The Unintended Consequences of Place-Based Industrial Policies: Evidence from Colombia

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Abstract

Special Economic Zones (SEZs) are a cornerstone of place-based industrial policy in developing economies, yet their success depends on how local labor markets adjust to new investment opportunities. Using newly assembled georeferenced data linking municipalities, firms, households, and zones, this paper exploits the staggered rollout of SEZs in Colombia between 2005 and 2018 in a difference-in-differences framework to assess their impact on local labor markets and firm dynamics. I find that, at the municipality level, SEZs increase informality without affecting total employment, indicating that the policy reallocates rather than expands local labor demand. This reallocation arises as high-skill–intensive firms entering SEZs compete for scarce skilled labor: wage pressures intensify, high-skill workers experience wage gains, and small, less-productive formal firms that cannot match rising labor costs exit the market. The resulting shift in labor demand reallocates high-skilled workers toward high-productivity firms, while displaced low-skill workers are absorbed into informal occupations. The findings reveal a distortion inherent to place-based industrial incentives: SEZs concentrate benefits among a narrow "club" of high-productivity firms, reshaping the composition of local economies and highlighting the limits of policies that rely on selective incentives.

Keywords: Place-Based Policies, Informality, Firms

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1 Introduction

Large and persistent spatial disparities are a defining challenge for economic development. A handful of regions concentrate production, high-paying jobs, and innovation, while large areas remain economically lagging. These imbalances shape fundamental dimensions of development, including access to jobs, productivity growth, and income inequality. Since market forces tend to reinforce spatial concentration, policymakers increasingly rely on place-based industrial policies to attract productive firms, stimulate private investment, and promote local employment. Their political economy appeal is straightforward, as they deliver visible and geographically concentrated gain, allowing policymakers to demonstrate rapid progress. However, whether such interventions create high-quality employment or primarily redistribute existing activity remains unclear.[1]

A central concern is that place-based incentives may shift economic activity across space rather than generate net growth, particularly when firms relocate or arbitrage tax exemptions (Neumark and Kolko, 2010; Kline and Moretti, 2013; Criscuolo et al., 2019). Existing evidence focuses largely on aggregate employment responses in developed countries, yet in many developing economies—where weak enforcement, high regulatory costs, and widespread informality shape firm and worker behavior—the most consequential adjustments may occur within local labor markets, through changes in who works, for whom, and under what contractual conditions. In these contexts, policies that alter the relative profitability of formal and informal production can therefore generate sizable general equilibrium effects on employment composition, wages, and welfare. By selectively reducing taxes, regulatory costs, or input prices for a subset of formal firms, place-based incentives shift local factor demand, tighten competitive pressure, and change the incentives to operate formally versus informally. Yet we know surprisingly little about these distributional margins and the mechanisms that generate them. Motivated by this gap, this paper asks: *How do place-based industrial policies affect local economic outcomes, and in particular how do they reshape the allocation of labor between formal and informal firms within treated municipalities, and through which mechanisms?*

To answer this question, I examine Colombia, a country with pronounced spatial inequality and widespread informality, where roughly half of urban workers and 60 percent of establishments operate outside the tax and social security system. Following a regulatory reform in 2005, Colombia expanded its Free Economic Zones (FEZs or *zonas francas* in Spanish), granting firms substantial exemptions from corporate income taxes, import tariffs, and selected administrative requirements. The stated objective was to promote regional competitiveness and formal employment. Yet by 2018, Colombia hosted around 22 percent of all Special Economic Zones (SEZs) in Latin America and the Caribbean—the largest share in the region—while ranking only seventh in zone-related employment, a pattern consistent with capital-intensive production structures and limited labor absorption in segmented labor markets (UNACTD, 2018; Cámara de Usuarios de Zonas Francas, ANDI, 2019).

[1]Most existing work is set in developed countries (Greenstone et al., 2010; Gobillon et al., 2012; Busso et al., 2012; Ehrlich and Seidel, 2018). Evidence on the effects of place-based industrial policies in less-developed countries is scarce, with studies on SEZs in China and India being the notable exception (Lu et al., 2019; Wang, 2013; Gallé et al., 2022).

This institutional environment characterized by weak enforcement, high regulatory costs, and rigid labor-market segmentation makes the setting well suited to study the unintended consequences of place-based industrial incentives.

To evaluate the impact of FEZs on local economic outcomes, I construct a novel georeferenced panel that combines administrative and survey microdata at the municipality, firm, and worker levels. Using machine-learning techniques and unstructured administrative reports, I recover the exact geographic coordinates and operational start dates of all zones and link them to detailed information on labor markets and firm dynamics from household surveys, structural censuses, and employer-employee records. This spatial linkage allows me to study the effects of FEZ establishment along complementary margins: (i) municipality-level labor-market aggregates, (ii) equilibrium consequences for workers in the same municipalities, (iii) direct effects on firms operating inside the zones, and (iv) indirect effects on other formal establishments competing in the same labor markets. Empirically, I exploit the staggered rollout of FEZs across space in an event-study difference-in-differences design comparing treated municipalities to never- and later-treated counterparts. The key identifying assumption is that, absent FEZ establishment, treated municipalities would have followed similar trends as controls—an assumption supported by flat pre-treatment dynamics and no evidence of anticipatory adjustments.

I begin by examining whether FEZs stimulate broad local labor demand. At the municipality level, total employment is not affected following FEZ establishment, despite persistent growth in fiscal revenues, nighttime luminosity, and tax collections. Instead, the informality rate increases by approximately five percentage points relative to baseline. This increase is driven by a reallocation of workers from formal into informal jobs rather than changes in unemployment or labor-force participation, indicating that FEZs alter the composition of employment rather than expand it. To unpack this further, sectoral decompositions show modest employment gains concentrated in manufacturing, with no detectable effects in services, consistent with a narrow adjustment margin rather than broad-based job creation. This pattern is consistent with "jobless industrialization" dynamics in late-industrializing economies, where manufacturing tends to generate substantial value added but absorb relatively few workers (Alfaro et al., 2023; Rodrik, 2015).

To understand the incidence of these adjustments across workers, I use individual-level data and compare workers in treated municipalities with those in untreated areas. The effects of FEZ exposure are heterogeneous across skill levels. I find a pronounced skill gradient: low-skilled workers are 6 percentage points more likely to work informally after FEZ implementation, while medium- and high-skilled workers are 1.9 and 1.6 percentage points less likely to be informally employed and earn wage premia of up to 4 percent, relative to low-skilled workers. At the same time, low-skilled workers are increasingly absorbed into informal occupations with stagnant earnings. Unemployment remains unchanged, indicating that displaced workers reallocate rather than exit the labor force. Overall, these results show that the increase in informality is concentrated almost entirely among low-skilled workers, widening both the formal–informal divide and wage dispersion within local labor markets.

This is consistent with a mechanism in which FEZs increase demand for skilled labor and bid up local wages, while generating limited additional demand for low-skilled workers, who are then absorbed into informal occupations.

Next, I examine the direct effects of FEZ incentives on firms that applied to join an FEZ using administrative financial statements. I find that FEZ firms expand wage bills by roughly 78 percent and invest heavily in capital and inventories.[2] These expansions are accompanied by substantial increases in long-term assets and inventories, consistent with program rules that mandate large upfront investment commitments. However, operating income does not improve, while net income before taxes increases by about 46 percent. This pattern indicates that gains are primarily driven by fiscal incentives—such as tariff exemptions and corporate tax relief—rather than productivity improvements. Larger and more capital-intensive firms capture the majority of these benefits, as they are better positioned to meet investment thresholds and amortize fixed costs. Consistent with this interpretation, balance-sheet measures show pronounced capital deepening without corresponding improvements in operating profitability, suggesting that FEZs support firms that are already productive and incentivize production technologies which could lead to limited labor absorption.

Do these localized gains spill over to other firms in the municipality? Using data for all formal manufacturing establishments, I find that employment increases by approximately 4.8 percent and total wages per worker by about 4 percent, consistent with tighter local labor markets and competition for scarce skills. I also show that firms located in treated municipalities expand output and become more productive following FEZ establishment, suggesting that access to local suppliers, logistics improvements, and demand complementarities extend beyond zone boundaries.[3] Consistent with the worker-level evidence, these patterns indicate that FEZ activity raises demand for medium- and high-skilled labor, bidding up local wages. Smaller and less productive firms are forced to increase compensation to retain qualified workers but can do so only selectively across occupations, increasing cost pressure for establishments operating near the productivity cutoff. The absence of effects on unemployment suggests that displaced workers continue to be absorbed elsewhere in the local economy, primarily through informal employment. Two institutional features limit broader labor absorption inside FEZs: eligibility rules that require large upfront capital investments encourage capital-intensive production technologies, and local skill scarcity constrains additional hiring even as output expands.

To examine how FEZs affect the structure of the formal firm population, I focus on entry and exit margins. I find that FEZ implementation raises formal firm exit rates by 7–8 percent, disproportionately among younger, smaller, and less productive establishments operating closer to the productivity

[2]The increase in the wage bill may reflect both hiring additional workers—consistent with program requirements to demonstrate job creation—and paying higher wages due to intensified competition for skilled labor. Unfortunately, I cannot distinguish between these channels with the available data.

[3]While these patterns are consistent with production-linkage spillovers, the available data do not allow me to directly trace input–output relationships between FEZ and non-FEZ firms. Documenting firm-to-firm linkages is an important avenue for future work.

cutoff. Entry rates remain unchanged, implying adjustment through selection rather than net firm creation. In line with this, heightened competition for skilled labor and higher local input prices disproportionately burden non-beneficiary firms.[4] Together, these findings reinforce the selection mechanism: FEZs raise the productivity cutoff for survival, reallocating employment toward capital- and skill-intensive firms that offer higher wage premia. The associated capital deepening increases capital per worker and limits labor absorption. Less productive firms exit and do not re-enter, while displaced workers are less likely to remain formally employed. This helps explain why aggregate formal employment does not increase despite the appearance of high-wage jobs within FEZs.

Finally, the evidence indicates that FEZs operate primarily through reallocation rather than expansion. By conferring fiscal advantages on capital- and skill-intensive producers, FEZs bid up local wages, tighten skill bottlenecks, and crowd out marginal formal establishments. Displaced workers reallocate to informal jobs, generating higher informality despite localized wage gains at the top of the skill distribution. To strengthen the credibility of these findings, I implement a series of robustness exercises addressing concerns related to treatment definition, measurement, and spurious correlation. First, I replace the binary FEZ indicator with the share of local employment in FEZ firms, providing a continuous measure of exposure and showing that results scale with treatment intensity. Second, I replicate the analysis using alternative definitions of informality drawn from both household survey and administrative sources, ruling out measurement artifacts. Third, I conduct placebo tests along several dimensions: (i) pre-trend placebos, which shift FEZ entry backward to detect spurious anticipation; (ii) spatial placebos, which randomly assign FEZs to untreated municipalities; and (iii) outcome placebos, estimating treatment effects on variables plausibly unrelated to industrial policy—such as rainfall or mortality. Across these exercises, the main patterns remain stable, confirming that FEZs reshape local labor markets by reallocating workers and firms rather than creating net employment.

This paper makes a central contribution by documenting a previously understudied distortion generated by place-based industrial incentives: FEZs reshape the allocation of labor within local economies in ways that can exacerbate informality and widen wage dispersion. This arises because preferential benefits are granted to a narrow subset of firms—a "club" of capital- and skill-intensive producers—while costly eligibility requirements exclude many potential beneficiaries. These club effects are driven by firms that, although highly productive, have limited labor absorption capacity, yet generate general equilibrium pressures that affect non-beneficiary firms competing in the same local markets. My paper highlights that industrial policies can therefore redistribute production toward firms with limited employment capacity, shifting the composition of local labor demand. These results underscore that, in institutional environments characterized by weak enforcement and high

[4]As an additional exercise, I document the role of firm heterogeneity in shaping these wage effects by using matched employer–employee data (PILA) to estimate an Abowd–Kramarz–Margolis (AKM) model and perform a Blinder–Oaxaca decomposition. I 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 offering higher wage premia. The decomposition confirms that differences in firm wage policies, rather than observable worker characteristics, drive most of the FEZ–non-FEZ wage gap.

regulatory costs, place-based interventions can unintentionally amplify labor-market segmentation, even as they appear successful along conventional measures.

Related Literature. I mainly contribute to multiple strands of economic literature. First, my paper relates to the sizable literature on the effects of place-based industrial policies. Most existing studies focus on developed economies, analyzing outcomes such as investment, productivity, employment, and firm creation (e.g., Neumark and Kolko 2010; Gobillon et al. 2012; Freedman et al. 2023; Kline and Moretti 2013; Mayer et al. 2015). In developing economies, however, evidence remains limited and mixed. For instance, the Chinese Special Economic Zones (SEZs) program 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. By contrast, evidence from India points to heterogeneous effects: 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. While this literature demonstrates that place-based programs can stimulate investment and productivity, it has largely overlooked how these policies reshape labor allocation within local labor markets—including movements between formal and informal jobs, across firms with different cost structures, and across segments of the skill distribution. I contribute by documenting a general equilibrium mechanism in which FEZs bid up local wages in settings with constrained labor supply, raise the productivity threshold for survival, and displace marginal formal firms that cannot match higher wage offers. Three features of the policy drive this reallocation: (i) a club effect, whereby preferential benefits accrue to a narrow subset of firms; (ii) eligibility rules that require large upfront capital investments, restricting access to already productive and capital-intensive establishments; and (iii) limited local supply of skilled labor, which amplifies wage pressures. Together, these forces reallocate employment toward firms with limited labor absorption capacity and push low-skill workers into informal employment.

Second, 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). More recently, Zárate (2022) show that infrastructure projects improving urban mobility can enhance allocative efficiency by reallocating workers from informal to formal establishments. I contribute to this literature by showing that place-based industrial incentives can inadvertently expand informality through equilibrium wage and cost pressures: as highly productive beneficiaries bid up local wages, marginal formal firms exit, and workers reallocate into informal employment. This mechanism highlights how spatially concentrated policies can reallocate resources across formality margins, reinforcing existing labor-

market distortions even in the absence of aggregate employment changes.

Third, my paper relates to the macro-development literature studying 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). Recent work emphasizes the importance of informality, labor-market frictions, and human capital in explaining why industrial expansion often fails to generate broad-based employment. I contribute to this debate by providing micro-level evidence showing that FEZs raise local productivity and wages among capital- and skill-intensive firms, but absorb relatively few additional workers, displace marginal formal firms, and reallocate low-skill labor into informal employment.

The remainder of the paper is organized as follows. Section 2 provides institutional background on Colombia's Free Economic Zones, describes the policy environment and the data. Section 3 outlines the empirical strategy, and Section 4 reports the main reduced-form results on municipalities, workers, and firms. Section 5 discusses policy implications and external validity. Section 6 concludes.

2 Institutional Context and Data

2.1 Free Economic Zones

Special Economic Zones (SEZs) are widely used place-based policy instruments in developing countries, designed to promote economic development, improve competitiveness, attract foreign direct investment (FDI) and generate employment. Globally, the total number of SEZs increased from 500 in 1995 to 5,400 in 2018, driven by globalization, trade liberalization, and integration programs (Facility for Investment Climate Advisory Services (2008); World Bank Group (2017); UNC (2019)). While institutional designs may vary, SEZs share the common characteristic of being established within clearly defined geographic areas, where firms located within the zone benefit from reduced tax and tariff rates or streamlined bureaucratic procedures (Akinci et al., 2008).

In the 1950s, Colombia established its first Special Economic Zone (Zona Franca in Spanish), becoming the first country in Latin America to adopt such a model. At the time, SEZs were conceived as public initiatives aimed at supporting import substitution in a closed economy, with ten zones established by the national government. By the late 1990s, globalization prompted a shift, allowing private sector management of existing zones and aligning policies with WTO regulations to promote a competitive export environment. Building on these challenges, and inspired by the success of Chinese SEZs, the Colombian government enacted the FEZ Act in 2005. This reform marked a turning point, transforming FEZs into fenced-in industrial enclaves with a new set of benefits, opening them to private investment, and creating a more flexible framework than the earlier state-owned,

centrally managed model.

The Act 1004 of 2005 formally set out four objectives for Colombia's Free Economic Zones: (i) to create employment opportunities and attract capital investments, (ii) to serve as a development hub that fosters regional competitiveness, (iii) to promote highly productive industrial processes through economies of scale, and (iv) to simplify international trade procedures. To achieve these goals, the Act provided a uniform legal framework for developing and doing business in FEZs and introduced a wide set of incentives. On the fiscal side, firms benefit from a flat corporate tax rate of 20%, lower than the 35% applied elsewhere in the domestic tariff area, and from exemptions on value-added taxes (VAT) on raw materials, supplies, goods exchanged within or between zones, and imported goods. [5]In terms of international trade and customs, firms are granted tariff-free imports and can indefinitely retain foreign-origin goods within the zones. [6] Finally, on the administrative side, authorized operations are exempt from customs declarations, significantly streamlining cross-border transactions. These benefits are not mutually exclusive or conditional on a certain type of activity, such as exporting.

Further reforms in 2007 refined the institutional design by creating three types of FEZs: (i) permanent zones, which host multiple firms in industrial parks or offshore complexes, (ii) special zones, dedicated to single-firm operations, and (iii) transitory zones, designated for temporary activities such as fairs and exhibitions. Over time, the Colombian government has issued several decrees regulating the FEZ regime, defining the functioning of FEZs, their economic obligations, tax incentives, and operational requirements. [7]

Applications to establish a Free Economic Zone in Colombia are well-established by the law. These are reviewed by the Intersectoral Commission of Free Economic Zones and the Ministry of Industry and Commerce. Proposals must comply with general criteria, including alignment with the municipality's development plan to ensure local economic impact, adherence to environmental regulations to avoid negative externalities, and provision of a contiguous area of at least 20 hectares, except in the case of special zones. In addition, zone-specific requirements apply: permanent zones must hold at least five firms within five years, while special zones must generate a minimum of 150 jobs within three years. Zone developers are required to present a *plan maestro de desarrollo general* (general development plan), which includes a set of economic projections, notably national and international sales targets, job creation commitments, investment amounts, and a timeline for meeting annual investment and direct employment obligations. Once this plan and other conditions are approved, the Ministry of Industry and Commerce formally declares the designated area as an FEZ.

Once a zone is declared, firms can apply to operate within the FEZ as "operator users" (*usuarios*

[5]Before 2017, the flat corporate tax rate was 15% in the zones, while outside it was 25%.

[6]Firms may indefinitely retain foreign-origin goods within FEZs, effectively turning them into duty-free warehouses. This provides flexibility in inventory management, reduces working-capital needs, and allows re-exports without incurring domestic duties.

[7]Most of the subsequent reforms have happened after 2016, falling outside the period of scope of this study.

operadores in Spanish).[8] To become qualified, they must conduct a lawful and formal activity, establish a new legal entity, and comply with the conditions set by Decree 2147 of 2016. The regime allows firms of any size and in any sector to qualify, even if they do not engage in imports or exports. However, becoming a qualified user involves a lengthy and costly approval process that requires multiple legal, financial, and technical submissions.[9] In addition, applicants must comply with specific investment and employment requirements, which are considerable for most firms. Depending on their total assets, companies must commit to new investments ranging from 5,000 to 11,500 SMMLV, equivalent to approximately USD 1.3 to 3.0 million, within three years and to create between 20 and 50 new formal jobs.

Once qualified, firms pay rent to the FEZ operator or developer in exchange for essential infrastructure, such as offices, warehouses, and production facilities. All firms are subject to the exclusivity principle, which requires production activities to remain within the FEZ and thereby reinforces its role as a place-based economic policy instrument. These conditions make the qualification process demanding and resource-intensive, effectively limiting participation to firms with substantial financial and organizational capacity. At the same time, the exclusivity requirement further restricts the participation of already established— and typically larger— firms, for whom the cost of relocating production facilities, given existing factories and the sunk costs associated with their plants, often outweighs the potential benefits of operating within an FEZ.

To remain in the regime, user firms must demonstrate their capacity to generate both investment and employment. Beginning in 2017, they were also required to submit quarterly reports during their operation. These reports track progress on the *plan maestro de desarrollo* and must include information on job creation, investment, economies of scale, the development of industrial processes in goods and services, proposals for sustainable development (covering environmental, social, and economic components), and financial performance.[10]

Colombian FEZs differ from many SEZs and other place-based programs worldwide, although they also share common features. First, in terms of size and physical design, they resemble the smaller industrial parks found in India, Vietnam, or the Dominican Republic, rather than the municipality-sized SEZs in China or the broad census tracts designated as Empowerment Zones in the United States. Colombian FEZs operate as fenced-in industrial parks where entry is restricted (see Appendix **??**). Second, in terms of governance, while the government retains the authority to declare and approve FEZs, the initiative typically originates from private investors or individual firms, contrasting with the more state-driven approach observed in China or the publicly coordinated planning of U.S.

[8] Qualified firms are classified as industrial goods users, industrial services users, or commercial user operators. Industrial users benefit from both tax and customs incentives, whereas commercial operators are limited to customs exemptions since they do not perform value-added activities.

[9] Firms must submit various documents, including their certificate of incorporation and legal representation, and are required to provide a detailed description of the project to be developed, as well as to present a financial and economic feasibility study demonstrating the project's soundness and the firm's capacity to carry out the proposed activities.

[10] The reporting obligation was introduced through Resolution 2135 of 2017, which falls outside the period analyzed in this study. It is included here only to illustrate the subsequent evolution of the FEZ regime.

Empowerment Zones. Third, with respect to procedural complexity, the creation of a zone in Colombia involves a lengthy and highly regulated approval process: proposals are subject to national-level evaluation and require detailed development plans. This process introduces substantial uncertainty for applicants, who often face long delays and limited information about whether and when their proposal will be approved. Consequently, the creation of a zone is partly endogenous to firms—since only those with sufficient financial and organizational capacity can bear the risks and costs of application—but exogenous to municipalities, which have no formal role in initiating or approving FEZs. Finally, Colombia is distinctive in allowing single firms to be declared as "Special" FEZs, effectively granting zone status to individual companies alongside the larger multi-firm permanent zones—an institutional feature uncommon among SEZ regimes worldwide.

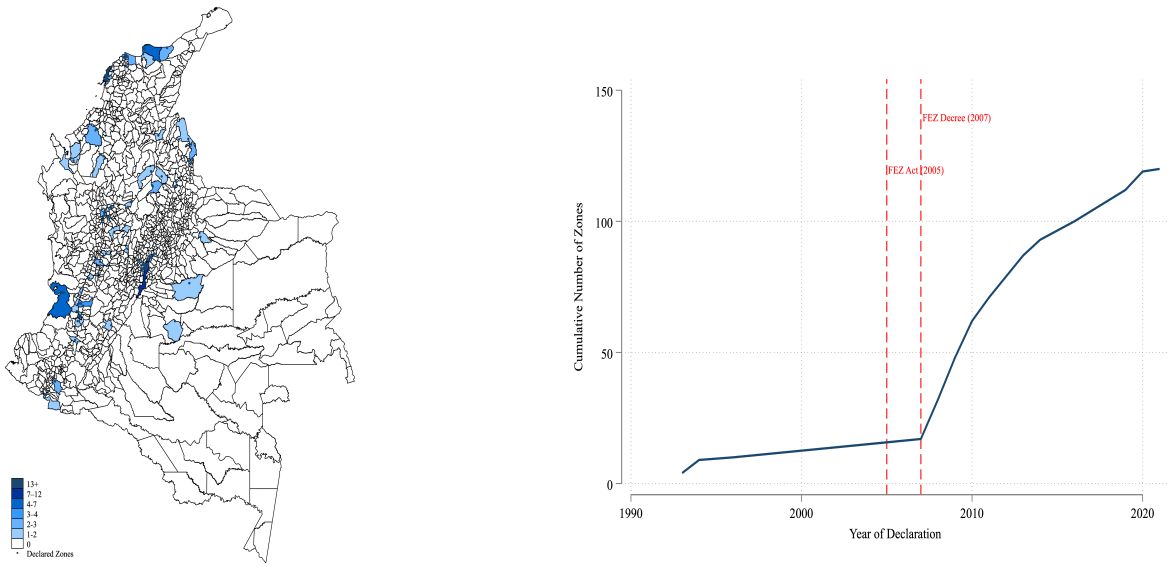
Since the enactment of the FEZ Act (2005) and the subsequent FEZ Decree (2007), the number of zones in Colombia has expanded rapidly (Figure 2a), surpassing 100 by 2015 and reaching 124 by 2024, marking a turning point that transformed FEZs into a widespread place-based policy instrument and positioning the country as the South American leader in SEZs. These zones, categorized into 42 "Permanent" and 82 "Special," are located in municipalities that contribute to more than half of Colombia's added value (Cámara de Usuarios de Zonas Francas, ANDI, 2019). FEZs support diverse sectors, including industry (60 zones), services (47 zones), and agroindustry (17 zones), and 87% of participating firms are medium-sized enterprises (Cámara de Usuarios de Zonas Francas, ANDI, 2019). The spatial distribution of FEZs (Figure 2a) reveals strong concentrations in Bogotá, Valle del Cauca, and the Caribbean coast, while large areas of the Amazon and Orinoquía remain without any zones.

Despite this expansion, Colombia's aggregate performance remains modest compared with other Latin American countries. As of 2022, exports from Colombian FEZs represented only 3.8% of total exports and contributed 0.8% to GDP, whereas FEZ exports accounted for 45.9% of national exports and 23% of GDP in Nicaragua, and for 37.3% and 15.8% in Honduras. Even smaller economies such as El Salvador and Costa Rica report much higher contributions from their FEZs. With 124 zones, second to the Dominican Republic's 246, Colombia thus stands out as a country with a large number of FEZs but a relatively limited aggregate economic impact. At the same time, the fiscal and trade incentives offered by Colombian FEZs are broadly competitive with those available elsewhere in the region, suggesting that the limited aggregate impact cannot be attributed to weaker benefits.[11]

Although FEZs aim to stimulate investment, employment, and industrial development, ongoing debates question their broader economic impact. Concerns focus on taxation inequality, high fiscal costs, and their alignment with competitive economic regimes. According to the Association of Free Trade Zones of the Americas (2022), in Latin American countries, every unit of local currency exempted as an incentive to free trade zones generated an average return to the economy of between two and seven times the value of the tax exemptions granted. These figures point to potentially

[11] Detailed comparative evidence on FEZ performance and incentives across countries is provided in Appendix **??**.

Figure 1: Free Economic Zones in Colombia



(a) Cross-Section of zones

(b) Number of zones over time

Panel (a) displays all the approved and declared zones in Colombia. Panel (b) shows the evolution of free economic zones, with known years of establishment, over time. Source: FEZ reported by the Ministry of Industry and Commerce of Colombia, and primary location data collected by the author.

positive effects, but their welfare implications depend on the broader institutional and fiscal context.

Given that FEZs grant preferential treatment to participating firms, they can reshape patterns of entry and competition. Some firms gain advantages from access to fiscal and trade incentives, while others outside the zones may face relative disadvantages (Departamento Nacional de Planeación, 2021). In developing country settings, where labor markets are often segmented and enforcement capacity is limited, these changes in firm dynamics can translate into broader shifts in employment structures. The coexistence of formal and informal activities within and around FEZs raises important questions about whether these zones reduce informality or reinforce it through indirect linkages. Barriers to entry may also limit the capacity of FEZs to absorb workers, so that adjustments occur partly through the informal sector. As highlighted by the World Bank (2020), FEZs are not substitutes for economy-wide reforms but rather partial instruments that accelerate adjustments in specific areas while broader reforms remain constrained by political economy considerations. Their developmental role depends on how they interact with existing institutions, regulations, and labor market structures. This paper contributes to this debate by examining the Colombian experience.

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 the FEZs.

2.2.1 *Free Economic Zones Data*

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 year of creation for each zone, which I use to define exposure since it marks the start of the institutional and investment processes associated with the program. To focus on zones with potential spatial and lasting effects, I restrict the sample to operating permanent and special FEZs, excluding offshore and transitory ones as well as those declared but never operational. Using machine learning algorithms, I match the administrative names of FEZs to their locations in Google Maps and georeference them precisely at the municipality level. FEZs and municipalities do not overlap, and all coordinates were manually verified against official documentation and local reports, ensuring 100% location accuracy. Unlike studies that rely on assumptions about circular areas (e.g., SEZs in India), I hand-collected information on the exact geographic boundaries and sizes of FEZs, which allows me to measure their actual spatial footprint. The geo-referencing further allows me to compute distances from sea ports, airports and highways that I will use as control variables in the empirical analysis.

To address potential discrepancies between the declaration and effective operation of zones, I use Google Earth's historical imagery and firm-level qualification records to identify when each FEZ became active. This validation enables me to match administrative dates with observable economic activity and to exclude zones that never started operations. The final dataset includes all operating FEZs in Colombia with detailed information on their geographic boundaries, area, and type. 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 A.

2.2.2 *Outcomes Data*

Municipality characteristics. Having information on the start of operation of each FEZ and the municipalities that host them, I rely on data compiled by the Center for Studies on Economic Development (CEDE, Universidad de los Andes) to measure local characteristics. This dataset provides a comprehensive overview of Colombian municipalities from the 1990s to the present, based primarily on administrative records from government agencies. It includes information on local tax revenues, public investment, population, and the urban–rural composition, as well as time-invariant features such as distance to major cities, distance to Bogotá, and land area.[12] I merge these data with the FEZ dataset by municipal code to ensure spatial consistency. To proxy for local economic development, I additionally use nighttime light intensity aggregated at the municipality level, combining data from the Defense Meteorological Satellite Program (DMSP) and the Visible

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

Infrared Imaging Radiometer Suite (VIIRS). These have been cleaned and harmonized into a single panel by Li et al. (2020). Nighttime lights provide a widely used proxy for local economic activity, particularly in settings where formal economic data are scarce or of limited quality.

Workers. To study labor market outcomes, I use data from 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 that covers approximately 240,000 households per year and provides nationally representative data on employment, sector of work, earnings, and social security coverage, as well as demographic and socioeconomic characteristics such as sex, age, marital status, and education. I identified 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 survey is aggregated at the municipality level using department-level survey weights, which ensures consistency in regional representation over time.

The GEIH is not representative for all municipalities, but it covers around 190 municipalities, which are grouped into treatment cohorts (ranging from 5 to 35 municipalities; see Table Appendix **??**). This aggregation mitigates concerns about sampling noise and allows for consistent comparisons across cohorts. I further collapse monthly data into annual averages to reduce measurement error. Importantly, estimates of formal employment from the GEIH closely match those from administrative records (PILA), supporting the reliability of the survey as a source for analyzing formal and informal labor outcomes.

Firms. To examine how FEZs affect firms, I combine several complementary sources of firm-level data.

To capture direct effects, I use administrative balance sheet and income statement data from the Superintendencia de Sociedades. Reporting to this agency is mandatory for firms with annual sales or assets exceeding 20,000 times the legal monthly minimum wage (approximately USD 4.1 million). The dataset covers medium and large firms and includes detailed financial information on sales, assets, liabilities, equity, and profits. These data allow me to track the performance of firms most likely to be directly affected by FEZ incentives and regulations.

To capture indirect effects of FEZs on manufacturing activity, I use data from the Encuesta Anual Manufacturera (EAM), collected annually by the National Statistics Office (DANE). The EAM is a census of all manufacturing establishments belonging to firms that own at least one plant with ten or more employees or whose annual production value exceeds a threshold close to USD 100,000. The survey spans 2001–2022 and includes between 4,000 and 5,000 plants per year, representing over 90 percent of Colombia's formal manufacturing production above the inclusion threshold. Each establishment is assigned a unique identifier that allows tracking over time, independently of ownership changes. The dataset reports detailed information on output value, sales, employment, wages, investment, and input use.

To analyze formal firm entry and exit dynamics, 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. 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 unique in Latin America (Salazar et al., 2017) and, as a result, holding an active license is widely used as an indicator of formality (Cárdenas and M, 2007; Galiani et al., 2016; Echeverry, 2010). Since 2011, Confecámaras has centralized records from all 57 local chambers, producing a unified database covering 2011–2018 with detailed firm entry and exit dates and precise geographic identifiers. This dataset represents the universe of registered formal firms in Colombia and allows me to analyze the extensive margin of firm dynamics—that is, the entry and exit of formal establishments across municipalities.

Finally, as a robustness source, I use 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, along with formal self-employed contributors. Each observation corresponds to a worker–employer match in a given month and year, reporting the worker's earnings base, sex, age, municipality, employment type, and a firm identifier. Although both firm and worker identities are anonymized—preventing direct linkage to FEZ exposure—the data provide valuable information on firm size, employment composition, and productivity-related measures that complement RUES.[13]

3 Research Design

This section describes the empirical strategies used to estimate the causal impact of Free Economic Zones (FEZs) on local economies, workers, and firms. Section 3.1 presents the identification strategy for analyzing the effects of FEZs on municipalities that host the zones. Section 3.2 outlines the research design used to examine labor market outcomes among workers. Finally, Section 3.3 details the empirical approach for studying both the direct effects on firms located within FEZs and the indirect effects on manufacturing firms operating in the same municipalities, for which zone affiliation cannot be directly observed.

3.1 Research Design: Hosting Municipalities

The ideal experiment to estimate the causal effect of Free Economic Zones (FEZs) on local economies would involve randomly assigning FEZs across municipalities. However, this is not feasible in practice, as the establishment of FEZs reflects both economic and political considerations. Local governments

[13]PILA is used as a robustness dataset rather than a main source, as its coverage ends in 2016 and identifiers are anonymized. Nonetheless, it offers useful complementary evidence on firm size and productivity.

emphasize factors such as proximity to major towns, ports, and rivers, as well as the availability of contiguous land parcels. At the same time, national objectives related to regional equalization and balanced growth, along with political factors, also influence their placement. Consequently, municipalities with higher economic potential may be more likely to receive a FEZ, while those with stronger infrastructure or political connections may both attract FEZs and experience stronger economic performance.

To address these concerns, I exploit the staggered rollout of FEZs across municipalities and over time through a difference-in-differences (DiD) framework. This approach leverages quasi-experimental variation in FEZ implementation and, under the assumption of parallel trends, identifies the causal effects of FEZs on local economic outcomes. The staggered timing of FEZ declarations, driven largely by administrative approval processes rather than local economic conditions, provides plausibly exogenous variation in treatment exposure. However, comparing municipalities that host FEZs with those that do not involves a trade-off. On the one hand, the staggered rollout generates quasi-experimental variation that mitigates concerns of self-selection into treatment, since local governments cannot unilaterally create a zone. On the other hand, FEZs may generate spatial spillovers to nearby areas—through labor mobility, supplier linkages, or local demand effects—which could violate the Stable Unit Treatment Value Assumption (SUTVA) if untreated municipalities in the same region are affected by nearby zones. To address this, I test for spatial spillovers and, in some specifications, exclude municipalities within a fixed distance of treated areas.

Econometric Framework. I estimate the causal and dynamic effects of FEZ implementation on local economic outcomes of municipality m by estimating the following event-study specification:

$$Y_{mt} = \alpha_m + \delta_t + \sum_{k=-7}^{8} \theta_k \mathbf{1}\{t = t^*(m) + k\} + \sum_{k=-7}^{8} \beta_k \mathbf{1}\{t = t^*(m) + k\} \times FEZ_m + \varepsilon_{mt}. \quad (1)$$

Here, Y_{mt} represents an outcome of interest (e.g., nightlight intensity or fiscal revenues) in municipality m and year t; α_m denotes municipality fixed effects, and δ_t denotes year fixed effects. FEZ_m is an indicator equal to one for municipalities that eventually host a FEZ, and $t^(m)$ denotes the year in which the first FEZ in municipality m becomes operational. Each event-time indicator $\mathbf{1}t = t^(m) + k$ refers to the time relative to FEZ implementation. The main coefficients of interest, β_k, capture—under the parallel trends assumption—the causal effect of hosting a FEZ on the outcome of interest. All event-time dummies are normalized relative to the period immediately before FEZ entry ($k = -1$), so that the β_k coefficients are interpreted as changes relative to that baseline year. Therefore, β_k for $k \geq 0$ measure the effect of FEZ implementation in a municipality on outcome Y_{mt}, k years after the opening or declaration of the zone, relative to one year prior to implementation and to untreated or not-yet-treated municipalities at each event time, conditional on municipality and year fixed effects. Robust standard errors are clustered at the municipality level.

Following Sun and Abraham (2021), the traditional two-way fixed-effects estimator may be biased if treatment effects vary across municipalities or over time. To address this concern, I also estimate the event-study using the doubly robust difference-in-differences estimator proposed by Callaway and Sant'Anna (2021), which accounts for heterogeneous treatment timing and cohort-specific effects. This framework provides a transparent visualization of pre-trends and the dynamic adjustment of local economies following FEZ establishment.

Identification Assumptions. The key identifying assumption of this model is that, in the absence of FEZ implementation, differences in outcomes between treated and control municipalities would have remained constant over time (the parallel trends assumption). Municipalities selected for FEZs may differ systematically from others in baseline characteristics—such as infrastructure quality, industrial composition, or political influence—which could independently affect economic outcomes. Therefore, I do not assume that FEZ placement is random with respect to local economic conditions. Instead, the identifying condition requires that unobserved time-varying shocks do not drive the staggered timing of FEZ declarations. The rapid rollout of zones across the country makes it unlikely that local economic dynamics fully explain their timing. In practice, the timing of FEZ approvals is primarily related to observable factors such as population size and regional location (see Appendix Table C.2). To mitigate potential biases, I include municipality and year fixed effects, as well as department-by-year fixed effects, which absorb all time-invariant municipal characteristics and common shocks at the departmental level. I also test the validity of the parallel trends assumption by estimating event-study models and conducting placebo tests. These exercises show no evidence of differential pre-trends in employment, wages, or fiscal revenues prior to FEZ establishment.

Another concern is that FEZs may be declared earlier in municipalities closer to, or better connected with, capital cities, exposing them to different spillover trends than later-treated cohorts. To address this, I re-estimate results under a conditional parallel trends framework, comparing municipalities with similar baseline characteristics (e.g., distance to major cities and level of rurality). Under this weaker assumption, results remain consistent, although estimates become less precise due to smaller cohort sizes. A further issue is that FEZs could generate spillover effects on neighboring municipalities through commuting, supplier linkages, or local demand, potentially violating the Stable Unit Treatment Value Assumption (SUTVA). To test for this, I explicitly assess spatial spillovers and, in some specifications, exclude municipalities located within a fixed radius of treated areas. Results remain robust to these restrictions.

Finally, I assume that municipalities do not adjust their economic behavior in anticipation of FEZ approval. This assumption is plausible given the lengthy approval process—often spanning some years between proposal and declaration—and the fact that local economic effects typically materialize gradually once zones become operational.

Together, these tests and robustness checks support the credibility of the identification strategy and the validity of the parallel trends assumption.

3.2 Research Design: Workers in Hosting Municipalities

This section describes the empirical strategy used to estimate the causal effect of FEZ implementation on workers' labor market outcomes. The objective is to measure how the creation of FEZs affects the composition, formality status, and earnings of workers living or employed in hosting municipalities. As with the municipality-level analysis, the identification challenge is that municipalities receiving FEZs may differ from others in characteristics that also influence labor market dynamics. To isolate the effect of FEZ exposure, I construct comparable groups of workers across treated and untreated municipalities and estimate difference-in-differences models with individual-level controls and fixed effects.

Workers in municipalities that host FEZs may differ systematically from those in other municipalities in both observable and unobservable ways. For instance, treated areas may have larger formal sectors, higher educational attainment, or stronger industrial bases. To mitigate these concerns, I restrict the analysis to urban workers aged 18–64 in municipalities covered by the GEIH labor force survey, and compare those employed in municipalities that eventually host a FEZ to those in municipalities that never host one. I exclude municipalities within a short distance of FEZs to avoid spillover contamination from commuting or supplier linkages. The comparison group thus consists of workers in similar municipalities not directly exposed to the program.

Given that the labor force survey is a repeated cross-section rather than a panel, I cannot follow the same individuals over time. Instead, I perform a *compositional change test*, comparing how the average characteristics and outcomes of the local labor force evolve across survey waves before and after FEZ implementation. This approach captures how FEZs reshape the structure of employment within municipalities—rather than the trajectories of specific workers—by focusing on shifts in the relative shares of employment by skill, sector, or formality status. To ensure comparability, I control for individual-level covariates such as age, gender, education, marital status, sector, and migration status, along with municipality and year fixed effects. These controls absorb persistent demographic and institutional differences across municipalities and guarantee that the observed effects are not mechanically driven by demographic or skill-based imbalances across municipalities

Econometric Framework. I estimate the following baseline specification to identify the average effect of FEZ exposure on workers' outcomes:

$$Y_{imt} = \alpha + \beta FEZ_{mt} + \theta \mathbf{X}imt + \lambda_t + \delta_m + \varepsilon imt, \tag{2}$$

where Y_{imt} denotes the outcome of individual i in municipality m and year t (such as formal employment or log earnings); FEZ_{mt} equals one if a FEZ is operational in municipality m at time t; $\mathbf{X}imt$ is a vector of individual-level controls; λ_t and δ_m are year and municipality fixed effects; and εimt is the error term. Standard errors are clustered at the municipality level. The coefficient β captures the average effect of FEZ exposure on workers living or employed in affected municipalities.

To explore heterogeneity by skill, I extend the baseline model by interacting FEZ exposure with a worker's skill level:

$$Y_{imt} = \alpha + \beta FEZ_{mt} + \gamma \left(FEZ_{mt} \times Skill_{it}\right) + \theta \mathbf{X} imt + \lambda_t + \delta_m + \varepsilon imt. \tag{3}$$

Here, $Skill_{it}$ indicates medium- or high-skilled workers, and the interaction term $FEZ_{mt} \times Skill_{it}$ captures the differential effect of FEZ exposure for these groups relative to low-skilled workers. This triple-difference structure follows the regional-shocks literature (Dix-Carneiro and Kovak, 2017; Kovak and Morrow, 2023), allowing identification of whether FEZs alter the relative demand for skilled labor or reinforce local segmentation across skill groups.

Because I cannot identify workers employed directly within FEZs, the estimated coefficients should be interpreted as *intention-to-treat–type* effects of FEZ exposure. They capture the average impact of living or working in a municipality affected by FEZs, encompassing both direct employment within the zone and indirect effects through local labor demand, wage pressures, and reallocation between formal and informal sectors. Identification relies on the assumption that, absent FEZ implementation, trends in employment, informality, and earnings would have evolved similarly across treated and untreated municipalities. This assumption is supported by pre-trend tests and placebo regressions showing no differential trajectories before FEZ establishment (Section 3.1).

This empirical design provides a framework to quantify how FEZs reshape the *composition* of local employment rather than its aggregate level. If FEZs attract high-productivity, skill-intensive firms, they may raise the relative demand for skilled labor, increasing wage differentials and displacing less-skilled formal workers. These displaced workers may transition into informal employment, consistent with a rise in overall informality despite stable aggregate employment. By decomposing the effects across skill groups, the compositional change test allows me to evaluate whether FEZs primarily induce upgrading through higher skill demand or reallocation through the displacement of less-skilled workers.

3.3 Research Design: Adopting Firms and Firms in Hosting Municipalities

After outlining the framework to study workers' outcomes, I now turn to firms, the key channel through which Free Economic Zones (FEZs) influence local economies. Firm dynamics can mediate the impact of FEZs by reshaping labor demand, wage-setting, and competitive conditions. I analyze two distinct levels of exposure. First, I study the *direct effects* of FEZ designation on firms that obtain FEZ status, using detailed financial statement data that track investment, profitability, and employment expenses. Second, I examine the *indirect effects* of FEZ exposure on the broader universe of formal manufacturing firms, analyzing how employment, production, and sales evolve in

municipalities hosting FEZs.[14]

As in the analysis for municipalities and workers, the empirical strategy relies on a Difference-in-Differences (DiD) design with two main comparisons: (i) firms before and after obtaining FEZ status, relative to those that never receive it; and (ii) firms located in municipalities with FEZs relative to those in municipalities without FEZs, before and after zone establishment. The first comparison captures the direct effects of FEZ designation on certified firms, while the second captures both direct and indirect effects on firms exposed to FEZ presence within their local markets.

Adopting Firms. I estimate firm-level effects using annual reports from the *Superintendencia de Sociedades* (Super-Sociedades), which provides detailed balance sheets and income statements for all registered corporations above mandatory reporting thresholds. The panel spans 2001–2015 and includes medium and large firms with sales or assets exceeding 20,000 times the legal monthly minimum wage (approximately USD 4.1 million).[15] The dataset is an *unbalanced panel*, as firms enter and exit the sample over time. Importantly, these entry and exit patterns reflect reporting thresholds and compliance, not necessarily firm creation or closure. Because the policy is implemented gradually and firms may appear intermittently in the records, I restrict the estimation to firms observed in or after the creation of the first FEZs to ensure consistent coverage of the treatment period.[16] To maintain internal consistency, I restrict to firms with valid accounting records (positive values for sales, assets, liabilities, and equity) and verify that the accounting identity holds in each report. These restrictions ensure that variation in FEZ status is compared across comparable firms and that changes in sample composition do not confound the estimated effects.

Formally, I estimate the following specification:

$$Y_{fmt} = \alpha + \beta FEZ_{ft} + \theta \mathbf{X} fmt + \lambda_t + \delta_m + \gamma_f + \varepsilon fmt, \tag{4}$$

where Y_{fmt} denotes outcome Y for firm f located in municipality m in year t; FEZ_{ft} equals one if firm f operates under FEZ status in year t; $\mathbf{X}_{fmt}$ includes time-varying firm characteristics (e.g., age, lagged assets); λ_t and δ_m are year and municipality fixed effects; and γ_f are firm fixed effects. Standard errors are clustered at the firm level.

Similar to the municipality and workers analysis, identification relies on the standard parallel trends assumption: in the absence of FEZ designation, treated and untreated firms would have followed similar trajectories. As before, FEZ placement is not assumed to be random with respect to

[14] The manufacturing census lacks firm-level identifiers distinguishing FEZ and non-FEZ firms within FEZ municipalities. Consequently, estimates are interpreted at the municipality level, capturing the average impact of FEZs on all firms in the area rather than isolating direct users.

[15] 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.

[16] As a robustness check, I also estimate specifications using a balanced subsample of firms continuously observed between 2001 and 2015, which isolates within-firm variation over time and I verify that FEZ designation is not systematically correlated with the probability of exiting the reporting panel, indicating that attrition does not bias the estimates (Appendix ?).

firm or local characteristics. The key identifying condition is that unobserved shocks are not systematically correlated with the staggered timing of FEZ declarations. The rapid nationwide rollout of FEZs and the multi-stage administrative approval process reduce the scope for firm-specific dynamics to drive treatment timing. To mitigate remaining concerns, the model controls for observable firm characteristics such as age, sector, and pre-policy characteristics. To test for pre-trends and to examine the dynamic adjustment of firms following FEZ designation, I estimate the event-study specifications.

Firms in Hosting Municipalities. I examine all formal manufacturing firms located in FEZ municipalities, to understand how Free Economic Zones affect the broader productive structure of local economies. This analysis captures the *indirect* effects of the program, those arising from changes in local demand, supplier linkages, and labor market competition that extend beyond the firms directly operating within FEZ boundaries. In contrast to the previous specification, which focuses on firms that formally obtained FEZ status, this analysis provides a municipality-level perspective on how the presence of a FEZ shapes manufacturing activity among nearby firms. It thus complements the worker-level framework by identifying whether local production adjusts through reallocation, expansion, or compositional shifts across firms.

The establishment of FEZs can affect firm outcomes through several channels. First, the entry of FEZ firms may alter local demand conditions and input–output linkages, benefiting suppliers and service providers in nearby areas. Second, increased competition for labor and capital may change production costs and the allocation of resources across firms. Third, FEZ-related public and private investments may improve infrastructure or market access, indirectly affecting the productivity of non-FEZ firms. These mechanisms parallel those identified by **?** in the context of multinational expansions, where both *labor market exposure* and *firm-level exposure* channels drive the transmission of local shocks through supply-chain and wage linkages. While the data do not allow me to observe firm-to-firm linkages or supply-chain transactions directly, most manufacturing production and intermediate input exchanges in Colombia occur locally. As a result, municipality-level changes in firm outcomes provide an informative measure of the local equilibrium effects of FEZ presence.

I use firm-level data from the *Encuesta Anual Manufacturera* (EAM, Annual Manufacturing Survey), collected by the Colombian statistical agency DANE. The analysis covers the period 2001–2018, restricting the estimation sample to firms that were already operating before 2005 and following them over time. This dataset complements the financial statement analysis in two important ways. First, while the *Superintendencia de Sociedades* data cover only the largest corporations and capture direct beneficiaries of FEZ status, the EAM includes the full universe of formal manufacturing firms, allowing the study of both direct and indirect effects within FEZ municipalities. Second, the EAM provides detailed information on production, employment, and output, enabling a closer examination of sectoral dynamics that are not visible in financial balance sheets.

Formally, I estimate the following specification:

$$Y_{fmt} = \alpha + \beta FEZ_{mt} + \theta \mathbf{X} fmt + \lambda_t + \delta_m + \chi_f + \varepsilon fmt, \tag{5}$$

where Y_{fmt} represents the outcome of firm f located in municipality m at time t (such as employment, output, or exports); FEZ_{mt} equals one if a FEZ is operational in municipality m at time t; $\mathbf{X}_{fmt}$ is a vector of time-varying firm characteristics (e.g., age, size, capital intensity); λ_t and δ_m denote year and municipality fixed effects; and χ_f are firm fixed effects. The coefficient of interest β captures the average impact of FEZ presence on all manufacturing firms located in treated municipalities—encompassing both direct beneficiaries and firms indirectly affected through changes in local demand, competition, and input linkages. In this sense, the results measure the equilibrium adjustments of the local manufacturing sector rather than the isolated effects of firm-level participation in FEZ programs.[17] Standard errors are clustered at the municipality level.

As in the previous analyses, identification relies on the parallel trends assumption: in the absence of FEZ designation, outcomes for firms in FEZ municipalities would have evolved similarly to those in non-FEZ municipalities. This requires that the timing of FEZ openings is not systematically related to unobserved shocks in firm performance. Given that the establishment of FEZs follows a national administrative process rather than firm-level decisions, the staggered rollout provides plausibly exogenous variation in treatment exposure. To further support identification, I estimate event-study models that test for pre-trends and allow for dynamic treatment effects.

4 The Causal Effect of FEZs on the Local Economy

This section discusses the effects of Free Economic Zones (FEZs) on local economies, workers, and firms. Section 4.1 covers the municipality-level effects, focusing on employment, nightlights, and fiscal revenues. Section **??** examines how FEZs reshape the composition of local employment and workers' earnings. Section **??** analyzes the response of firms, distinguishing between those that obtain FEZ status, those located in hosting municipalities, and firm dynamics. Finally, Section **??** discusses robustness checks and alternative mechanisms.

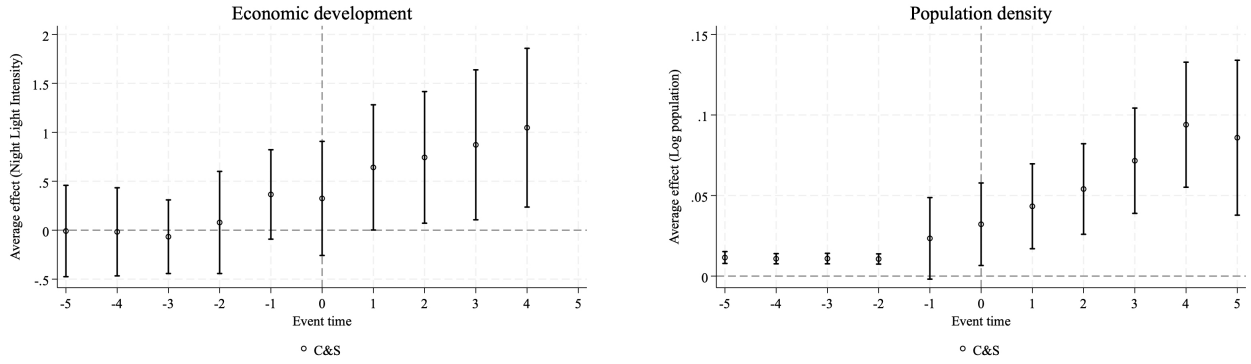
4.1 Main Results: Effects of FEZs on Municipalities

4.1.1 Economic Development

I begin by examining the broader local development effects of Free Economic Zones (FEZs) on hosting municipalities. Figure 3a reports the estimated coefficients β_k from Equation 1 for nightlight intensity, a widely used proxy for local economic activity and infrastructure utilization. Treated municipalities experience a marked and persistent rise in luminosity, reaching approximately 1–1.5

[17] Because I cannot directly identify which firms operate within FEZ boundaries, the estimated coefficients should be interpreted as *intention-to-treat (ITT)-type effects.*

standard deviations above pre-treatment levels within four years of FEZ establishment. Although the confidence intervals remain wide—reflecting intrinsic measurement noise and local heterogeneity—the overall pattern points to a sustained increase in commercial, industrial, and possibly public infrastructure activity following the creation of the zones. This evidence aligns with the view that FEZs foster localized agglomeration and intensify infrastructure use.[18]



(a) Night-light intensity

(b) Population density

Figure 3: Event-study estimates of FEZ effects on local development outcomes.

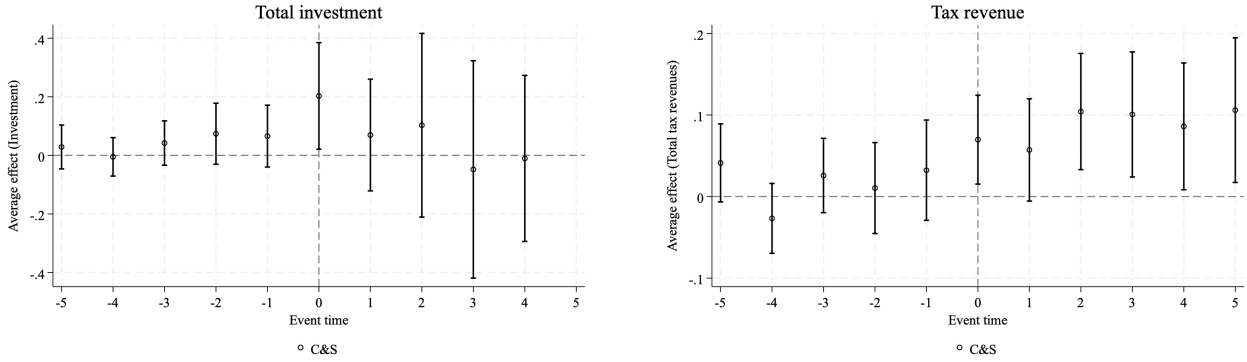
Notes: Panels 3a and 3b display event-study coefficients and 95% confidence intervals from specification (1). Panel 3a shows the effect of FEZ openings on night-light intensity, measured in standard-deviation units, and Panel 3b shows the effect on the logarithm of population density. The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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 3b shows the estimated effect of FEZ implementation in a municipality on the log population density. The results show that treated municipalities increase the population density gradually over the sample period; however, mild positive pre-trends complicate a strict causal interpretation. Treated areas were already on a trajectory of demographic growth prior to FEZ designation, making it difficult to disentangle the zone's effect from underlying regional dynamics. This pattern suggests that FEZs have not, within the observed period, accelerated population inflows or retention beyond existing trends.

Municipal tax revenues provide a key measure of local fiscal capacity and, more broadly, of whether FEZs generate positive externalities to municipalities that extend beyond firm-level activity. If FEZs successfully stimulate business expansion, property investment, or commercial transactions, such changes should be reflected in higher local tax collections. Figure 4b shows that municipal tax revenues exhibit a steady and persistent upward trend following FEZ introduction, with effects reaching approximately 0.15 log points by year five. Importantly, there is no evidence of differential pre-trends between treated and control municipalities, supporting a causal interpretation of this

[18] A natural extension would be to investigate agglomeration economies more directly by geolocating firms and tracing their clustering dynamics. While feasible, this requires constructing a detailed geocoded firm-level database, which is beyond the scope of the present paper.

fiscal improvement. These revenues are primarily drawn from three local sources: the Industry and Commerce Tax (ICA) on commercial, industrial, and service activities; the Unified Property Tax, levied on real estate based on cadastral valuation; and the fuel surtax, applied to gasoline and other liquid fuels to finance transport and infrastructure projects. Consistent with this, Appendix ? results show a significant rise in property tax collections in treated municipalities, which may reflect either an expansion of the tax base or higher assessed property values. Overall, these fiscal gains suggest that FEZs can strengthen municipal budgets, potentially enhancing local governments' capacity to finance infrastructure and public services.



(a) Municipal investment

(b) Municipal tax revenues

Figure 4: Event-study estimates of FEZ effects on local fiscal outcomes.

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 municipal investment, and Panel 4b 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). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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, attract additional private investment, and sustain local growth beyond the initial setup phase. By examining public investment dynamics, I provide insight into whether FEZs generate broader fiscal multipliers or remain isolated enclaves of activity. Figure 4a shows that FEZs have only a modest and short-lived effect on public investment, with an average increase of roughly 5 percent in the year of implementation. This pronounced spike is consistent with upfront expenditures on infrastructure, utilities, and administrative coordination required to operationalize the zones. However, the increase dissipates quickly, with investment levels reverting to those of untreated municipalities within one to two years. This pattern suggests that while FEZs may trigger an initial burst of public capital spending, these investments are not sustained over time, limiting their potential to generate lasting developmental spillovers.

In summary, these results indicate a nuanced view of FEZs' role in local development. On one

hand, FEZs expand municipal fiscal capacity and stimulate localized economic activity, as reflected in the sustained rise of tax revenues and night-light intensity. On the other, their broader effects on sustained growth, population dynamics, and public investment remain limited within the study horizon. This disconnect underscores a central challenge of place-based industrial policies: while they can generate concentrated gains in fiscal resources and infrastructure, translating these into durable and inclusive development outcomes is far less straightforward. Assessing the extent to which these localized gains translate into labor market adjustments is essential for understanding the broader effectiveness of FEZs.

4.1.2 Local Labor Market

I next examine the effects of FEZs on local labor market outcomes with broad measures at the municipality level by estimating Equation 1.

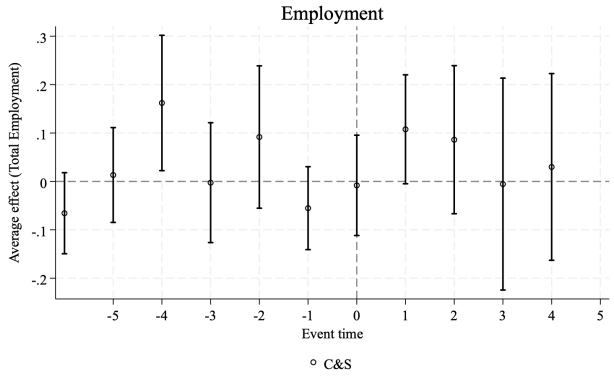


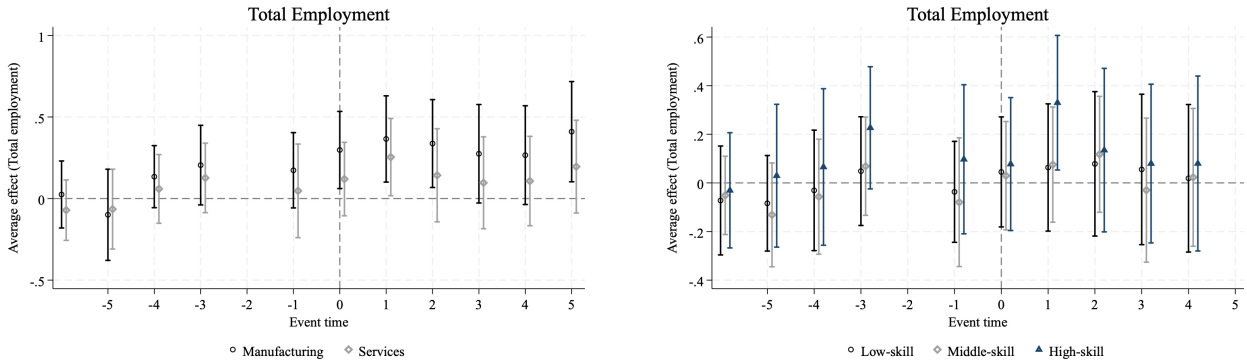
Figure 5: Event-study estimates of total employment following FEZ implementation.

Notes: Figure 5 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). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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.

The central stated objective of FEZs is to stimulate local job creation by attracting new firms and expanding existing production. Measuring total employment at the municipality level therefore provides an initial test of whether FEZs generate aggregate labor demand effects. Figure 5 shows no statistically significant effect on total employment in municipalities hosting FEZs. Estimated coefficients fluctuate around zero before and after FEZ introduction, with wide confidence intervals and no discernible upward trend. This pattern indicates that FEZs do not generate broad-based job creation at the aggregate local level. Instead, their influence appears to operate through changes in the composition of employment across sectors, firms, and worker groups—mechanisms explored in the following subsections.[19]

[19] Appendix results using the employment-to-population ratio, based on both administrative and survey data, cor-

To shed light on why FEZs do not expand overall employment, I decompose the effects by sector and skill. Figure 6a shows that employment gains are concentrated in manufacturing, while the services sector exhibits a muted response. Similarly, the skill-level decomposition in Figure 6b indicates that gains are driven primarily by high-skilled workers, with low- and middle-skilled labor experiencing comparatively smaller increases. These patterns suggest that FEZs channel employment toward specific industries and higher-skilled segments of the workforce, resulting in a reallocation of jobs rather than a broad-based expansion. Such targeted adjustments highlight the importance of compositional dynamics in evaluating the labor market impacts of place-based industrial policies.



(a) Employment effects by sector

(b) Employment effects by skill level

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

Notes: Panels 6a and 6b display event-study coefficients and 95% confidence intervals from specification (1). Panel 6a shows the effect of FEZ openings on the logarithm of total employment by sector, and Panel 6b 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). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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.

I estimate the effect of FEZs on the informality rate, defined as the ratio of total informal employment to total employment, to assess how FEZs reshape 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 often absorbs workers displaced from the formal sector. Changes in the informality rate thus capture reallocation between formal and informal employment. Figure 7 presents the estimated effects: consistent with a shift from the formal to the informal sector, the informality rate rises steadily by approximately five percentage points (around 5% relative to baseline), indicating a substantial compositional adjustment in local labor markets.[20]

roborate this finding: there is no evidence of increased labor market engagement following FEZ implementation. 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).

[20]These results, however, should be interpreted with caution. The informality rate in Colombia has declined steadily over time at the national level, reflecting broader structural trends. The estimates presented here indicate that, following the implementation of a FEZ, treated municipalities experienced a slower decline (or a relative increase) in informality compared with control municipalities.

The increase in informality is evident across multiple sectors, with Figure 8a showing the largest rises in manufacturing and more moderate increases in services.[21] Informality also increases disproportionately among low-skilled workers (Figure 8b), whereas changes among middle- and high-skilled workers are comparatively muted. These patterns suggest that the expansion of informal employment spans several parts of the labor market but is concentrated among lower-skilled segments that are typically more vulnerable to local economic shocks. The evidence points to a reallocation process in which FEZs alter the structure of local employment rather than its overall level, with adjustments occurring along the formality and skill margins.

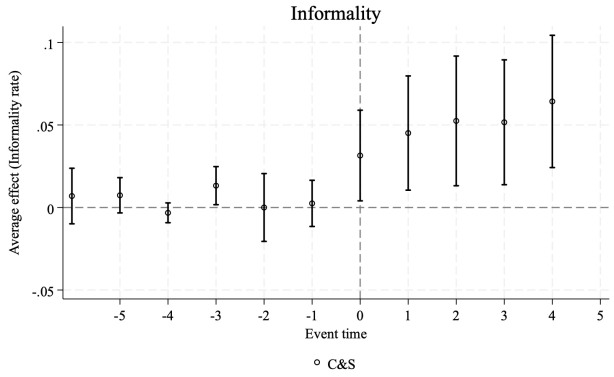


Figure 7: Event-study estimates of the overall informality rate following FEZ implementation.

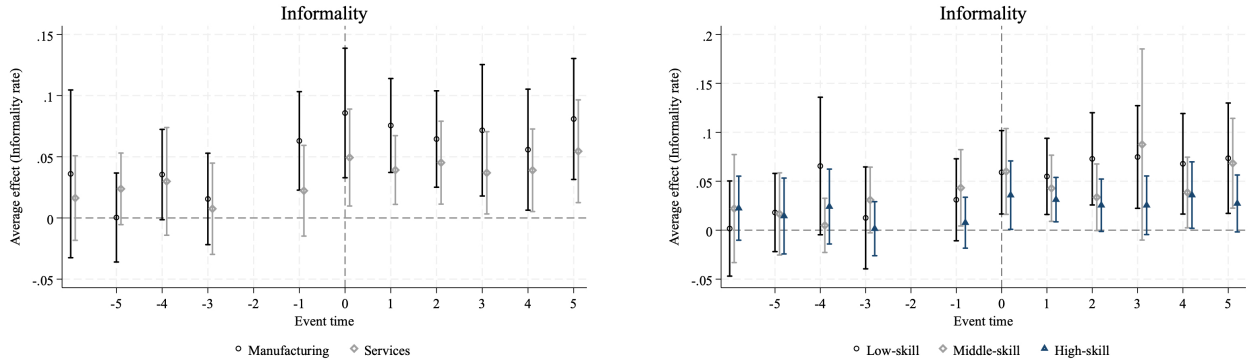
Notes: Figure 7 displays event-study coefficients and 95% confidence intervals from specification (1). The outcome is the informality rate, defined as the share of employed workers without social security contributions over total employment. Coefficients are expressed in percentage points (pp). The treatment is defined at the municipality level, and the sample includes municipalities that never hosted a zone (i.e., never-treated). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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.

Consistent with this reallocation mechanism, I find no significant effects on unemployment or labor force participation (Appendix Figure ?), indicating that workers displaced from the formal sector are absorbed into informal jobs rather than exiting the labor force. 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.[22]

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 likely masks heterogeneous wage adjustments across workers and firms: FEZs may generate upward pressure on wages for specific skill groups or

[21] 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.

[22] 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 8: Anatomy of the FEZ effects on informality, by sector and skill level.

Notes: Panels 8a and 8b 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 8a shows effects by sector, while Panel 8b 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). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. 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.

sectors, but these gains are offset by stagnant or declining pay among workers in non-beneficiary firms or informal employment. In this sense, worker-level analyses, examining distributional outcomes such as within-skill variation—provide a more accurate measure of how FEZs reshape earnings dynamics. Taken together, these results suggest that FEZs alter the composition of local employment and relative wage pressures without significantly affecting aggregate labor-market tightness or overall wage levels.

In summary, FEZs reshape local labor markets primarily through reallocation rather than expansion—shifting employment across sectors and skill groups and increasing informal work, without generating broad-based job creation or affecting unemployment. This nuanced pattern underscores the limits of place-based industrial policies in fostering inclusive growth: while FEZs can attract investment and raise local productivity, these gains do not necessarily translate into broader employment opportunities. In developing economies, where formal–informal segmentation and skill heterogeneity are pronounced, such dynamics fundamentally shape local adjustment to spatially concentrated policy shocks. Understanding how workers and firms respond to these incentives is therefore crucial for identifying the mechanisms that underlie these aggregate labor-market outcomes.

4.2 Worker-level Results: Compositional Change Test

Having established that FEZs do not generate local employment gains but instead induce compositional adjustments, this section examines the treatment effects of FEZs on worker-level labor market outcomes. The analysis identifies which groups of workers benefit or lose from these reallocations.

The compositional change test provides a way to assess whether observed aggregate stability masks underlying heterogeneity in workers' outcomes, conditional on a rich set of individual characteristics.

I begin by examining changes in employment, informality, and unemployment status. Table 1 reports difference-in-differences estimates at the individual level using the estimator proposed by Callaway and Sant'Anna (2021). Column (1) shows the average effect of FEZ implementation on the probability of being employed for individuals working in treated municipalities. On average, the estimates reveal no statistically significant effect—consistent with the aggregate null result documented earlier. However, once I account for heterogeneity in worker skills, clear compositional differences emerge. The triple-difference estimates in Column (2) indicate that the probability of being employed increases by 1.9 percentage points for medium-skilled workers and by 1.6 percentage points for high-skilled workers relative to low-skilled workers. These effects remain robust after controlling for a rich set of individual-level covariates, including age, gender, education, marital status, migration status, and sector of employment, indicating that the observed heterogeneity is not merely driven by demographic composition or sectoral sorting across municipalities but reflects genuine differences in labor demand by skill level.

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: Callaway and Sant'Anna aggregate estimates (columns 1). TWFE (columns 2). 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, the estimates in Table 1 show that FEZs significantly increase the likelihood of informal employment. In the specification with skill interactions (Column 4), the estimated coefficients indicate that low-skilled workers experience the largest rise in informality, with an effect of about 6.3 percentage points. By contrast, the interaction terms show that this effect is considerably smaller for medium- and high-skilled workers. When combining coefficients, the total effect amounts to roughly 0.1 percentage points for medium-skilled workers and 2 percentage points for high-skilled workers. The baseline (low-skilled) group thus drives the increase in informality, while

FEZ exposure mitigates or reverses that effect for more skilled groups—not because informality falls overall, but because these workers are less affected by, or shielded from, informalization pressures. This pattern suggests that FEZs deepen labor market segmentation by disproportionately displacing low-skilled workers into informal jobs while offering relatively better opportunities for those with higher skills.

As a robustness exercise, I adopt a broader definition of informality that accounts for multiple dimensions of labor market status. Informal workers are defined as individuals lacking formal employment protections and operating outside established regulatory frameworks. This includes salaried employees or domestic workers without social security contributions, unpaid workers, self-employed individuals, and employers in unregistered firms or firms without proper accounting practices. Using this alternative measure, Table B.4 shows that FEZs are associated with a significant increase in informality of about 2 percentage points. Skill-level interactions again reveal strong heterogeneity: informality declines for medium-skilled (–6.2 percentage points) and high-skilled workers (–4.2 percentage points), while low-skilled workers remain the primary group driving the aggregate increase. To further disentangle the drivers of this shift, I decompose informality into its components. Table B.5 shows that the increase is largely explained by declines in business registration, higher incidence of unpaid work, and lower rates of social security contributions among low-skilled workers. These findings confirm that FEZs facilitate formalization for skilled workers but simultaneously expand precarious forms of work among the less skilled.

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 or higher-skill formal jobs. 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 low-skilled workers experience no improvement. These heterogeneous effects mirror the patterns observed for employment and informality. Skilled workers benefit from FEZ-related labor demand, transitioning into new or expanding firms, whereas less-skilled workers are mainly displaced toward informal occupations rather than unemployment.[23]

Finally, I turn to wages. Table 2 shows that FEZs have 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 suggests that FEZs are associated with modest but significant wage gains at the local level. Column (2) shows a clear pattern for skill heterogeneity. The direct FEZ effect for low-skilled workers becomes negative but statistically insignificant, implying that wage gains are not evenly distributed across the workforce. The interaction terms reveal that

[23]The estimated effects capture genuine within-market adjustments rather than shifts in workforce composition, since I control for a different set of covariates, like age and migration status.

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: Callaway and Sant'Anna aggregate estimates (columns 1). TWFE (columns 2). 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

medium-skilled workers experience a wage increase of roughly 0.3 percent, while high-skilled workers earn about 4 percent more relative to low-skilled workers in FEZ municipalities.

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 largely unaffected. This pattern aligns with the broader 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 create opportunities for skilled workers to transition into more stable and better-paid employment, 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.[24] 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).

[24] 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).

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 or declining 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.

4.3 Firms-level Results: Firms as a Channel of Adjustment

4.3.1 Direct Effects from Firm-Level Financial Statements

Formally, I estimate Equation 4, 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).[25]

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 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 × 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 3 reports the effects of FEZ designation on firm performance outcomes. The results show no statistically significant effects on operating income, suggesting little systematic enhancement in firms' core productive efficiency. 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. The divergence between operating and net income indicates that FEZs improve firms' financial outcomes primarily through fiscal and financial channels—such as tax

[25] Detailed estimates and event-study dynamics are reported in Appendix B.2.1.

exemptions, tariff relief, and other policy incentives—rather than through higher productivity. These incentives strengthen firms' balance sheets and liquidity positions through cost reductions and rent capture without necessarily expanding production or efficiency.

This pattern has implications for the incidence of FEZ benefits. Gains accrue disproportionately to firms that were already productive or capital intensive, while the absence of corresponding improvements in operating income suggests limited spillovers to workers or local suppliers. FEZs therefore enhance profitability at the top of the firm distribution but do not appear to diffuse efficiency gains across the wider economy.

I then examine labor costs and intensities. Table 4 shows that FEZ designation substantially increases wage bills by about 78%, with salary expenditures rising sharply while labor shares in total costs and revenues remain stable. This implies that firms pay higher average wages without proportionally expanding employment, generating upward pressure on wages through competition for skilled labor. Although I do not observe the number of workers directly, the results are consistent with either rising wages per worker or modest increases in headcount. Figure B.2 shows that these effects are particularly pronounced among manufacturing and medium-sized firms, which are most directly exposed to competition for skilled labor following the creation of nearby FEZs. This suggests that the incidence of FEZ incentives extends beyond direct beneficiaries: non-FEZ firms, especially smaller and labor-intensive ones, face higher wage costs as skilled workers are drawn toward FEZ firms offering better compensation. These findings reinforce the worker-level results of skill-biased reallocation and rising informality, highlighting that FEZ-induced wage competition can redistribute rather than expand labor market opportunities.

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
Year FE	X	X	X	X
Sector 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 × year, firm age, and ownership type, as well as firm, year, and sector fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Additionally, I examine balance sheets and production capacity. FEZ designation triggers a pronounced expansion in firm balance sheets (Table B.6). 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. The incidence of FEZ benefits therefore falls primarily on capital accumulation—through expanded borrowing and asset growth—rather than on cost savings passed through to workers or consumers.

FEZ designation also reshapes firms' production profiles (Table B.7). 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 with intensified production processes and higher output volumes. These patterns align with program requirements that condition FEZ eligibility on large upfront investment commitments. 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, which helps explain why FEZ firms do not hire proportionally more workers despite expanding output. 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 reveals that FEZs primarily benefit capital and skilled labor. FEZ firms expand their scale and improve their financial standing through fiscal advantages and capital deepening, substituting toward physical capital rather than hiring proportionally more workers. Combined with the documented scarcity of skilled labor, this helps explain why FEZ firms do not absorb the broader local workforce. At the same time, nearby competitors face higher labor costs and limited productivity spillovers. These distributional patterns mirror the labor market results: employment and wage gains concentrated at the top of the skill distribution, coupled with displacement and informality at the bottom. The next section broadens the analysis to all manufacturing firms, including both FEZ participants and those indirectly affected in hosting municipalities, to capture the wider equilibrium adjustments in the local productive structure.

4.3.2 Direct and Spillover Effects from Manufacturing Firms

I estimate the effects of being located in a municipality with a FEZ on firm-level outcomes for manufacturing firms using Equation 5. 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 significantly increases employment in formal manufacturing firms by 4.8%, demonstrating that zones generate additional formal job opportunities within host municipalities. I find no effect on unpaid employment. Total wages per worker rise by 4.0%, indicating that FEZs not only expand formal employment but also increase labor compensation. These patterns are consistent with firms scaling up production and competing more intensely for skilled workers.

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. 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.

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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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.

Taken together, the firm-level evidence points to an amplification mechanism. FEZ activity increases demand 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: skilled workers experience wage gains and transitions into formal employment, 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.

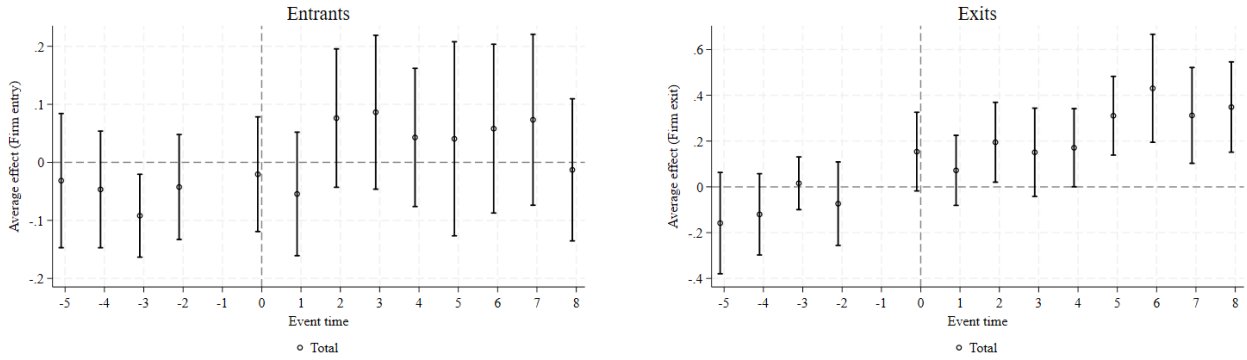
First, eligibility rules require large upfront investment commitments, encouraging firms to adopt more capital-intensive production processes rather than labor-absorbing expansion. Second, skill scarcity limits firms' ability to hire additional workers even as output expands. These forces generate heterogeneous 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.

4.4 Dynamics of Formal Firms

I estimate Equation 1, which captures the effects of FEZ designation on formal firm entry and exit at the municipality level. 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 **??** 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. Despite reductions in tariffs and taxes, FEZs do not induce new firm formation in the short to medium run. By contrast, Figure **??** 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).



(a) Entry rate (b) Exit rate

Notes: Panels 9a and 9b display event-study coefficients and 95% confidence intervals from specification (1). 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). Figure ? reports similar results excluding never-treated municipalities and using the Callaway and Sant'Anna (2021) estimator. Standard errors are clustered at the municipality level, and all regressions include municipality and time fixed effects.
Source: Authors' calculations using RUES and FEZ information.

To better understand which firms exit, Figure **??** disaggregates the treatment effects by firm age. The increase in exit rates is disproportionately concentrated among young firms, which tend

to be less productive and more susceptible to competitive pressures (Hopenhayn, 1992). The post-treatment effects for this group are consistently positive and grow over time, consistent with a rising productivity cutoff for survival induced by FEZs. The absence of new formal entry reinforces this interpretation: reallocation occurs through the expansion of more productive incumbents and the exit of marginal firms rather than through the creation of new firms.

The analysis above relies on the RUES registry, which covers the universe of firms that register and renew their *matrícula mercantil*. RUES is well-suited to measure entry and exit dynamics of formal firms, but it does not allow tracking worker outcomes or capturing the transition of individuals out of formality. To complement this, I use administrative records from the social security system (PILA), which provide firm–worker level information on wages, hours, age, and sector of activity. While the RUES data reveal how FEZs affect the stock and turnover of formal firms, the PILA data 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. Together, the two datasets provide a consistent picture of how FEZs reshape both firms and workers in the formal economy.

Social Security Data. As a complementary exercise, I use administrative records from the social security system (PILA) to study firm exit and subsequent worker reallocations. Unlike the RUES registry, PILA contains firm–worker level information on wages, hours worked, worker age, and sector of activity, which allows me to trace both firm dynamics and worker trajectories. 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 without reappearing in another registered firm.[26] 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.

To align with the baseline strategy, I estimate the same event-study difference-in-differences specification as in Equation 1, replacing the outcome Y_{it} with firm exit rates and worker exit rates computed from PILA. This ensures comparability with the RUES-based analysis while exploiting the richer worker-level dimension of social security records. The coefficients β_k are interpreted as differential changes in exit rates in treated municipalities relative to controls at event time k from FEZ entry. This approach highlights not only whether firms exit after FEZ creation, but also how their workers reallocate across the formal and informal margins, reinforcing the interpretation that FEZs raise the productivity threshold for survival rather than altering aggregate labor supply.

Figure **??** shows that FEZs increase exit rates among smaller (less productive) firms, consistent with a higher productivity cutoff. Following firm exit, affected workers are considerably more likely to leave formal employment than to transition to a new formal employer. Because PILA only

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

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 a selection mechanism: FEZs raise the profitability of high-productivity formal firms, allowing them to expand, invest in capital, and bid up wages—particularly for medium- and high-skill occupations. This intensifies competition in local labor markets and increases the survival threshold, pushing out less productive formal firms, especially younger and smaller ones. At the same time, FEZ eligibility rules incentivize large upfront capital investments, encouraging firms to adopt more capital-intensive production processes with limited scope for additional labor absorption. The absence of new formal entry reinforces this interpretation: reallocation occurs primarily through the expansion of productive incumbents and the exit of marginal firms rather than through net firm creation. These dynamics help 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 higher informality. Taken together, the evidence points to a cascade mechanism in which FEZs elevate productivity cutoffs, favor capital- and skill-intensive production, and generate local wage pressures that reshape the composition of both firms and jobs without increasing total employment.

4.4.1 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 **??** provides full details.

The AKM results in Table **??** 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 **??** 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. The associated capital deepening increases capital per worker and limits labor absorption, particularly for low-skill workers. 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 provide a microfoundation for the structural model in the next section, where productivity cutoffs, capital deepening, skill heterogeneity, and labor market frictions jointly determine equilibrium outcomes.

4.5 Robustness and Alternative Explanations

The baseline results suggest that FEZs raise local wages and increase informality by pushing low-productivity formal firms out of the market. To strengthen the credibility of this interpretation, I examine a range of alternative mechanisms and potential confounding factors. I first rule out competing explanations—such as migration, changes in labor force participation, sectoral reallocations, land market pressures, and product-market crowding-out—that could mechanically shift the composition of employment without operating through firm-level selection. I then assess robustness through alternative measures of FEZ exposure and a battery of placebo tests, including pre-trend, spatial, and outcome falsifications. Together, these exercises show that the main findings are not driven by specification choices or unrelated local shocks, but rather by labor market pressures induced by FEZ activity.

4.5.1 Alternative Explanations

The results presented in Section 4 show that the creation of FEZs is associated with rising wages and higher informality, consistent with a mechanism in which wage pressures induce low-productivity formal firms to exit and displaced workers to move into informal jobs. A natural concern, however, is that these patterns may instead reflect other forces that mechanically alter local employment composition. In particular, migration inflows, changes in labor force participation, sectoral reallocation, land price competition, or demand-side crowding-out could all, in principle, generate shifts toward informal employment unrelated to firm-level selection. To address these concerns, I examine each of these channels in turn. The evidence shows that none of these mechanisms can account for the rise in informality, reinforcing the interpretation that the results are driven by labor market pressures originating from FEZs.

Compositional changes. A natural concern is that the observed rise in informality reflects compositional shifts in the local labor force rather than firm selection.

Three alternative channels are worth considering: (i) migration inflows could increase informality if new arrivals disproportionately take informal jobs. However, as shown in Equation **??**, controlling for migrant status leaves the FEZ estimates unchanged. Moreover, since aggregate employment does not change, this explanation would require a one-for-one displacement of local workers, which

is not supported by the data. Interacting FEZ exposure with migrant status in household surveys further shows that the increase in informality is concentrated among local residents.[27]; (ii) FEZs could attract additional labor supply if they 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, ruling out supply-driven compositional effects; and (iii) sectoral reallocation could matter if workers moved 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, indicating that workers are not simply shifting toward high-informality industries.

Together, these results show that compositional changes in migration, labor force participation, or sectoral structure cannot account for the rise in informality, reinforcing the interpretation that FEZs operate through labor market pressures and firm-level selection.

Land competition. FEZ entry could also raise local land values, either through infrastructure investments, speculative activity, or clustering, thereby 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, one would expect stronger exit effects in urban municipalities. Instead, the evidence points to similar patterns across space (Appendix Figure **??**), suggesting that land competition is not the primary channel.

Market power and demand constraints. Another potential explanation is that FEZ firms capture market share in imperfectly competitive product markets, reducing demand for non-FEZ producers. In such a scenario, incumbents would exit not because they face higher labor costs, but because they lose customers to FEZ firms supplying similar products at lower prices. This mechanism yields a clear empirical prediction: exit should be concentrated among medium- and large-sized firms that directly compete in product markets, rather than only among small firms. In addition, relatively productive non-FEZ firms should also be forced to exit if they are close competitors.

The data do not support this pattern. I show that 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, we would observe broader exit up the productivity distribution. Instead, the fact that high-productivity firms persist and grow is consistent with a rising wage bill that only less productive firms cannot sustain. This pattern reinforces the interpretation that labor-cost pressures, rather than reductions in product demand, drive the observed increase in formal firm exits and, ultimately, informality.

[27]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.

4.5.2 Robustness Checks

To strengthen the credibility of the baseline results, I implement a range of robustness exercises addressing potential concerns about treatment definition, sample composition, and spurious correlations. First, I re-define treatment intensity by replacing the binary FEZ indicator with the share of local employment in FEZ firms, which provides a more continuous measure of exposure. Second, I replicate the analysis using alternative outcome definitions of informality, drawing from both household survey and administrative sources, to ensure that the main findings are not an artifact of measurement. Third, I conduct a series of placebo tests: (i) pre-trend placebos, which artificially shift FEZ entry dates backwards to check for false effects; (ii) spatial placebos, which randomly assign FEZs to untreated municipalities; and (iii) outcome placebos, where I re-estimate the main specifications on outcomes plausibly unrelated to FEZs, such as rainfall or mortality.

In addition, I prove the sensitivity of the results to alternative samples. Specifically, I (i) split the sample into urban and rural municipalities, (ii) re-estimate the effects after dropping outlier municipalities such as the largest metropolitan areas or the smallest rural jurisdictions, and (iii) exclude municipalities with other large-scale place-based programs to avoid confounding policy shocks. I also explore heterogeneous treatment effects as a robustness check: the model predicts stronger impacts of FEZs in municipalities with higher baseline shares of low-skill workers, greater poverty, or more inelastic labor supply. Testing for differential effects along these margins provides an additional credibility check on the proposed mechanism. Finally, I cross-validate the results using alternative datasets when available, comparing household survey evidence with firm registries and administrative data on employment. Together, these exercises ensure that the estimated effects of FEZs on informality are not driven by a particular specification, sample, or data source.

As a robustness exercise to capture a better control group, I replicate the analysis using a more granular definition of the relevant labor market. Specifically, I define a labor market as a two-digit industry $\times$ region cell and exploit the fact that not all subsectors within a broad sector are represented in FEZs. Within manufacturing, for instance, only certain activities are present in zones. This design allows me to compare represented and non-represented subsectors within the same municipality and broad sector, controlling for sector- and place-specific shocks. Identification in this setting rests on the idea that both groups share local and national trends but differ in their direct exposure to FEZs. This within-sector comparison mitigates potential sectoral composition bias and strengthens the interpretation of results as reflecting reallocation among similar firms competing for scarce skills and inputs. I observe similar results to the baseline specification.

I additionally refine the comparison group using propensity score matching (PSM). Municipalities without FEZs are matched to treated municipalities based on pre-treatment characteristics—including population, industrial structure, wage levels, baseline informality, and distance to ports—reducing imbalance in observable determinants of FEZ placement. Re-estimating the baseline specification on the matched sample yields estimates similar in magnitude and precision, suggesting

that results are not driven by differential selection into treatment.

Together, these exercises demonstrate that the estimated effects of FEZs on informality are robust to alternative treatment definitions, placebo assignments, sample restrictions, heterogeneous effects, and matching-based reweighting, strengthening the interpretation that the results are driven by labor market pressures originating from FEZ activity.

5 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 documents 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.

More broadly, the estimates presented here are reduced-form responses. They show how FEZs reallocate employment and production within local economies, but assessing aggregate welfare implications—and the overall gains from spatial reallocation—requires a structural framework capable of mapping these observed margins into general equilibrium outcomes. 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.

6 Conclusion

Place-based industrial policies are often justified by their potential to stimulate growth and employment in economically lagging areas. Yet in contexts characterized by high informality and institutional weakness, such interventions can generate unintended redistributive effects—not only across regions but within them. This paper shows that Colombia's Free Economic Zones (FEZs), rather than producing broad-based development, 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: 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. 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. 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 Appendix to Section ??

Table A.1: Exports and GDP of AZFA Member Countries (2022 figures)

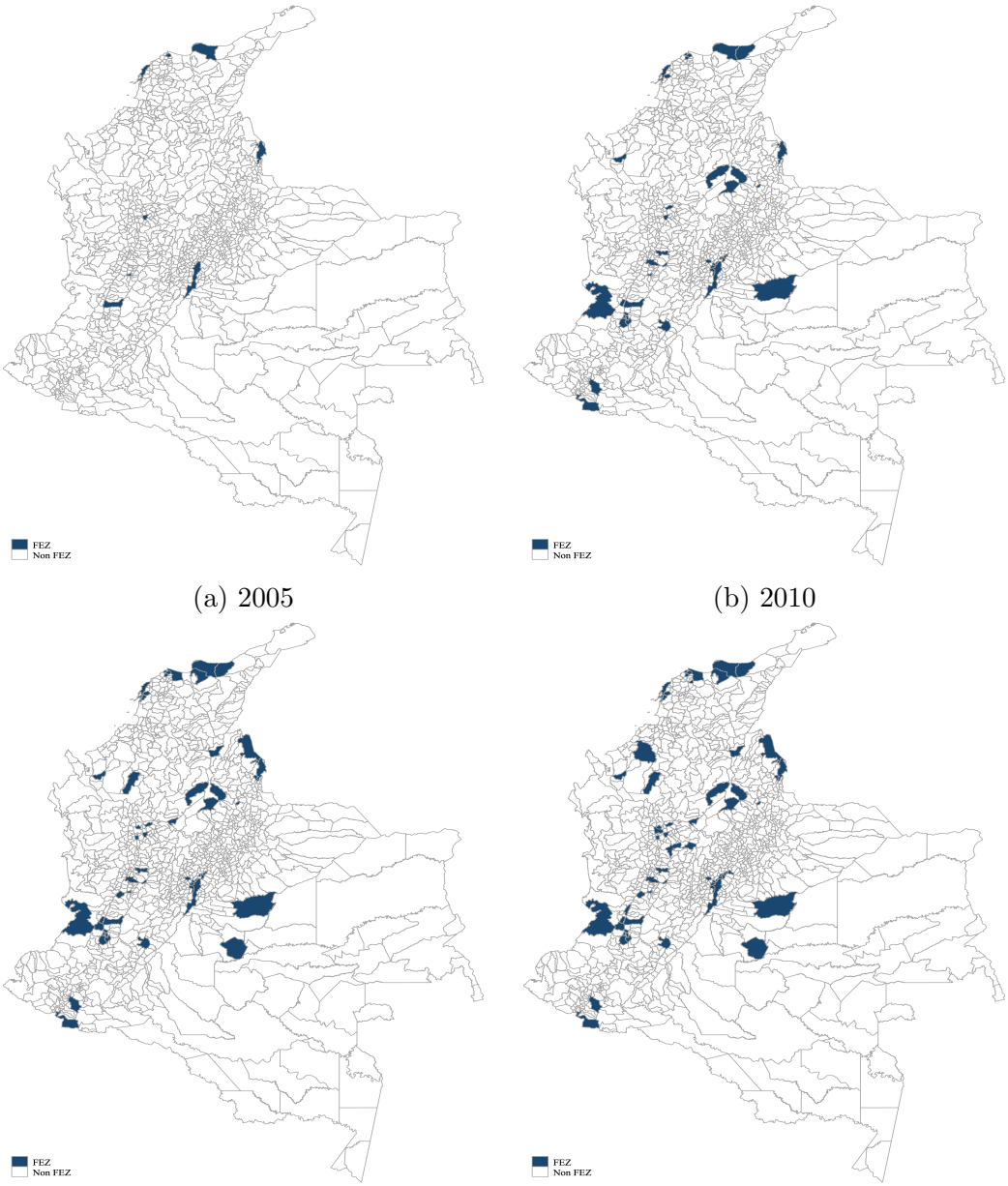
Country	**Population** (thousands)	**GDP total** (million USD)	**Exports** (million USD)	**Exports from FEZs** (million USD)	**FEZ share of exports** (%)	**FEZ share of GDP** (%)
Nicaragua	6,948	15,711	7,870	3,616	45.9	23.0
Honduras	10,433	31,710	13,394	5,000	37.3	15.8
El Salvador	6,336	32,189	10,164	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

Source: Authors' compilation based on AZFA reports.

Table A.2: Tax incentives of Free Economic Zones (FEZs) in selected Latin American and Caribbean countries

Country	**Corporate Tax**		**VAT**		**Tariff**		**Dividends**	
	FEZ	**Outside FEZ**	**FEZ**	**Outside FEZ**	**FEZ**	**Outside FEZ**	**FEZ**	**Outside FEZ**
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%

Source: Authors' compilation based on AZFA reports.



(a) 2005 (b) 2010

(c) 2015 (d) 2018

Figure A.1: Evolution of the spatial distribution of Free Economic Zones (FEZs) across municipalities, 2005–2018.

B Appendix to Section 4

Table B.3: 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.

B.1 Workers

Table B.4: 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 B.5: 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.2 Firms

B.2.1 Financial Statement Firm Results

Table B.6: 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

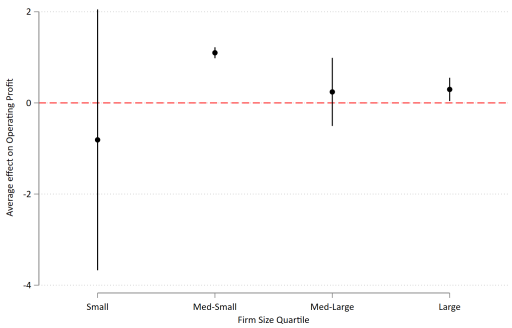
Notes: t-statistics in parentheses. All variables are deflated using industry-specific yearly Producer Price Index and expressed in logarithms. The control group are non-FEZ firms. Specifications control for pre-policy assets × year, firm age, and ownership type, as well as firm, year, and sector fixed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

Table B.7: Regression Results: Firm Assets and Composition

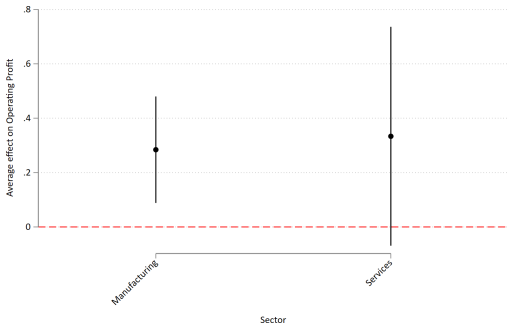
	Net PPE	Raw Materials	Finished Goods	Land	Real Estate for Sale	Capital Intensity	Investments
FEZ	0.633***	0.683*	1.360***	0.00135	0.0401	0.238**	-0.808
	(0.182)	(0.399)	(0.473)	(0.0128)	(0.0595)	(0.117)	(0.561)
Observations	203,407	80,417	73,528	153,318	153,286	193,832	169,694
R^2	0.812	0.898	0.896	0.801	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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

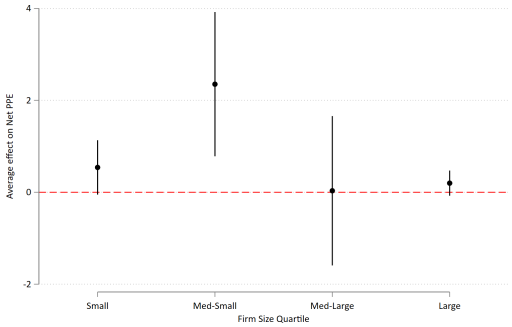
Figure B.2: Anatomy of the FEZ effects on firm performance, by size and sector.



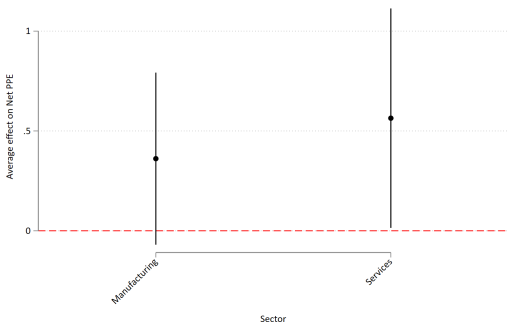
(a) By size



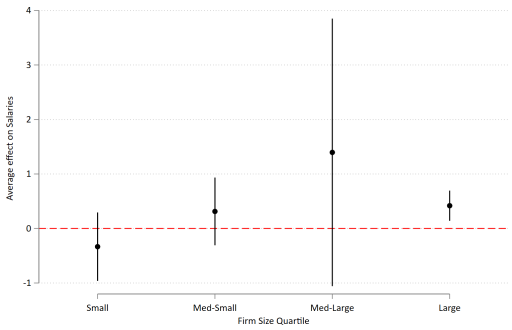
(b) By sector



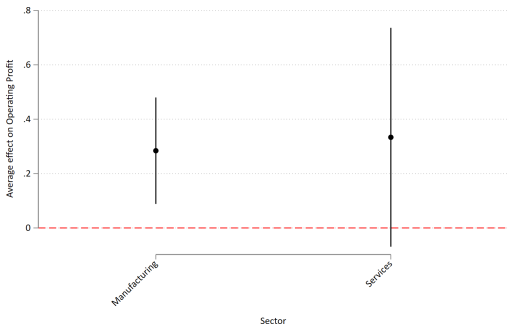
(c) By size



(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.

B.2.2 Micro-businesses

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

$$Y_{fmt} = \alpha_{fmt} + \beta FEZ_{mt} + +\theta \mathbf{X}_{fmt} + \lambda_t + \delta_m + +\epsilon_{fmt} \tag{6}$$

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 B.8), 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 B.10). 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 B.9), 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 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 B.8: 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 B.9: 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 B.10: Regression Results: Total Employment and Informal Workers (Unpaid)

	Total Employment			Informal Workers (Unpaid)		
	Informal	Formal	All	Informal	Formal	All
Variable	(1)	(2)	(6)	(1)	(2)	(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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B.3 Estimating Firm and Worker Effects Using the AKM Model

The model indicates that firm productivity is an important predictor of the effects of FEZs. With access to the full count of formal workers and firms in Colombia, I construct a measure of firms' wage premiums, serving as a proxy for productivity. To achieve this, I estimate the canonical AKM model proposed by **?**, which decomposes the contribution of firm-specific and worker-specific constant characteristics to log formal wages:

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

where α_i captures the unobserved worker effect, ψ_j the unobserved firm effect, and $j(i,t)$ refers to the firm j where worker i is employed in period t. X_{it} is a vector of controls including normalized age (squared and cubic) and year fixed effects. ε_{it} is the error term. To avoid endogenous workers' movements driven by the FEZ treatment itself, I estimate the model in the pre-policy period (2010–2015, August waves, $T = 6$), restricting the sample to the largest set of firms connected by worker mobility.

Firm effects ψ_j are identified through workers' movements across firms, assumed to be exogenous conditional on worker and firm fixed effects, i.e., $E[\varepsilon_{it}|\alpha_i, \psi_{j(i,t)}, X_{it}] = 0$. As noted by **?**, AKM models may present biases in estimating $\text{Var}(\hat{\psi}_j)$ or $\text{Cov}(\hat{\alpha}_i, \hat{\psi}_j)$ when worker mobility is limited. Since I focus on the estimated vectors of firm and worker FEs $(\hat{\psi}_1, \ldots, \hat{\psi}_J)$ and $(\hat{\alpha}_1, \ldots, \hat{\alpha}_N)$, limited mobility bias affects only precision, not consistency. Nevertheless, I implement the leave-out method proposed by **?** when decomposing sources of wage inequality.

To quantify the role of FEZs more precisely, I perform a Blinder–Oaxaca decomposition following **?** and **?**, which decomposes the cross-sectional difference in mean log real wages between workers in FEZ firms and those in non-FEZ firms into three components:

$$\begin{aligned} E[\ln w_{it}|FEZ=1] - E[\ln w_{it}|FEZ=0] = & \underbrace{E[\alpha_i|FEZ=1] - E[\alpha_i|FEZ=0]}_{\text{Skill differences}} \\ & + \underbrace{E[\hat{\psi}^{FEZ}_{J(i,t)}|FEZ=1] - E[\hat{\psi}^{FEZ}_{J(i,t)}|FEZ=0]}_{\text{Differential sorting}} \\ & + \underbrace{E[\hat{\psi}^{FEZ}_{J(i,t)} - \hat{\psi}^{nonFEZ}_{J(i,t)}|FEZ=0]}_{\text{Differential pay-setting}}. \end{aligned} \tag{15}$$

I focus on the second term, as the first (skill differences) and third (within-firm pay-setting) require estimating separate AKM models by FEZ status. Given the limited number of firms with

movers observed in both FEZ and non-FEZ groups, the analysis centers on the sorting component. This term captures the average difference in firm fixed effects between FEZ and non-FEZ workers. For instance, if the difference in mean firm effects is 0.20 log points while the total wage gap is 0.40, this implies that sorting accounts for 50% of the FEZ wage premium.

Such a result would indicate that much of the observed wage premium in FEZ municipalities is explained not by FEZ firms paying more per se, but by the fact that they disproportionately employ workers in higher-paying firms. This mechanism is consistent with the model's prediction that FEZs raise the local survival threshold and reallocate workers across the firm pay distribution. It also helps to rationalize why total employment does not increase, despite the appearance of higher-paying jobs inside FEZs.

Table B.11: 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

This table reports the variance decomposition of log formal wages estimated from the model in Card et al. (2016), using the largest connected set of workers and firms. Year fixed effects are included as controls. The full decomposition follows: $\text{Var}(\ln w_{it}) = \text{Var}(\alpha_i) + \text{Var}(\psi_{j(i)}) + \text{Var}(X'_{it}\gamma) + \text{Var}(v_{it}) + 2\,\text{Cov}(\alpha_i, \psi_{j(i)}) + 2\,\text{Cov}(\alpha_i, X'_{it}\gamma) + 2\,\text{Cov}(\psi_{j(i)}, X'_{it}\gamma)$, and I focus only on the most relevant terms in this table.

Table B.11 presents the decomposition of the variance of wages $\text{Var}(\ln w_{it})$ in Colombia's formal sector. Worker effects explain 59.4% of the variance, and firm effects explain 19.24%, consistent with the ranges reported in **?**. Relative to other middle-income countries, Colombia's firm component is lower than in South Africa and Mexico but similar to Brazil (**?**). A plausible explanation is Colombia's relatively high and binding minimum wage, which compresses wage-setting policies of low-paying firms, reducing the importance of firm effects. Positive sorting of high-wage workers into high-wage firms accounts for another 13.24% of the variance, a share comparable to upper bounds reported for European countries and the US (**?**).

Table B.12: 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). Group 1 corresponds to non-FEZ municipalities.