firms by Size Class and FEZ Status

	Non-FEZ	FEZ	All
Micro	0.0	0.0	0.0
Small	2.2	0.3	2.2
Medium	28.0	8.4	27.9
Large	69.8	91.4	69.9
Total	100.0	100.0	100.0

Notes: Column percentages of firm-years in the Superintendencia de Sociedades panel. Size classes follow the asset bands of Ley 590 de 2000 as amended by Ley 905 de 2004, evaluated at the 2010 minimum wage and applied to assets deflated to 2010 pesos, so that class changes reflect real growth rather than annual minimum-wage adjustments. The absence of micro firms and the small share of small firms follow from the Superintendencia's reporting threshold.

are not, however, structurally different: the ratio of net fixed capital to total assets is 0.20 in both groups and leverage does not differ significantly. Beneficiaries are distinguished by scale rather than by capital intensity at the point of entry.

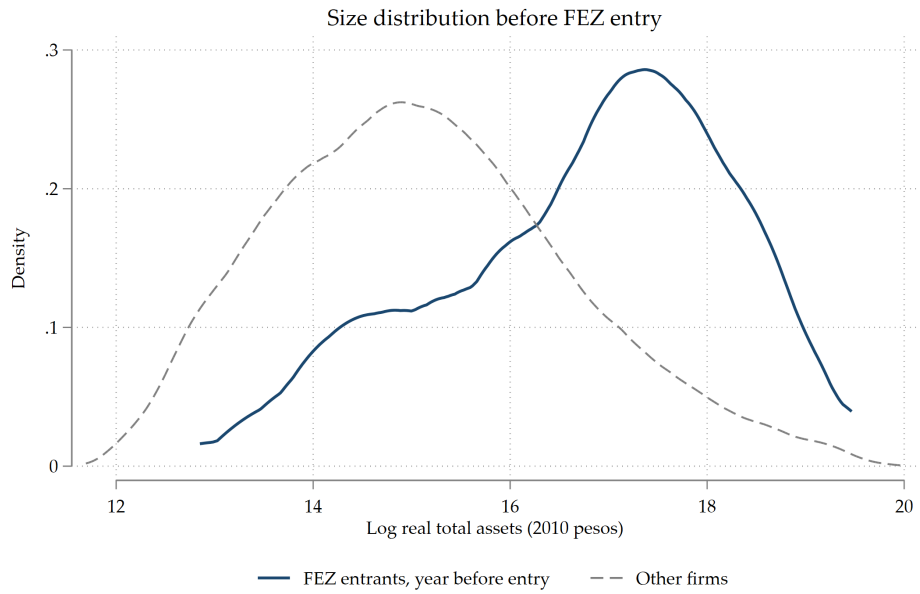
Table A.10: Characteristics of FEZ Entrants Before Entry

	Other firms	FEZ entrants (year before entry)	Difference
Log real sales	14.816	16.808	1.992***
Log real total assets	15.120	16.753	1.633***
Log real net PPE	12.808	14.461	1.653***
Net PPE / total assets	0.199	0.201	0.002
Liabilities / total assets	0.506	0.429	−0.077
Observations	270,480	56	270,536

Notes: Columns (1) and (2) report means. Column (3) reports the difference between FEZ entrants and other firms, estimated conditional on calendar-year fixed effects with standard errors clustered at the firm level. FEZ entrants are observed in the year immediately preceding entry; other firms are observed in the same calendar years. Monetary variables are deflated to 2010 pesos using the producer price index. The Superintendencia data do not record employment, so neither employment nor labor cost per worker is reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.8 shows the full distributions behind these means. The distribution of entrants is shifted markedly to the right, with a mode roughly two and a half log points above that of non-participating firms, but the two overlap substantially over the range in which most firms lie. Selection into the regime is therefore a shift along the size distribution rather than a separation into two distinct populations: entrants are drawn disproportionately, though not exclusively, from its upper tail.

Figure A.8: Size Distribution Before FEZ Entry



Notes: Kernel densities of log real total assets, deflated to 2010 pesos. The solid line covers FEZ entrants in the year immediately preceding entry; the dashed line covers all other firms observed in the Superintendencia de Sociedades panel in the same calendar years. Both distributions are truncated from below by the Superintendencia's reporting threshold. *Source:* Author's calculations using Superintendencia de Sociedades data.

C Derivations and Proofs

C.1 The unit labor cost index

A firm with status s minimizes labor costs,

$$\tilde{w}_1 l_1 + \tilde{w}_2 l_2,$$

subject to

$$\left(\eta_s l_1^{\rho} + (1-\eta_s) l_2^{\rho}\right)^{1/\rho} = \ell.$$

The first-order conditions imply

$$\frac{\tilde{w}_1}{\tilde{w}_2} = \frac{\eta_s}{1-\eta_s}\left(\frac{l_1}{l_2}\right)^{\rho-1},$$

and therefore

$$\frac{l_1}{l_2} = \left(\frac{\eta_s}{1-\eta_s}\right)^{\sigma}\left(\frac{\tilde{w}_2}{\tilde{w}_1}\right)^{\sigma} = \lambda_s\left(\frac{\tilde{w}_2}{\tilde{w}_1}\right)^{\sigma}, \qquad \sigma \equiv \frac{1}{1-\rho},$$

which gives (16). Substituting into the production constraint yields the conditional labor demands

$$l_1 = \ell\,\eta_s^{\sigma}\left(\frac{c_s}{\tilde{w}_1}\right)^{\sigma}, \qquad l_2 = \ell\,(1-\eta_s)^{\sigma}\left(\frac{c_s}{\tilde{w}_2}\right)^{\sigma},$$

where the unit labor cost index is

$$c_s(\mathbf{w}) = \left[\eta_s^{\sigma}\tilde{w}_1^{1-\sigma} + (1-\eta_s)^{\sigma}\tilde{w}_2^{1-\sigma}\right]^{\frac{1}{1-\sigma}},$$

as reported in (9).

By Shephard's lemma,

$$\mathrm{d}\ln c_s = s_1^s \mathrm{d}\ln \tilde{w}_1 + s_2^s \mathrm{d}\ln \tilde{w}_2,$$

where

$$s_1^s = \eta_s^{\sigma}\tilde{w}_1^{1-\sigma}c_s^{\sigma-1}, \qquad s_2^s = 1 - s_1^s.$$

The high-skill cost share s_1^s is increasing in η_s, so Assumption 1 implies

$$s_1^z \geq s_1^f > s_1^i.$$

Finally, because η_s changes the definition of the labor composite itself, c_s is the unit cost of a status-specific composite. Its level therefore does not have a direct economic interpretation across technologies with different η_s. Cost shares and the response of c_s to a common change in wages, by contrast, are directly comparable across statuses.

C.2 The profit function

Given the unit labor cost c_s, a registered firm chooses the labor composite to solve

$$\max_{\ell}\left\{(1-\tau_s)\theta\ell^{\alpha} - c_s\ell\right\}.$$

The first-order condition,

$$\alpha(1-\tau_s)\theta\ell^{\alpha-1} = c_s,$$

gives

$$\ell^* = \left[\frac{\alpha(1-\tau_s)\theta}{c_s}\right]^{\frac{1}{1-\alpha}}.$$

Substituting back into the objective function yields

$$\Pi_s = (1-\tau_s)\theta(\ell^*)^{\alpha} - c_s\ell^* = \left(\frac{1}{\alpha}-1\right)c_s\ell^* = (1-\alpha)\alpha^{\frac{\alpha}{1-\alpha}}\left[(1-\tau_s)\theta\right]^{\frac{1}{1-\alpha}}c_s^{-\frac{\alpha}{1-\alpha}},$$

which gives (10). The corresponding elasticities are

$$\frac{\mathrm{d}\ln\ell}{\mathrm{d}\ln(1-\tau_s)} = \frac{\mathrm{d}\ln l_1}{\mathrm{d}\ln(1-\tau_s)} = \frac{\mathrm{d}\ln l_2}{\mathrm{d}\ln(1-\tau_s)} = \frac{1}{1-\alpha}, \qquad \frac{\mathrm{d}\ln\Pi_s}{\mathrm{d}\ln(1-\tau_s)} = \frac{1}{1-\alpha}, \tag{27}$$

and

$$\frac{\mathrm{d}\ln\Pi_s}{\mathrm{d}\ln c_s} = -\frac{\alpha}{1-\alpha}, \qquad \frac{\mathrm{d}\ln\Pi_s}{\mathrm{d}\ln\theta} = \frac{1}{1-\alpha}. \tag{28}$$

Since the conditional demands in (9) are proportional to ℓ^*, while their relative proportions depend only on $(\tilde{w}_1, \tilde{w}_2, \eta_s)$, changes in τ_s or θ affect both skill types proportionally. Hence,

$$\frac{\mathrm{d}\ln(l_1/l_2)}{\mathrm{d}\ln(1-\tau_s)} = 0, \qquad \frac{\mathrm{d}\ln(l_1/l_2)}{\mathrm{d}\ln\theta} = 0,$$

which establishes Proposition 2.

For later use, combining the scale response with the conditional labor demands gives

$$\frac{\partial \ln l_2}{\partial \ln w_1} = s_1^s \left[\sigma - \frac{1}{1-\alpha}\right]. \tag{29}$$

The first term captures substitution toward low-skill labor when the skilled wage rises, while the second captures the contraction in firm scale. Thus, when $\sigma < 1/(1-\alpha)$, the scale effect dominates and demand for low-skill labor falls.

C.3 The regime premium and the adoption cutoff

Deriving Γ. From (7) and (10), for $s \in \{f, z\}$,

$$\pi_s(\theta) = (1-t_s)\kappa_0 \left[(1-\tau_s)\theta\right]^{\frac{1}{1-\alpha}} c_s^{-\frac{\alpha}{1-\alpha}} - \bar{c}_s.$$

Since $\theta^{1/(1-\alpha)}$ enters profits in the same way under both registered statuses, the FEZ profit differential can be written as

$$\pi_z(\theta) - \pi_f(\theta) = \underbrace{\kappa_0 \left[(1-t_z)(1-\tau_z)^{\frac{1}{1-\alpha}} c_z^{-\frac{\alpha}{1-\alpha}} - (1-t_f)(1-\tau_f)^{\frac{1}{1-\alpha}} c_f^{-\frac{\alpha}{1-\alpha}}\right]}_{\Gamma} \theta^{\frac{1}{1-\alpha}} - \Delta\bar{c},$$

which gives (11)–(12). Factoring out the ordinary-formal term yields

$$\Gamma = (1-t_f)\kappa_0 (1-\tau_f)^{\frac{1}{1-\alpha}} c_f^{-\frac{\alpha}{1-\alpha}} (\Psi - 1),$$

where Ψ is defined in (13). Multiplying by $\theta^{1/(1-\alpha)}$ and using (10) gives

$$\Gamma \theta^{\frac{1}{1-\alpha}} = (\Psi - 1)(1-t_f)\Pi_f(\theta).$$

Taking logs of Ψ gives (14). Hence $\Gamma > 0$ if and only if $\Psi > 1$. Since Ψ does not depend on θ, the FEZ regime provides a constant proportional operating gain, while the value of that gain in levels increases with productivity.

Remark (The technology term). The term

$$-\frac{\alpha}{1-\alpha} \ln\left(\frac{c_z}{c_f}\right)$$

in (14) reflects differences in the labor technologies associated with ordinary formal and FEZ status. Its level should be interpreted with caution because c_f and c_z are unit costs of different

CES composites. In particular, their ratio depends on the normalization used to compare technologies with different η_s. Rescaling one labor composite and adjusting productivity accordingly leaves production, input choices, and profits unchanged while changing the level of c_z/c_f.

This normalization issue does not affect the equilibrium comparative statics used in the analysis. Wage changes affect unit costs according to

$$\mathrm{d}\ln c_s = s_1^s \mathrm{d}\ln w_1 + s_2^s \mathrm{d}\ln w_2,$$

and the cost shares s_k^s are directly comparable across statuses. Thus, the equilibrium results rely on differences in the sensitivity of unit costs to wages rather than on the level comparison between c_z and c_f. The assumption $\Psi > 1$ simply requires that, under the chosen normalization, the fiscal advantages of FEZ status are sufficient to make adoption profitable for some registered firms.

The cutoff. A registered firm adopts FEZ status when

$$\frac{\pi_z(\theta) - \pi_f(\theta)}{\kappa} \geq E_z - E_f,$$

or, equivalently,

$$\Gamma\theta^{\frac{1}{1-\alpha}} \geq \kappa(E_z - E_f) + \Delta\bar{c}.$$

For $\Gamma > 0$, the left-hand side is strictly increasing in θ. Provided the cutoff lies in the support of the productivity distribution, the adoption condition therefore single-crosses at

$$\theta_z^{\frac{1}{1-\alpha}} = \frac{\kappa(E_z - E_f) + \Delta\bar{c}}{\Gamma},$$

which gives (15). The adoption set is the upper interval $[\theta_z, \infty)$, over which the FEZ branch of $\widetilde{V}_f$ in (3) dominates ordinary formal operation. This establishes the cutoff result in Proposition 1. ■

Comparative statics. From (15),

$$\mathrm{d}\ln\theta_z = -(1-\alpha)\mathrm{d}\ln\Gamma$$

for changes that leave the incremental participation cost unchanged. Since τ_z affects Γ through Ψ,

$$\frac{\mathrm{d}\ln\Gamma}{\mathrm{d}\ln(1-\tau_z)} = \frac{\Psi}{\Psi-1}\frac{1}{1-\alpha},$$

and hence

$$\frac{\mathrm{d}\ln\theta_z}{\mathrm{d}\ln(1-\tau_z)} = -\frac{\Psi}{\Psi-1} < 0.$$

A larger operating advantage therefore lowers the productivity threshold for FEZ adoption.

The skilled wage affects both the level of ordinary formal profits and the relative advantage of FEZ status. Using $\mathrm{d}\ln c_s / \mathrm{d}\ln w_1 = s_1^s$,

$$\frac{\mathrm{d}\ln \Gamma}{\mathrm{d}\ln w_1} = -\frac{\alpha}{1-\alpha} s_1^f + \frac{\Psi}{\Psi - 1}\frac{\alpha}{1-\alpha}\left(s_1^f - s_1^z\right),$$

which implies

$$\frac{\mathrm{d}\ln \theta_z}{\mathrm{d}\ln w_1} = \alpha\left[s_1^f + \left(s_1^z - s_1^f\right)\frac{\Psi}{\Psi - 1}\right] > 0,$$

under Assumption 1. The first term captures the decline in the level of registered-sector profits as the skilled wage rises. The second captures the additional erosion of the FEZ premium when FEZ production is more skill-intensive than ordinary formal production. A higher skilled wage therefore raises the adoption threshold through both channels.

C.4 Sorting on the informal–formal margin

By the envelope theorem applied to (5) and (6), operating profits under both informal and formal status are increasing in productivity. The difference between the two statuses is how labor costs vary with firm size. Registered firms face the constant effective wage $\tilde{w}_k$, whereas informal firms face the size-dependent expected cost of detection $\tau_i(\ell_i)$, which is increasing and convex. As productivity and optimal scale rise, informality therefore becomes progressively more costly relative to registration. Under the single-crossing property established in Ulyssea (2018), the relative value of formal operation is increasing in productivity,

$$\frac{\partial}{\partial \theta}\left[V_f(\theta, \mathbf{w}) - V_i(\theta, \mathbf{w})\right] > 0.$$

The same ordering carries over to the entry decision. Since $F(\theta \mid \nu)$ is ordered by first-order stochastic dominance in ν, a higher signal places greater probability on productivity realizations for which registration is relatively more valuable. Hence

$$V_f^e(\nu, \mathbf{w}) - V_i^e(\nu, \mathbf{w})$$

is increasing in ν. The entry decision therefore has a cutoff structure: potential entrants with $\nu < \bar{\nu}_i$ do not enter, those with $\nu \in [\bar{\nu}_i, \bar{\nu}_f)$ enter informally, and those with $\nu \geq \bar{\nu}_f$ register, with the thresholds determined by (4).

C.5 Relative demand and the wage equation

Using (16), aggregate high-skill employment can be written as

$$L_1 = \left(\frac{w_2}{w_1}\right)^{\sigma} \sum_s \lambda_s \int l_2^s \, \mathrm{d}\mu_s = \left(\frac{w_2}{w_1}\right)^{\sigma} \bar{\lambda} L_2,$$

where

$$\omega_s \equiv \frac{\int l_2^s \, \mathrm{d}\mu_s}{L_2}, \qquad \bar{\lambda} \equiv \sum_s \omega_s \lambda_s.$$

It follows that

$$\ln \frac{L_1}{L_2} = \ln \bar{\lambda} - \sigma \ln \frac{w_1}{w_2}.$$

Since skill supplies are fixed, $L_1/L_2 = \bar{L}_1/\bar{L}_2$ in every equilibrium. Subtracting the pre- and post-reform market-clearing conditions therefore gives

$$\Delta \ln \bar{\lambda} - \sigma \Delta \ln \frac{w_1}{w_2} = 0,$$

and hence

$$\Delta \ln \frac{w_1}{w_2} = \frac{\Delta \ln \bar{\lambda}}{\sigma},$$

which is (19). This relation is exact rather than a first-order approximation.

Because $\bar{\lambda} > 0$, $\Delta \ln \bar{\lambda}$ has the same sign as $\Delta \bar{\lambda}$. Moreover, since $\sum_s \Delta \omega_s = 0$,

$$\Delta \bar{\lambda} = \sum_s \lambda_s \Delta \omega_s.$$

Thus, the direction of the relative-wage response is determined by whether low-skill employment weight shifts toward statuses with higher or lower λ_s. FEZ adoption and beneficiary expansion shift weight toward z, while displacement into informality shifts weight toward i. Proposition 3 follows from the balance of these two compositional forces. Corollary 3.1 follows directly from (19): conditional on the equilibrium composition shift, the relative-wage response depends on the substitutability parameter σ, not on the level of the low-skill endowment. ■

Remark (What (19) is and is not). The composition term $\bar{\lambda}$ is itself an equilibrium object. The employment weights ω_s depend on firms' scale, the adoption cutoff θ_z, and the survival and entry margins, all of which respond to equilibrium wages. Equation (19) therefore does not solve for the wage response independently of the rest of the model. Rather, it isolates the relationship between changes in the composition of labor demand and relative wages, and clarifies the role of substitutability in mediating that response.

C.6 Incumbent response and the exit cutoff

Part (ii) of Proposition 4 follows directly from (28) and

$$\mathrm{d} \ln c_f = s_1^f \mathrm{d} \ln w_1 + s_2^f \mathrm{d} \ln w_2.$$

For part (iii), combining

$$\mathrm{d} \ln \ell = -\frac{1}{1-\alpha} \mathrm{d} \ln c_f$$

with the conditional demands in (9) gives

$$\begin{aligned} \mathrm{d}\ln l_2^f &= \left[\sigma - \frac{1}{1-\alpha}\right]\mathrm{d}\ln c_f - \sigma\,\mathrm{d}\ln w_2 \\ &= s_1^f\left[\sigma - \frac{1}{1-\alpha}\right]\mathrm{d}\ln w_1 - \left[\sigma s_1^f + \frac{s_2^f}{1-\alpha}\right]\mathrm{d}\ln w_2, \end{aligned} \tag{30}$$

$$\begin{aligned} \mathrm{d}\ln l_1^f &= \left[\sigma - \frac{1}{1-\alpha}\right]\mathrm{d}\ln c_f - \sigma\,\mathrm{d}\ln w_1 \\ &= -\left[\sigma s_2^f + \frac{s_1^f}{1-\alpha}\right]\mathrm{d}\ln w_1 + s_2^f\left[\sigma - \frac{1}{1-\alpha}\right]\mathrm{d}\ln w_2. \end{aligned} \tag{31}$$

The own-wage effects are negative for any $\sigma > 0$. The cross-wage effects satisfy

$$\frac{\partial \ln l_2^f}{\partial \ln w_1} = s_1^f\left[\sigma - \frac{1}{1-\alpha}\right], \qquad \frac{\partial \ln l_1^f}{\partial \ln w_2} = s_2^f\left[\sigma - \frac{1}{1-\alpha}\right],$$

and are therefore negative if and only if $\sigma < 1/(1-\alpha)$. Under this condition, the two skill types are gross complements in firm-level demand: an increase in the wage of either skill type reduces demand for both. The first of these expressions is the elasticity reported in Proposition 4(iii).

For part (iv), the zero-profit cutoff for ordinary formal firms satisfies

$$(1-t_f)\Pi_f(\bar{\theta}_f) = \bar{c}_f.$$

Using (10),

$$\bar{\theta}_f = \left[\frac{\bar{c}_f}{(1-t_f)\kappa_0}\right]^{1-\alpha}(1-\tau_f)^{-1}c_f^{\alpha},$$

so, holding the policy parameters and fixed operating cost constant,

$$\mathrm{d}\ln\bar{\theta}_f = \alpha\,\mathrm{d}\ln c_f.$$

An increase in the unit labor cost therefore raises the productivity required for ordinary formal operation. Firms with productivity between the old and new cutoffs earn negative profits and exit. Since optimal firm size is increasing in θ, exit is concentrated among the smallest formal firms. For a given flow of formal entrants, (8) then implies a decline in the mass of active ordinary formal firms. ■

Remark (The fixed cost of operation). I express $\bar{c}_f$ in units of the final good. Ulyssea (2018) instead indexes the fixed operating cost to the low-skill wage. This distinction matters here because the two skill-specific wages need not move in the same direction. Indexing $\bar{c}_f$ to w_2 would introduce an additional term into the response of the survival cutoff and could attenuate the exit effect when the low-skill wage falls. I therefore keep the fixed operating cost in final-good units and make this modeling choice explicit.

C.7 Wage movements under inelastic supply

Throughout this subsection I assume $\sigma < 1/(1-\alpha)$ and that the reform induces a positive mass of adopters.

Lemma 1 (Wages). *Suppose $\Delta\bar{\lambda} > 0$. Then the skill premium rises,*

$$\Delta \ln \frac{w_1}{w_2} > 0.$$

Moreover, if the expansion induced by FEZ adoption must be accommodated within the fixed local labor endowment, the equilibrium adjustment requires $\mathrm{d}\ln c_f > 0$ and hence $\mathrm{d}\ln w_1 > 0$.

Proof sketch. Relative market clearing immediately gives

$$\Delta \ln \frac{w_1}{w_2} = \frac{\Delta \ln \bar{\lambda}}{\sigma} > 0.$$

To establish the response of ordinary formal labor costs, consider first the case $\mathrm{d}\ln w_2 > 0$. The increase in the skill premium then implies $\mathrm{d}\ln w_1 > \mathrm{d}\ln w_2 > 0$, and therefore

$$\mathrm{d}\ln c_f = s_1^f \mathrm{d}\ln w_1 + s_2^f \mathrm{d}\ln w_2 > 0.$$

Now suppose $\mathrm{d}\ln w_2 \leq 0$. If, contrary to the claim, $\mathrm{d}\ln c_f \leq 0$, (30) implies that surviving ordinary formal firms weakly increase their demand for low-skill labor:

$$\mathrm{d}\ln l_2^f = \left[\sigma - \frac{1}{1-\alpha}\right] \mathrm{d}\ln c_f - \sigma \mathrm{d}\ln w_2 \geq 0,$$

where the inequality follows from $\sigma < 1/(1-\alpha)$. At the same time, FEZ adopters expand by Proposition 2. Thus, absent an offsetting contraction elsewhere, aggregate low-skill demand would increase against the fixed supply $\bar{L}_2$. In the equilibrium characterized here, this adjustment is accommodated through higher labor costs for ordinary formal firms, so $\mathrm{d}\ln c_f > 0$.

Finally, when $\mathrm{d}\ln w_2 \leq 0$,

$$\mathrm{d}\ln c_f = s_1^f \mathrm{d}\ln w_1 + s_2^f \mathrm{d}\ln w_2 > 0$$

requires $\mathrm{d}\ln w_1 > 0$. Hence the skilled wage rises and ordinary formal firms face a higher unit labor cost. □

Why condition (20) implies $\mathrm{d}\ln w_2 < 0$. Suppose (20) holds. Since $\bar{L}_2$ is fixed, the decline in registered low-skill employment must be matched by an increase in informal low-skill employment,

$$\Delta L_2^i = -\Delta\left(L_2^z + L_2^f\right) > 0.$$

By Lemma 1, $\mathrm{d}\ln w_1 > 0$. An increase in the skilled wage raises the cost of the skill mix used by informal firms and, absent an offsetting decline in w_2, cannot generate an increase in their

demand for low-skill labor. For informal employment to expand, the relative price of low-skill labor must therefore fall sufficiently to improve the cost position of informal production. In particular, the informal unit labor cost satisfies

$$\mathrm{d}\ln c_i = s_1^i \mathrm{d}\ln w_1 + s_2^i \mathrm{d}\ln w_2,$$

so a decline in c_i when $\mathrm{d}\ln w_1 > 0$ requires $\mathrm{d}\ln w_2 < 0$. The resulting fall in low-skill labor costs raises the scale and profitability of informal production and, through the entry and survival margins, allows the informal sector to absorb the low-skill employment released by registered firms. Hence condition (20) implies $\mathrm{d}\ln w_2 < 0$.

C.8 Sorting margins

Lemma 2 (Informal profits depend on wages only through c_i)**.** *Informal operating profits can be written as*

$$\Pi_i(\theta, \mathbf{w}) = \max_{\ell} \left\{ \theta \ell^{\alpha} - \tau_i(\ell) c_i(\mathbf{w}) \ell \right\},$$

where c_i is the CES unit labor cost index in (9), evaluated at raw wages (w_1, w_2) and skill weight η_i. Consequently, Π_i is strictly decreasing in c_i.

Proof. For any level of the labor composite ℓ, minimizing the wage bill $w_1 l_1 + w_2 l_2$ yields the CES conditional demands and minimized cost $c_i \ell$. Since $\tau_i(\ell)$ multiplies the entire wage bill, it does not affect the optimal skill mix conditional on ℓ. The problem therefore reduces to the one-dimensional program above. By the envelope theorem,

$$\frac{\partial \Pi_i}{\partial c_i} = -\tau_i(\ell_i^*)\ell_i^* < 0.$$

□

Proof of Proposition 6. Suppose $\mathrm{d}\ln c_i < 0$. For part (i), the informal zero-profit cutoff satisfies

$$\Pi_i(\bar{\theta}_i, \mathbf{w}) = \bar{c}_i.$$

Since Π_i is increasing in θ and decreasing in c_i by Lemma 2, a decline in c_i lowers the productivity required for informal operation, so $\bar{\theta}_i$ falls.

For part (ii), $V_i(\theta, \mathbf{w}) = \max\{0, \pi_i(\theta, \mathbf{w})/\kappa_i\}$ rises pointwise when c_i falls. Hence

$$V_i^e(\nu, \mathbf{w}) = \int V_i(\theta, \mathbf{w}) \, \mathrm{d}F(\theta \mid \nu)$$

also rises for every ν. Since V_i^e is increasing in ν, the free-entry condition $V_i^e(\bar{\nu}_i, \mathbf{w}) = E_i$ implies that $\bar{\nu}_i$ falls.

For part (iii), the formal-entry margin satisfies

$$V_f^e(\bar{\nu}_f, \mathbf{w}) - V_i^e(\bar{\nu}_f, \mathbf{w}) = E_f - E_i.$$

Write

$$V_f^e(\nu, \mathbf{w}) = V_{f,\text{ord}}^e(\nu, \mathbf{w}) + \Omega(\nu, \mathbf{w}),$$

where $V_{f,\text{ord}}^e$ is the expected value of remaining an ordinary formal firm and

$$\Omega(\nu, \mathbf{w}) = \int_{\theta_z}^{\infty} \left[\frac{\pi_z(\theta, \mathbf{w}) - \pi_f(\theta, \mathbf{w})}{\kappa} - (E_z - E_f) \right] \mathrm{d}F(\theta \mid \nu) \geq 0$$

is the value of the option to adopt FEZ status. The increase in c_f lowers $V_{f,\text{ord}}^e$, while the decline in c_i raises V_i^e. The adoption option works in the opposite direction. If θ_z lies sufficiently far in the right tail of $F(\cdot \mid \bar{\nu}_f)$, the probability that a marginal formal entrant eventually adopts is small, and so is the corresponding option-value effect. In that case, the value of formal relative to informal entry falls at the initial cutoff, and restoring the entry condition requires $\bar{\nu}_f$ to rise.

For part (iv), $M_i = [G(\bar{\nu}_f) - G(\bar{\nu}_i)]M$ rises when $\bar{\nu}_i$ falls and $\bar{\nu}_f$ rises. At the same time, the decline in $\bar{\theta}_i$ raises informal survival. Equation (8) therefore implies an increase in the stationary mass of informal firms. Conversely, formal entry falls and the formal survival cutoff rises by Proposition 4(iv), reducing the stationary mass of ordinary formal firms. ■

On the sign of $\mathrm{d}\ln c_i$**.** Condition (20) implies $\mathrm{d}\ln w_1 > 0 > \mathrm{d}\ln w_2$, but does not by itself determine the sign of the informal unit labor cost,

$$\mathrm{d}\ln c_i = s_1^i \mathrm{d}\ln w_1 + s_2^i \mathrm{d}\ln w_2.$$

The informal cost index falls if and only if

$$\frac{s_2^i}{s_1^i} > \frac{\mathrm{d}\ln w_1}{|\mathrm{d}\ln w_2|}.$$

Thus, the firm-sorting results above require informal production to be sufficiently low-skill intensive relative to the equilibrium wage movements. This condition is more likely to hold when s_1^i is small, as implied by $\eta_i < \eta_f$, but it is an additional restriction rather than a consequence of Proposition 5.

C.9 When does the reform formalize?

The same framework can also generate a decline in informality. The distinction between the two regimes is whether FEZ employment growth more than offsets the contraction of low-skill employment elsewhere in the registered sector.

Corollary 6.1 (The formalization regime)**.** *Suppose registered low-skill employment rises on net,*

$$\Delta L_2^z + \Delta L_2^{f,\text{intensive}} + \Delta L_2^{f,\text{exit}} > 0.$$

Then informal low-skill employment falls one for one, $\Delta L_2^i < 0$*. If the associated wage adjustment raises the unit labor cost of informal firms,* $\mathrm{d}\ln c_i > 0$*, informal survival and entry also decline. Ordinary formal incumbents may nevertheless contract and marginal formal firms exit if their unit labor costs rise.*

Proof. Since low-skill labor supply is fixed,

$$\bar{L}_2 = L_2^i + L_2^f + L_2^z,$$

and therefore

$$\Delta L_2^i = -\Delta\big(L_2^f + L_2^z\big).$$

Hence, if registered low-skill employment rises on net, informal low-skill employment necessarily falls by the same amount.

Suppose in addition that the associated wage adjustment raises the informal unit labor cost, $\mathrm{d}\ln c_i > 0$. By Lemma 2, informal operating profits fall at every productivity level. Since the informal zero-profit cutoff satisfies

$$\Pi_i(\bar{\theta}_i, \mathbf{w}) = \bar{c}_i,$$

and Π_i is increasing in θ, the cutoff $\bar{\theta}_i$ rises and informal survival falls. Likewise, $V_i(\theta, \mathbf{w})$ falls pointwise, so the expected value of informal entry $V_i^e(\nu, \mathbf{w})$ falls for every ν. The free-entry condition then implies that $\bar{\nu}_i$ rises, reducing informal entry.

These implications do not require ordinary formal firms to expand. If the equilibrium wage adjustment raises c_f, Proposition 4 continues to apply: ordinary formal incumbents contract and the formal survival cutoff rises. Formalization can therefore coexist with displacement within the registered sector, provided that FEZ employment growth is sufficiently large to dominate the contraction and exit of ordinary formal firms. □

Three margins determine which side of the condition in Corollary 6.1 the economy falls on. First, the skill intensity of FEZ production determines how strongly adoption shifts the composition of labor demand. When $\eta_z = \eta_f$, FEZ expansion does not alter the skill composition of demand within the registered sector, making it more likely to draw low-skill workers from informal employment rather than displace them from ordinary formal firms. As $\eta_z - \eta_f$ increases, the expansion becomes increasingly concentrated in skills that are relatively scarce in the informal labor reserve. Second, substitutability across skill types determines how this shift affects incumbent employment. Under $\sigma < 1/(1-\alpha)$, high- and low-skill labor are gross complements, so skilled-wage pressure reduces incumbents' demand for low-skill labor as well; greater substitutability weakens this displacement channel and makes reallocation from informal to registered employment more likely. Third, the mass of ordinary formal firms near the zero-profit cutoff determines the strength of the extensive-margin response. When many incumbents operate close to this threshold, a given increase in labor costs generates more exit

and a larger loss of registered low-skill employment; when incumbents are farther from the survival margin, the exit response is smaller.

The framework therefore admits both formalization and increased informality following the same policy shock. Beneficiary expansion alone does not determine which regime obtains. What matters for low-skill informality is whether the low-skill employment created by FEZ firms exceeds the low-skill employment displaced elsewhere in the registered sector. The sign of (20) is therefore an equilibrium outcome rather than an assumption of the framework.

C.10 Extension: elastic labor supply

The baseline model assumes that local labor supply is fixed. To relax this assumption, replace the fixed endowments with a quasi-linear household, $U = \sum_t \beta^t [x_t - \sum_k v_k(L_{kt})]$, with $v_k(L) = \bar{L}_k^{-1/\varepsilon} L^{1+1/\varepsilon}/(1+1/\varepsilon)$. Labor supply is then isoelastic,

$$L_k(w_k) = \bar{L}_k w_k^{\varepsilon},$$

with $\varepsilon = 0$ recovering the baseline model. Quasi-linearity eliminates income effects, while assuming a common elasticity across skill types keeps relative supply log-linear in the relative wage, just as relative demand is under the CES technology. With skill-specific elasticities, relative supply would also depend on the wage level, but this does not change the mechanism of interest. I interpret ε as a reduced-form elasticity of local labor supply. Allowing labor supply to respond to wages attenuates the relative-wage response without changing its logic. Combining relative labor supply with (17) gives

$$\Delta \ln \frac{w_1}{w_2} = \frac{\Delta \ln \bar{\lambda}}{\sigma + \varepsilon}.$$

The expression remains exact: labor-supply responsiveness simply joins substitutability across skill types in absorbing a shift in the composition of labor demand. As ε increases, a larger share of FEZ expansion can be accommodated through additional labor supply and less through relative wages and displacement. For sufficiently small ε, the wage and cost responses in Lemma 1, and hence the contraction mechanism in Proposition 4, continue to hold by continuity.

What changes more directly is the accounting behind Proposition 5. With fixed supply, any decline in registered low-skill employment must be absorbed one for one by the informal sector. With elastic supply, low-skill employment also responds to w_2, so this identity no longer holds exactly. Informality rises when the contraction in registered low-skill employment is large enough to dominate the labor-supply response. This additional margin is of order ε and disappears as $\varepsilon \to 0$.

Total employment is likewise no longer fixed:

$$\mathrm{d} \ln L = \varepsilon\big(\omega_1 \mathrm{d} \ln w_1 + \omega_2 \mathrm{d} \ln w_2\big),$$

where ω_k is skill group k's share of employment. In the displacement regime, $\mathrm{d} \ln w_1 > 0 >$

$d \ln w_2$, so the two terms work in opposite directions. The sign of the aggregate employment response is therefore not determined analytically. Because low-skill workers account for most employment, however, a sufficiently large fall in w_2 can generate a small decline in total employment even as FEZ firms expand. The model can therefore accommodate substantial reallocation across firms and sectors with little change in aggregate employment. The fixed-employment result in the baseline model is the limiting case as $\varepsilon \to 0$.

C.11 Omitted channel: the intensive margin of informality

The baseline framework abstracts from the intensive margin of informality within registered firms. In Ulyssea (2018), a registered firm can hire workers off the books at a cost $\tau_{fk}(l_k)w_k$ that is increasing and convex in the number of informal workers hired, while a formally hired worker costs $(1+\tau_w)w_k$. The switching threshold therefore satisfies

$$\tau'_{fk}(\bar{l}_k) = 1 + \tau_w,$$

so the wage cancels and $\bar{l}_k$ is independent of w_k.

This margin has two implications for the mechanism studied here. First, a firm employing more than $\bar{l}_k$ workers of skill type k hires $\bar{l}_k$ of them informally. Its within-firm informality share is therefore $\bar{l}_k/l_k$, which rises mechanically when the firm contracts. Ordinary formal incumbents can thus respond to the FEZ-induced increase in labor costs not only by reducing employment or exiting, as in Proposition 4, but also by increasing the share of their remaining workers hired off the books. This generates an additional channel through which worker informality can rise without requiring a change in the firm's registration status.

Second, with the estimated enforcement-cost functions in Ulyssea (2018), the threshold is higher for low-skill workers, $\bar{l}_2 > \bar{l}_1$, so this adjustment is concentrated among the less educated. Introducing the intensive margin would therefore provide an additional mechanism reinforcing the increase in low-skill worker informality characterized in Proposition 5. It would also further separate worker and firm informality: worker informality may rise even if the number or share of informal firms does not. I abstract from this margin to keep the mechanism transparent and because my data do not allow me to distinguish whether displaced formal workers are absorbed by informal firms or through informal employment within registered firms.

D Treatment Design and Identification

D.1 Treatment Cohorts

Table B.11 reports the distribution of GEIH municipalities across treatment cohorts. The 31 treated municipalities span 11 cohorts between 2005 and 2017. The largest cohorts are 2005, 2008, and 2010, each comprising 5 to 6 municipalities, which correspond to the period of most rapid FEZ expansion following the enactment of Act 1004 of 2005 and its implementing decree

of 2007. Later cohorts are smaller, reflecting the gradual saturation of the FEZ program in GEIH-covered areas. Two cohort years within the study period—2006 and 2012—have no treated municipalities in the GEIH sample, indicating that FEZs declared in those years were located in municipalities not covered by the survey. The 307 never-treated municipalities provide a broad and heterogeneous comparison group spanning all major regions of Colombia.

Table B.11: Treatment Cohorts: GEIH Municipalities by Year of First FEZ

Cohort (year of first FEZ)	Municipalities	Share of treated (%)
2005	6	19.4
2007	2	6.5
2008	5	16.1
2009	4	12.9
2010	5	16.1
2011	1	3.2
2013	3	9.7
2014	1	3.2
2015	1	3.2
2016	2	6.5
2017	1	3.2
Treated subtotal	31	100.0
Never treated	307	–
Total	**338**	

Notes: Each cohort corresponds to the calendar year in which the first Free Economic Zone (FEZ) was declared in a municipality. The sample is restricted to the 338 municipalities covered by the *Gran Encuesta Integrada de Hogares* (GEIH) labor force survey, defined as municipalities with at least one non-missing observation of the working-age population (*Población en Edad de Trabajar*, PET). Municipalities in the *Never treated* group did not host a FEZ at any point during the study period (2000–2018) and serve as the comparison group in the difference-in-differences estimation. Cohort sizes range from 1 to 6 municipalities.

D.2 FEZ Placement and Timing

Table B.12 examines the determinants of both the location and timing of Free Economic Zone (FEZ) implementation.

Panel A focuses on the likelihood that a municipality ever receives a FEZ. The results indicate that larger municipalities are significantly more likely to be selected: a one-unit increase in log population is associated with a 4.2 to 9.0 percentage point higher probability of FEZ placement across specifications. In addition, municipalities located farther from Bogotá are more likely to receive a FEZ, suggesting that zones are disproportionately placed outside the main economic center. Other geographic variables, such as distance to the departmental capital or to the nearest city, are generally not statistically significant once controls are included. The rural index becomes positive and significant in the full specification, indicating that, conditional on other characteristics, FEZs tend to be located in more rural areas.

In contrast, pre-existing labor market conditions display little systematic relationship with

Table B.12: Determinants of FEZ Location and Declaration Timing

	Geography	Lagged outcomes	Full
Panel A. Ever receives a FEZ (Probit marginal effects)			
Geographic and structural characteristics			
Log population	0.042**		0.090**
	(0.019)		(0.042)
Distance to department capital (km)	-0.000		-0.001
	(0.000)		(0.001)
Distance to Bogotá (km)	0.001**		0.001
	(0.000)		(0.000)
Distance to nearest city (km)	-0.001		0.000
	(0.000)		(0.001)
Rural index	-0.061		0.386**
	(0.061)		(0.191)
Lagged labor market outcomes (2000–2004)			
Employment rate		2.275	1.726
		(10.178)	(4.583)
Unemployment rate		3.299	1.939
		(6.026)	(2.644)
Informality rate		-0.322	-0.295
		(0.359)	(0.213)
Activity rate		-3.090	-2.722
		(9.190)	(4.176)
Log wage		0.383**	0.030
		(0.151)	(0.085)
Observations	338	338	338
Panel B. Timing of FEZ declaration (OLS)			
Geographic and structural characteristics			
Log population	200.790***		306.003***
	(40.547)		(66.065)
Distance to department capital (km)	-0.449		0.449
	(0.592)		(1.022)
Distance to Bogotá (km)	1.100**		0.822
	(0.509)		(0.884)
Distance to nearest city (km)	-1.596*		-2.287*
	(0.830)		(1.336)
Rural index	-117.405		728.365**
	(176.635)		(294.377)
Lagged labor market outcomes (2000–2004)			
Employment rate		4988.702	11969.5
		(17066.9)	(17385.2)
Unemployment rate		5306.183	8688.797
		(9954.5)	(9896.3)
Informality rate		-1322.6*	-572.878
		(685.840)	(676.327)
Activity rate		-5667.4	-13330.8
		(15689.5)	(16025.5)
Log wage		175.174	57.630
		(193.862)	(167.310)
Observations	338	338	338
R-squared	0.333	0.281	0.405

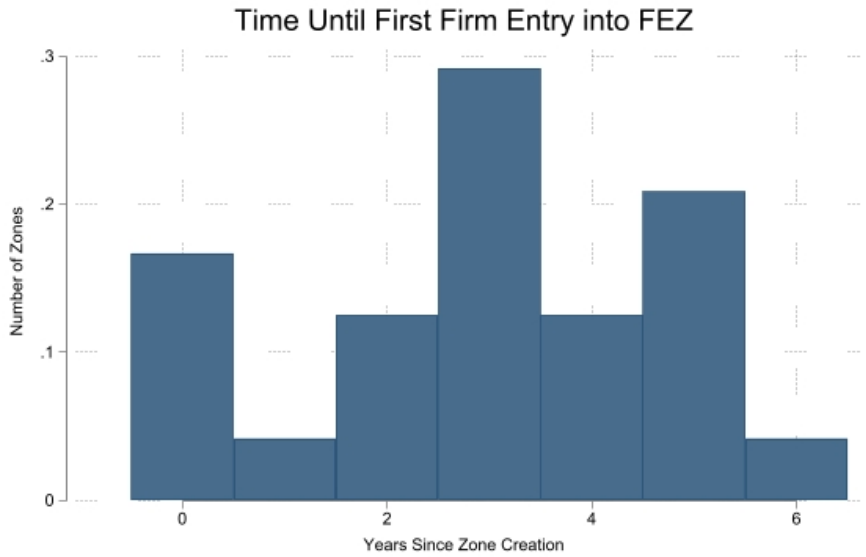
Notes: Panel A reports average marginal effects from a probit model of an indicator for ever receiving a Free Economic Zone (FEZ). Panel B reports OLS estimates of the year of first FEZ declaration. All covariates are measured as pre-treatment averages over 2000–2004. All specifications include department fixed effects. Robust standard errors are reported in parentheses.
Geographic characteristics—particularly population size and distance to Bogotá—are consistently associated with both FEZ location and timing, whereas pre-existing labor market conditions display limited explanatory power once geographic controls are included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

FEZ placement. While higher pre-treatment wages are positively associated with FEZ selection when only labor market variables are included, this effect disappears once geographic controls are added. Other labor market indicators—employment, unemployment, informality, and activity rates—are imprecisely estimated and not statistically significant. Overall, these results suggest that FEZ placement is not driven by prior labor market performance.

Panel B turns to the timing of FEZ declaration. A similar pattern emerges. More populous municipalities tend to receive FEZs earlier, with large and precisely estimated coefficients across specifications. Municipalities located farther from major cities experience delays in implementation, consistent with the idea that infrastructure and connectivity constraints slow down the establishment of zones. As in Panel A, lagged labor market outcomes are generally not predictive of timing once geographic controls are included.

These patterns indicate that both the location and timing of FEZ implementation are primarily driven by structural and geographic factors—such as population size and spatial location—rather than by pre-existing labor market trends. This alleviates concerns that FEZs were systematically targeted to municipalities on differential economic trajectories before treatment.

Figure B.9 complements this analysis by examining the delay between FEZ authorization and the entry of the first firm. The distribution is centered around relatively short delays, with most zones receiving their first firm within two to four years of authorization. At the same time, there is substantial dispersion, with some zones taking up to six years to attract their first firm. This heterogeneity underscores that the formal authorization of a FEZ does not necessarily coincide with the onset of economic activity. Instead, there is often a lag reflecting the time required for infrastructure development, regulatory compliance, and firm-level investment decisions. This distinction motivates the use of first firm entry as a more economically meaningful measure of treatment timing.

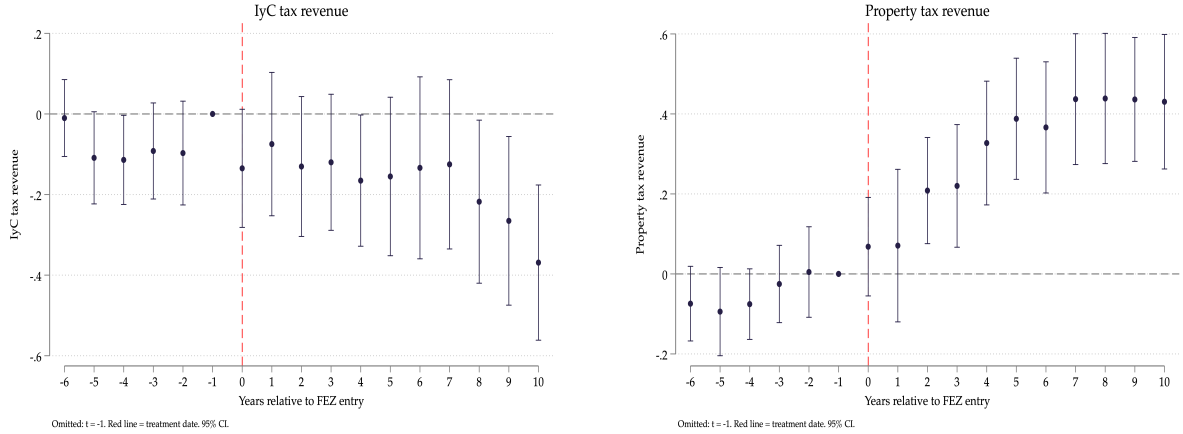


Notes: This figure displays the distribution of the number of years between FEZ authorization and the entry of the first qualified firm, for all operating zones declared after 2005. Each observation corresponds to one zone. A delay of zero indicates that the first firm was qualified in the same year as the zone's declaration.
Source: Ministry of Industry, Commerce, and Tourism of Colombia; firm qualification records compiled by the author.

Figure B.9: Distribution of Delay Between FEZ Authorization and First Firm Entry

E Additional results

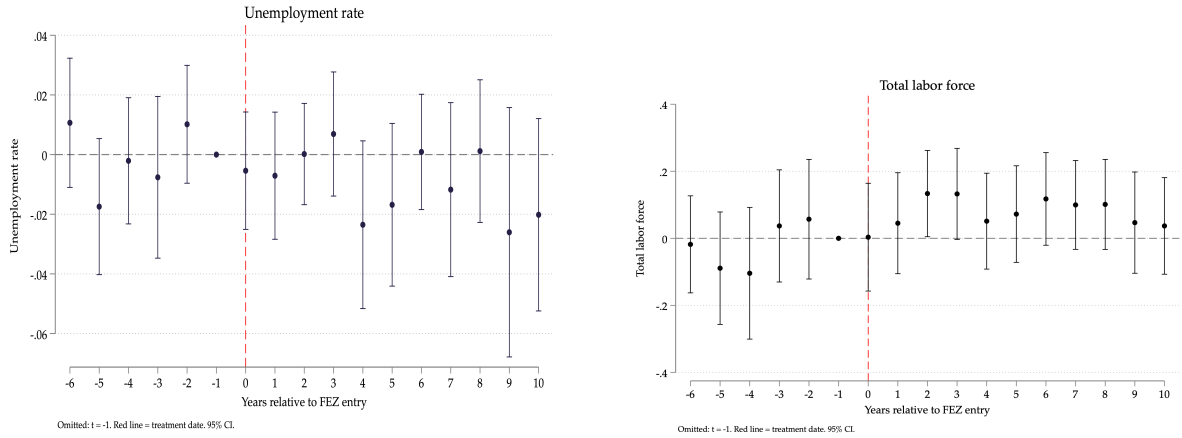
E.1 Municipalities



(a) Industry and commerce tax (IyC) (b) Property tax (predial)

Figure C.10: Effects of FEZs on municipal tax revenues

Notes: Panels C.10a and C.10b display event-study coefficients and 95% confidence intervals from specification (1). The outcome variables are the logarithm of municipal tax revenues from the industry and commerce tax (IyC) and the property tax (predial), respectively. Coefficients can be interpreted as approximate percentage changes. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using administrative fiscal data and FEZ information.



(a) Unemployment rate

(b) Labor force participation

Figure C.11: Effects of FEZs on labor market participation and unemployment

Notes: Panels C.11a and C.11b display event-study coefficients and 95% confidence intervals from specification (1). The outcome variables are the unemployment rate and the labor force participation rate, respectively. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using GEIH (LFS) and FEZ information.

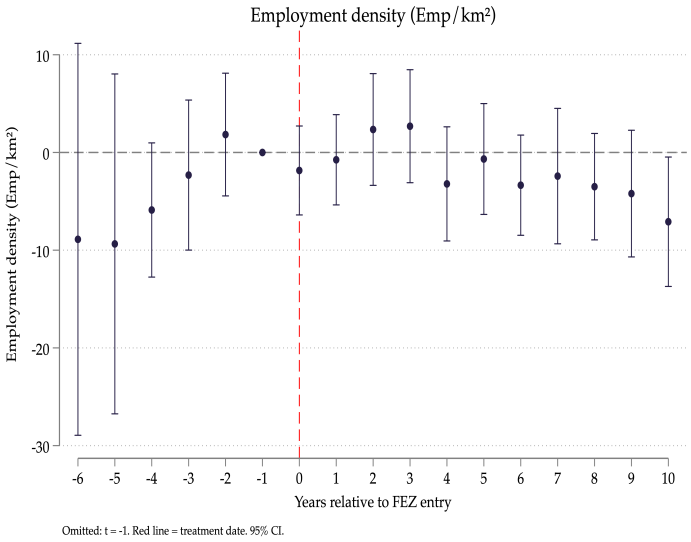
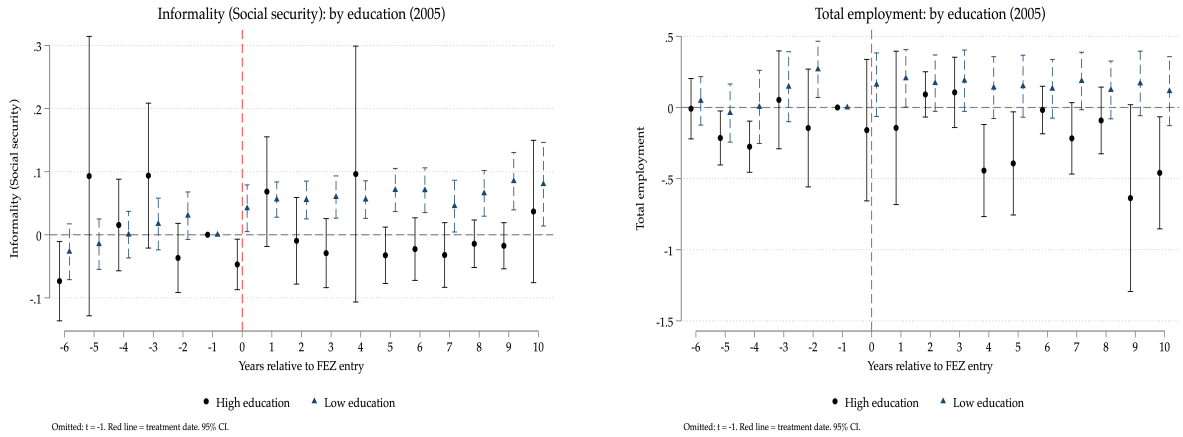


Figure C.12: Effects of FEZs on the employment-to-population ratio

Notes: This figure displays event-study coefficients and 90% confidence intervals from specification (1). The outcome variable is the employment-to-population ratio, defined as the share of employed individuals over the working-age population. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using GEIH (LFS) and FEZ information.



(a) Informality rate

(b) Employment-to-population ratio

Figure C.13: Heterogeneous effects of FEZs by baseline skill composition

Notes: Panels C.13a and C.13b display event-study coefficients and 90% confidence intervals from specification (1), allowing for heterogeneous effects by baseline skill share at the municipality level. The outcome variables are the informality rate and the employment-to-population ratio, respectively. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using GEIH (LFS) and FEZ information.

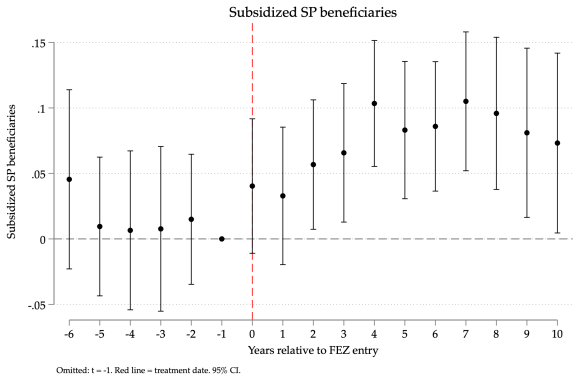


Figure C.14: Event-study estimates of the share of the municipal population registered in SISBEN following FEZ implementation.

Notes: Figure C.14 displays event-study coefficients and 95% confidence intervals from specification (1). The outcome is the share of the municipal population registered in SISBEN, the administrative registry used to target subsidized social protection, employed here as a proxy for the local prevalence of informality that does not depend on the GEIH sampling design. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated).
Source: Authors' calculations using SISBEN and FEZ information.

E.2 Workers

In the baseline analysis, informality is defined using workers' self-reported contributions to the social security system for health care and pensions. To better understand the mechanisms underlying the aggregate increase in informality, I next examine how FEZ exposure affects specific margins of informal employment, rather than introducing an alternative definition. Table C.13 reports difference-in-differences estimates for distinct components of informality, including self-employment, unpaid work, lack of social security contributions, and firm registration status. Consistent with the baseline results, FEZ exposure is associated with an overall increase in informality of approximately 2 percentage points, concentrated among low-skilled workers; the differential effects for medium-skilled (-6.2 percentage points) and high-skilled workers (-4.2 percentage points) offset nearly all of the increase for these groups. To further pinpoint the margins driving this shift, Table C.14 decomposes informality into its underlying components. The results show that the increase in informality is primarily driven by declines in business registration, higher incidence of unpaid work, and lower rates of social security contributions among low-skilled workers. Together, these findings indicate that while FEZs facilitate formalization for skilled workers along several dimensions, they simultaneously expand more precarious forms of work among the less skilled. The decline in business registration is consistent with the sorting margin of the framework (Proposition 6), in which the same wage adjustment that reallocates workers toward informality also makes informal operation relatively more attractive for marginal firms.

Table C.13: Regression Results: Informality Components

	Self-employment		Official		Unpaid	
	(1) Baseline	(3) Skill interactions	(1) Baseline	(3) Skill interactions	(1) Baseline	(3) Skill interactions
FEZ	-0.0201***	-0.000266	0.0204***	0.0628***	0.000739	0.00995***
	(-11.14)	(-0.12)	(11.46)	(29.39)	(0.95)	(10.60)
Medium-skilled		-0.0756***		-0.0219***		0.000479
		(-70.56)		(-20.86)		(1.04)
High-skilled		-0.240***		-0.190***		-0.0320***
		(-188.24)		(-152.41)		(-57.97)
FEZ×Medium-skilled		-0.0396***		-0.0617***		-0.0164***
		(-23.03)		(-36.57)		(-22.16)
FEZ×High-skilled		-0.00428*		-0.0420***		-0.00530***
		(-2.26)		(-22.65)		(-6.49)
Constant	0.365***	0.249***	0.740***	0.620***	0.334***	0.313***
	(133.06)	(92.31)	(274.21)	(233.92)	(281.77)	(268.52)
Observations	1,908,735	1,910,463	1,908,735	1,910,463	1,888,846	1,890,243
R^2	0.137	0.140	0.0977	0.105	0.0899	0.0913
F	24,488.4	17,734.7	8,154.7	7,339.4	17,638.5	12,677.7

Notes: *t*-statistics in parentheses. Controls: migration status, age, age squared, number of children under 12, marital status, head of household status, education, and gender. All specifications include municipality and year fixed effects. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.14: Regression Results: Social Security, Accounts, and Registration

	Social Security		Accounts		Registration	
	(1) Baseline	(3) Skill interactions	(1) Baseline	(3) Skill interactions	(1) Baseline	(3) Skill interactions
FEZ	0.000222	0.0270***	-0.000850	0.0258***	-0.0126**	-0.0311***
	(0.13)	(13.64)	(-0.35)	(9.17)	(-2.67)	(-5.70)
Medium-skilled		-0.134***		-0.0422***		-0.133***
		(-138.10)		(-33.60)		(-55.21)
High-skilled		-0.452***		-0.154***		-0.313***
		(-390.92)		(-92.28)		(-96.09)
FEZ×Medium-skilled		-0.0431***		-0.0158***		0.0214***
		(-27.61)		(-7.12)		(5.04)
FEZ×High-skilled		-0.0167***		-0.0649***		0.0366***
		(-9.76)		(-25.19)		(7.35)
Constant	1.417***	1.188***	1.147***	1.064***	1.227***	1.113***
	(558.27)	(478.75)	(293.17)	(279.70)	(152.05)	(141.12)
Observations	1,872,191	1,873,829	362,848	363,154	234,331	234,587
R^2	0.246	0.259	0.121	0.133	0.290	0.291
F	46,430.4	36,397.9	2,963.2	2,598.7	2,575.3	1,837.4

Notes: t-statistics in parentheses. Controls: migration status, age, age squared, number of children under 12, marital status, head of household status, education, and gender. All specifications include municipality and year fixed effects. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

E.3 Firms

E.3.1 Financial Statement

Table C.15 examines whether FEZ certification or exposure affects firm exit from the *Superintendencia de Sociedades* reporting panel. I consider three margins: the direct effect on treated firms (column 1), local spillovers on firms operating in the same municipality and 4-digit sector (column 2), and sectoral spillovers on firms in the same 4-digit sector located in other municipalities (column 3). Across all specifications, the estimated coefficients are small and statistically insignificant. This indicates that neither FEZ certification nor exposure to FEZs—whether within the same local market or within the same sector across space—systematically affects the probability of exiting the reporting panel. These results alleviate concerns that the main findings are driven by selective attrition or compositional changes in the sample following FEZ implementation.

Table C.15: Attrition Check: FEZ Certification and Panel Exit

	Exit from reporting panel		
	Direct effect	Municipality × sector	Sector (other municipalities)
FEZ certification	0.118		
	(0.116)		
FEZ in municipality (same 4-digit sector)		-0.0007	
		(0.0009)	
FEZ in other municipality (same 4-digit sector)			0.0061
			(0.0108)
Observations	1,887	899	1,449
R^2	0.382	0.393	0.120
Mean Dep. Var.			
Controls	X	X	X
Firm FE	X	X	X
Year × Sector FE	X	X	X

Notes: The dependent variable equals one in a firm's final observed year (defined as a gap of at least two consecutive missing years) and zero otherwise; observations after exit are excluded. The unit of observation is the firm-year. Column (1) reports the direct effect of FEZ certification. Column (2) captures within-market spillovers for firms operating in the same municipality and 4-digit sector. Column (3) captures sectoral spillovers for firms in the same 4-digit sector located in other municipalities. Specifications include firm fixed effects and year × sector fixed effects, and control for firm age, ownership status, multinational status, and pre-policy assets interacted with year. Standard errors are clustered at the municipality × 4-digit sector level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.16: Regression Results: Firm Assets, Liabilities, and Costs

	Balance Sheet					Costs
	Total Assets	Current Assets	Non-current Assets	Liabilities	Equity	Total Costs
FEZ	0.274***	0.184*	0.510***	0.429***	0.241**	0.393**
	(0.0792)	(0.105)	(0.140)	(0.110)	(0.106)	(0.153)
Observations	204,611	184,386	184,064	204,514	201,334	176,471
R^2	0.931	0.849	0.805	0.849	0.913	0.893
Mean Dep. Var.	15.34	14.44	13.75	14.24	14.54	14.87
Controls	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Sector FE	X	X	X	X	X	X

Notes: *t*-statistics in parentheses. All variables are deflated using industry-specific yearly Producer Price Index and expressed in logarithms. The control group are non-FEZ firms. Specifications control for pre-policy assets × year, firm age, and ownership type, as well as firm, year, and sector fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

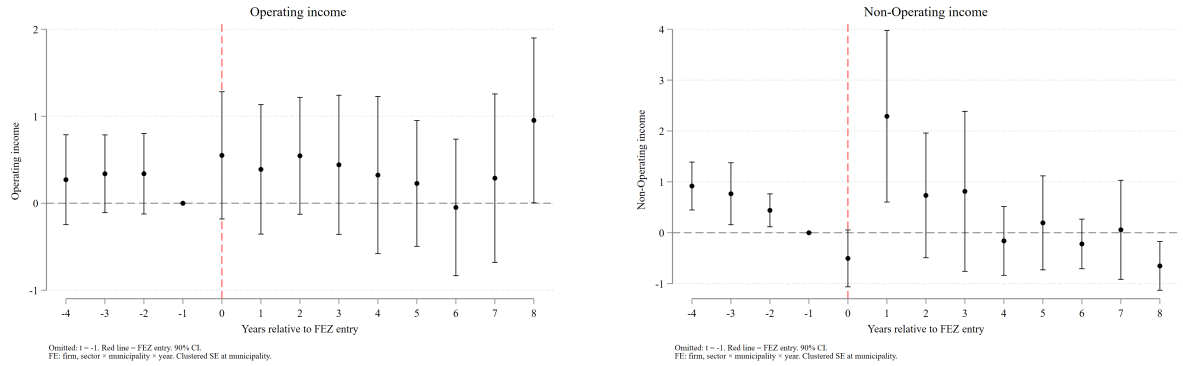
Table C.17: Regression Results: Firm Assets and Composition

	Net PPE	Raw Materials	Finished Goods	Land	Real Estate for Sale	Capital Intensity	Investments
FEZ	0.633***	0.683*	1.360***	0.00135	0.0401	0.238**	-0.808
	(0.182)	(0.399)	(0.473)	(0.0128)	(0.0595)	(0.117)	(0.561)
Observations	203,407	80,417	73,528	153,318	153,286	193,832	169,694
R^2	0.812	0.898	0.896	0.801	0.768	0.792	0.657
Mean of Dep. Variable	12.44	7.042	6.069	0.345	0.353	2.304	5.270
Controls	X	X	X	X	X	X	X
Firm FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Sector FE	X	X	X	X	X	X	X

Notes: t-statistics in parentheses. All variables are deflated using industry-specific yearly Producer Price Index and expressed in logarithms. The control group are non-FEZ firms. Specifications control for pre-policy assets × year, firm age, and ownership type, as well as firm, year, and sector fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Event-study results. Figure C.15 and Figure C.16 report the event-study dynamics of firm-level outcomes around FEZ qualification. The patterns are broadly consistent with the difference-in-differences estimates presented in Tables 3 and 4. Operating income shows no clear post-treatment increase, with coefficients remaining close to zero throughout the post-treatment period. By contrast, net income before taxes rises sharply following FEZ qualification, with effects that remain positive. A similar pattern is observed for non-operating income, which increases immediately after treatment.

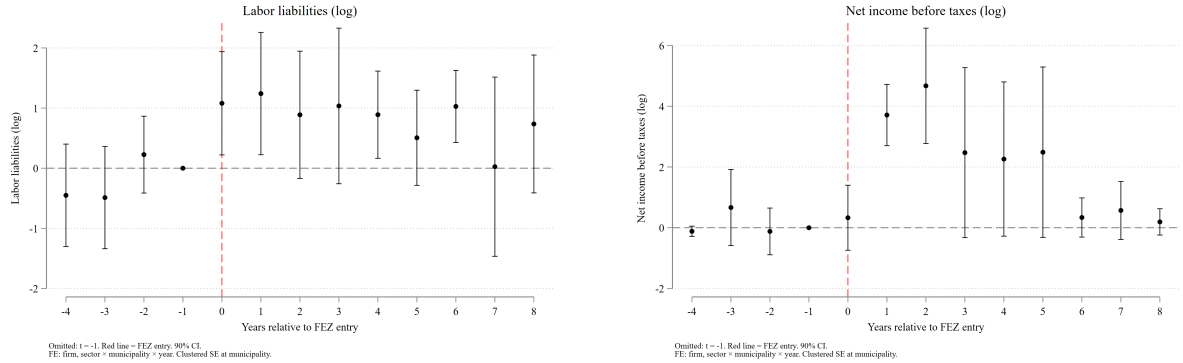
Labor costs also increase markedly after FEZ qualification. The wage bill rises sharply in the years following treatment, with no evidence of pre-trends, indicating that the increase is closely tied to FEZ designation. The absence of a corresponding increase in operating income suggests that these higher labor costs are not offset by gains in core production outcomes.



(a) Operating income (b) Non-operating income

Figure C.15: Effects of FEZ qualification on firm income components

Notes: Panels C.15a and C.15b display event-study coefficients and 90% confidence intervals from a two-way fixed effects specification with firm fixed effects and sector × municipality × year fixed effects. The outcome variables are log operating income and log non-operating income, respectively, deflated using sector-specific producer price indices. The treatment indicator equals one from the year of first FEZ qualification. The omitted period is $t = -1$. Standard errors are clustered at the municipality level. *Source:* Authors' calculations using firm-level financial data and FEZ information.



(a) Wage bill (labor costs)

(b) Net income before taxes

Figure C.16: Effects of FEZ qualification on firm labor costs and profits

Notes: Panels C.16a and C.16b display event-study coefficients and 90% confidence intervals from a two-way fixed effects specification with firm fixed effects and sector × municipality × year fixed effects. The outcome variables are log wage bill (labor liabilities) and log net income before taxes, respectively, deflated using sector-specific producer price indices. The treatment indicator equals one from the year of first FEZ qualification. The omitted period is $t = -1$. Standard errors are clustered at the municipality level. *Source:* Authors' calculations using firm-level financial data and FEZ information.

E.3.2 Micro-businesses

Now, I focus on the impact of FEZs on micro-businesses. The baseline estimating equation is:

$$Y_{fmt} = \alpha_{fmt} + \beta FEZ_{mt} + +\theta \mathbf{X}_{fmt} + \lambda_t + \delta_m + +\epsilon_{fmt} \tag{32}$$

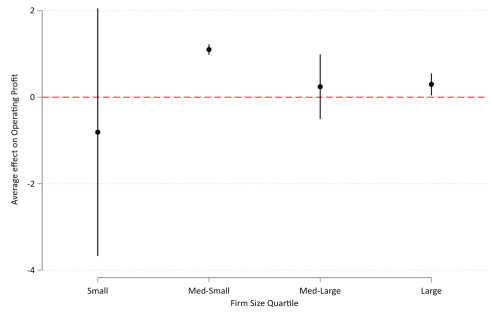
where Y_{fmt} represents the outcome for firm f at event time t. The term FEZ_{mt} includes dummies for each event time t and municipality. The term X contains a full set of firm controls.

I find that FEZs significantly increase the likelihood of being an informal firm by 3.03% at the extensive margin (Table C.18), indicating a rise in the number of informal micro-businesses being established or operating in FEZ-municipalities.

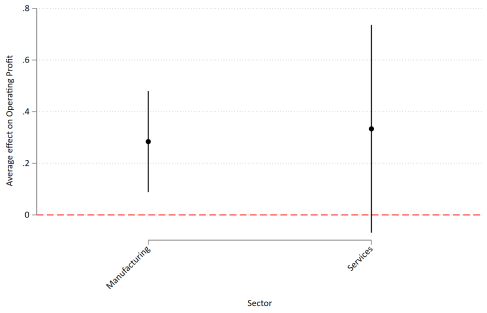
To account for heterogeneous effects across the formality status of the firm, I split the sample into two groups: formal firms (registered businesses) and informal firms (unregistered businesses). The results suggest that FEZs have no significant effect on total employment for informal or formal micro-businesses (Table C.20). However, unpaid informal work increases significantly by 10.1% for informal micro-businesses, indicating that the rise in economic activity spurred by FEZs is accompanied by greater reliance on unpaid labor in the informal sector. This may reflect a growing use of family or community labor to support operations in informal enterprises. The lack of significant effects on formal employment indicates that, despite fostering market activity, FEZs may reinforce informality in micro-businesses rather than promoting their transition into the formal economy.

In terms of sales and productivity (Table C.19), FEZs lead to significant increases in sales for both informal and formal micro-businesses. Sales rise by 9.95 million COP for informal businesses and 18.57 million COP for formal businesses, with an average increase of 16.58

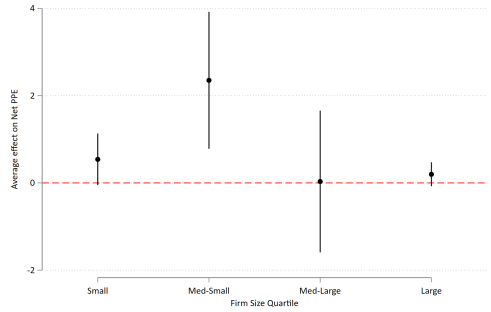
Figure C.17: Anatomy of the FEZ effects on firm performance, by size and sector.



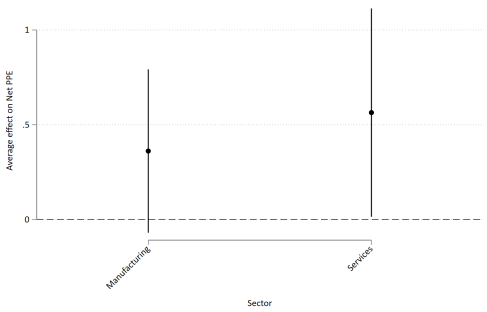
(a) By size



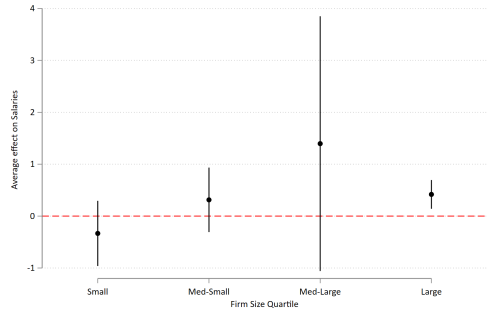
(b) By sector



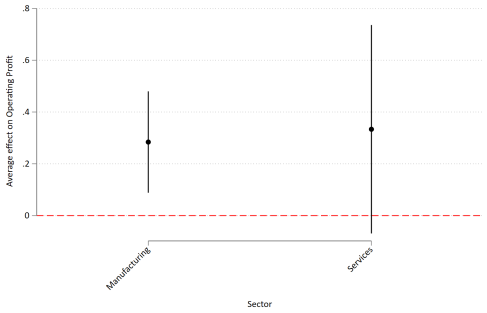
(c) By size



(d) By sector



(e) By size



(f) By sector

Notes: Panels (a)–(b) display the effects of FEZs on firms' operating income. Panels (c)–(d) show the effects on firms' capital (proxied by net property, plant, and equipment). Panels (e)–(f) report the effects on labor costs and liabilities. Each pair compares heterogeneous responses by firm size (left panels) and by sector (right panels). All effects are estimated using firm-level data and reported relative to pre-treatment periods.

million COP across all businesses. This highlights how FEZs create demand spillovers that benefit micro-businesses across the formal-informal divide, likely through heightened economic activity and improved access to customers and supply chains. Additionally, FEZs improve overall productivity by 13.6%, with gains observed for both informal and formal businesses. These findings suggest that FEZs enhance economic output and efficiency, benefiting businesses regardless of their formal status.

Table C.18: Regression Results: Extensive Margin

	Extensive Margin
FEZ	0.0303***
	(5.25)
Constant	0.243***
	(5.16)
R^2	0.757
Adjusted R^2	0.649
Observations	79,576
F-statistic	19.92

Notes: *t*-statistics in parentheses. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.19: Regression Results: Sales and Productivity

	Sales			Productivity		
	Informal	Formal	All	Informal	Formal	All
Variable	(1)	(2)	(6)	(1)	(2)	(6)
FEZ	9,948,866.4***	18,571,729.3***	16,580,711.6***	0.142	0.134**	0.136**
	(6.93)	(5.56)	(6.58)	(2.08)	(3.97)	(3.49)
Constant	33,003,527.6***	99,424,787.4***	80,288,901.1***	16.34***	17.02***	16.84***
	(31.09)	(40.41)	(43.12)	(323.22)	(681.83)	(583.21)
R^2	0.662	0.711	0.708	0.652	0.626	0.638
Adjusted R^2	0.511	0.601	0.605	0.496	0.483	0.510
Observations	24,105	70,862	101,601	24,105	70,862	101,601
F-statistic	47.98	30.89	43.35	4.331	15.73	12.18

Notes: *t*-statistics in parentheses. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table C.20: Regression Results: Total Employment and Informal Workers (Unpaid)

	Total Employment			Informal Workers (Unpaid)		
Variable	Informal (1)	Formal (2)	All (6)	Informal (1)	Formal (2)	All (6)
FEZ	0.0116	0.0565	0.0507	0.101**	0.0142	0.0363
	(0.77)	(1.44)	(1.69)	(3.12)	(0.37)	(1.03)
Constant	1.561***	2.442***	2.194***	1.038***	0.908***	0.947***
	(140.78)	(84.64)	(99.10)	(43.59)	(32.08)	(36.50)
R^2	0.701	0.775	0.769	0.485	0.500	0.482
Adjusted R^2	0.568	0.689	0.688	0.255	0.308	0.299
Observations	24,105	70,862	101,601	24,105	70,862	101,601
F-statistic	0.593	2.081	2.865	9.737	0.136	1.068

Notes: t-statistics in parentheses. Robust standard errors clustered at the municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

E.4 Social Security Data

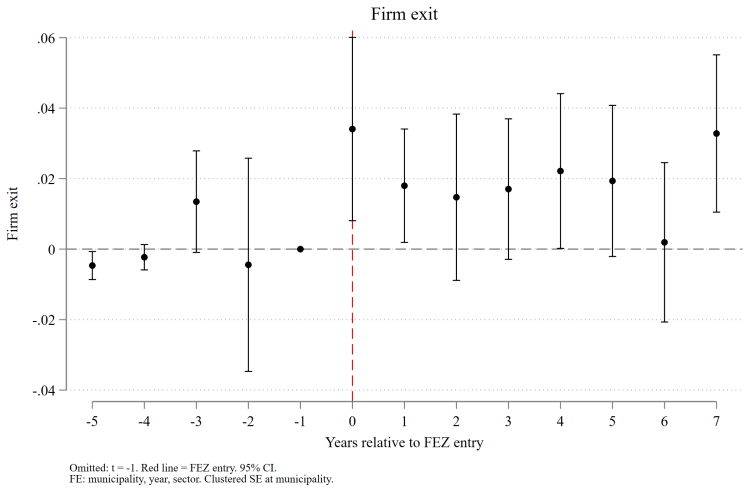


Figure C.18: FEZ effects on firm exit

Notes: This figure displays event-study coefficients and 95% confidence intervals from a two-way fixed effects specification. The outcome variable is the number of firm exits the social security reporting panel in a given year in a municipality m. The treatment indicator equals one from the year the municipality receives its first Free Economic Zone. The omitted period is $t = -1$. The specification includes municipality, year, and sector fixed effects. Standard errors are clustered at the municipality level.
Source: Authors' calculations using *Planilla Integrada de Liquidación de Aportes* (PILA) social security records and FEZ entry dates.

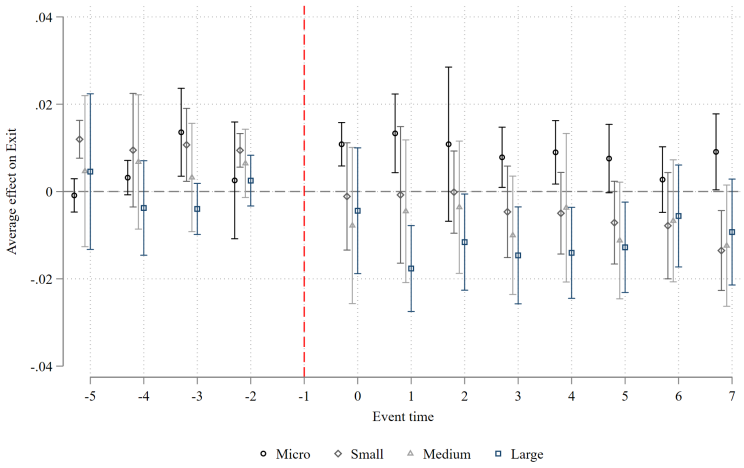


Figure C.19: FEZ effects on firm exit by size

Source: Authors' calculations using *Planilla Integrada de Liquidación de Aportes* (PILA) social security records and FEZ entry dates.

E.5 Estimating Firm and Worker Effects Using the AKM Model

The model predicts that firm productivity plays a central role in shaping the local effects of FEZs. To quantify this channel, I construct a measure of firm wage premia, a proxy for firm productivity, using matched employer–employee data covering the universe of formal workers and firms in Colombia.

I estimate the canonical AKM model proposed by **?**, which decomposes log wages into worker and firm components:

$$\ln w_{it} = \alpha_i + \psi_{j(i,t)} + X_{it}'\beta + \varepsilon_{it}, \tag{14}$$

where α_i captures worker-specific heterogeneity, ψ_j firm-specific wage premia, and $j(i,t)$ denotes the firm employing worker i at time t. The vector X_{it} includes a flexible polynomial in age and year fixed effects.

To avoid contamination from endogenous worker reallocation induced by FEZs, I estimate the model using only pre-policy data (2010–2015, August waves), restricting the sample to the largest connected set of firms. Identification relies on worker mobility across firms, under the assumption that $\mathbb{E}[\varepsilon_{it}|\alpha_i, \psi_{j(i,t)}, X_{it}] = 0$.

Table C.21 shows that firm effects account for 19.3% of the variance in log wages. This is within the range of estimates reported by Card et al. (2017) for other countries, though somewhat below the levels observed in economies with more flexible wage-setting institutions. One possible explanation is Colombia's relatively high and binding minimum wage, which may compress wage differences across firms, particularly at the lower end of the distribution

(Delgado-Prieto, 2025). At the same time, the covariance between worker and firm effects accounts for 13.2% of wage dispersion, indicating substantial positive sorting. This suggests that differences in how workers are allocated across firms are quantitatively important for understanding wage inequality in this setting.

Table C.21: Variance decomposition of log formal wages

Component	Share of wage variance (%)
Worker fixed effects ($\text{Var}(\alpha_i)$)	59.43
Firm fixed effects ($\text{Var}(\psi_j)$)	19.34
Sorting ($2 * \text{Cov}(\alpha_i, \psi_j)$)	13.24

Notes: This table reports the variance decomposition of log formal wages estimated from the AKM model using the largest connected set of workers and firms. Year fixed effects are included as controls.

These magnitudes highlight two key features of the Colombian labor market. First, firm-level heterogeneity is economically meaningful: nearly one-fifth of wage dispersion reflects differences in firm wage-setting. Second, sorting is quantitatively important, implying that reallocating workers across firms can generate sizable wage effects even in the absence of changes in within-firm pay policies.

To assess whether FEZs operate through this reallocation channel, I perform a Blinder–Oaxaca decomposition of the cross-sectional wage gap between workers in FEZ and non-FEZ municipalities, following Card et al. (2016), Dostie et al. (2021) and Delgado-Prieto (2025):

$$\mathbb{E}[\ln w_{it}|FEZ=1] - \mathbb{E}[\ln w_{it}|FEZ=0] = \underbrace{\Delta\mathbb{E}[\alpha_i]}_{\text{Worker composition}} + \underbrace{\Delta\mathbb{E}[\psi_j]}_{\text{Firm composition}} + \underbrace{\text{Residual}}_{\text{Within-firm pay-setting}}. \tag{15}$$

Table C.22 reports the results. The overall wage gap is small and negative (-0.023 log points), indicating slightly lower average wages in FEZ municipalities. This gap is almost entirely explained by differences in composition (-0.0226), while the unexplained component is economically negligible (-0.0008).

Decomposing the explained component reveals two offsetting forces. Workers in FEZ municipalities have lower worker fixed effects on average (-0.0304), consistent with negative selection in terms of skills. At the same time, they are employed in higher-paying firms, as reflected in the positive firm component ($+0.0078$). This pattern provides direct evidence of reallocation across the firm wage distribution. FEZs are associated with a shift of workers toward firms with higher wage premia, but this effect is not large enough to offset the negative selection in worker composition. The negligible unexplained component further indicates that differences in within-firm pay-setting play a minimal role.

These results indicate that differences in firm composition partly offset the negative selection in worker characteristics, but not fully. This pattern is consistent with a setting in which higher-

Table C.22: Blinder–Oaxaca decomposition of the FEZ vs non-FEZ log wage gap

Component	Coefficient	z-statistic
Overall decomposition		
Difference (Group 1 – Group 2)	−0.0234	−119.91
Explained component	−0.0226	−131.04
Unexplained component	−0.0008	−9.59
Explained:		
Worker fixed effects (α_i)	−0.0304	−235.42
Firm fixed effects (ψ_j)	+0.0078	+85.51
Unexplained:		
Worker fixed effects (α_i)	−0.00086	−134.27
Firm fixed effects (ψ_j)	−0.00002	−15.10
Constant	+0.00006	+0.68

Notes: The table reports coefficients and z-statistics from the Blinder–Oaxaca decomposition of the log formal wage gap between municipalities with (Group 2) and without (Group 1) Free Economic Zones (FEZs).

paying firms are more prevalent in FEZ municipalities, while the pool of workers remains, on average, lower in terms of measured productivity. The magnitude of the firm component suggests that reallocation across firms contributes meaningfully to wage differences, even if it does not dominate overall wage determination.

More broadly, these findings are consistent with a reallocation of workers toward higher-paying firms in FEZ areas, rather than large changes in within-firm wage-setting. This pattern helps reconcile the presence of higher-paying firms in FEZ municipalities with the absence of aggregate employment gains documented in the main analysis.

F Robustness Checks

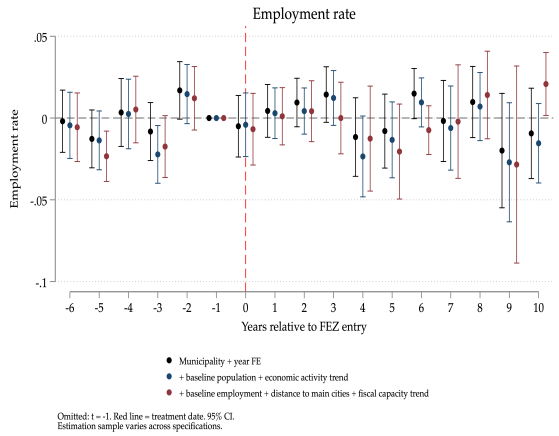
F.1 Controlling for Differential Trends

FEZs tend to locate in larger and more economically active municipalities (Appendix Table A.5). These municipalities may have followed different trajectories even in the absence of FEZs. I account for this possibility by allowing outcomes to evolve differently according to predetermined municipal characteristics.

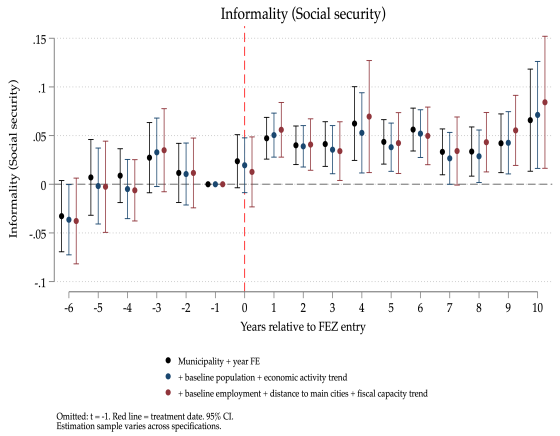
Figure D.20 reports three nested specifications. The first includes municipality and year fixed effects. The second additionally interacts baseline population and nightlights with year effects. The third adds interactions with baseline employment, distance to the nearest major city, and fiscal capacity. All characteristics are measured before FEZ entry.

The estimated treatment paths are very similar across the three specifications. The increase in informality remains similar in magnitude and timing, while the estimates for employment and

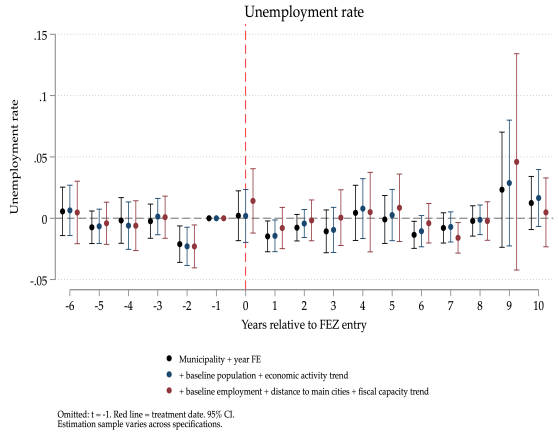
unemployment remain close to zero. The results are therefore robust to allowing municipalities with different initial economic conditions to follow different trends.



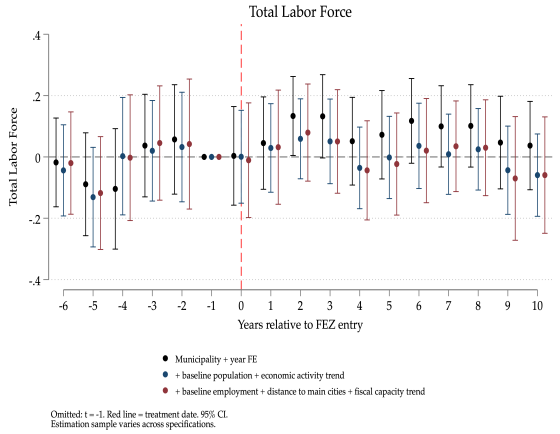
(a) Employment rate



(b) Informality rate



(c) Unemployment rate



(d) Labor force

Figure D.20: Labor Market Effects Allowing for Differential Trends

Notes: Each panel reports event-study coefficients and 95% confidence intervals from three nested specifications. The first includes municipality and year fixed effects. The second additionally allows for differential trends by baseline population and nightlights, interacting each with year effects. The third further allows for differential trends by baseline employment, distance to the nearest major city, and fiscal capacity. Baseline characteristics are measured before FEZ entry. Treatment is defined at the municipality level, and the comparison group consists of municipalities that never hosted a zone. Standard errors are clustered at the municipality level.
Source: Author's calculations using GEIH, DANE population projections, and FEZ information.

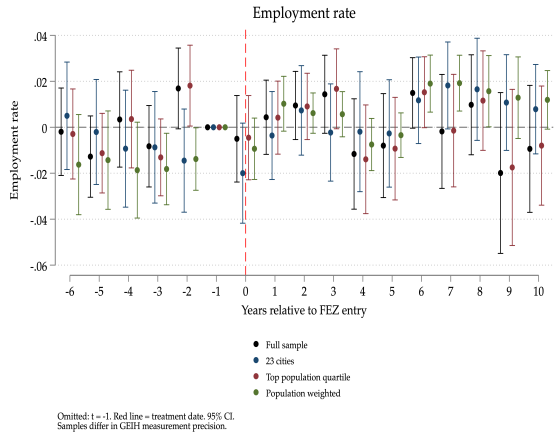
F.2 Representativeness of the Labor Force Survey

The Gran Encuesta Integrada de Hogares (GEIH) is designed to produce representative estimates for a set of geographic domains that does not coincide with the municipality. Its sampling frame supports inference for the municipalities comprising the 23 main cities and metropolitan areas, for departments, and for national urban and rural aggregates. Municipality-level estimates outside these domains rely on smaller numbers of sampled households and are therefore measured with greater sampling error, particularly in small and remote municipalities. This appendix assesses whether differences in survey precision affect the municipality-level estimates reported in the main text.

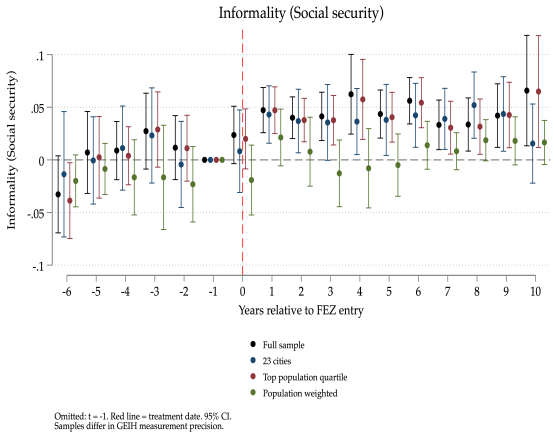
The main concern is that municipality-year cells are measured with unequal precision. Cells based on relatively few sampled households provide noisier estimates of local labor-market outcomes than cells based on larger samples, yet the baseline specification gives them equal weight. If this sampling variation is unrelated to the timing of FEZ entry, it primarily reduces precision; more generally, however, differences in measurement quality across municipalities could affect the estimated treatment paths.

I assess the importance of this issue by re-estimating the event-study specification using samples and weighting schemes that reduce the influence of municipalities for which GEIH outcomes are measured less precisely. Figure D.21 reports the results for four outcomes. Within each panel, estimates are shown for the full municipal panel; for municipalities comprising the 23 main cities and metropolitan areas, where the survey is explicitly designed to provide representative estimates; for the largest quartile of municipalities by baseline population, where municipality-year cells tend to contain more sampled households; and for a population-weighted specification, which gives greater influence to larger municipalities without discarding observations. Comparing the estimated treatment paths across these specifications assesses whether the baseline patterns are sensitive to the inclusion and weighting of noisily measured municipality-year cells.

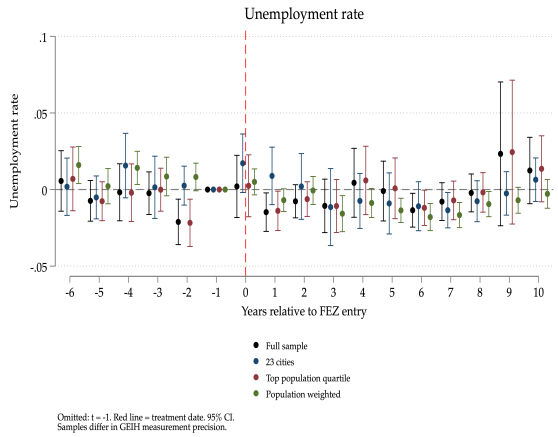
Figure D.21 shows that the main labor-market conclusions are not driven by municipality-year cells for which GEIH outcomes are measured less precisely. The absence of a sustained employment or unemployment response persists when I restrict the analysis to the 23 main cities and metropolitan areas or to the largest quartile of municipalities by baseline population. The increase in informality is likewise preserved under both restrictions, with post-treatment estimates generally similar in magnitude to those from the full panel. The population-weighted estimates of informality are smaller and generally close to zero. Because population weighting changes the relative contribution of municipalities to the estimand, this difference need not reflect survey precision alone and is consistent with a weaker informality response when greater weight is placed on the largest municipalities. The labor-force estimates are less stable across specifications, with a larger but imprecisely estimated increase among the 23 main cities. Overall, the employment and informality results are not an artifact of including small municipalities for which GEIH outcomes are measured with greater sampling error.



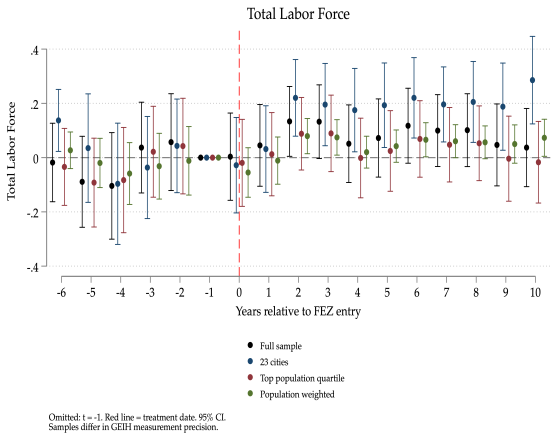
(a) Employment rate



(b) Informality rate



(c) Unemployment rate



(d) Labor force

Figure D.21: Labor Market Effects of FEZs across Samples of Differing Survey Precision

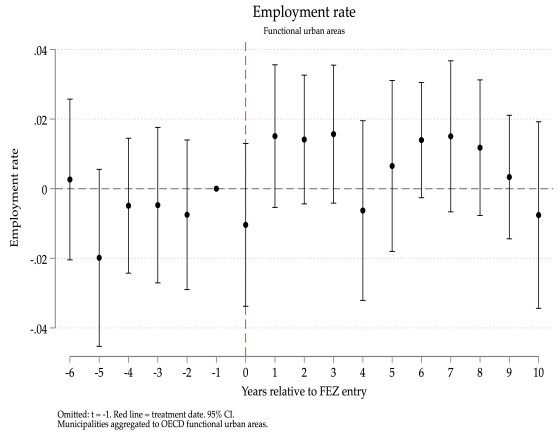
Notes: Each panel reports event-study coefficients and 95% confidence intervals from specification (1) under four samples or weighting schemes: the full municipal panel; the municipalities comprising the 23 main cities and metropolitan areas, for which the GEIH sampling design supports representative estimates; the largest quartile of municipalities by baseline population; and the full panel weighted by baseline population. Treatment is defined at the municipality level, and the comparison group consists of municipalities that never hosted a zone. All specifications include municipality and year fixed effects, with standard errors clustered at the municipality level.
Source: Author's calculations using GEIH and FEZ information.

F.3 Functional Urban Areas

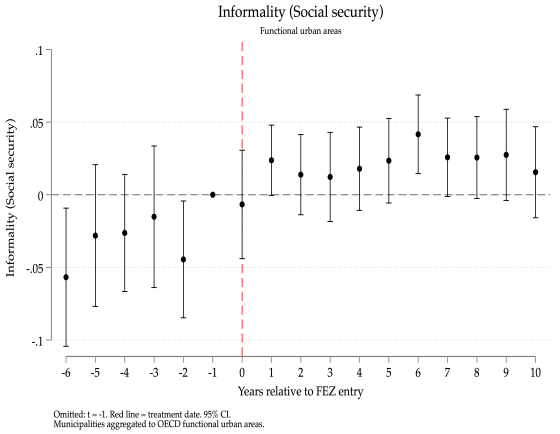
Municipal boundaries may not coincide with local labor markets because workers commute across them. This is particularly relevant for the GEIH, which records employment at the worker's place of residence rather than the place of work. A job created by an FEZ may therefore be observed in a neighboring municipality rather than in the municipality hosting the zone. To account for this, I redefine local labor markets using functional urban areas (FUAs), which group municipalities into commuting zones based on observed commuting flows to a core urban center (Sánchez-Serra, 2016). The Colombian delineation comprises 51 functional urban areas covering 129 municipalities.

I aggregate employment, the labor force, and population across municipalities within each FUA and construct rates from the corresponding area-level totals. An FUA becomes treated when the first FEZ within the area begins operating, while areas that never contain an FEZ form the comparison group. Aggregating to this level captures labor-market adjustment that crosses municipal boundaries but remains within the same commuting zone. It also improves the precision of GEIH outcomes by pooling sampled households across municipalities, although at the cost of reducing the number of geographic units to 51.

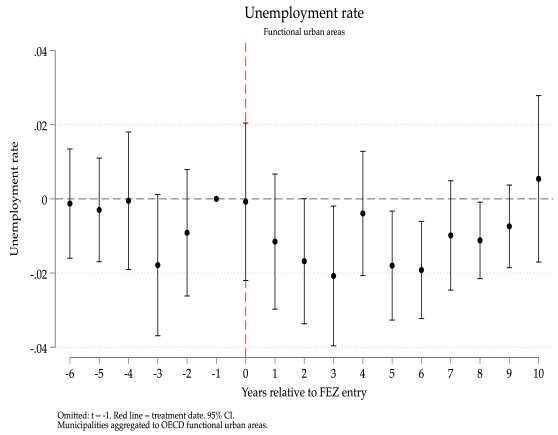
Figure D.22 reports the event-study estimates for the four main labor-market outcomes. The estimated effects are broadly similar in magnitude and timing to the municipality-level results. In particular, the increase in informality remains visible, while employment and unemployment show little systematic response. The estimates are less precise, as expected given the reduction to 51 functional urban areas. Overall, the similarity of the point estimates suggests that the municipality-level results are not driven by labor-market adjustment occurring across municipal boundaries within the same commuting area.



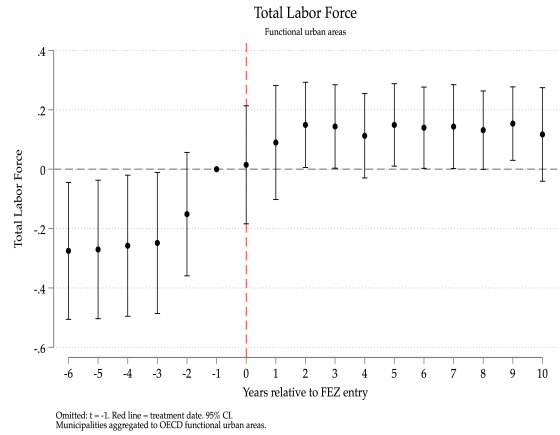
(a) Employment rate



(b) Informality rate



(c) Unemployment rate



(d) Labor force

Figure D.22: Labor Market Effects of FEZs across Functional Urban Areas

Notes: Each panel reports event-study coefficients and 95% confidence intervals from specification (1), estimated at the level of functional urban areas. Municipalities are grouped into commuting zones following Sánchez-Serra (2016). Employment, the labor force, and population are aggregated within each area, and rates are constructed from the corresponding totals. An area becomes treated in the year in which its first FEZ begins operating; areas that never contain an FEZ form the comparison group. All specifications include area and year fixed effects, with standard errors clustered at the functional urban area level.
Source: Author's calculations using GEIH and FEZ information.

F.4 Alternative Treatment Measures and Spatial Spillovers

A potential concern for the identification strategy is that untreated municipalities located near treated areas may themselves be affected by FEZ activity through spatial spillovers—for example, via labor mobility, supplier linkages, or local demand effects. Such spillovers could contaminate the control group and bias the estimated treatment effects. To assess the extent of this concern, and to construct alternative treatment definitions, I compute a set of time-varying spatial exposure measures for all municipalities in Colombia over the period 2005–2018. These measures are based on the exact geographic boundaries of FEZ polygons, which I match to municipality and urban area perimeters. For each municipality-year, exposure is defined relative to the urban centroid, capturing the location of economic activity and population, rather than the full municipal boundary, which often includes large uninhabited or rural areas.

Figure D.23 illustrates these measures for a representative year. Panel A maps the straight-line distance from each municipality's urban centroid to the nearest active FEZ, highlighting the strong spatial concentration of zones along the Caribbean coast, the Andean coffee region, and the Bogotá metropolitan area. Panel B overlays the exact FEZ polygons—constructed from hand-collected boundary data—on municipality and urban area shapefiles, showing that FEZs are typically small industrial enclaves located within or adjacent to urban centers rather than covering entire municipalities.

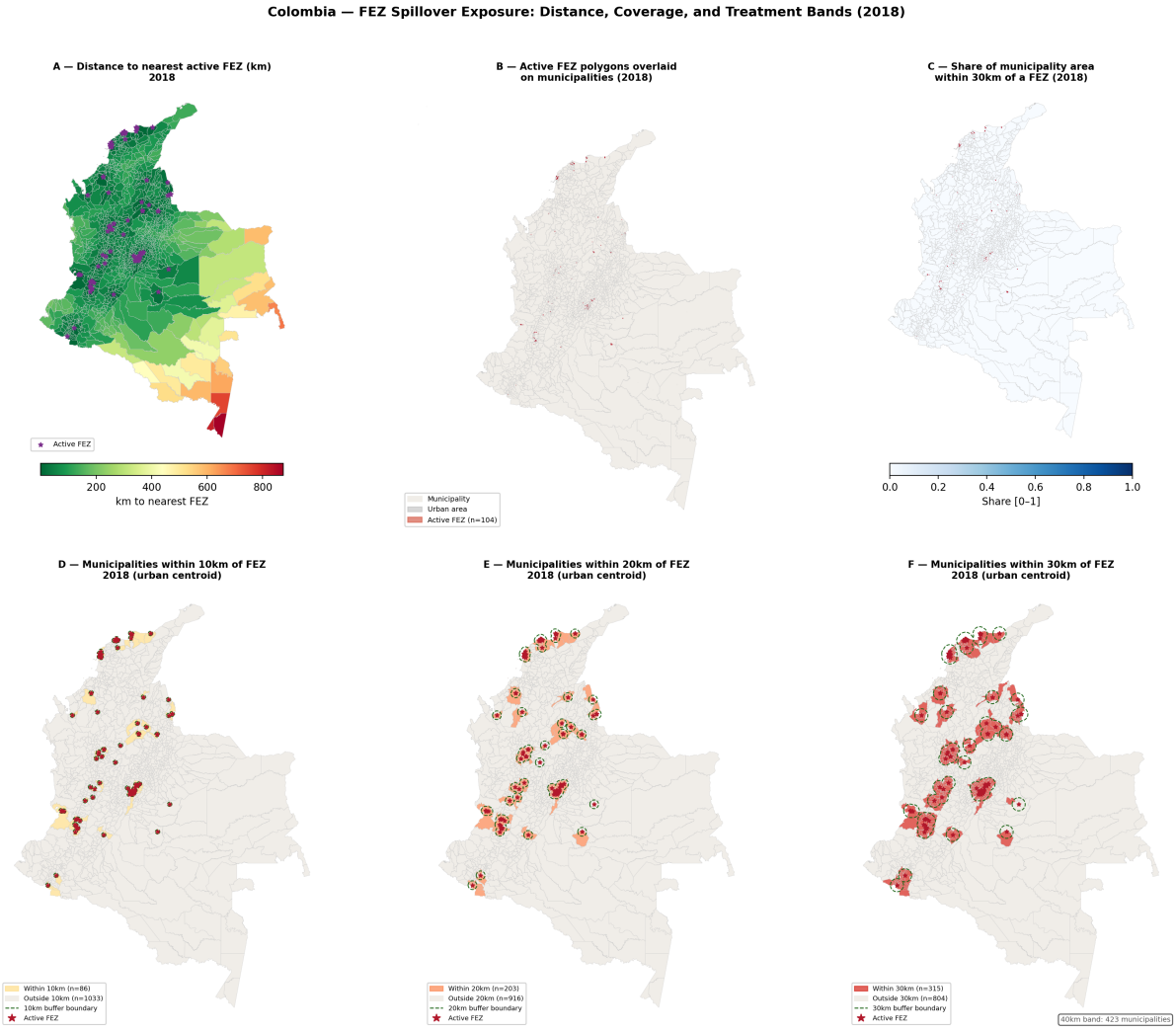
Panels C through F introduce alternative measures of treatment intensity and exposure. Panel C reports the share of each municipality's area covered by a 30-kilometer buffer around active FEZs, providing a continuous measure of proximity. Panels D–F define binary treatment indicators based on whether a municipality lies within 10, 20, or 30 kilometers of an active FEZ. These thresholds are used to construct "donut" samples that exclude municipalities located within a given radius of treated areas. Comparing results across these samples allows me to distinguish between true absence of spillovers and potential contamination of nearby control municipalities.

To illustrate the spatial logic underlying the exposure measures, I examine two municipalities that differ substantially in their relationship to nearby Free Economic Zones (FEZs): Armenia (Quindío) and Gachancipá (Cundinamarca). Both municipalities host at least one active FEZ, but they represent distinct configurations of local and regional exposure.

Armenia is a large departmental capital that hosts its own zone and is located within a broader regional cluster of FEZs across the coffee-growing region. As such, it combines direct treatment with relatively diffuse surrounding exposure. Gachancipá, by contrast, is a small municipality on the Bogotá savanna that hosts a single zone but is embedded in a dense cluster of additional FEZs within a 15-kilometer radius. This configuration implies that, although the municipality itself is directly treated, it is also exposed to nearby zones that may generate independent spillover effects on surrounding areas.

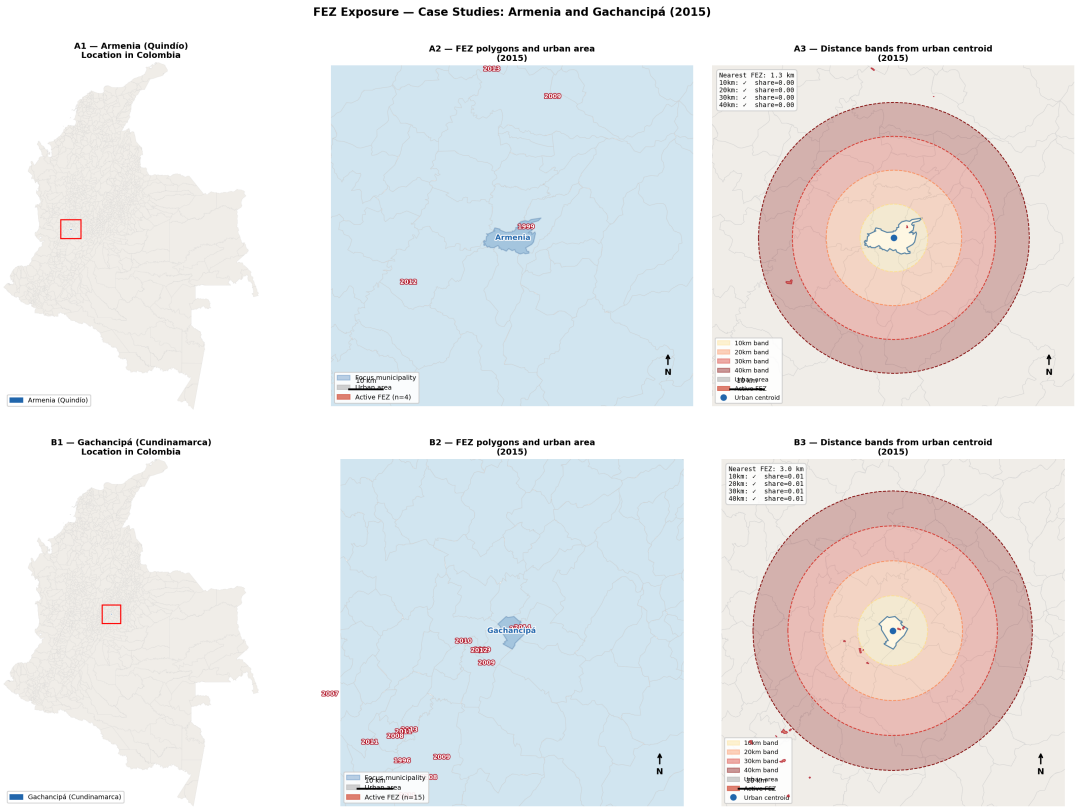
Figure D.24 presents three panels for each municipality. The first panel situates the municipality within Colombia, with a red rectangle indicating the zoomed-in area shown in the subsequent panels. The second panel provides a detailed view of the municipality boundary,

Figure D.23: FEZ Spatial Exposure Measures across Colombian Municipalities (2015)



Notes: Each panel displays a different measure of municipality-level exposure to Free Economic Zones (FEZs) in 2015. Panel A: straight-line distance (km) from the urban centroid of each municipality to the nearest active FEZ. Panel B: actual FEZ polygon boundaries overlaid on municipality and urban area shapefiles. Panel C: share of the full municipality area covered by a 30-kilometer buffer around active FEZ centroids. Panels D–F: binary treatment indicators at 10, 20, and 30 kilometer thresholds from the municipal urban centroid; the dashed circle shows the corresponding buffer boundary. A FEZ is classified as active in a given year if its first qualified firm entered on or before that year. Exposure measures are computed separately for each year over 2005–2018. Geographic calculations use Colombia's official projected coordinate system (MAGNA-SIRGAS, EPSG:3116). *Source:* FEZ boundary data compiled by the author; municipality and urban area shapefiles from IGAC (Instituto Geográfico Agustín Codazzi).

Figure D.24: FEZ Exposure: Case Studies for Armenia and Gachancipá (2015)

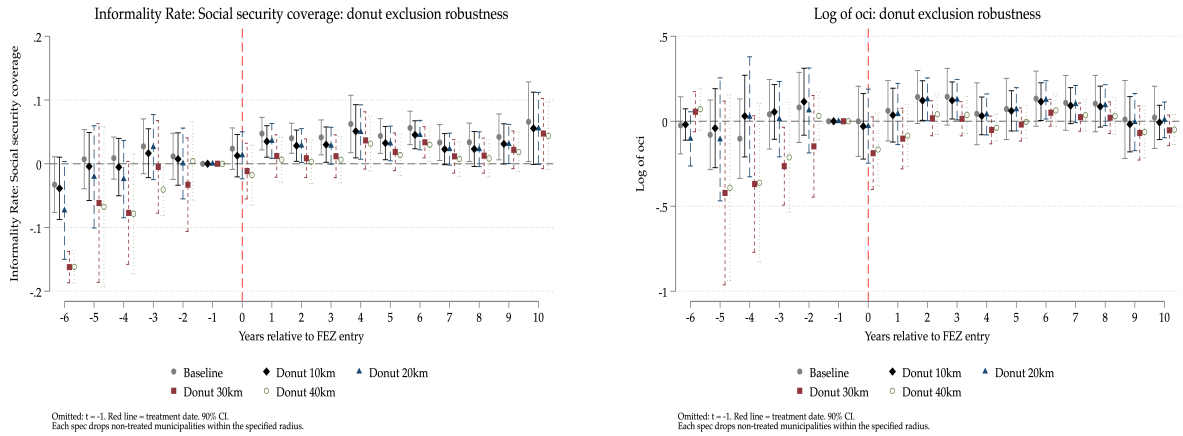


Notes: Each row presents three panels for one municipality. Left panel: location within Colombia, with the zoom area indicated by a red rectangle. Center panel: zoomed view showing the municipality boundary (blue), urban area (grey), and active FEZ polygons (red) within 60 kilometers, labelled by opening year. Right panel: concentric distance rings at 10, 20, 30, and 40 kilometers from the urban centroid, with a summary box reporting computed exposure measures for 2015 (distance to nearest FEZ, binary treatment indicators, and share of municipality area covered). Armenia (top row) and Gachancipá (bottom row). *Source:* FEZ boundary data compiled by the author; municipality and urban area shapefiles from IGAC.

its urban area, and the active FEZ polygons in the region, with each zone labeled by its opening year and distance from the urban centroid. The third panel overlays concentric 10, 20, 30, and 40 kilometer rings centered on the urban centroid, along with a summary of the corresponding exposure measures for 2015.

These highlight that the two municipalities exhibit qualitatively different exposure profiles despite both being directly treated. In Armenia, FEZ activity is largely contained within the municipality itself, whereas in Gachancipá, the local zone is part of a denser regional cluster that blurs the distinction between direct treatment and spatial spillovers. This contrast motivates the use of distance-based treatment measures and donut exclusion samples in Section 5.4, which test the sensitivity of the results to the inclusion of municipalities located near—though not hosting—FEZs.

Figure D.25 reports event-study coefficients for the main outcomes under each exclusion radius, alongside the baseline specification. The estimated effects are stable across all four donut thresholds. Neither the magnitude nor the timing of the treatment effects changes meaningfully as the exclusion radius expands from 10 to 40 kilometers, and pre-treatment coefficients remain close to zero throughout. These results indicate two things. First, the baseline comparison group is not systematically contaminated by spillovers from nearby FEZs. Second, the reallocation mechanism identified in the main analysis operates primarily through the internal labor market dynamics of host municipalities rather than through spatial diffusion to neighboring areas. This is consistent with the institutional structure of Colombian FEZs, which operate as fenced-in industrial enclaves with restricted entry and limited direct linkages to surrounding labor markets in the short to medium run.



(a) Informality rate

(b) Employment-to-population ratio

Figure D.25: Robustness to spatial spillovers: donut exclusion

Notes: This figure reports event-study estimates for the main outcomes under alternative donut exclusions of the control group. In each year, never-treated municipalities are excluded if their urban centroid falls within 10, 20, 30, or 40 kilometers of an FEZ that is already operational. Distances are computed using hand-collected FEZ boundary polygons and Colombia's official projected coordinate system (EPSG:9377). The figures plot the baseline estimates together with the estimates obtained under each exclusion radius. The stability of the coefficients across specifications indicates that the main results are not driven by spillovers from nearby FEZs contaminating the comparison group. Standard errors are clustered at the municipality level. *Source:* Authors' calculations using GEIH (LFS) and FEZ information.

Continuous treatment The baseline specification treats FEZ exposure as a binary indicator, comparing municipalities that host a zone to those that never do. This approach abstracts from potential heterogeneity in treatment intensity. To assess whether this masks meaningful variation, I exploit proximity to the nearest active FEZ as a continuous measure of exposure. I compute the straight-line distance from each municipality's urban centroid to the nearest operational FEZ in each year, using the hand-collected zone boundary polygons described in Section 2.[39] Among treated municipalities, I split the sample at the median pre-treatment distance, defining a *close* group (below-median distance) and a *far* group (above-median distance). The pre-treatment distance is computed as the average distance over all years prior to each municipality's first FEZ declaration, rather than at a fixed baseline year, to account for the staggered rollout of the program.[40]

Figure D.26 reports event-study estimates separately for the close and far groups for the main outcomes. Two patterns emerge. First, the estimated effects are stronger for municipalities located closer to active FEZs: increases in informality are larger, wage gains among high-skilled workers are more pronounced, and formal firm exit is more substantial relative to municipalities farther away. Second, pre-treatment coefficients remain close to zero for both groups, providing support for parallel trends within each exposure category.

The results are consistent with a gradient in treatment intensity by proximity. The attenuation of effects with distance suggests that the patterns documented in the main analysis are not driven solely by discrete treatment status, but also reflect variation in exposure across space. More broadly, the evidence is consistent with local labor market adjustments that weaken with distance from FEZ locations.

F.5 Placebo Tests

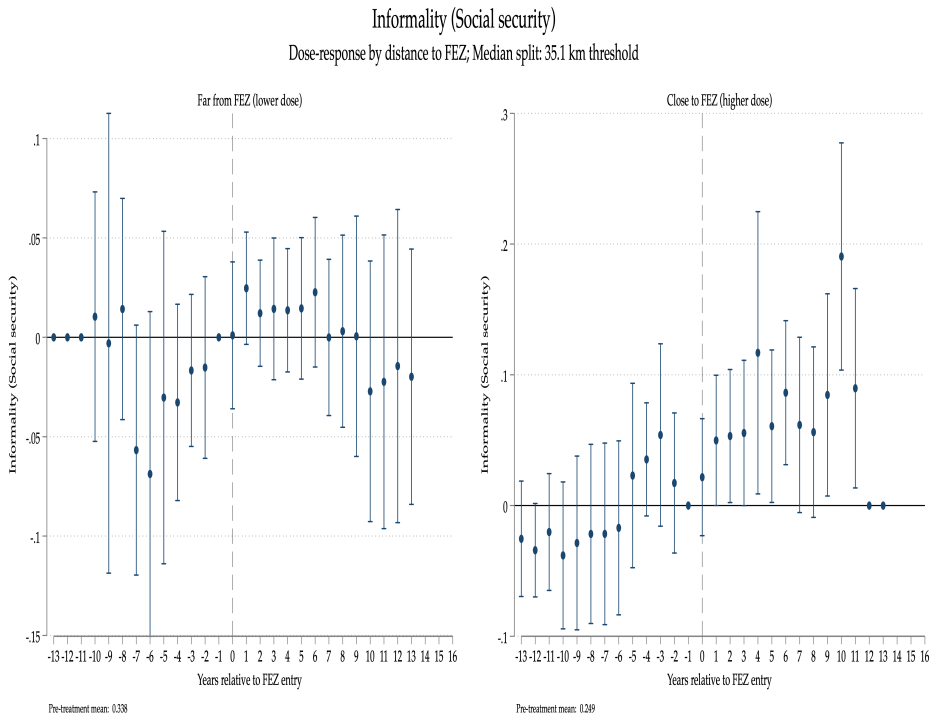
To further assess the credibility of the identifying assumption, I implement three complementary placebo exercises.

The first is a *pre-trend placebo*, which shifts the FEZ entry date backward by one, two, and three years and re-estimates the main specification using these falsified treatment dates. If the baseline results reflected differential trends already underway before FEZ entry, assigning treatment to these earlier dates should generate similar effects. Instead, the placebo estimates are small and statistically indistinguishable from zero. In particular, the increase in informality observed under the actual treatment timing does not emerge when FEZ entry is artificially assigned to earlier years. These results provide additional evidence that the estimated post-treatment effects are not driven by pre-existing differential trends.

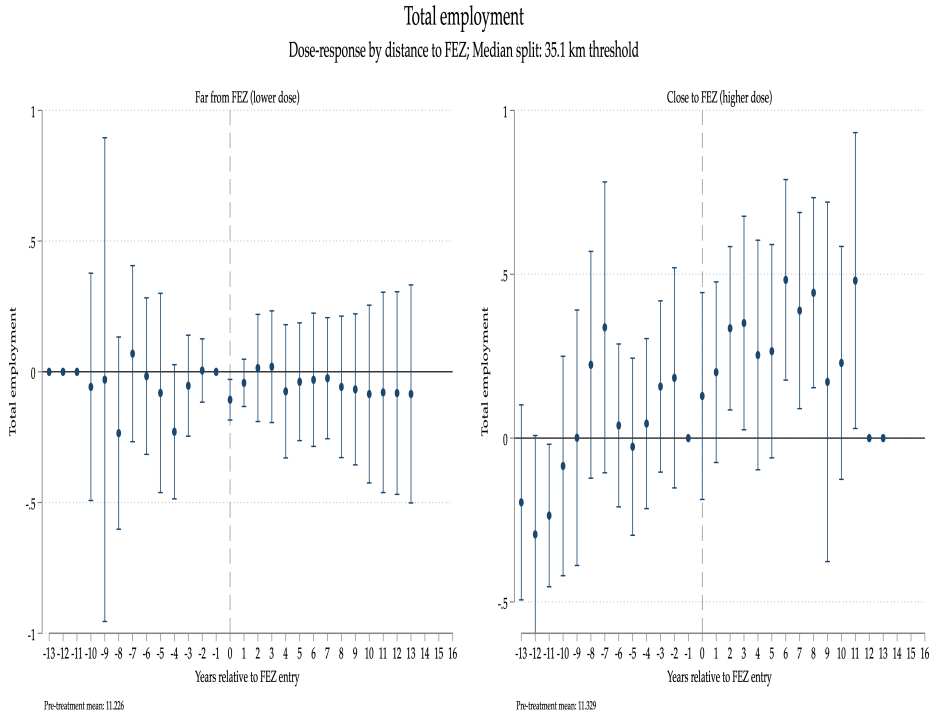
The second exercise is a *spatial placebo*, which randomly reassigns FEZ treatment to munici-

[39] Distances are computed annually and are therefore time-varying: as new zones become operational, some municipalities move closer to the treatment frontier. The urban centroid is used rather than the geographic centroid to better reflect the location of economic activity.

[40] Using a fixed baseline year (e.g., 2005) would conflate underlying geography with the evolution of the FEZ network, as municipalities treated in later cohorts would be assigned distances that predate the existence of zones they eventually host.



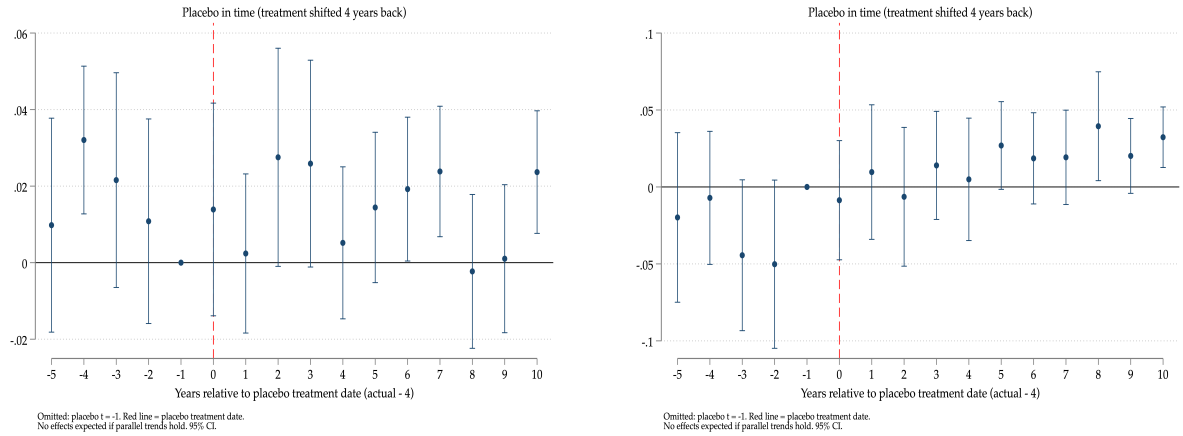
(a) Informality rate



(b) Total employment

Figure D.26: Dose-response by proximity to the nearest FEZ

Notes: This figure reports event-study estimates separately for municipalities with high and low exposure to FEZs, defined by their pre-treatment distance to the nearest FEZ. The *close* group includes municipalities below the median distance, while the *far* group includes those above the median. Distances are computed as straight-line distances between each municipality's urban centroid and the nearest operational FEZ in each year. The figures display the baseline specification estimated separately for each group. The larger effects observed among closer municipalities are consistent with stronger exposure to FEZ-related economic activity. Standard errors are clustered at the municipality level. *Source:* Authors' calculations using GEIH (LFS) and FEZ information.



(a) Employment-to-population ratio

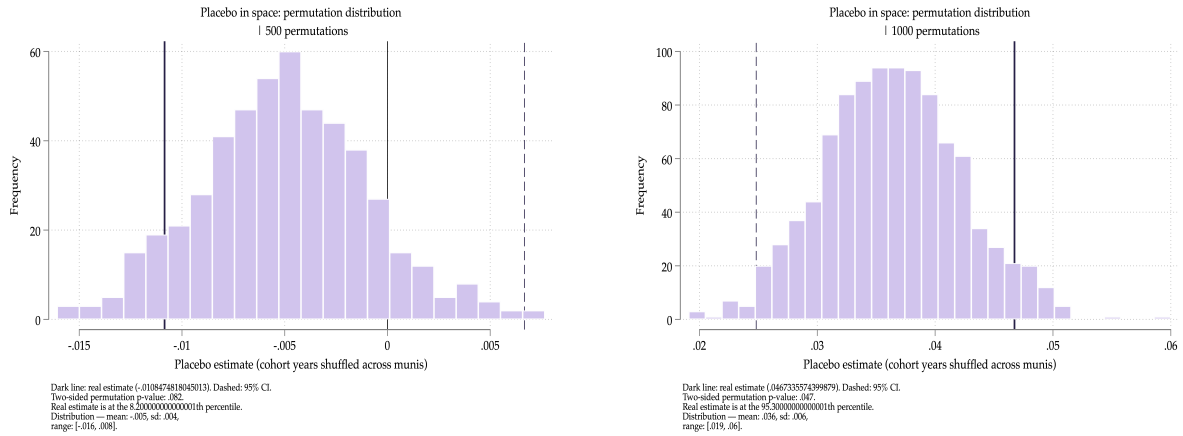
(b) Informality rate

Figure D.27: Placebo Tests in Treatment Timing

Notes: The figure reports placebo estimates obtained by shifting the year of first FEZ entry backward by one, two, and three years and re-estimating the baseline specification using each falsified treatment date. The estimate based on the actual treatment timing is reported for comparison. Points indicate estimated treatment effects and vertical bars report 95% confidence intervals. Placebo estimates close to zero indicate that the baseline results do not emerge when treatment is artificially assigned before actual FEZ entry.
Source: Author's calculations using GEIH and FEZ information.

palities that never hosted a zone. I draw 500 random permutations of the treatment assignment, preserving the number of treated municipalities and the cohort structure in each wave, and re-estimate the baseline specification for each draw. Figure D.28 reports the distribution of average post-treatment coefficients across permutations together with the actual estimate. The observed effect lies well outside the permutation distribution for the informality outcome, indicating that the baseline estimates are unlikely to be driven by chance variation in treatment assignment.

These exercises support the interpretation that the effects documented in the main analysis reflect genuine labor market adjustments rather than pre-existing trends, spurious spatial correlations, or chance assignment of treatment.



(a) Employment-to-population ratio (b) Informality rate

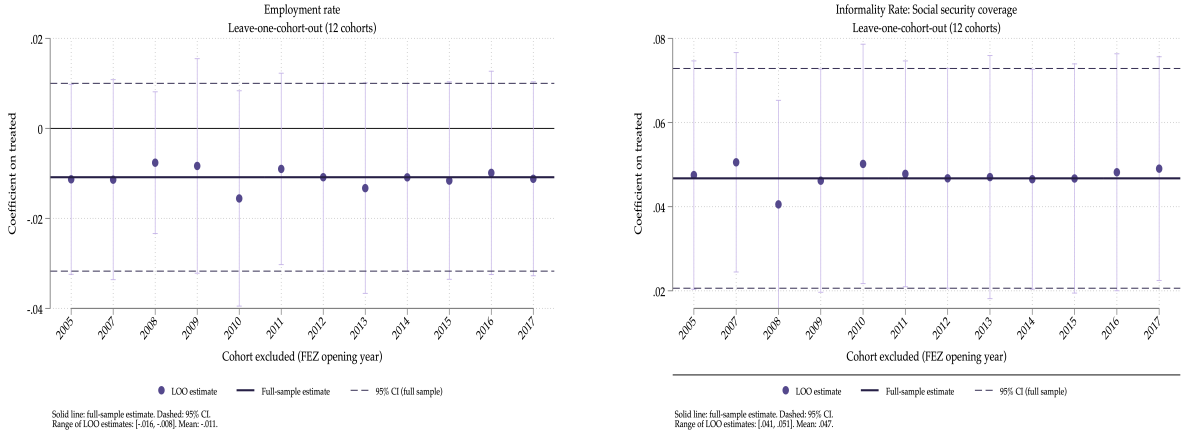
Figure D.28: Placebo tests in space

Notes: This figure presents randomization inference results based on 500 placebo assignments. Cohort timing is held fixed, while municipalities are randomly reassigned across treatment cohorts. The distribution shows the estimated coefficients under placebo assignments. The solid vertical line indicates the baseline estimate, and dashed lines correspond to its 90% confidence interval. A distribution centered around zero suggests the absence of mechanical bias, while the position of the baseline estimate in the tail supports its statistical significance under non-parametric inference.
Source: Authors' calculations using GEIH (LFS) and FEZ information.

F.6 Sensivity: Leave-One-Out

Another concern with the baseline estimates is that a small number of influential treatment cohorts may drive them. The results could be sensitive to the inclusion of particular waves. To assess this, I implement a leave-one-out exercise that systematically excludes one treatment cohort at a time and re-estimates the main specifications on the remaining sample. This approach allows me to evaluate whether any single cohort disproportionately affects the estimated treatment effects.

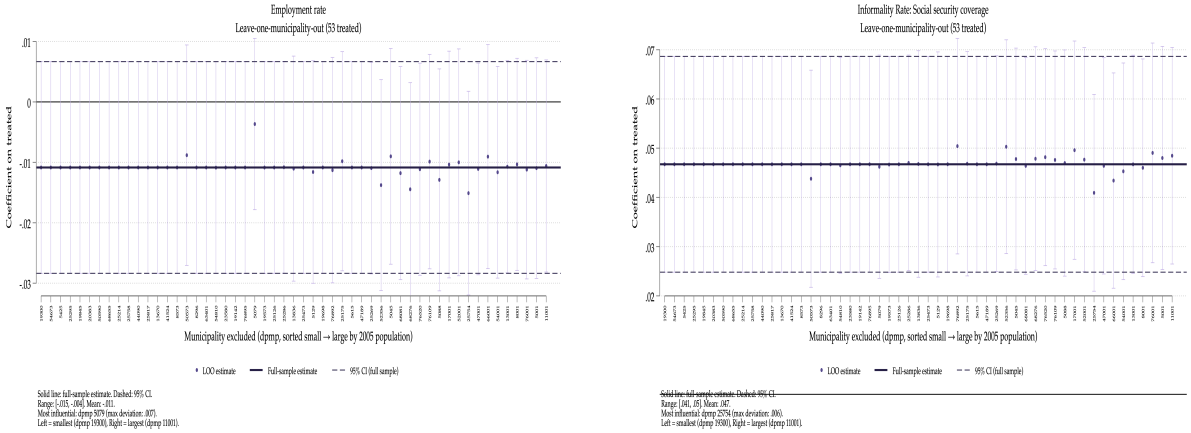
Figure D.30 reports the distribution of leave-one-out estimates for the main outcomes alongside the baseline estimate. Each point corresponds to the average post-treatment effect obtained after excluding one cohort. The estimates remain stable across all specifications. The distribution of coefficients is tightly centered around the baseline, and excluding any individual cohort does not reverse the sign or materially change the magnitude of the effects.



(a) Employment-to-population ratio (b) Informality rate

Figure D.29: Leave-one-cohort-out robustness

Notes: Each point reports the TWFE estimate obtained after excluding one treatment cohort at a time. The solid line indicates the full-sample estimate, and dashed lines denote its 90% confidence interval. The stability of the estimates across cohort exclusions indicates that the results are not driven by any single treatment wave.
Source: Authors' calculations using GEIH (LFS) and FEZ information.



(a) Employment-to-population ratio (b) Informality rate

Figure D.30: Leave-one-municipality-out robustness

Notes: Each point reports the TWFE estimate obtained after excluding one treatment municipality at a time. The solid line indicates the full-sample estimate, and dashed lines denote its 90% confidence interval. The stability of the estimates across cohort exclusions indicates that the results are not driven by any single treatment wave.
Source: Authors' calculations using GEIH (LFS) and FEZ information.