

Cash Transfers, Demand Composition, and Labor Reallocation*

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Abstract

This paper shows that who receives a cash transfer shapes not only redistribution but also the composition of local demand, the allocation of labor between formal and informal firms, and aggregate productivity. We exploit the 2019 universalization of Mexico's non-contributory pension, which extended pension benefits across the income distribution. Using difference-in-differences designs at the household and municipality levels, we find that beneficiary households increased consumption by 5.7%, with spending disproportionately directed toward formal establishments, consistent with non-homotheticities in formal-sector demand. Formal employment increased by 6.1% in municipalities highly exposed to the reform relative to less exposed municipalities, with no statistically significant effect on informal employment. This expansion was concentrated among firms in industries where household estimates show the largest increases in demand. Formalization gains were larger where new beneficiaries ranked higher in the local resource distribution and where the rise in transfers was driven by expanded coverage to richer households rather than increased benefits for existing recipients. These patterns are consistent with a demand-led reallocation toward the formal sector induced by the pension reform. A sufficient-statistic calculation shows that the transfer-induced reallocation raised aggregate productivity by 0.32%. A model in which the marginal propensity to spend formally rises with household resources and formal firms face larger average labor wedges rationalizes these findings.

O11, O14, O17, H55, D24, D12

Keywords: Cash Transfers; Social Pensions; Labor reallocation; Informality; Non-homothetic preferences; Structural transformation; Misallocation; Aggregate productivity

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1 Introduction

Over 1.4 billion people in the developing world receive some form of cash transfer (Niehaus and Suri, 2026). One of the main design questions of cash transfers that governments face is who should receive them. Programs usually target the poorest, on the redistributive logic that an extra dollar is worth more to a household with fewer resources (Coady et al., 2004; Grosh et al., eds, 2022). Recent evidence, however, shows that the effects of cash transfers may extend beyond their direct beneficiaries to other economic actors. By changing beneficiaries' demand for goods and services, transfers can also affect local firms, workers, and non-beneficiary households through local multiplier effects (Egger et al., 2022; Gerard et al., 2026; Angelucci and Attanasio, 2009). Accounting for these indirect effects is therefore essential for evaluating both the incidence and the aggregate consequences of cash-transfer programs.

The characteristics of cash-transfer beneficiaries can shape where the additional demand generated by the transfer is directed. Households in different parts of the income distribution allocate their income to different types of establishments. Poor households buy a larger share of their consumption at informal street vendors and public markets, while richer households buy a larger share at formal supermarkets, chain stores, and restaurants (Bachas et al., 2024). Transfers to households at the bottom of the income distribution are therefore likely to generate demand that is directed more toward informal establishments than transfers to richer households and, consequently, that is likely to affect different firms. Therefore, the disbursement of cash transfers to different segments of the population closely interacts with the type of local multiplier effects it generates.

Understanding the extent to which heterogeneity in beneficiaries' consumption patterns across the formal and informal sectors translates into aggregate effects is crucial for assessing the ultimate impact of cash transfers on local economies. Firms in the formal sector tend to be larger, more productive, and subject to greater labor-market distortions than informal firms (Hsieh and Klenow, 2009; Ulyssea, 2018; Dix-Carneiro et al., 2026; Atkin and Khandelwal, 2020). As a result of these higher distortions, formal firms tend to exhibit higher marginal revenue products of labor. Therefore, when the additional demand generated by a transfer affects the allocation of workers across the formal and informal sector, it can impact allocative efficiency and, consequently, aggregate productivity.

Evidence on this question is scarce because cash transfers in developing countries have almost always been targeted to low-income households, either through means testing or targeting of poor communities. Work on local multiplier effects of cash transfers estimates multipliers in settings with little formal activity (Egger et al., 2022), or focuses on formal sector responses (Gerard et al., 2026), but none assess how heterogeneity in households' responses to transfers shapes their effects on local economies.

In this paper we address this question by assessing whether extending a transfer to households higher in the income distribution can change the composition of local demand, reallocate labor from informal to formal firms, and raise aggregate productivity. To answer this question, we exploit the 2019

universalization of Mexico’s non-contributory old-age pension.¹ Before 2019, the program paid a flat benefit of approximately \$31 per month to individuals aged 65 and older who lacked a contributory pension or received one below a defined threshold. In January 2019, the government reformed the non-contributory pension system by eliminating means-testing and extending eligibility to all individuals above a specified age threshold. The reform also introduced a gradual increase in pension benefits for all eligible individuals, which reached US\$126 in 2023.² The universalization offers an ideal setting to study these questions because, unlike most cash transfers, the expanded eligibility and the benefit increase introduced by this reform raised transfers for households across the income distribution, with most benefits accruing to newly eligible middle- and high-income households.³ This allows us to study heterogeneity in household responses to the cash transfer by their income status, and to assess how these heterogeneous individual patterns shape the aggregate effects of the transfer.

Guided by a model of a local economy that links household demand to the allocation of labor across the formal and informal sectors, we provide novel empirical evidence of demand-driven reallocation towards the formal sector following the universalization of the pension. Using difference-in-differences designs, we first draw on detailed household-level consumption surveys to estimate the effect of the transfer on recipients’ consumption patterns, and we assess how these responses vary by household income, product, and place of purchase. We then leverage administrative individual-level microdata on beneficiaries, sales data from one of the biggest retailers in the country, and Mexico’s Economic Census, which covers the universe of firms and workers in both the formal and informal sectors, to examine the reallocative effects of the pension across firms and local labor markets. Finally, combining the empirical estimates with the model structure, we quantify the productivity consequences of the resulting reallocation.

We begin by developing a model of a local economy to structure the empirical analysis. In the model, the economy has a formal and an informal sector that use local labor as an input. Firms face labor wedges that are larger on average in the formal sector. Therefore, consistent with empirical evidence, formal firms have a higher MRPL than informal firms on average. Households have non-homothetic preferences in which formal goods are luxuries and the marginal propensity to spend in the formal sector increases with household resources. We then consider an exogenous increase in monetransfers to local households, financed with resources from outside the municipality. The model delivers four empirically testable predictions, one at the household level and the other three at the local economy level. First, households increase their formal consumption share and their marginal formal consumption monotonically as they move across the income distribution. Second, due to this non-homothetic demand structure, an exogenous transfer to households can shift the formal composition of local demand, reallocating labor across the formal and informal sector. Third, the direction and extent of reallocation depends on who receives the transfer. Allocating the marginal transfer to richer

¹Mexico is one of a growing number of countries, including Argentina, Kenya, and Chile, that have made old-age transfers universal in the last decade.

²Eligibility was set at age 68 in 2019 and 2020 and lowered to 65 from 2021 onward, and existing beneficiaries kept their transfers throughout.

³Before the reform, 60% of low-income households received some cash transfer, compared with 25% of middle-income and 4% of high-income households.

rather than poorer households generates a larger reallocation towards the formal sector, because richer households spend a larger share of the additional income on formal goods. Fourth, for a transfer to change allocative efficiency, two conditions are necessary: a positive wedge gap between formal and informal firms and non-homothetic demand for formal-sector goods.⁴

We empirically test the model predictions by assessing the individual and local economy level effects of the transfer.⁵ To estimate the household responses to the universalization, we exploit the age-eligibility cutoff in a difference-in-differences design. We compare households with a member just above the eligibility age to households with a member just below it, before and after the reform, within a window of ages 60 to 73.⁶ The narrow window limits the scope for contemporaneous shocks, including the pandemic, to affect the two groups differently.

The reform raised per capita pension income in eligible households by 400%, with strongest increases among high-income households who were previously ineligible for pension benefits. Treated households increase total consumption by 4.3%, with most of the additional spending directed toward formal-sector establishments. Purchases from formal establishments rise by 5%, while the estimate for informal establishments is 2.3%. As a consequence, the formal share of consumption rises by 1.4 percentage points (2.5%) for the average treated household, consistent with non-homothetic preferences for formal goods. The additional demand falls mainly on non-tradable goods and services, and the strongest rise is observed in the following consumption categories: food and beverages, household items, transport, cleaning products and personal care.

We next show that, for each additional peso of pension transfers received, the rise in formal consumption is highest among richer households. Instrumenting pension income with eligibility, we find that an additional peso of pension income raises formal expenditure by 0.15 pesos among low-income households, 0.09 among middle-income households, and 0.22 among high-income households. Using a continuous household asset index, rather than comparing only three broad socioeconomic groups, shows similar patterns. For a household at the mean of the pre-reform asset distribution, an additional peso of pension income raises formal expenditure by 0.11 pesos; one standard deviation higher in the asset distribution, the implied response nearly doubles to 0.21. Informal expenditure per peso of transfer received moves in the opposite direction, declining with household resources. Together, these results show that the marginal propensity to spend formally increases with household resources, consistent with the non-homothetic demand structure in the model. This heterogeneity is central to the model's aggregate mechanism: by directing a larger share of the marginal transferred

⁴A notable feature of the model is that a transfer can generate reallocation toward the formal sector and gains in aggregate productivity purely through the economy's demand structure and the initial distribution of wedges, without requiring nominal price or wage rigidities.

⁵Three main features of the program help us attribute the changes we observe to the reform. First, the benefit level and eligibility rules of the means-tested program had been essentially unchanged in the years leading up to the reform, which gives us a stable pre-reform window. Second, the federal government financed the expansion by reallocating existing funds rather than raising taxes, and because the program is federally financed and administered, municipal budgets were not called on to fund it. Third, the main concurrent change in social policy, the replacement of the conditional cash transfer program *Prospera* with other national programs, is uncorrelated with pension exposure across municipalities (the correlation between the two changes is -0.01).

⁶We recenter the window on the cutoff for each year.

peso toward formal goods, transfers to higher-resource households generate a stronger shift in demand toward the formal sector. We therefore empirically assess whether the additional demand generated by the transfer produced such a reallocation.

To trace the demand shock from households to firms and local economies, we exploit cross-municipality variation in the increase in pension transfers generated by the reform. Using administrative records of beneficiary payments, we measure each municipality’s exposure as the change in per capita pension transfers from 2015–2018 to 2019–2023. Exposure varies because municipalities differed before the reform in both their elderly population and the share of elderly residents already receiving a pension. Our baseline specification classifies municipalities as treated when their increase in transfers per capita is above the within-state median, and control otherwise.⁷

We then leverage rich firm-level microdata from Mexico’s Economic Census, covering all formal and informal firms in the economy, to estimate the effect of the transfer on local economies’ formal and informal employment. Our main specification consists of a difference in differences analysis where we compare high vs low exposed municipalities, before and after the universalization.⁸ We carry out two complementary specifications: one using firm level observations, and one at the municipality level. The firm-level specification allows us to estimate the effects on firms in highly exposed municipalities while controlling for time-varying sector-specific shocks and changes in firm composition across municipalities. The municipality-level specification lets us validate the firm-level results and conduct a model-guided decomposition of the forces driving local labor market effects.

We begin by assessing the impact of the universalization on firms operating in industries corresponding to the household expenditure categories with the largest increases in consumption following universalization.⁹ These firms are the most directly exposed to the demand expansion generated by the reform. Within these industries, formal firms in municipalities with high pension exposure increased sales by 1.3% and employment by 1.4% relative to those in less exposed municipalities, while we find no statistically significant effects among informal firms. We complement this evidence with a triple-difference-in-differences specification where we compare firms in high vs low demand industries, in treated vs control municipalities. We find that following the reform, among formal firms, the difference in sales and employment growth between firms in high- and low-demand industries was larger in municipalities with high pension exposure than in those with low exposure, while we find no significant effects among informal firms. These findings are consistent with a direct impact of the rise in beneficiaries’ consumption, which focused on certain goods and services in the formal sector and drove an expansion among formal firms supplying these goods and services.

Moreover, we find that the universalization led to an expansion in formal employment, sales and

⁷The pension transfers represent a quantitatively important economic shock for high-exposed municipalities: pension transfers after the reform amount to 3.7% of household income on average among treated locations, compared with 2.4% in less-exposed municipalities.

⁸Importantly, the reform increased transfers everywhere. Our estimates therefore do not compare treated and untreated municipalities; instead, they capture the effect of receiving a larger pension-induced income shock.

⁹These NAICS sectors are 722, 461, and 462 (food & beverage consumption); 4631, 8121, 4651, and 8114 (personal care); 485, 811, and 468 (transport); and 488 and 8122 (cleaning, household care, and furniture).

production beyond highly exposed sectors. The average formal firm in highly exposed municipalities increased employment and sales by 0.8%, relative to firms in low exposure municipalities. In our municipality level specifications we see that highly exposed municipalities experience a rise in formal employment of approximately 6.1%, and a 10.2% increase in formal sales. Meanwhile we find no significant effects on informal firms in our firm level analysis, while in the municipality level analysis effects on informal employment and sales are negative but imprecise. These results are consistent with the model’s second prediction: the universalization led to a demand-driven reallocation of labor toward the formal sector.

We complement these results with an analysis using establishment-level microdata from one of the largest formal retail chains in Mexico. While this dataset covers only a subset of formal retail establishments, it provides annual information on prices and quantities separately. The results confirm the patterns observed in the Economic Census. Following the reform, sales increase by 4% and quantities by 3.9% in highly exposed municipalities. Prices increase by at most 1.6%, indicating that the demand shock translated primarily into higher quantities, with only modest price effects.

Consistent with the model’s third prediction on recipient composition, formalization gains are larger where the marginal transfer reaches relatively higher-resource households. For similar increases in transfers per capita, we find no significant effect in municipalities where prospective new recipients were concentrated at the bottom of the local resource distribution, while the formal employment share rises by 2.0 and 3.7 percentage points in the middle and top terciles, respectively.¹⁰ Additionally, formal employment effects are larger in municipalities where few elderly residents received non-contributory-pension before the reform and, consequently, a larger share of the post-reform increase in transfers came from expanding coverage to newly eligible individuals, rather than from higher benefits for pre-reform beneficiaries. Because the newly covered elderly are richer on average than pre-reform beneficiaries, this pattern is consistent with an expansion in benefit eligibility to higher income households generating a larger formal-demand response than an equally sized deepening of benefits among existing low-income recipients. These patterns highlight the role of non-homotheticities in formal consumption in shaping how the aggregate effects of cash transfers vary with beneficiary characteristics.

Finally, we quantify the productivity effects of this reallocation by estimating a model-derived sufficient statistic in the spirit of [Baqaee and Farhi \(2020\)](#). In our setting, productivity gains arise from improved allocative efficiency, as the transfer reallocates labor from informal firms with a low marginal revenue product of labor (MRPL) to formal firms with a high MRPL. Our estimates imply an allocative-productivity gain of approximately 0.32% in highly exposed municipalities relative to less exposed ones, and a 0.65% gain among the sectors most exposed to the demand shock.

This paper contributes to three strands of literature. First, we contribute to a small but growing literature on the local-economy effects of cash transfers in developing countries. [Egger et al. \(2022\)](#) show that an unconditional transfer in Kenya raised the earnings of non-recipients through general-equilibrium channels. [Banerjee et al. \(2023\)](#) find that a long-term universal basic income in Kenya

¹⁰We rank municipalities according to the average percentile position of prospective recipients within their own municipality’s pre-reform asset distribution.

expanded economic activity and shifted workers from wage employment toward self-employment. [Gerard et al. \(2026\)](#) exploit the expansion of Bolsa Família to show that the transfer raised formal employment and local economic activity in treated municipalities. In these studies, as in most of the literature on cash-transfers, the recipients are poor households. Work on targeting compares program designs on redistribution, take-up, and labor supply ([Hanna and Olken, 2018](#)), and has paid much less attention to how the characteristics of targeted individuals shape aggregate effects of cash transfers. We contribute in two main ways. First, the universal nature of the transfer allows us to provide the first evidence on the effects of cash transfers on local economies when they reach households across the income distribution, and how these effects vary according to beneficiary income. Second, to the best of our knowledge, this paper is the first to quantify the allocative efficiency gains from demand-driven labor reallocation induced by cash transfers.¹¹

Second, we contribute to the literature on demand-driven structural transformation in developing countries. While the traditional literature defines structural transformation as the reallocation of economic activity across agriculture, manufacturing, and services ([Herrendorf et al., 2014](#)), more recent views also include movements from home to market production, from self-employment to wage work, and from informal to formal activity ([Gollin and Kaboski, 2023](#)). Recent work in this area emphasizes the growth of demand for locally non-tradable consumer services in driving the expansion of a modern service sector ([Fan et al., 2023](#); [Peters et al., 2026](#); [Ramos-Menchelli and Sverdlin-Lisker, 2024](#)). In closely related work, [Schwartzman \(2026\)](#) develops a modern-service multiplier in which rising income redirects demand from traditional toward modern service firms. We make two contributions to this literature. First, we use the universalization of the pension as an exogenous household income shock to show that additional income raises the formal share of consumption, whereas existing evidence relies primarily on cross-sectional variation in income and consumption. This also allows us to show novel evidence that marginal formal expenditure is increasing in household resources. Second, we show that non-homothetic demand can affect sectoral employment within a few years of a policy change, and not only over the long horizons typically associated with structural transformation, and that its productivity consequences depend on which sector labor moves into ([Duarte and Restuccia, 2010](#)).

Third, we contribute to the literature on misallocation and aggregate productivity. A large body of work documents substantial dispersion in marginal products across firms in developing countries and its consequence for aggregate productivity ([Hsieh and Klenow, 2009](#); [Baqae and Farhi, 2020](#)). A related literature studies how particular policies affect allocative efficiency, including capital-market integration ([Bau and Matray, 2023](#)), trade liberalization ([Atkin and Khandelwal, 2020](#)), and transportation infrastructure ([Zárate, 2022](#)). We contribute by showing that a demand-side policy can improve allocative efficiency without directly relaxing firms' wedges, by directing demand towards the firms with higher marginal revenue products of labor.

The rest of the paper proceeds as follows. Section 2 documents the three facts that motivate the analysis. Section 3 presents the model. Section 4 describes the reform and the data. Section 5 estimates

¹¹The labor supply responses of recipients and their family members, which earlier work on non-contributory pensions emphasizes, are small in our setting. We study them in a companion paper [Colonna et al. \(2026\)](#), and [Galvez-Soriano and Ramirez Peralta \(2026\)](#) document a similar pattern for the same reform.

the effects on household consumption. Section 6 traces the demand shock to firms and municipalities and quantifies the productivity gains. Section 7 concludes.

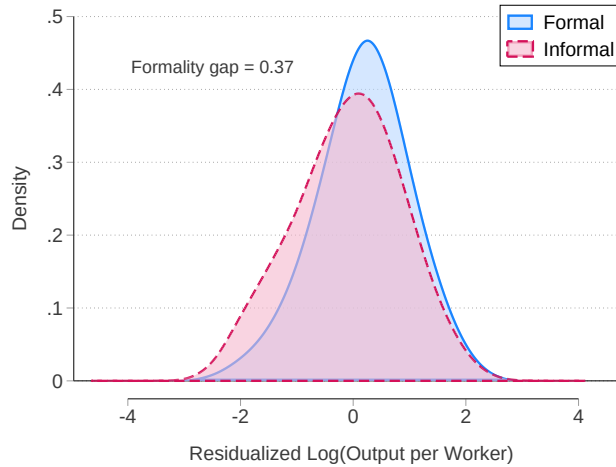
2 Motivating Facts

In this section, we document two empirical facts that motivate the paper’s main mechanism. First, the formal sector faces larger wedges than the informal sector and is therefore inefficiently small. Second, as households and local economies become richer, they direct a larger share of expenditure toward formal establishments. The model in the next section formalizes our main mechanism, showing how the interaction between these patterns shapes the effects of cash transfers on the formal composition of aggregate demand, labor allocation across sectors, and aggregate productivity.

2.1 A Distorted Formal Sector

In this section, we show evidence that formal firms in Mexico face higher distortions than informal firms. As widely documented in the literature, a defining feature of firms in the formal sector is that they are subject to regulations, such as minimum wages, workplace standards, taxes, and union agreements. On the contrary, the enforcement of such regulations is significantly weaker among firms operating in the informal sector (La Porta and Shleifer, 2014; Ulyssea, 2018; Levy, 2008). This asymmetry imposes larger regulatory distortions on formal firms, raising their marginal costs relative to those of informal firms. Assuming firms equate marginal cost to marginal product, this raises formal firms’ marginal revenue product of labor, and leaves the formal sector inefficiently small (Hsieh and Klenow, 2009; Dix-Carneiro et al., 2026).

Figure 1: MRPL Distribution by Sector



Notes: This figure plots kernel density estimates of residualized log output per worker from formal and informal firms using the full sample from the 2018 wave of the Economic Census. A firm is defined as formal if it is registered with the tax authority (SAT). Residuals come from an OLS regression of log output per worker on firm age, form of ownership, size category, and municipality $\times$ 4-digit SCIAN industry fixed effects, estimated on the pooled sample of formal and informal firms. The reported formality gap is the coefficient on an indicator for formality in the regression, in log points.

In Figure 1, we empirically evaluate these distortions by assessing the distribution in the marginal

revenue product of labor (MRPL) across formal and informal firms.¹² Using the 2018 wave of the Economic Census, we proxy each firm’s marginal revenue product of labor (MRPL) using the ratio between firm revenue and total labor force. We then regress the log firm MRPL on firm age, form of ownership, firm-size category, and municipality $\times$ 4-digit industry fixed effects and plot the distribution of the residuals separately for formal and informal firms. The formal distribution is shifted to the right: the gap between the average (residual) MRPL of formal and informal firms is 0.37 log points.¹³ The two distributions also overlap substantially, so not every formal firm faces a larger wedge than every informal firm.¹⁴

2.2 Non-Homotheticity in Formality

As recent studies have shown, consumption composition varies systematically across the income distribution. In both developed and developing economies, as households grow richer, they shift consumption toward modern, more productive service firms (Faber and Fally, 2022; Schwartzman, 2026; Ramos-Menchelli and Sverdlin-Lisker, 2024) and toward formal outlets more broadly (Bachas et al., 2024). In Figure 2 we document these regularities across households and municipalities in Mexico. Panel (a) plots a degree-two local polynomial of the formal budget share against log income per capita at the household level, using the 2014, 2016, and 2018 waves of the Income and Expenditure Household Survey (ENIGH).¹⁵ The figure reveals a clear and monotonically increasing relationship between household income and the share of expenditures made in the formal sector.

Figure B.2, Panels (a) and (b), provides additional evidence for this relationship. First, we use log expenditure per capita as the running variable instead of income, since household income is often measured with more error in developing countries (Deaton and Zaidi, 2002). Second, we use the socioeconomic classification produced by the Bureau of Statistics, which groups households into strata, incorporating dwelling, asset, and locality characteristics, that we label low-, middle-, and high-income.¹⁶ In both cases we find the same monotonically increasing gradient.

¹²The relation between the MRPL and the wedge is stated explicitly in the FOC in Equation (10) in Section 3. Interpreting the gap between MRPL as a wedge relies on four assumptions: (i) firms operate a Cobb-Douglas technology, so the MRPL is proportional to the average revenue product of labor; (ii) formal and informal firms in the same industry $\times$ municipality share the same labor elasticity α_s ; (iii) firms in the same municipality $\times$ industry pay a common wage per efficiency unit of labor, which requires that workers move freely across sectors, face equal disutility of working in either sector (no compensating differentials), and do not differ systematically in quality by formality within a cell; and (iv) firms choose labor optimally given their wedge, and sales and employment are measured with comparable error in both sectors. Any formal-informal difference in markups is also loaded into the measured wedge. These assumptions are standard in the literature (Bau and Matray, 2023; Goldberg and Pavcnik, 2003; Foster et al., 2008).

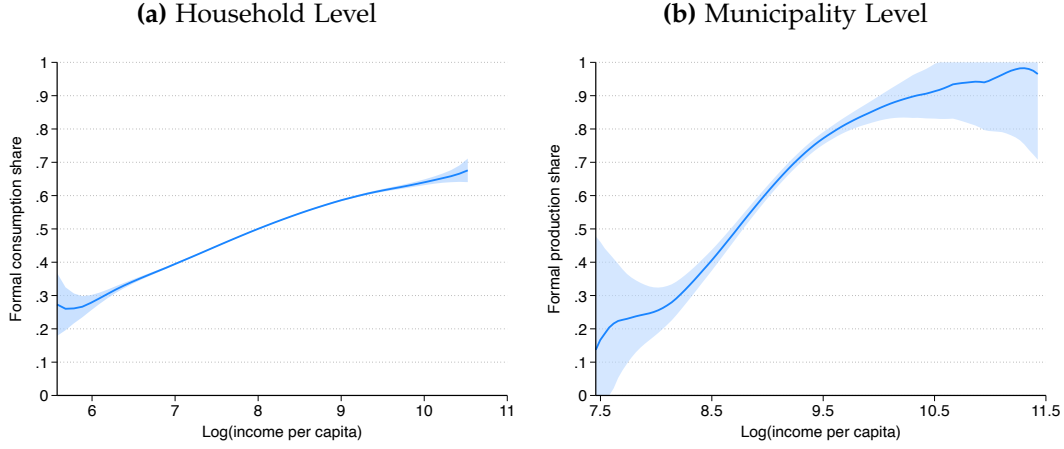
¹³Figure B.1 in the Appendix reports this coefficient together with less demanding specifications.

¹⁴In Section 6 we recover each firm’s wedge directly from its labor first-order condition, $(1 + \tau_j) = \alpha^L \cdot \frac{pY_j}{wL_j}$, using the firm’s own revenue and wage bill from the 2018 Economic Census (net of social security contributions and income-tax withholdings) together with labor elasticities estimated by 2-digit industry following Levinsohn and Petrin (2003). Because this measure uses each firm’s own wage bill, it does not rely on formal and informal firms paying a common wage. Formal firms again face larger wedges: the average τ is 3.21 among formal firms and 1.06 among informal firms (Figure B.32 in the Appendix).

¹⁵For certain categories, such as food, beverages, tobacco, personal care, clothes and dining out, the place of purchase is recorded in ENIGH. We use this reported place of purchase to classify expenditures as formal or informal, following Bachas et al. (2024).

¹⁶The classification has four categories: low, middle-low, middle-high, and high; we pool the top two for statistical power.

Figure 2: Formal Non-Homotheticity



Notes: This figure documents the non-homotheticity of formal consumption at the household level and formal production at the municipality level. Panel (a) plots the share of formal expenditure in total classifiable expenditure against log income per capita at the household level, where expenditures are classified as formal or informal following [Bachas et al. \(2024\)](#) (see Table B.19). Panel (b) plots the share of formal output against log income per capita at the municipality level. The solid lines are local polynomial estimates of degree two and the shaded area denotes the 95% confidence interval.

Panel (b) repeats the analysis at the municipality level, plotting a degree-two local polynomial of the share of formal production in a municipality (from the 2018 Economic Census) against log municipal household income per capita (from the Bureau of Statistics' ICM database). The same monotonically increasing gradient appears, suggesting that as residents grow richer, their demand shifts toward formal goods, raising the share of formal production and employment in the municipality. Figure B.3 in the Appendix repeats the exercise using the share of formal sales and the share of formal employment, and finds the same pattern.

3 Model

This section develops a model of a local economy in which a cash transfer can reallocate labor from informal to formal firms by shifting the composition of household demand. Since formal firms face larger labor wedges, this reallocation results in productivity gains for the economy. The model has four main ingredients. First, each local economy has two sectors: the formal and the informal sector. Second, consistent with evidence in Section 2.2, household preferences for formal and informal goods is non-homothetic such that the formal share of spending rises with household resources. Third, firms use local labor as an input and they face heterogeneous labor wedges, with formal firms facing larger wedges on average, consistent with the patterns shown in Section 2.1. Fourth, labor is mobile across sectors within a local labor market and immobile across local labor markets. Therefore, aggregate labor supply in each local market is fixed, and changes in demand composition result in a reallocation of labor across formal and informal firms.

The model delivers four predictions that organize the empirical work, one at the household level and the remaining three at the local economy level. First, households increase both their formal share

of consumption, and the formal content of marginal increases in consumption monotonically as they move across the income distribution. Second, due to these non-homotheticities in formal consumption, an externally financed transfer can shift the composition of local demand and reallocate employment between the formal and informal sectors. Third, the effect on demand composition and employment depends on who receives the transfer. As richer households have a higher marginal propensity to spend an extra peso formally, a transfer to these households generates a larger shift towards the formal sector. As a result, universalizing a means-tested benefit generates a larger reallocation towards the formal sector per peso than increasing the benefit for existing low-income recipients. Fourth, the effect of the transfer on aggregate productivity depends on the size of the wedge and the labor reallocation. Section 5 estimates the household demand response, and Section 6 tests the implied employment reallocation and measures its effect on productivity. All derivations and proofs are in Appendix A.

3.1 Environment

Consider a small and open local economy (a municipality in our empirical setting). The economy has two local sectors, formal (F) and informal (I), and each sector contains a continuum of firms. Firms use a tradable good as input, which is imported at a price fixed in national markets and normalized to one. Households spend all their budget on the goods produced by the two local sectors.¹⁷

The economy has a single local labor market. Labor is mobile across the two sectors and earns a common wage w per unit. Aggregate labor supply is fixed.¹⁸

$$L_F + L_I = \bar{L}, \quad (1)$$

where L_s is employment in sector s .

There is a unit mass of households indexed by i . Each household i inelastically supplies z_i units of labor, with $\int z_i di = \bar{L}$. Households also receive nonlabor income y_i and a pension transfer T_i , both measured in units of the tradable numeraire, and the latter is financed with resources outside the municipality. Households live only one period, so their nominal expenditure equals their total resources:

$$e_i = wz_i + y_i + T_i. \quad (2)$$

Aggregate expenditure is

$$E \equiv \int e_i di = w\bar{L} + Y_0 + T, \quad Y_0 \equiv \int y_i di, \quad T \equiv \int T_i di. \quad (3)$$

We assume $Y_0 + T > 0$ and that every household has positive resources.

¹⁷Allowing households to also spend a share of the marginal peso on tradables would rescale the direct local demand response by that share and leave its composition unchanged (Remark 1).

¹⁸As mentioned in Section 5, the labor supply response of recipients is small. Section 6 finds no significant change in total local employment in response to the reform.

The national government pays the pension transfers from the national budget. The transfers are financed by issuing debt, which gets repaid in a second period. The model here describes the first period of this two period economy. Since current households live only for one period, do not have bequest motives, and the government promises to repay raising taxes on a future generation (period two), current households do not internalize them. Hence the transfer increases recipients' resources without causing a reduction in current expenditure in anticipation of future taxes.

3.2 Household demand

Households have PIGL preferences over the formal and informal local goods (Muellbauer, 1975; Boppart, 2014), with indirect utility function

$$V(e_i; p_F, p_I) = \frac{1}{\varepsilon} \left(\frac{e_i}{P} \right)^\varepsilon + \nu \log \left(\frac{p_F}{p_I} \right), \quad 0 < \varepsilon < 1, \quad P \equiv p_F^\omega p_I^{1-\omega}, \quad 0 < \omega < 1, \quad (4)$$

where p_F and p_I are the prices of the formal and informal goods. P is the Cobb-Douglas price index, and the parameter ν controls the non-homotheticity between the goods.

Assumption 1 (E1: the formal good is a luxury). $\nu > 0$.

Roy's identity gives the formal spending share of a household with resources e_i ,

$$s_F(e_i) = \omega - \nu \left(\frac{e_i}{P} \right)^{-\varepsilon}, \quad (5)$$

so formal and informal spending are given by $D_F^i = s_F(e_i) e_i$ and $D_I^i = e_i - D_F^i$. Under Assumption 1, the formal spending share rises with resources toward the asymptote ω , and it is positive for $e_i > \underline{e} = P(\nu/\omega)^{1/\varepsilon}$. We restrict attention to equilibria in which all households have $e_i > \underline{e}$, such that all buy some formal good and demand is interior. This is consistent with Figure 2, which shows that the formal spending share rises with resources across households, yet formal consumption share is still positive for the lowest income households.

While share $s_F(e_i)$ gives the formal share of the household's average peso of spending, the marginal propensity to spend formally is defined as the increase in formal expenditure per additional peso of household resources:

$$\sigma(e_i) \equiv \frac{\partial D_F^i}{\partial e_i} = \omega - (1 - \varepsilon) \nu \left(\frac{e_i}{P} \right)^{-\varepsilon}. \quad (6)$$

Lemma 1 (Marginal formal spending). *Under Assumption 1:*

(i) *the marginal peso is more formal than the household's average peso,*

$$\sigma(e_i) - s_F(e_i) = \varepsilon [\omega - s_F(e_i)] > 0; \quad (7)$$

(ii) *the marginal propensity to spend formally is strictly increasing in resources, $\sigma'(e_i) > 0$, a property we label E2.*

The two parts of the lemma do different work in the model. Part (i) is what makes a transfer shift demand toward the formal sector, and part (ii) is what makes the allocation of the transfer across households matter. The lemma shows that E2 follows from E1 in our demand system.¹⁹ Section 5 tests both parts of the lemma using the universalization of pensions in Mexico as an income shock: we assess the average effect of the transfer on formal composition, and how this effect varies with beneficiary composition.²⁰

3.3 Firms

Each sector $s \in \{F, I\}$ has a continuum of firms $j \in \mathcal{F}_s$. Each firm j uses local labor l_j and imported input x_j to produce the sector good:

$$q_j = A_j l_j^{\alpha_s} x_j^{\beta_s}, \quad \alpha_s > 0, \quad \beta_s > 0, \quad \gamma_s \equiv \alpha_s + \beta_s < 1, \quad (8)$$

where A_j is the firm's productivity and $\gamma_s < 1$ implies decreasing returns to scale. Each firm faces a labor wedge τ_j , with $1 + \tau_j > 0$. τ_j represents a distortion that creates a wedge between the price of labor paid by the firm and the wage received by workers, and it can be negative when the distortion lowers the firm's labor cost below the market wage. Such wedges have been shown to be prevalent in labor markets in developing countries, and can arise from multiple sources (Atkin and Khandelwal, 2020). For instance, they may reflect payroll taxes, social security contributions, different monopsony power, and compliance costs that raise firms' marginal cost of labor above the wage received by workers. We assume the resources from the wedge go outside the local economy, and the profits go to firm owners, also outside the local economy, so none of them returns to households' budgets.

Firm j in sector s takes p_s and w as given and solves

$$\max_{l_j, x_j} \left\{ p_s A_j l_j^{\alpha_s} x_j^{\beta_s} - (1 + \tau_j) w l_j - x_j \right\}. \quad (9)$$

With revenue $R_j \equiv p_s q_j$, the first-order conditions are

$$\alpha_s \frac{R_j}{l_j} = (1 + \tau_j) w, \quad \beta_s \frac{R_j}{x_j} = 1. \quad (10)$$

The labor condition defines the measured marginal revenue product of labor, $\text{MRPL}_j \equiv \alpha_s R_j / l_j = (1 + \tau_j) w$. Because (10) holds firm by firm at a common wage, differences in measured MRPL are a direct proxy of the wedge. Thus, under our assumptions, MRPL dispersion indicates misallocation (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Bau and Matray, 2023): firms with higher

¹⁹Under Stone–Geary preferences, the formal spending share can rise with resources while the marginal propensity to spend formally is constant above subsistence. In that case the reallocation caused by a transfer would not depend on the position of recipients in the income distribution, contrary to what we find.

²⁰The two-good demand system can be generated by aggregation over product categories in two ways. Richer households can shift their basket toward categories that are mostly sold by formal firms, or they can buy the same products from formal sellers more often. The decomposition in Table B.3 and Figure B.7 in the Appendix shows that most of the rise in the formal share happens within categories. Appendix A.2 develops that foundation, where v is the average of the within-category slopes weighted by budget shares, and it microfound the classification of spending by place of purchase used in Sections 2 and 5.

wedges have higher MRPL and employ less labor than in the absence of those wedges.

The employment-weighted average wedge of sector s is given by:²¹

$$1 + \bar{\tau}_s \equiv \frac{\int_{j \in \mathcal{F}_s} (1 + \tau_j) l_j dj}{L_s}, \quad L_s \equiv \int_{j \in \mathcal{F}_s} l_j dj, \quad (11)$$

Assumption 2 (Ranking of the wedges). $\bar{\tau}_F > \bar{\tau}_I$: the formal sector has a larger employment-weighted average wedge.

This assumption is in line with the empirical evidence shown in Figure 1 in Section 2 and Figure B.1 in the Appendix. Notably, the assumption does not require every formal firm to have a higher wedge than every informal firm, consistent with the overlapping empirical distributions of residualized MRPL across the two sectors shown in Figure 1.

Let $D_s \equiv \int D_s^i di$ denote the aggregate spending on the sector s good in the municipality. Goods market clearing equates household demand for good s with sector output, $D_s/p_s = \int_{j \in \mathcal{F}_s} q_j dj$. Equivalently, at common price p_s , aggregate spending equals sector revenue, $D_s = \int_{j \in \mathcal{F}_s} R_j dj$. Integrating the labor condition in (10) over the sector gives the sector employment

$$L_s = \kappa_s \frac{D_s}{w}, \quad \kappa_s \equiv \frac{\alpha_s}{1 + \bar{\tau}_s}, \quad (12)$$

and the input first order condition gives sector aggregate imported inputs $X_s = \beta_s D_s$. The term κ_s is the share of a peso spent in sector s that becomes direct wage payments in that sector. The rest of the peso goes to imported inputs, profits, and the labor wedge.²²

3.4 Equilibrium

Let $\bar{s} \equiv D_F/E$ denote the municipal formal expenditure share. Aggregating formal spending across households gives,

$$\bar{s} = \omega - \nu \mathcal{A} \left(\frac{E}{P} \right)^{-\varepsilon}, \quad \mathcal{A} \equiv \frac{\int e_i^{1-\varepsilon} di}{E^{1-\varepsilon}} \in (0, 1], \quad (13)$$

This expression shows that local markets with higher average expenditure levels will feature higher formality shares, consistent with the empirical evidence shown in Panel (b) of Figure 2 in Section 2.²³

²¹Because (A_j, τ_j) are fixed in our setting, and technology is Cobb–Douglas with the same returns to scale for every firm in a sector, any change in (p_s, w) moves every firm within its sector by the same proportion, so firm employment shares within a sector stay constant.

²²We assume $1 + \bar{\tau}_s > \alpha_s$, so that $\kappa_s \in (0, 1)$ and a peso of spending creates positive wage payments of less than a peso.

²³The term $\mathcal{A}$ shows that the composition of municipal demand depends not only on mean expenditure, but also on the entire distribution of resources. As $e \mapsto e^{1-\varepsilon}$ is concave, for a given average municipal expenditure, municipalities with greater expenditure dispersion across households have higher formality shares. Figure B.9 in the Appendix provides suggestive evidence of this result: conditional on average municipal income, greater within-municipality income dispersion is associated with larger formal shares of sales and production.

Adding (12) across sectors and imposing labor-market clearing gives

$$w\bar{L} = \bar{\kappa}(\bar{s})E, \quad \bar{\kappa}(\bar{s}) \equiv \bar{s}\kappa_F + (1 - \bar{s})\kappa_I. \quad (14)$$

Aggregate wage payments equal total expenditure times the weighted average of the shares of spending paid out as local wages. The formal employment share follows from (12) and (14), $\ell_F \equiv L_F/\bar{L} = \kappa_F \bar{s}/\bar{\kappa}(\bar{s})$, and it is an increasing function of the formal spending share.

Local prices respond to the wage and to the allocation of employment. For any marginal change, the local price index moves as $d \log P = \varphi_w d \log w + \varphi_\ell d \ell_F$, where the pass-through of the wage, φ_w , is positive, and the pass-through of the reallocation, φ_ℓ , can take either sign.²⁴

Definition 1 (Equilibrium of the local economy). *An equilibrium of the local economy is a wage w , prices (p_F, p_I) , household spending $\{D_F^i, D_I^i\}$, and firm choices $\{l_j, x_j\}$ such that: (i) households maximize (4) subject to their budget constraint (2); (ii) firm choices solve (9); (iii) the labor market clears, (1); and (iv) both goods markets clear, $D_s = \int_{j \in \mathcal{F}_s} R_j dj$ for $s \in \{F, I\}$.*

Appendix A.7 proves that an equilibrium exists, and shows that under Assumption 3 it is unique.

3.5 The effect of the transfer

Consider a transfer change dT given to households as $\{dT_i\}$ with $\int dT_i di = dT$. To assess the impact of the transfer on consumption and labor we define:

$$\bar{\sigma}_R \equiv \frac{\int \sigma(e_i) dT_i di}{dT}, \quad g_R \equiv \bar{\sigma}_R - \bar{s}. \quad (15)$$

The statistic $\bar{\sigma}_R$ is the average marginal propensity to spend formally among recipients, weighting them by the pesos they receive, where the subscript R stands for the specific allocation of dT across recipients of a particular transfer. g_R is the Engel gap of the reform, which measures how much more formal the transferred (marginal) peso is than the average local peso. These objects capture the first-order reallocation effects of a transfer. The shift in spending composition reallocates labor across sectors, affecting local wages and prices. These wage and price changes, in turn, affect households' real resources and—through the non-homotheticity of formal demand—induce further adjustments in spending composition, generating higher-order reallocation effects. This feedback loop between spending, reallocation, and prices is explained in detail in Appendix Section A.6, which derives a two-equation system that determines $d\bar{s}$ and dw and yields the responses in Proposition 1 below. Assumption 3 in the Appendix ensures that the equilibrium responses to the transfer are well behaved (stable and unique).

Proposition 1 (Formal spending share, wage, and employment responses). *Under Assumptions 1 and 3,*

²⁴The full derivation of the price index response and its weights is done in Appendix A.5.

a marginal transfer changes the municipal formal spending share and the wage by

$$d\bar{s} = \mathcal{G} \frac{dT}{E}, \quad dw = \frac{\bar{\kappa} \Lambda_P + \Delta\kappa g_R}{\mathcal{D}} \frac{dT}{\bar{L}}, \quad (16)$$

and moves employment by

$$dL_F = \frac{\kappa_F \kappa_I}{\bar{\kappa}^2} \cdot \mathcal{G} \cdot \bar{L} \cdot \frac{dT}{E}, \quad dL_I = -dL_F. \quad (17)$$

where $\Delta\kappa \equiv \kappa_F - \kappa_I$ and

$$\mathcal{G} \equiv \frac{(1 - \bar{\kappa}) g_R + \bar{\kappa} g_W}{\mathcal{D}}.$$

The term $\mathcal{G}$ represents the net formalization content of the marginal peso, accounting for both first-order demand effects and general equilibrium effects.²⁵ The terms g_W and Λ_P are defined in equations (A.24) and (A.25) in the Appendix, and capture the feedback between spending composition, wages, and prices. In particular, g_W captures the effect of additional wage income on the formal share of expenditure, while Λ_P captures feedback between the price index and employment reallocation.²⁶ The denominator $\mathcal{D}$, also defined in the Appendix, scales the direct effect of the transfer through the feedback. The responses scale with the fiscal exposure dT/E , the size of the transfer relative to the local economy. All terms, including E , take their pre-reform values. When $\mathcal{G} > 0$ formal employment rises and informal employment falls. This is the reallocation pattern that we test at the municipality level in Section 6.²⁷

The homothetic benchmark. To see the important role of non-homotheticity in our results, consider a benchmark model in which everything is unchanged except that demand is homothetic.

Corollary 1 (Neutrality under homothetic demand). *If $\nu = 0$, then $s_F(e) = \omega$ for every e , $\bar{s} = \omega$, $\bar{\sigma}_R = \bar{\sigma}_W = \omega$, and $g_R = g_W = 0$. For any size and any targeting of the transfer, the employment of every firm is unchanged, $d\bar{s} = d\ell_F = 0$, and the first-order change in real local value added, defined in Section 3.6, is zero. The transfer still raises expenditure, the wage, both local prices, and imports.*

Corollary 1 separates our mechanism from standard local multiplier effects of transfer injections (Nakamura and Steinsson, 2014; Egger et al., 2022). Under homothetic preferences, transfers affect the levels of wages, prices, and imports but leave the formal–informal composition of local demand and employment unchanged. For the transfer to induce a change in the formal composition of demand and, consequently, of employment, it is necessary that the marginal peso spent from the transfer differs in composition from the average peso spent.

²⁵The transfer also raises the level of spending, but the shift in the composition of spending $d\bar{s}$ is what reallocates labor across sectors. From the resource identity (3), expenditure rises with the transfer and with the induced wage income, $dE = dT + \bar{L} dw$. Formal spending then rises for two reasons, $dD_F = \bar{s} dE + \mathcal{G} dT$: a scale effect, since the transfer raises total spending, and a composition effect, since it shifts spending toward the formal good.

²⁶Reallocating labor across sectors changes local prices through the composition of the price index, and, consequently, households' real resources, inducing further adjustments in the formal share of expenditure.

²⁷The effect on formal employment in percentage terms can be expressed as $\hat{L}_F = \frac{\kappa_I}{\bar{\kappa}^2} \cdot \mathcal{G} \cdot \frac{dT}{E}$.

3.5.1 Which municipalities adjust the most

For municipality m , Proposition 1 gives

$$d\ell_{F,m} = \underbrace{\frac{dT_m}{E_m}}_{\text{fiscal exposure}} \times \underbrace{\mathcal{G}_m}_{\text{net formalization content}} \times \underbrace{\frac{\kappa_{F,m} \kappa_{I,m}}{\bar{\kappa}_m^2}}_{\text{employment pass-through}}. \quad (18)$$

The formula highlights three different sources of heterogeneity across municipalities. The first term is fiscal exposure, the size of the transfer relative to the local economy, which is the treatment measure that Section 6 uses. The second term is the net formalization content $\mathcal{G}_m$, which is larger when recipients have a higher marginal propensity to spend formally, i.e. when they are located higher on the local Engel curve. The third term is the baseline formal-informal structure of the economy, which determines how a change in the spending share translates into a change in the employment share. The employment pass-through can also be written as $\ell_{F,m}(1 - \ell_{F,m})/[\bar{s}_m(1 - \bar{s}_m)]$.

Proposition 2 (Recipient resources). *Under the assumptions of Proposition 1:*

- (i) *Holding the baseline municipal equilibrium fixed, the formal spending share increases with recipients' marginal propensity to spend formally:*

$$\frac{\partial \mathcal{G}_m}{\partial \bar{\sigma}_{R,m}} = \frac{1 - \bar{\kappa}_m}{\mathcal{D}_m} > 0. \quad (19)$$

- (ii) *For a given positive fiscal exposure dT_m/E_m , and holding the remaining equilibrium terms fixed, the responses of the formal spending and employment shares increase with the Engel gap $g_{R,m} = \bar{\sigma}_{R,m} - \bar{s}_m$.*

Part (i) shows that, conditional on municipal characteristics, who receives the transfer matters for the municipal formalization response. Since the marginal propensity to spend formally rises with household resources, directing a given transfer toward richer households means more of it is spent formally and, all else equal, generates a larger shift toward formal demand. Part (ii) concerns targeting *across* municipalities, and also accounts for the composition of spending in the municipality as a whole. Conditional on fiscal exposure and the remaining equilibrium terms, the formalization response is larger where the transferred peso is spent more formally relative to the average local peso.

Corollary 2 (Cash Transfer Universalization vs. Deepening). *Consider two equal budget transfer allocations A and B. If the peso-weighted distribution of recipients' pre-transfer resources under allocation A first-order stochastically dominates that under allocation B, then $\bar{\sigma}_{R,m}^A \geq \bar{\sigma}_{R,m}^B$, with strict inequality when the dominance is strict.*

Corollary 2 implies that, starting from a means-tested program in which households with income $Y_i < \bar{Y}$ receive transfers $T_i > 0$, an allocation dT that universalizes the program by extending transfers to households with $Y_i \geq \bar{Y}$ (a *universalization* transfer) shifts demand toward the formal sector more, and hence induces greater labor reallocation toward formal firms, than an equally sized allocation that increases transfers to households eligible for the means-tested benefit (a *deepening* transfer).

3.6 Productivity

We now derive the implications of the demand-driven reallocation generated by the transfer for aggregate productivity in the local economy. Define the real local value added of the economy at pre-reform prices as the gross local production net of imported inputs:

$$Y^L \equiv \sum_s \int_{j \in \mathcal{F}_s} (p_s q_j - x_j) dj = (1 - \beta_F) D_F + (1 - \beta_I) D_I, \quad (20)$$

In (20) we fix prices at their pre-reform values, so changes in Y^L are changes in real quantities, and the second equality holds at the pre-reform equilibrium, where sector spending equals sector revenue. We define the change in local TFP as $d \log \text{TFP} = dY^L/Y^L$. Since the firms import inputs, separating gross output and value added matters here for accounting. Gross sector output can rise because firms use and import more tradable inputs, which have zero net value added at the optimum.

Proposition 3 (Productivity gain from reallocation). *Let $\lambda_j \equiv R_j/Y^L$ denote the firm Domar weight. To first order in the reform,*

$$d \log \text{TFP} = \sum_s \int_{j \in \mathcal{F}_s} \lambda_j \alpha_s \frac{\tau_j}{1 + \tau_j} d \log l_j, \quad (21)$$

and, aggregated across firms using Lemma 2, the change in real value added is

$$dY^L = w (\bar{\tau}_F - \bar{\tau}_I) dL_F. \quad (22)$$

We derive Equation (21) in Appendix Section A.8, which is the reallocation formula from Baqaee and Farhi (2020). Equation (21) shows that local value added gains come from the reallocation of the local factor, labor, across firms with different wedges. Moving a worker from a low distorted low-distorted (low MRPL) firm to a highly distorted (high MRPL) firm raises value added by the difference in their marginal revenue products of labor. Equation (22) highlights the two forces in our model driving the change in allocative efficiency: the demand system, which determines how much labor moves across sectors, and the wedge gap, which determines the value of that movement. Note that we don't necessarily need the wedge in the informal sector to be zero, both sectors may be distorted, but only the gap between average distortions matters.

In the following sections, we test the model's predictions by exploiting a reform that expanded coverage and generosity of non-contributory pensions in Mexico. Section 5 assesses Lemma 1 by estimating household-level responses to the transfer. Section 6 assesses Propositions 1 and 2 by evaluating the pension's effects on reallocation in local markets and how these effects vary with beneficiary composition. Finally, Section 6.6 combines the estimated effects of the reform with empirical counterparts of the components of Equation (21) in Proposition 3 to assess the reform's implications for aggregate productivity.

4 Institutional Background and Data

4.1 Institutional Background

In developing economies, a large share of workers reach old age without a pension. Globally, only about a quarter of the working-age population contributes to a pension scheme, and in many low-income countries, fewer than 20% of those who reach retirement age receive one ([International Labour Organization, 2024](#)). Several factors drive this gap. Contributory pensions require sustained formal employment, which is scarce where informal work predominates. Careers are also fragmented: workers move in and out of formal jobs, interrupt work to care for dependents, or leave during downturns, accumulating gaps rather than the continuous contribution histories that qualifying period rules demand. Low earnings compound the problem, as low-paid workers have limited capacity to contribute even when eligible. Mexico exhibits all of these features: by 2018, only 24% of individuals aged 65 and older held a contributory pension.

To fill this gap, governments have introduced a non-contributory pillar into pension systems. In Mexico, before 2019, this pillar operated through the *Programa Pensión para Adultos Mayores* (65 y Más), which paid a flat transfer to individuals aged 65 and above who did not receive a contributory old-age pension or whose contributory pension income did not exceed MXN 1,092 per month. In 2018, the program paid roughly 580 pesos per month (approximately \$31 USD), reached about 5.1 million beneficiaries, and covered around 51% of Mexico's population aged 65 and over, predominantly low-income individuals (see Section 5). Benefit levels and eligibility rules had remained essentially unchanged in the years leading up to the reform, providing a stable pre-reform baseline.

In December 2018, the incoming administration reformed the non-contributory pension, relaunching it as *Pensión para el Bienestar de las Personas Adultas Mayores* (Welfare Pension for Older Adults). The reform introduced three changes. First, it eliminated means-testing, extending the pension to all older adults regardless of income or contributory pension status.²⁸ Second, the monthly benefit more than doubled, rising from \$31 to \$67 USD in 2019 and reaching \$126 by 2023. Third, it initially raised the eligibility age from 65 to 68, before lowering it back to 65 in 2021.²⁹ The expansion was financed through the liquidation of over 100 federal trust funds, rather than through new taxes or a decrease in the budget received by municipalities.³⁰

Figure B.10 and Table B.1 summarize the resulting expansion. Coverage grew from 5.1 to 11.4 million beneficiaries (representing 97% of individuals ages 65 and above) between 2018 and 2023,

²⁸The pension is paid bimonthly through direct deposit onto a *Banco del Bienestar* debit card, with little intermediation. In the early years of the reform, many beneficiaries collected their payments in cash at bank branches; the government subsequently expanded the *Banco del Bienestar* branch network, adding thousands of new branches, and shifted disbursement toward direct deposits onto beneficiary cards.

²⁹The existing beneficiaries kept their pensions throughout, so the higher threshold applied only to new entrants.

³⁰Other reforms occurred during the same period, such as the replacement of the conditional cash transfer *Prospera* (previously *Progresá/Oportunidades*) with more universal programs. These changes were national in scope. The cancellation of *Prospera* did not reallocate any funds to the pension and did not operate on the same margin: at the municipality level, the change in per capita pension transfers from universalization is weakly and even negatively correlated with the change in *Prospera* transfers ($\rho = -0.01$).

raising the program’s fiscal cost to roughly 1.4% of GDP (about \$19 billion per year) from 0.5% of GDP before universalization. This places it among the larger transfer programs in the developing world: Brazil’s Bolsa Família, by comparison, costs about 0.8% of GDP (Gerard et al., 2026), while in advanced economies old-age systems are larger still, with U.S. Social Security spending about 5.2% of GDP in 2023 (Congressional Budget Office, 2023). At the household level, the transfer is sizable: at roughly \$126 per month in 2023, it amounts to about 40% of the minimum wage (Comisión Nacional de los Salarios Mínimos, CONASAMI). Two features of the expansion shape our analysis. First, because the previous program was means-tested, the newly covered beneficiaries were on average richer than existing ones, so universalization extended the pension disproportionately toward middle- and higher-income households (see Section 5), a source of heterogeneity we exploit throughout. Second, although individuals aged 60 and older represent only 12% of the population, close to 30% of households include at least one such member, so the reform can be seen as a broad income shock reaching nearly a third of Mexican households.

4.2 Data

We draw on four categories of data, described briefly below. Additional detail on sample construction and linkages across datasets is in Appendix B.

A. Administrative pension records. We use monthly administrative records from the *Secretaría de Bienestar* covering the universe of non-contributory pension payments from 2016 to 2024. The data report the number of beneficiaries and the total transfer amount per municipality and month, disaggregated by program (*65 y Más* before 2019, *Pensión para el Bienestar* thereafter). These records allow us to construct our main measure of treatment intensity at the municipality level: the change in per capita pension transfers between the pre- and post-reform periods and to document that the cross-municipal variation we exploit emerges only with the reform rather than from pre-existing divergence in coverage.

B. National Household of Income Expenditure Survey (ENIGH). ENIGH is a biennial state representative survey recording household income, detailed consumption by good category, savings, and household composition. ENIGH has two features central to our analysis. First, it records whether each household member receives non-contributory pension transfers. Second, it classifies food and beverage expenditure items by place of purchase, which lets us infer the formality status of the establishment where each purchase was made (we follow Bachas et al. (2024) in their classification). This allows us to proxy the household’s formal budget share directly and to document its non-homotheticity. We use ENIGH waves from 2014, 2016, 2018, 2020, 2022, and 2024.

C. Establishment census (Economic Census). We use establishment-level microdata from INEGI’s *Economic Census* (Censos Económicos), which covers the universe of economic establishments in Mexico, including the formal and the informal sector. Two waves span the reform period: the 2019 Census (reference year 2018, pre-reform) and the 2024 Census (reference year 2023, post-reform), each containing approximately five million establishments. For each establishment, the data report output, employment, value added, input purchases, location, 6-digit industry, and formality status (defined by

registration with the tax authority). We construct a balanced panel of roughly two million establishments present in both waves to study intensive-margin responses, to compute the establishment-level marginal revenue product of labor (MRPL), and to measure within-industry productivity dispersion following [Hsieh and Klenow \(2009\)](#). We also exploit the census coverage to aggregate outcomes to the municipality level. For our main analysis, we restrict the sample to municipalities with a minimum formal-sector presence of 4%³¹ (15th percentile), and exclude Mexico City. This leaves us with 2,070 municipalities (out of a total of 2,463 municipalities).

D. Private Retail Data. We complement the survey and administrative sources with proprietary transaction data from one of the largest retail chains in Mexico. The data cover more than 3,000 stores across more than 700 municipalities over the period 2015–2023, at a monthly frequency. For each store, we observe prices, physical quantities sold, and total revenue at the product level. The chain sells primarily food and beverages, along with some financial services (e.g., bill payment, ATM access, etc.) and health and personal care products. Because the data report both prices and quantities, we can separate changes in nominal sales into price and quantity components. We aggregate the data to the municipality $\times$ month level and merge them with our treatment measure.

Appendix Table [B.2](#) reports summary statistics for the main samples used in the analysis.

5 Effect on household consumption

5.1 Research Design: Difference-in-Differences

To estimate the effect of the universal pension on household outcomes, we exploit the strict age eligibility cutoff together with the timing of universalization. We compare households with at least one member just above the age threshold (treated) to households with at least one member just below it (control), before and after the reform.³² We restrict the sample to households with members aged 60 to 73, which keeps treated and control households as comparable as possible.³³

We estimate two difference-in-differences specifications: a reduced form and an instrumental variables specification. The reduced form identifies the intention-to-treat effect of eligibility. The IV specification instruments household pension income with the interaction between the number of age-eligible members (*NElderly*) and the post-reform indicator, expressing expenditure responses per additional peso of pension income induced by the reform. This normalization matters for two reasons. First, the increase in pension income varied across the income distribution because take-up differed and low-income households were more likely to receive the means-tested pension before the reform,

³¹We measure formal-sector presence as the share of formal workers in the municipality; using the share of formal firms or formal output instead yields similar results. Below this level, the formality share is zero.

³²Eligibility for the pre-reform means-tested non-contributory pension was restricted to individuals aged 65 and above. Following the 2019 reform, the eligibility threshold was initially raised to age 68 for the 2019–2020 period, before returning to age 65 from 2021 onward.

³³For the 2014–2018 and 2021–2024 surveys we use ages 60–64 for the control group and 65–73 for the treated group; for 2019 and 2020 we adjust the window to 60–67 and 68–73 to match the temporary threshold. Results are robust to holding the age-65 cutoff constant throughout.

making their incremental transfer smaller. Second, the IV estimates for formal spending in levels allow us to construct an empirical counterpart to the marginal propensity to spend formally $\sigma(e)$ defined in Section 3, allowing us to assess Lemma 1 from our model.

Reduced Form:

$$y_{h,s,t} = \sum_{k \neq 2018} \beta_k NElderly_h \mathbb{1}\{t = k\} + \delta NElderly_h + X'_{h,t} \gamma + \lambda_{s,t} + \theta_{l,t} + \varepsilon_{h,s,t} \quad (23)$$

IV:

$$Pension_{h,s,t} = \pi(NElderly_h \times Post_t) + \delta^{(1)} NElderly_h + X'_{h,t} \gamma^{(1)} + \lambda_{s,t}^{(1)} + u_{h,s,t}, \quad (24)$$

$$y_{h,s,t} = \beta \widehat{Pension}_{h,s,t} + \delta^{(2)} NElderly_h + X'_{h,t} \gamma^{(2)} + \lambda_{s,t}^{(2)} + \varepsilon_{h,s,t}. \quad (25)$$

Equation (23) estimates the intention-to-treat effect of eligibility, where $y_{h,s,t}$ is the outcome of interest for household h in state s in year t . The coefficients of interest β_k capture the difference between households with elderly members and those without in year k , relative to the same difference in 2018, the year prior to universalization. $\lambda_{s,t}$ denotes state $\times$ year fixed effects, and $X_{h,t}$ is a vector of household controls including household size, the number of male members, the number of members under 12, the age, sex, and education of the household head, the INEGI socioeconomic stratum, household type, locality size, and the minimum wage.

The IV specification is estimated using two-stage least squares. Equation (24) is the first stage, which instruments household pension income with the interaction between the number of age-eligible members and $Post_t$, an indicator for the post-reform period. The coefficient π captures the differential increase in pension income after the reform associated with an additional eligible household member. The specification includes the main effect of $NElderly_h$, household controls, and state $\times$ year fixed effects. Equation (25) is the second stage, where $\widehat{Pension}_{h,s,t}$ denotes pension income predicted by the first stage. Under the IV identifying assumptions, β measures the effect of an additional peso of pension income induced by the reform. The same household controls and state $\times$ year fixed effects are also included in the second stage.

Our main identification assumption is that treated and control households would have followed parallel outcome trends absent the reform. The absence of pre-trends across the results in Section 5.2 provides initial support for this assumption. Moreover, restricting the sample to households with members just above and just below the age threshold makes it unlikely that a contemporaneous shock is both correlated with the timing of the reform and differentially affects one group but not the other.³⁴ A separate concern is that, because the pension is a permanent transfer, households may anticipate eligibility and begin smoothing consumption and labor earnings before becoming elderly. Such anticipation would attenuate our estimates, making our results a lower bound. We provide

³⁴For COVID-19 or any other aggregate shock, the impact on a 64-year-old and a 67-year-old, and their households, is plausibly similar.

evidence of no anticipation in the following subsection by showing that the effects emerge precisely at the age eligibility cutoff. One potential explanation is limited access to credit. According to Mexico’s national survey of financial inclusion (ENIF), roughly 65% of households near the cutoff are credit-constrained,³⁵ which likely prevents households from bringing future transfers forward. Finally, Table B.4 reports descriptive statistics for the two groups. The treated and control households are similar in size, composition, income per capita, and spending behavior, both in levels and in the share spent in the formal sector. Treated households have fewer working members and lower social security coverage, reflecting that individuals above 65 have largely left the labor force. Figure B.13 shows that households do not modify their composition in response to the reform.

The IV estimates require two additional conditions: relevance and exclusion. Relevance is directly testable: given the strict age threshold, the instrument is strong (see F-stats in Table 1). Exclusion requires that the differential change in outcomes for households with more age-eligible members after the reform operates only through the pension transfer, conditional on controls. We argue this is plausible for two reasons. First, we restrict the comparison to demographically similar households in a narrow age window around the threshold, so any confounding shock would need to be correlated with both the timing of the reform and the age-eligibility cutoff to threaten identification; smooth aggregate shocks such as COVID-19, which affect a 63-year-old and a 67-year-old similarly, do not meet this requirement. Second, households did not alter their demographic composition in response to the reform (Figure B.13), ruling out the concern that the instrument operates through endogenous household formation rather than through extra income.

5.2 Results

In this section we estimate the effect of the universal pension on beneficiaries’ household consumption. We first document the change in coverage and in per capita non-contributory pension income after universalization, and show that changes in both take-up and per capita pension income were largest among middle and high income households. We then show that households affected by the income shock from the non-contributory pension increased their consumption on average, with formal consumption accounting for most of the increase. Finally, we test the assumption that the marginal propensity to spend formally rises with household resources (property E2 in Lemma 1), using the pension as an income shock that traces out the marginal formal expenditure along the income distribution.

5.2.1 Effect on Coverage

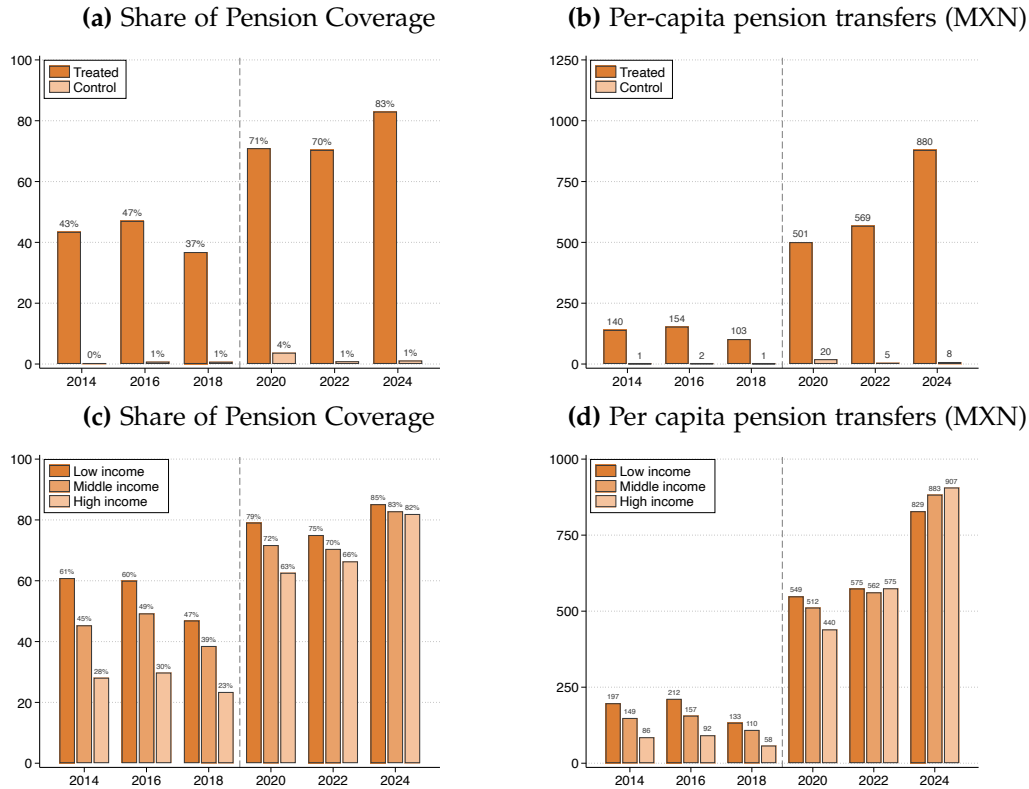
Figure 3 Panel (a) and (b) show the evolution of non-contributory pension coverage and per capita pension income for treated households (those with at least one member at or above the age cutoff) and control households (those with members just below it) respectively. Before universalization, 41% of households with members aged 65 or older reported receiving a non-contributory pension.³⁶ Reported

³⁵75% of households report not having any form of credit. Of these, around 87% state that the reason is an external barrier rather than a lack of demand for credit.

³⁶Reported take-up below official coverage rates is a common feature of household surveys, attributable to misreporting, recall error, and stigma (Meyer et al., 2015; Celhay et al., 2025)

coverage rose to 71% within two years of the reform and reached 83% by 2024. In monetary terms, universalization raised average per capita pension income by MXN 545 among households with at least one member above 65, a 400% increase, equivalent to 25% of the monthly minimum wage before the reform.

Figure 3: Pension Amount and Coverage Evolution



Notes: This figure plots the evolution of non-contributory pension coverage using household level data from ENIGH 2014-2024. Panels (a) and (c) plot share of households receiving a pension. Panels (b) and (d) plot per-capita pension income (in 2018 MXN) for treated households, defined as those with at least one member at or above the age-eligibility cutoff. Panels (c) and (d) shows these statistics, split by socio-economic status using the ENIGH classification. Darker to lighter shades denote low, middle, and high-income households, respectively. The vertical dashed line marks the 2019 universalization reform.

Figure 3 Panel (c) and (d) presents the same results by socioeconomic status. Two observations are relevant. First, as most cash transfers, pre-reform non-contributory pension was concentrated among low-income households, in 2016 take-up among low-income households was 60%, compared to 49% for middle-income and 30% for high-income households, with per capita transfers of MXN 287, 230 and 129, respectively.³⁷ Second, universalization erased this gradient. By 2024, take-up had risen to 85%, 83% and 82%, an increase of 25, 34 and 52 percentage points relative to 2016, while per capita transfers

³⁷We benchmark against 2016 rather than 2018 because reported coverage falls across all groups in the 2018 wave. ENIGH 2018 fieldwork took place between August and November 2018, during the presidential transition, and its six-month income recall window overlaps the 2018 federal campaign period, during which social program payments are suspended by electoral law. Both factors depress reported pension receipt in that wave.

rose to MXN 1,787, 1,836 and 1,875, so that by the end of the period high-income households received slightly more per capita than low-income households. This expansion of resources to households across the income distribution, particularly among middle and high income households, is central to our analysis, since it allows us to test for differential responses across the income distribution and their aggregate implications.

5.2.2 Effect on Overall, Formal and Informal Consumption

Figure 4 summarizes the effects of pension universalization on total, formal, and informal consumption. We measure consumption using the *National Survey of Household Income and Expenditure (ENIGH)*, which records expenditures across eight broad categories: food and beverages; clothing and footwear; housing and utilities; cleaning and household articles; health; transportation and communications; education and recreation; and personal care. For a subset of products, including food, beverages, and cigarettes, the survey also records the place of purchase, allowing us to classify expenditures as formal or informal following [Bachas et al. \(2024\)](#).

Panel (a) reports the effect on total consumption, including expenditures that cannot be classified as formal or informal.³⁸ Following universalization, total consumption increased by 4.7%. Panel (b) shows that this increase was larger at formal establishments.³⁹ Formal consumption rose by a statistically significant 5.1%, compared with 2.8% for informal consumption; the difference between these responses is statistically significant ($p = 0.026$). Panel (c) captures the resulting shift in spending composition: the formal share of classifiable consumption increased by 0.6 percentage points per recipient, equivalent to approximately 1 percentage point for the average treated household. Panels (d), (e), and (f) plot the corresponding means for treated and control households, normalized to 100 in 2018, illustrating the post-reform divergence in their consumption paths. Panel A of Table 1 shows that the results are robust to alternative fixed effects and the inclusion of sociodemographic controls.

We additionally assess which sectors most strongly drive this increase in consumption. Dividing spending into the eight main ENIGH purchase categories, the largest responses are in food (2.5%), personal expenses (3.5%), cleaning products (5.6%), and transportation (2.7%) (See Figure B.17 in the Appendix). We revisit these findings in Section 6, where we estimate the effects of the pension on these highly exposed sectors. We additionally evaluate the effects separately on tradable and non-tradable goods by classifying each product by whether it can be produced away from where it is consumed. Non-tradables comprise services and locally provided goods food away from home, passenger transportation, housing rents and utilities, domestic and cleaning services, personal care services, education, health services, vehicle maintenance, repairs of clothing and footwear, and recreational and cultural services. Tradables comprise physical goods such as packaged food and beverages, clothing, medicines, and household supplies (see Table B.17 for the full classification).⁴⁰ Figure B.18 shows that non-tradable

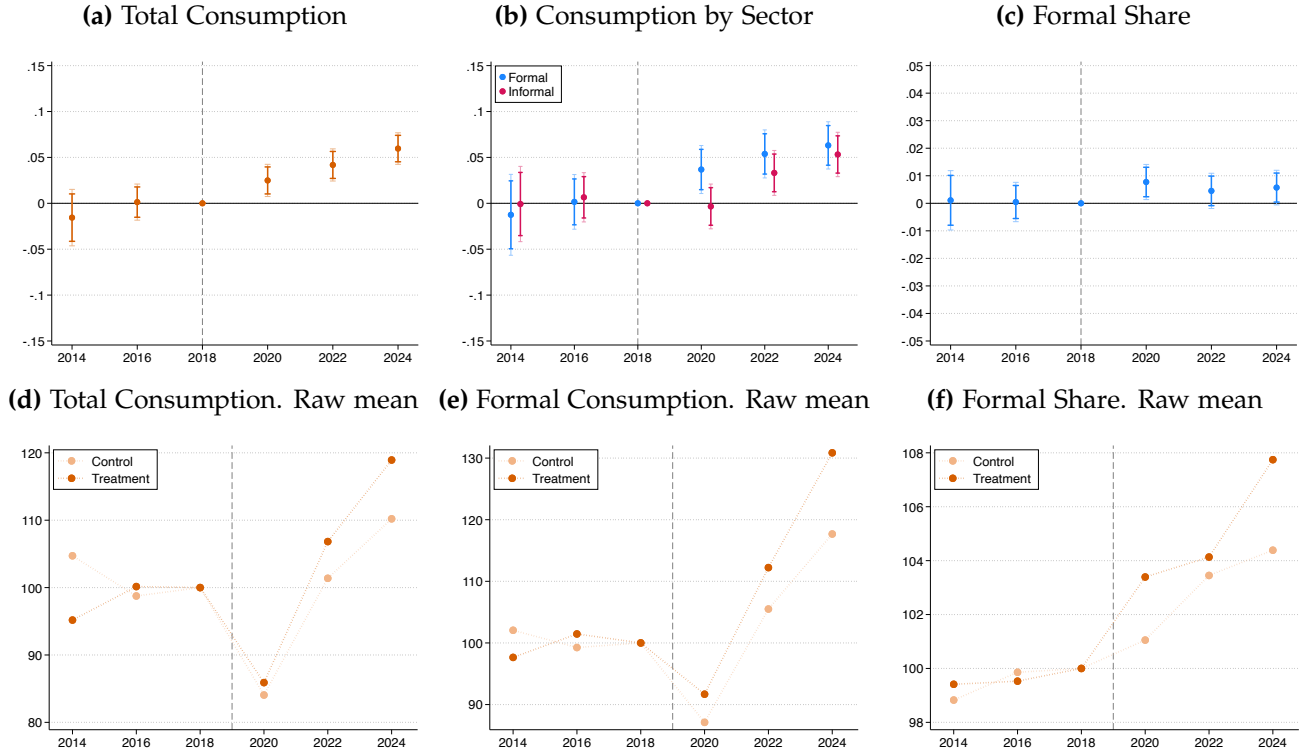
³⁸Figure B.16 in the Appendix restricts total consumption to products for which the place of purchase is observed and yields similar results.

³⁹Although the place of purchase is not observed for all products, those for which it is available account for 68% of total household spending. Table B.18 in the Appendix lists the products covered, and Table B.19 reports the classification of purchase locations as formal or informal.

⁴⁰Durable goods, including appliances, furniture, recreational equipment and vehicles, are classified as tradables.

consumption rises by 7.6%, compared with 3.5% for tradables ($p = 0.007$ for the difference). This larger response in non-tradable spending is particularly relevant for the local effects of the pension, as spending on non-tradables has been shown to generate larger local effects (Nakamura and Steinsson, 2014; Gerard et al., 2026).

Figure 4: Effects on Consumption



Notes: This figure plots the effect on household consumption. The top row reports event-study coefficients from equation 23, with 2018 as the omitted reference year. Panel (a) shows the effect on log total consumption, panel (b) on log formal and log informal consumption, and panel (c) on the share of formal consumption. Vertical bars represent 95% confidence intervals with robust standard errors. The bottom row plots the mean of each outcome by year for treatment and control households (Group Mean), normalized to 100 in 2018: total consumption in panel (d), formal consumption in panel (e), and the formal share in panel (f). The dashed vertical line marks the 2019 reform. We classify household expenditures as formal or informal following Bachas et al. (2024), based on the place of purchase reported in the ENIGH (see Table B.19). Informal purchases include those from street vendors, street markets, public markets and acquaintances.

Column (1) of Table 1 Panel B reports the IV estimates for the formal consumption share. In the specification with state $\times$ year fixed effects and sociodemographic controls, each additional MXN 1,000 of pension income raises the formal share by 0.5 percentage points. Equivalently, a pension-income increase equal to the average pension income received by beneficiary households during 2019–2024 (MXN 2,865) results in an increase of 1.4 percentage points, or 2.5% relative to the baseline formal share. Columns (2) and (3) of Panel B show that this pattern is explained by a larger increase in formal-sector expenditure per peso transferred: 5%, compared with only 2% for informal expenditure. These results provide evidence of non-homothetic demand for formal consumption. At fixed relative prices, homothetic preferences imply that an income increase raises formal and informal consumption proportionally, leaving the formal share unchanged. The estimated increase in the formal share is

inconsistent with this prediction.⁴¹ To our knowledge, this is among the first studies to document this non-homotheticity using an exogenous household-level income shock.⁴²

A related question is whether the pension affects the labor supply of recipients and their household members. We examine these responses in detail in a companion paper (Colonna et al., 2026), covering both recipients' own labor supply and spillovers to family members. For this paper, the relevant finding is that these adjustments appear modest in aggregate, limiting the extent to which reductions in labor earnings offset the transfer-induced increase in household resources.⁴³ This makes the reform a useful setting for studying non-homothetic consumption responses to an income increase. Three facts support this interpretation. First, only 33% of individuals at or above the eligibility age were working pre-reform. Second, among those still working, average weekly hours were 36, while the estimated reduction was only 0.07 hours per week. Third, the estimated reduction among family members was also small (0.05 hours per week) and concentrated among informal workers with low baseline hours. The household-level estimates in Figure B.20, which incorporate adjustments by both recipients and family members, are consistent with this limited response. The probability that a household receives any labor income declines by 1.3 percentage points (1.6% relative to its baseline level), while we see no significant effects on the intensive-margin. Panel (d) of Figure B.15 provides complementary descriptive evidence, showing similar labor income trends for the control and treated group before and after the reform.

Finally, we examine two potential threats to our interpretation. First, households approaching pension eligibility may anticipate the transfer and adjust their behavior before receiving it, potentially affecting our control group. Figures B.14 and B.15 provide little evidence of such anticipatory responses. Figure B.14 reports estimates from Equation (23) using an alternative control group consisting of households whose oldest member is between 40 and 50 years old. We estimate separate regressions for treatment groups defined by the oldest member's age, using two-year age bins from 51 to 76. The estimated effects on household income and consumption remain largely flat through age 64 and increase sharply at age 65. The raw age profiles in Figure B.15 show a similar pattern, with adjustments concentrated among households that have reached eligibility.⁴⁴ Second, because consumption is measured through expenditures, the estimated increases could partly reflect higher prices rather than greater quantities purchased. State $\times$ year fixed effects absorb price changes common to households within each state and year. To assess more localized price adjustments, Section 6 examines price and quantity responses across municipalities with different exposure to the reform. In municipalities receiving larger increases in transfers, the price response is small relative to the quantity response. This evidence

⁴¹This response operates along the intensive margin. Before the reform 98% of treated households already reported some purchase in a formal establishment, and the indicator of any formal purchase does not respond to the reform (see Figure B.19 in the Appendix). We return to this distinction in Section 5.2.3.

⁴²A household panel would allow us to follow the same households as their pension income changes. ENIGH is a repeated cross-section, so we cannot track these individual transitions. Our design uses reform-induced income variation to complement existing evidence based on cross-sectional expenditure patterns.

⁴³In the limiting case, a one-for-one reduction in household labor earnings would fully offset the transfer, leaving total household income unchanged.

⁴⁴The sharp increase in estimated effects at the eligibility threshold is also less consistent with broader shocks whose effects vary smoothly with age, including potential age-related effects of the COVID-19 pandemic.

supports the interpretation that the increase in spending largely reflects higher real consumption.

Table 1: Effect on Consumption-Reduced Form and IV

	Formal share (1)	Formal (2)	Informal (3)	Total (4)
<i>Panel A: Reduced form ($NElderly \times Post$)</i>				
Year + State FE	0.008*** (0.002)	0.061*** (0.010)	0.022*** (0.008)	0.049*** (0.007)
Year $\times$ State FE	0.008*** (0.002)	0.059*** (0.010)	0.021*** (0.008)	0.048*** (0.007)
Year $\times$ State FE + Controls	0.006*** (0.002)	0.052*** (0.009)	0.025*** (0.008)	0.043*** (0.006)
<i>Panel B: IV (Pension)</i>				
Year + State FE	0.007*** (0.002)	0.055*** (0.009)	0.020*** (0.007)	0.044*** (0.006)
Year $\times$ State FE	0.007*** (0.002)	0.054*** (0.009)	0.019*** (0.007)	0.043*** (0.006)
Year $\times$ State FE + Controls	0.005*** (0.002)	0.047*** (0.008)	0.022*** (0.007)	0.039*** (0.005)
First-stage F	20786.7	20605.7	20454.0	20786.3
Mean dep. var.	0.557	7.719	7.412	8.917
Observations	94,533	93,147	90,817	94,577

Notes: This table reports the effects on household consumption using the reduced-form specification (Equation 23) in Panel A and the corresponding IV specification in Panel B (Equation 25), where pension income is instrumented by the number of pension-eligible household members interacted with the post-reform period. The unit of observation is the household, and expenditure outcomes are monthly household totals. Each row within a panel corresponds to a fixed-effects specification, adding state and year effects separately, their interaction, and household controls. All outcomes are in logs except the share of formal consumption in column (1), which is a proportion, so its coefficients are in percentage points. Column (1) reports the effect on the share spent in the formal sector; column (2) shows the effect on formal consumption; column (3) shows the effect on informal consumption based on the place of purchase; column (4) shows the effect on total household consumption, including the classifiable and unclassifiable components (see Table B.18 for the products whose place of purchase is observed). All specifications control for household size and the number of age-eligible household members ($NElderly$). In Panel B the endogenous variable is monthly household income from the non-contributory pension, measured in thousands of pesos, so the coefficients give the response per additional MXN 1,000 of monthly pension income. Household controls are the number of male members, the number of members under 12, the age, sex, and education of the household head, the INEGI socioeconomic stratum, locality size, and the minimum wage. The first-stage F-statistic (Kleibergen–Paap), mean dependent variable, and number of observations correspond to the most saturated specification (Year $\times$ State FE + Controls). Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2.3 The Income Gradient in Formal Consumption

In the previous section, we established that the pension shifts household consumption toward formal establishments, rejecting homotheticity. We now characterize the *shape* of this non-homotheticity. A growing literature, mostly on structural transformation, has emphasized the importance of non-homothetic preferences in different settings (Boppart, 2014; Peters et al., 2026; Schwartzman, 2026; Ramos-Menchelli and Sverdlin-Lisker, 2024). Different preference structures, however, imply different profiles for how the marginal peso is allocated across the income distribution and, consequently, different effects of transfer targeting on the local economy.

Under Stone–Geary preferences (Geary, 1950; Stone, 1954), non-homotheticity arises from a subsistence requirement $\bar{c}_I$ for the informal necessity. Above subsistence, the marginal propensity to spend

formally, $\sigma(e)$, is constant, so every household allocates an additional peso in the same way. Recipient composition therefore does not affect the formalization generated by a given amount of transfers.

Schwartzman (2026) and Ramos-Menchelli and Sverdlin-Lisker (2024) instead generate non-homotheticity through a fixed cost of accessing formal or modern retail. Households below an expenditure threshold e^* consume no formal goods, whereas households sufficiently above it allocate a constant share of expenditure to the formal sector. The marginal formal expenditure response therefore jumps around the access threshold and is approximately flat away from it. Under this mechanism, transfers generate the largest formalization response when they are directed toward households close to the threshold, plausibly in the lower-middle part of the distribution.

Finally, in our model the marginal propensity to spend formally, $\sigma(e)$, increases across the income distribution: richer households allocate a larger fraction of each additional peso of expenditure to formal goods (Equation (6)).⁴⁵ The three classes can therefore be distinguished by the profile of the causal formal-expenditure response across household resources: constant under Stone–Geary, concentrated around an access threshold under fixed costs, and increasing in our specification. In this section, we use the pension as an exogenous income shock to distinguish among these alternatives.

To separate between alternative demand systems, we use two complementary measures of households’ economic resources.⁴⁶ The first is a categorical socioeconomic classification constructed by the Bureau of Statistics (INEGI), which assigns each household to one of four groups: low, lower-middle, upper-middle, and high. We pool the upper-middle and high groups for statistical power. Table B.5 compares household characteristics across the resulting three groups before the reform. The second measure is a continuous asset index constructed from the ownership of 33 durable goods and dwelling characteristics.⁴⁷ Unlike the categorical INEGI classification, this measure allows us to exploit continuous variation in household resources, including substantial variation within the broad socioeconomic groups.⁴⁸

For the resource measure based on socioeconomic classes, we estimate Equation (25) separately for each of the three socioeconomic groups, using total expenditure, formal expenditure and informal

⁴⁵We use PIGL preferences (Muellbauer, 1975; Boppart, 2014) as a tractable specification that delivers this property. An increasing marginal propensity to spend on the formal good can also arise under PIGLOG preferences (Deaton and Muellbauer, 1980) and non-homothetic CES preferences (Comin et al., 2021) under appropriate parameter restrictions.

⁴⁶We do not classify households using contemporaneous reported income for two reasons. First, around 25% of households report zero labor income, reflecting the fact that recipients are close to or past retirement age. Second, pension income directly enters measured household income, so defining groups using contemporaneous income would introduce selection based on a post-treatment variable. For the same reason, we do not rank households using contemporaneous expenditure.

⁴⁷The index is the first principal component of 33 indicators of durable ownership, dwelling materials, and access to services; Appendix Table B.20 reports the full set of indicators and their loadings. We estimate the component once using the pre-reform waves (2014–2018) and apply the resulting loadings to all survey years. We standardize the score to mean zero and unit variance in the pre-reform sample, so that positions in post-reform waves are measured relative to the pre-reform distribution. The first component explains 23.7% of the total variance in the estimation sample.

⁴⁸The two measures are imperfectly related. The mean standardized asset index rises from -1.11 among low-income households to -0.07 among middle-income households and 0.75 among high-income households, but the three socioeconomic groups explain only 40% of the variation in the index. The asset index also captures differences in household expenditure beyond those captured by the socioeconomic groups: these groups alone explain 12% of the variation in total expenditure, while adding the asset index raises the explained share by 16 percentage points, to 28%.

expenditure as outcomes. This provides us with a separate estimate of the average marginal propensity to consume overall and in the formal and informal sectors for each socio-economic group.

For the second measure, based on household assets, we assess how marginal consumption responses vary with household resources by interacting pension income with the standardized asset index in Equation (25). Specifically, we instrument pension income and its interaction with the standardized asset index using the eligibility-based instrument and its corresponding interaction with the index.⁴⁹

Table 2 reports the results of the two approaches. Panel A reports the marginal expenditure for the three socio-economic groups. Consistent with our model's demand specification, Column (1) shows that the formal expenditure response is largest among high-income households: an additional peso of pension income raises formal expenditure by 0.2 pesos, compared with 0.14 among low-income households and 0.08 among middle-income households. The high-low difference is positive but not statistically significant ($p = 0.240$, one-sided). Column (2) displays the opposite pattern for informal expenditure. Per peso of transfer, low-income households increase informal expenditure by 0.08 pesos per peso of pension income, middle-income households by 0.059, while the response among high-income households is only 0.030 and is not statistically different from zero. Thus, although the socioeconomic classification does not generate a strictly monotonic formal-spending profile across all three groups, it points toward a higher formal marginal expenditure among households at the top of the distribution.

Panel B shows additional evidence in support of Lemma 1. A household at the mean of the pre-reform asset distribution, an additional peso of pension income raises formal expenditure by 0.107 pesos. A one-standard-deviation increase in the asset index raises the formal response by a further 0.096 pesos ($p = 0.010$, one-sided). Informal expenditure moves in the opposite direction: at the mean of the asset distribution, the response is 0.055 pesos, while a one-standard-deviation increase in assets reduces this response by 0.034 pesos. Thus, as household resources increase, the composition of the marginal peso shifts away from informal toward formal establishments. While this specification summarizes the gradients using a linear approximation Table B.6 relaxes this restriction by allowing the marginal response to vary across asset-index quartiles. The formal expenditure coefficients rise monotonically from 0.036 in the lowest quartile to 0.092, 0.137, and 0.212 in the subsequent quartiles, and the difference between the top and bottom quartiles is statistically significant under a one-sided test ($p = 0.074$). The quartile estimates therefore support the increasing profile without imposing linearity.

Importantly, the increasing formal response is not driven by higher-resource households having a

⁴⁹We estimate

$$y_{hst} = \beta \text{Pension}_{hst} + \phi \text{Pension}_{hst} \times A_h + \delta \text{NEligible}_h + \delta_A \text{NEligible}_h \times A_h + \psi A_h + \gamma' X_{ht} + \lambda_{st} + \eta_t A_h + \rho_s A_h + \varepsilon_{hst},$$

where A_h denotes the asset index, standardized relative to its pre-reform distribution. We instrument Pension_{hst} and $\text{Pension}_{hst} \times A_h$ with $\text{NEligible}_h \times \text{Post}_t$ and $\text{NEligible}_h \times \text{Post}_t \times A_h$, respectively. The terms $\eta_t A_h$ and $\rho_s A_h$ allow the relationship between the asset index and expenditure to vary flexibly by year and state, while λ_{st} denotes state-by-year fixed effects. The coefficient β measures the marginal expenditure response for a household at the mean of the pre-reform asset distribution, and ϕ measures how this response changes with a one-standard-deviation increase in pre-reform asset position.

larger overall marginal propensity to consume. In Panel A, Columns (3) and (4) show fairly similar marginal responses of classifiable and total expenditure across socioeconomic groups. Panel B shows that households with higher asset indexes are not significantly more likely to show higher classifiable total consumption, , while the corresponding relationship for total expenditure is only marginally significant at the 10% level. Moreover, the magnitude of the interaction coefficient is substantially larger for formal spending: a one-standard-deviation increase in the asset index raises the formal expenditure response by about 90% relative to its value at the mean of the asset distribution, compared with approximately 39% for classifiable expenditure and 56% for total expenditure.⁵⁰

Table 2: Marginal Expenditure Response

	Formal (1)	Informal (2)	Classifiable (3)	Total (4)
<i>Panel A: IV by Socioeconomic Group</i>				
Low income	0.136*** (0.046)	0.087*** (0.033)	0.219*** (0.060)	0.283*** (0.100)
Middle income	0.104** (0.042)	0.051** (0.021)	0.164*** (0.049)	0.341*** (0.092)
High income	0.169** (0.081)	0.037 (0.027)	0.196** (0.088)	0.255 (0.174)
$p(\text{High} > \text{Low}), 1\text{-tail}$	0.363	0.876	0.588	0.556
First-stage F	1264.1	1264.1	1264.1	1264.2
Observations	94,686	94,686	94,686	94,688
<i>Panel B: IV by Continuous Asset Index</i>				
Pension	0.105*** (0.030)	0.052*** (0.015)	0.159*** (0.034)	0.250*** (0.065)
Pension $\times$ Asset Index	0.090** (0.042)	-0.032** (0.016)	0.059 (0.045)	0.119 (0.090)
$p(\text{interaction} > 0), 1\text{-tail}$	0.015	0.978	0.095	0.092
First-stage F	2984.5	2984.5	2984.5	2985.7
Observations	94,683	94,683	94,683	94,685

Notes: This table reports two-stage least squares estimates of marginal expenditure per peso of pension income. The unit of observation is the household. Expenditure and pension income are monthly household totals, so the coefficients give pesos of expenditure per peso of monthly household income from the non-contributory pension. Column (1) reports expenditure at formal establishments, column (2) expenditure at informal establishments, column (3) total expenditure on the same set of goods for which place of purchase can be classified (see Table B.19), and column (4) total monetary expenditure across all categories. All expenditure and pension income measures are expressed in 2018 MXN. Panel A estimates Equation (25) with pension income interacted with indicators for the three socioeconomic groups, so that the three coefficients come from a single regression and each reports the marginal expenditure response within a group. Panel B estimates Equation (49), interacting pension income with the continuous standardized pre-reform asset index and instrumenting pension income and its interaction with eligibility and eligibility interacted with the asset index. Regressions in Panel A include socioeconomic group by state, socioeconomic group by year, and state by year fixed effects. Regressions in Panel B include state by year fixed effects and allow the coefficient on the asset index to vary by state and year. Both panels include household controls. $p(\text{High} > \text{Low})$ and $p(\text{interaction} > 0)$ report one-sided tests in the indicated direction. The first-stage F -statistic is the Kleibergen–Paap Wald statistic. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A potential measurement concern is that the subset of expenditures for which ENIGH records place of purchase may represent different fractions of the consumption basket across the income distribution. In particular, a positive association between marginal formal consumption and socioeconomic status

⁵⁰We interpret these coefficients as marginal expenditure responses within the ENIGH measurement window rather than as structural lifetime marginal propensities to consume. The survey need not capture the full intertemporal adjustment to a permanent income increase, particularly when expenditure on durables or other lumpy purchases occurs outside the reporting window.

could arise if the survey captures a larger fraction of the relevant consumption of richer households. This does not appear to be the case in our setting. The classifiable basket is heavily concentrated in food and beverages, a necessity that accounts for a larger share of expenditure among poorer households.⁵¹ Thus, if anything, the products for which we observe place of purchase are relatively more important in the budgets of poorer households, rather than richer ones.

Finally, the gradient is unlikely to be generated by an extensive-margin access mechanism in which the transfer allows households to cross a fixed cost of shopping formally, as modeled in [Ramos-Menchelli and Sverdlin-Lisker \(2024\)](#); [Schwartzman \(2026\)](#). Before universalization, 98% of treated households and 95% of low-income treated households already reported some expenditure in a formal establishment. Moreover, [Figure B.19](#) shows no response in the probability of making any formal purchase after the reform: all estimated effects are below 0.5 percentage points and none are statistically different from zero. The observed response therefore operates almost entirely along the intensive margin, consistent with an increase in the formal content of marginal expenditure rather than an access threshold.

Taken together, the evidence in this section is consistent with the demand specification in our model: the positive correlation between formal expenditure and household resources comes from individual preferences that generate increasing marginal formal expenditures ([Lemma 1](#)). On average, additional resources are disproportionately spent at formal establishments, and the formal marginal expenditure increases with household resources. While this section focuses on household-level responses to the pension, given the scale of the pension expansion, these household-level responses are likely to have shifted the composition of aggregate demand toward the formal sector. The remainder of the paper therefore traces this demand shift from households to firms and local labor markets. We first test whether greater exposure to the pension expansion caused a reallocation of employment toward the formal sector, and then quantify the aggregate productivity gains generated by this reallocation.

6 Effect on labor reallocation in the local economy

In this section, we assess the impact of the universal pension on local economies. As documented in the previous section, the pension increased beneficiaries' consumption, with the additional spending disproportionately directed toward the formal sector. The framework in [Section 3](#) shows how these individual-level responses can shift the composition of local demand and reallocate employment between formal and informal firms. Guided by [Propositions 1 and 2](#) of the model, we first estimate the pension's effects on formal and informal employment across municipalities using both firm-level and municipality-level regressions. We then quantify the productivity implications of this reallocation following [Proposition 3](#) of the model.

⁵¹The share of total expenditure accounted for by classifiable goods is negatively correlated with both socioeconomic status and the asset index, with correlations of -0.21 and -0.26 , respectively.

6.1 Empirical design

6.1.1 Construction of the treatment variable

We begin by describing our identification strategy. As discussed in Section 4, the incoming government announced in December 2018 that non-contributory pensions would be universalized starting in January 2019, with the new transfers financed entirely by the federal government using trust resources and no increase in state or municipal taxation.⁵² From the perspective of a municipality, the reform generated a liquidity shock whose magnitude depended on two factors. First, the local age structure determined the share of residents eligible for the pension and therefore the number of potential beneficiaries. Second, the pre-reform income distribution of the elderly determined the extent to which the reform increased pension income, as lower-income elderly individuals were more likely to have already been receiving a non-contributory pension prior to universalization. Therefore, a municipality with a larger elderly population and a smaller share of elderly residents previously covered by the program experienced larger increases in transfer income following the reform. Following Nakamura and Steinsson (2014), Pennings (2021), and Gerard et al. (2026), we exploit the heterogeneity in transfer disbursements across municipalities to estimate the local economy effects. Specifically, we define a municipality's exposure to the pension as the change in average per capita transfers to the municipality from national non-contributory pension plans between the pre- and post-reform periods:

$$Exposure_m = \frac{1}{5} \sum_{t=2019}^{2023} \left(\frac{\text{Non-contributory pension income}}{\text{population}} \right)_{mt} - \frac{1}{4} \sum_{t=2015}^{2018} \left(\frac{\text{Non-contributory pension income}}{\text{population}} \right)_{mt} \quad (26)$$

Because the reform expanded both pension coverage and benefit generosity, exposure varies across municipalities through two margins: transfers to new beneficiaries and benefit increases for existing recipients. On average, transfers to new beneficiaries account for 63.5% of the increase in municipal income from non-contributory transfers following the reform. Transfers to new beneficiaries also account for approximately 64% of the cross-municipality variation in exposure, while benefit increases for existing recipients account for the remaining 36%.⁵³ Panel (a) of Figure B.22 illustrates this decomposition by showing how the cross-municipality variation in changes in per capita pension transfers can be attributed to transfers to new beneficiaries, and increases in benefits received by individuals who were already beneficiaries before the reform. Panel (b) of Figure B.22 shows that, except at the very bottom of the income distribution, the dispersion in exposure is similar across average municipal income levels.

We then define a municipality (m) as treated if its value of $Exposure_m$ is above the state-level median of $Exposure_m$. This results in a total of 1,046 treated (high-exposed) municipalities and 1,024

⁵²Importantly, the universalization did not coincide with differential exposure to other concurrent policy changes, such as the cancellation of Prospera and the programs that replaced it. The correlation between our treatment variable and the change in Prospera transfers across municipalities is -0.01 .

⁵³The variance of $Exposure_m$ is decomposed into the two components using the identity $Exposure_m = \Delta(B_m P_m) = \Delta B_m P_{m,1} + \Delta P_m B_{m,0}$, where B_m is the beneficiary share in municipality m and P_m is pension income per beneficiary. We assign the share of exposure variation explained by each component as $\text{Cov}(C_{jm}, Exposure_m) / \text{Var}(Exposure_m)$.

(low-exposed) control municipalities. Panel (a) of Figure B.11 shows the geographic distribution of the two groups. Panel (b) plots the evolution of per capita transfers for the two groups. Treated municipalities received slightly higher per capita pension benefits before 2018 and diverged sharply after the reform: treated municipalities received, on average, 2,430 MXN per capita during the post-reform period, compared with 1,470 MXN in control municipalities.⁵⁴

We exploit this variation using a difference-in-differences strategy over the 2016–2023 period. We estimate three complementary specifications, which allow us to establish the relationship between the rise in demand observed in the previous section and changes in local activity. First, we focus on firms operating in sectors where beneficiary consumption increased the most by estimating a firm-level triple-difference design that compares firms in highly and less exposed sectors across treated and control municipalities. Second, we estimate a firm-level specification to assess whether the effects extend to firms in less exposed sectors and to provide an input for estimating productivity gains from improvements in allocative efficiency. Third, we estimate municipality-level regressions that map closely to model-derived equations and allow us to provide several pieces of evidence on our main mechanism.

6.1.2 Triple difference-in-differences design: impact on firms in high-demand sectors

Our first empirical specification consists of a triple-difference design, where we exploit the fact that the pension increased household expenditure in specific sectors (hereafter, high-demand sectors). Specifically, we compare firms in high-demand and low-demand sectors across municipalities with high (treated) and low (control) pension exposure, before and after the introduction of the pension. This specification allows us to control for time-varying shocks that differentially affect treated and control municipalities, as well as for time-varying shocks specific to high-demand sectors. We estimate the following specification:

$$y_{j,m,f,t} = \theta^{ddd} (T_{fm} \times \mathbb{1}_{t>2018}) + \mu_j + \delta_{fm} + \delta_{ft} + \delta_{mt} + \varepsilon_{j,m,f,t} \quad (27)$$

where $y_{j,m,f,t}$ denotes the outcome of interest for firm j in municipality m , industry f , and year t . The treatment indicator T_{fm} equals one for firms operating in high-demand sectors located in municipalities with high pension exposure, and zero otherwise. We include firm fixed effects μ_j , municipality $\times$ industry fixed effects δ_{fm} , municipality $\times$ year fixed effects δ_{mt} , and industry $\times$ year fixed effects δ_{ft} . As treatment is defined according to the firm's municipality and whether it belongs to a high- or low-demand sector, standard errors are clustered at the municipality $\times$ high-/low-demand-sector level.

The triple-difference coefficient, θ^{ddd} , captures whether the difference in outcomes between firms in high- and low-demand sectors is larger in treated municipalities than in control municipalities after the reform. Because firms in low-demand sectors may also benefit from spillover effects, θ^{ddd} should be interpreted as the relative effect of the pension on firms in high-demand sectors, rather than as the

⁵⁴Panel (a) of Figure B.21 plots the distribution of the change in per capita transfers for treated and control municipalities. The two distributions overlap, as treatment is defined according to the within-state median, yet the distribution for treated municipalities is significantly shifted to the right.

absolute effect of the pension on these firms.

6.1.3 Firm-level difference-in-differences

We then estimate the impact of the pension on local economies through a difference-in-differences design that compares firms located in treated versus control municipalities, before and after the introduction of the universal pension. We estimate the following specification:

$$y_{j,m,t} = \theta^{dd}(T_m \times \mathbb{1}_{t>2018}) + \mu_j + \nu_m + \Gamma\mathbf{X}_{jt} + \varepsilon_{j,m,t} \quad (28)$$

where $y_{j,m,t}$ denotes the outcome of interest for firm j in municipality m at time t , and T_m is an indicator equal to one for treated municipalities. We include firm fixed effects μ_j and municipality fixed effects ν_m , while $\Gamma\mathbf{X}_{jt}$ consists of state $\times$ year fixed effects and 2-digit NAICS industry $\times$ year fixed effects.⁵⁵ The coefficient θ^{dd} captures the differential change in firm outcomes between high- and low-exposure municipalities after the reform.

As outlined in the theoretical framework in Section 3, non-homothetic demand for formal and informal goods, combined with the reform's coverage of many higher-income older adults, implies heterogeneous effects across firms by formality status, with employment gains concentrated among formal firms. To study this heterogeneity (as well as heterogeneity by estimated pre-reform labor wedges, given the correlation between formality and MRPL), we extend the empirical specification as follows:

$$y_{j,g,m,t} = \theta_{g0}^{dd}(T_m \times \mathbb{1}_{g=0} \times \mathbb{1}_{t>2018}) + \theta_{g1}^{dd}(T_m \times \mathbb{1}_{g=1} \times \mathbb{1}_{t>2018}) + \gamma_{gt} + \mu_j + \nu_m + \Gamma\mathbf{X}_{jt} + \varepsilon_{j,g,m,t} \quad (29)$$

where g indexes the firm group of interest (e.g., whether the firm is formal or informal). We include the same set of fixed effects as in Equation (28). We additionally include group-by-time fixed effects γ_{gt} to allow for differential growth rates between firms in different groups. The coefficients θ_{g0}^{dd} and θ_{g1}^{dd} capture the differential post-reform changes in firm outcomes between treated and control municipalities for firms belonging to groups $g = 0$ and $g = 1$, respectively.

Table B.7 in the appendix summarizes the main characteristics of the sample of approximately 1.4 million firms, dividing them into those belonging to treated and control municipalities. Reassuringly, treated and control firms are similar in total workers, production, and average productivity.⁵⁶ Because treatment is defined relative to the within-state median exposure, Table B.8 additionally shows that there are no significant within-state differences between firms in treated and control municipalities.

⁵⁵A very small proportion of firms change municipality between 2018 and 2023.

⁵⁶We restrict the firm-level analysis to establishments with two or more employees. Single-person establishments lack hired labor, making the marginal revenue product of labor undefined and the intensive margin of employment adjustment absent. Moreover, many single-person establishments reflect underemployment or subsistence self-employment and the formality classification is also less reliable.

6.1.4 Municipality-level difference-in-differences

Finally, we complement the firm-level analysis with a specification at the municipality level, comparing highly exposed (treated) municipalities against less exposed ones (control) before and after universalization in a difference-in-differences design. This step allows us to connect the aggregate results to the theoretical model of Section 3. We estimate the following specification:

$$y_{m,s,t} = \sum_{k \neq 2018} \theta_k T_m \mathbb{1}\{t = k\} + v_m + \lambda_{s,t} + \alpha_{Q_m^{pop},t} + \gamma_{Q_m^{inf},t} + \chi_{Q_m^{firm},t} + \varepsilon_{m,s,t} \quad (30)$$

where $y_{m,s,t}$ is an outcome of interest for municipality m in state s in year t . We include municipality fixed effects v_m , state $\times$ year fixed effects $\lambda_{s,t}$, population-size-quartile $\times$ year fixed effects $\alpha_{Q_m^{pop} \times t}$, informal-share-quartile $\times$ year fixed effects $\gamma_{Q_m^{inf} \times t}$, and number-of-firms-quartile $\times$ year fixed effects $\chi_{Q_m^{firms} \times t}$. The last three sets of fixed effects allow municipalities with different population sizes, numbers of firms, and baseline informality rates to follow different trends. Standard errors are clustered at the municipality level. The coefficients of interest θ_k capture differences in outcomes between treated and control municipalities in each year.⁵⁷

6.2 Descriptive statistics across municipalities

Table B.10 shows that high- and low-treatment municipalities have similar average levels of household income, the formal share of firms and total output, the share of women in the population, the share of non-tradable output, and excess mortality during the COVID-19 period. Treated municipalities have a higher share of elderly residents, which is expected by construction, as the transfer increase is mechanically larger in municipalities where more individuals became eligible for the pension. Notably, treated municipalities are, on average, smaller than control municipalities, as reflected in their lower total population, gross output, number of firms, and number of workers. We account for these differences by including firm-count-quartile $\times$ year fixed effects and population-quartile $\times$ year fixed effects in the estimation. Panel (b) of Figure B.21 shows that the pension represented a quantitatively important income shock: average annual income from the non-contributory pension between 2018 and 2024 represented 4.6% of total municipal household income, on average.⁵⁸

As shown in Equation (18), there are three components that determine the effect on formal employment in a given municipality. The first is the size of the transfer shock relative to the local economy. The second is the composition of the marginal peso generated by the reform. The third is how a change in spending composition maps into a change in employment composition (employment pass-through $\mathcal{H}$). Therefore, for the comparison between treated and control municipalities —from equations (28)

⁵⁷Because municipalities in the comparison group also experienced some increase in transfers, our design should be interpreted as a comparison between high- and low-intensity exposure to the expansion, not between treated and untreated municipalities. The identifying assumption is that, absent the differential increase in transfer intensity, high- and low-exposure municipalities would have followed parallel trends in outcomes. We support this assumption with pre-trend evidence where available and by allowing municipalities with different population sizes and informality rates to follow separate trends.

⁵⁸Among control municipalities, the mean and median share of pension income relative to total household income is 3%.

and (30)— to mainly capture variation in municipalities’ fiscal exposure (dT_m/E_m), rather than systematic variation in the other components of Equation (18), treated and control municipalities should be similar in the observable determinants of $\mathcal{G}_m$ and in their employment pass-through.⁵⁹ Table B.10 shows that the observable determinants of $\mathcal{G}_m$ and $\mathcal{H}_m$ are similar across exposure groups. The pre-universalization formal sales share is very similar across treated and control municipalities (proxy for $\bar{s}$). Additionally, the mean and variance of income per capita are very similar across treated and control group, as is the share of beneficiaries among the elderly is comparable in treated and control municipalities (41% versus 38%). Therefore, it is likely that the formal share of spending out of the marginal peso transferred to treated and control municipalities is similar across both groups. In addition, we compute each municipality’s employment pass-through using its formal shares of labor and sales. A t-test finds no significant difference in the mean employment pass-through between treated and control municipalities ($p > 0.98$).⁶⁰ Taken together, these similarities imply that the post-reform difference in transfers per capita between treated and control municipalities, shown in Panel (b) of Figure B.11 and Figure B.21, provides variation that allows us to recover the first component of equation (18).

6.3 Effects on firms in highly exposed sectors

In this section, we assess the impact of the pension on firms operating in sectors that are highly exposed to the demand shock generated by the universal pension. We classify a firm as belonging to a highly exposed sector if its industry is among those for which household consumption increases significantly following the reform, namely the sectors connected to the following expenditure categories: food, beverage, and tobacco consumption; personal care; cleaning products and services; household care; household furniture and utensils; and transport (see Section 5.2.2).⁶¹ Because these firms produce the goods and services for which demand rises the most, they are expected to be those most directly affected by the increase in purchasing power induced by the pension. On average, firms in highly exposed sectors represent approximately 55% of all firms in a municipality.

We estimate the effects on firms in highly exposed sectors using the triple difference-in-differences approach in Equation (27). As this specification compares firms in high- and low-demand sectors across treated and control municipalities, it allows us to control for shocks that may differentially affect municipalities with different levels of exposure. Figure 5 reports the estimated coefficients from Equation (27), estimated separately for formal and informal firms. Panel (a) shows that formal firms in highly exposed sectors located in highly exposed municipalities experience a 1.3% larger increase in total employment relative to firms in less exposed sectors, compared with the corresponding difference

⁵⁹The net formalization content also incorporates the general-equilibrium wage and price feedback through g_W and $\mathcal{D}$. We do not observe all of these objects municipality by municipality. The balance exercise therefore establishes similarity in observable determinants of the model’s remaining terms rather than equality of $\mathcal{G}_m$ itself.

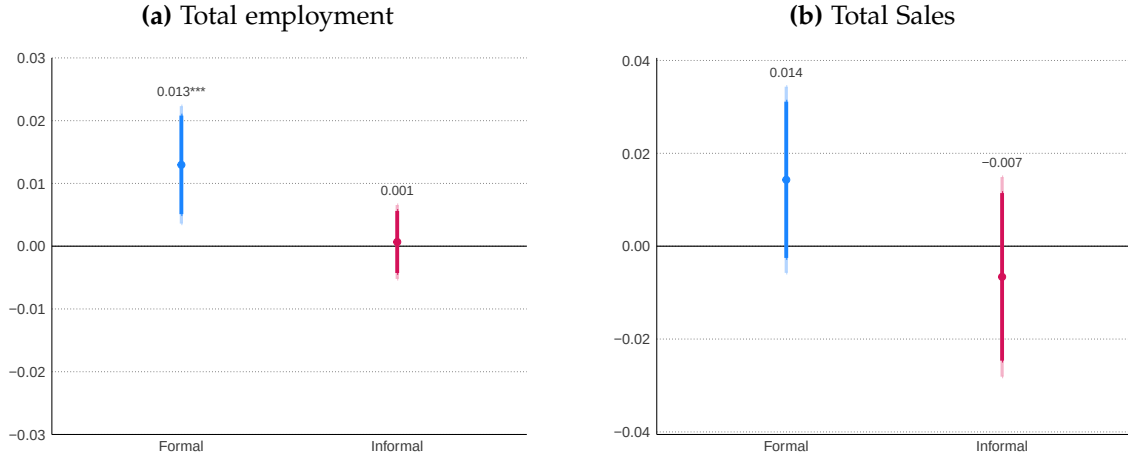
⁶⁰The employment pass-through can be written as

$$\mathcal{H}_m = \frac{\ell_{F,m} (1 - \ell_{F,m})}{\bar{s}_m (1 - \bar{s}_m)}.$$

⁶¹The corresponding highly exposed NAICS sectors for each category are: 722, 461, and 462 (food & beverage consumption); 4631, 8121, 4651, and 8114 (personal care); 485, 811, and 468 (transport); and 466 and 8122 (cleaning, household care, and furniture).

in municipalities with lower pension exposure. In contrast, informal firms in highly exposed sectors exhibit no differential employment growth. Panel (b) reports the same specification using sales as the outcome. Although the estimate is noisier ($p = 0.16$), it has the expected sign and a magnitude similar to the employment effect, with sales increasing by 1.4%.

Figure 5: Triple differences-in-differences results



Notes: This figure plots the estimated θ^{ddd} from the triple difference-in-differences regression defined in (27). The coefficients in black restrict the estimation sample to formal firms, while the coefficients in gray restrict it to informal firms. Highly exposed sectors are defined as those in which beneficiaries increased demand after the reform, i.e. the sectors connected to the following expenditure categories: food, beverage, and tobacco consumption; personal care; cleaning products and services; household care; household furniture and utensils; and transport. Highly exposed municipalities are those with an above-within-state-median increase in pension transfers after the reform. The outcome variables are log total workers (Panel (a)) and log total annual income (Panel (b)). Regressions are estimated on a balanced sample of firms from the 2018 and 2023 Economic Censuses with at least 2 workers. Standard errors are clustered at the municipality $\times$ high-/low-demand-sector level. Wide lines correspond to 90% confidence intervals, while narrow lines correspond to 95% confidence intervals.

Figure B.23 shows that these results hold when, instead of estimating a triple-difference regression, we compare firms in highly exposed sectors located in treated municipalities versus control municipalities by estimating the difference-in-differences specifications (28) and (29) using the subsample of firms operating in highly exposed sectors. Panel (a) shows the results. Among firms in highly exposed sectors, formal firms in highly exposed municipalities experience a 1.3% increase in total employment relative to firms in municipalities with lower pension exposure. Panel (b) shows that the effects on formal firm sales are very similar in magnitude, albeit less precisely estimated. In contrast, we find no significant effects on sales or employment among informal firms in highly exposed municipalities, and the coefficients are very close to zero. Moreover, the coefficients at the bottom of Figure B.23 show that the gains in firm activity are also concentrated among firms with higher wedges at baseline.

Reassuringly, the estimates for formal-sector sales and employment from the triple difference-in-differences regression are almost identical to those from the difference-in-differences regression. This suggests that (i) highly exposed municipalities were not subject to time-varying shocks that affected the difference-in-differences estimates, adding credibility to these estimates, and (ii) spillovers to less exposed sectors in highly exposed municipalities were limited. We assess these spillovers in more detail in the next subsection.

Overall, the results in this section support the demand-driven reallocation toward the formal sector predicted by Proposition 1. The increase in beneficiary consumption documented in Section 5, concentrated in specific goods and services and disproportionately directed toward formal establishments, led to an expansion in aggregate demand in these sectors. This, in turn, increased sales and employment precisely in the sectors toward which the additional consumption was directed.

6.4 Effects on the broader local economy

In this section, we assess whether the universalization of the pension affected firms beyond those operating in highly exposed sectors, and examine its consequences for the allocation of labor within highly exposed municipalities. We first present firm-level and municipality-level results using data from the 2018 and 2023 Economic Censuses, and then complement this analysis with evidence from a large Mexican retailer.

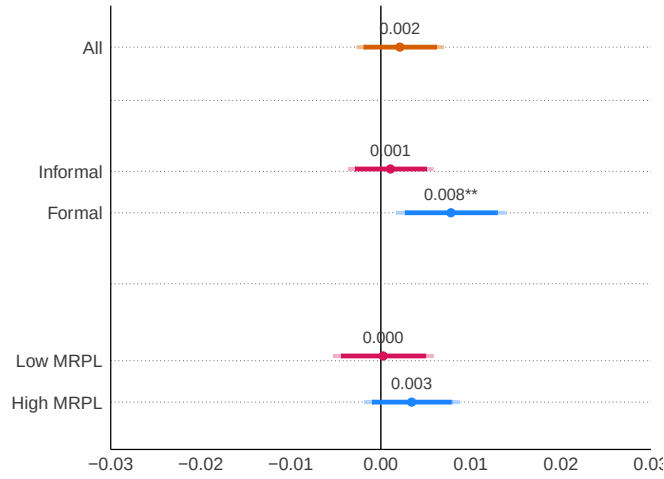
Firm-level regressions. We begin by estimating the effects on firms in highly exposed municipalities using Equation (28) and firm-level microdata from the 2018 and 2023 Economic Censuses. Figure 6 shows the estimates of θ_{dd} from Equation (28) and the estimates of $\{\theta_{g0}^{dd}, \theta_{g1}^{dd}\}$ from Equation (29), where $g \in \{0, 1\}$ corresponds to firms' formality status (informal-formal) and pre-reform estimated labor wedges (high/low MRPL), for different subsamples of firms across sectors. Five years after universalization, formal firms experience a 0.8% increase in employment, while high-MRPL firms experience a 0.3% increase. We find no statistically significant effects for informal firms or firms with below-median MRPL. Figure B.24 Panel (a) shows very similar results for firms in the non-tradable sector, consistent with the finding from the literature that local demand effects are stronger if the rise in demand is focused on local goods and services (Nakamura and Steinsson, 2014). Figure B.24 shows that the estimated effects on firm sales are also positive for formal firms and firms with high pre-reform wedges (high-MRPL) and are similar in magnitude to the corresponding employment effects.

Table B.9 shows that these results are robust to alternative specifications. Column (4) shows that they are robust to dividing firms into high and low productivity according to value added per worker. Columns (5) and (6) show that they are robust to restricting the sample to municipalities in the top and bottom quartiles of pension exposure. Columns (7) and (8) show that they are robust to controlling for firms' pre-reform sales quintiles $\times$ year fixed effects to control for shocks that may have differentially affected firms of different sizes. In Columns (9) and (10), we repeat the main specifications using predicted treatment exposure rather than actual treatment exposure to define treatment. Specifically, for each municipality, we construct predicted treatment exposure as the sum of two components (as a share of total municipality population): (i) the average post-reform non-contributory pension benefit multiplied by the number of new beneficiaries, assuming that all individuals aged 65 or older who were not receiving a non-contributory pension before the reform begin receiving it after the reform; and (ii) the number of pre-reform beneficiaries of non-contributory pensions multiplied by the change in the average pension benefit per beneficiary between the pre- and post-reform periods.⁶² Notably, results are very similar under this specification, with employment increasing 1.3% for formal establishment in

⁶²The first component captures the extensive margin of treatment exposure, while the second captures the intensive margin.

the retail sector, and 0.8% for high-productivity establishments.

Figure 6: Effect on Employment at the Firm Level



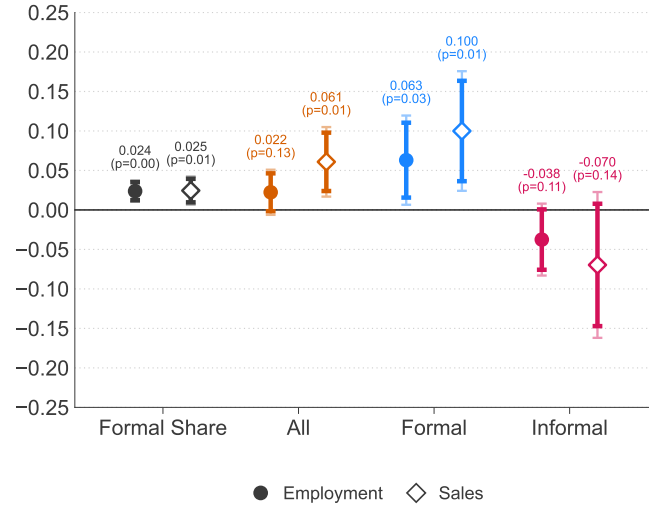
Notes: This figure plots the estimated effects of the pension on log total firm employment of firms in highly exposed municipalities. The ‘All’ coefficient corresponds to the estimated θ^{dd} from Equation 28. The Informal-Formal coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm is classified as formal. The Low-High MRPL coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm’s MRPL was above the 2-digit NAICS sector median in 2018. Regressions are estimated on a balanced sample of firms from the 2018 and 2023 Economic Censuses with at least 2 workers. Standard errors clustered at the municipality level. Wide lines correspond to 90% CI while narrow lines correspond to 95% CI.

Municipality-level regressions.

Figure 7 shows that we observe very similar patterns when estimating the effects on total formal and informal employment at the municipality level with Equation (30).⁶³ Highly treated municipalities experienced an increase in formal employment of 6.1% and in formal output of 10.2%. Informal employment and output moved in the opposite direction, although these estimates are less precise. As a result, both the formal employment share and the formal output share increased (2.2% and 2.3% respectively). Total output increased by 6.5%, while total employment changed little (2.2%, $p = 0.15$). Thus, most of the increase in formal employment reflects a reallocation of workers across sectors rather than an increase in total local employment. The combination of higher output and little change in total employment provides a first indication that this reallocation raised aggregate productivity. We quantify the productivity value of this reallocation below.

⁶³We use the balanced panel for two reasons. First, it holds the composition of firms fixed. Second, it is closest to our model, in which reallocation operates through changes in the employment of incumbent firms. Panel (a) in Figure B.25 reports the corresponding results allowing for entry and exit, with similar estimates. Figure B.26 shows why: we find no significant effect on the total number of firms or within size categories. The only coefficient close to conventional significance levels is the number of formal firms, which could reflect entry in response to higher demand for formal products.

Figure 7: Effect on Employment and Output at the Municipal Level



Notes: This figure plots the coefficient θ_{2023} from Equation (30) and 90% (dark) and 95% (light) confidence intervals, five years after universalization, using data from the 2018 and 2023 Economic Census. Filled circles correspond to employment and hollow diamonds to sales. The first group of estimates uses the formal share of employment and output as the dependent variable, in levels; the remaining groups use the log of total, formal, and informal employment and sales. A firm is formal if it reports being registered with the fiscal authority, and we aggregate by formality status at the municipality level. Treatment is an indicator for municipalities above the median change in pension transfers per capita between 2018 and 2023, and 2018 is the reference year. All specifications include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Evidence from retailer microdata. We complement the Census results using sales data from one of the largest formal retailers in Mexico, with a presence in more than 700 municipalities. The data provide an independent measure of formal demand and allow us to separate changes in sales into quantities and prices. They also allow us to verify that treated and control municipalities followed similar trends before the reform and to trace the response over time. Our main specification uses a balanced panel of stores that opened before universalization and remained open through 2023. We measure treatment using the predicted change in cash transfers, as in Columns (9) and (10) of Table B.9, rather than the realized increase used in the Census analysis.⁶⁴

Figure 8 shows the results using Equation (30), with 2018, the year before universalization, as the reference year.⁶⁵ Panel (a) reports the event-study estimates for sales across all products,⁶⁶ while Panel

⁶⁴During the transition years, realized transfers reflect each municipality's take-up, and the full realized change, once take-up approaches 100% in 2022 (see Table B.1), is only observed at the end of the panel. Assigning this change to earlier outcomes would therefore incorporate future information. This issue does not arise in the Census analysis: outcomes are observed only in 2018 and 2023, the same window over which the realized change is measured, so the treatment is fully determined when the post-reform outcome is observed. The correlation between the two treatment definitions is $\rho = 0.87$.

⁶⁵The retail data are at the municipality-by-month level. We therefore augment the fixed effects in Equation (30) in two ways. We include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality, and replace the interacted year fixed effects with their month-level counterparts, so that state-by-month and quartile-by-month fixed effects absorb monthly shocks that affect states and baseline municipality groups differentially.

⁶⁶We exclude financial services such as deposits, withdrawals, and online gift cards. In developing countries, retail stores serve not only as places to purchase food and beverages but also as banking correspondents where households carry out financial transactions (Jack and Suri, 2014).

(c) restricts the outcome to the highly exposed product categories identified in Section 5.2 that are sold by the retailer.⁶⁷ Panels (b) and (d) provide a complementary descriptive view of the same comparison. They plot mean sales for high- and low-exposure municipalities after residualizing out state-by-year fixed effects and normalizing both groups to 100 in 2018.

The event-study estimates show no evidence of differential pre-trends. After universalization, treated municipalities experience an increase of 4% in sales across all products and 3.7% in sales of highly exposed products, consistent with the increase in formal spending documented at the household level and with the sales response in the Economic Census. The residualized means show the same pattern descriptively: high- and low-exposure municipalities follow similar paths before the reform and diverge afterward, with sales rising more rapidly in treated municipalities. Figure B.27 repeats the analysis without restricting the sample to a balanced panel of stores, with similar effects of approximately 4% for sales in both all products and highly exposed categories.⁶⁸

The annual frequency also allows us to trace the dynamics of the response. The effect increases during the first years after universalization and declines afterward. Since the Economic Census measures outcomes in 2023, after this decline, the Census estimates capture the effect several years after the initial transfer shock rather than its peak response. Several mechanisms could account for the decline, including the gradual incorporation of the transfer into households' intertemporal budgets as new cohorts become eligible, as well as increased competition in response to the initial demand shock.

We subject the Census and retail results to a series of robustness checks in Tables B.11 and B.12. The estimates retain the expected sign across specifications and most remain statistically significant. The results are robust to restricting the comparison to the top and bottom quartiles of the treatment distribution (column 2); using the change in per capita transfers, scaled per \$1,000, as a continuous treatment variable (column 3); weighting municipalities by population (column 4); reweighting low-treatment municipalities through entropy balancing (Hainmueller, 2012) to match high-treatment municipalities on pre-reform population, number of firms and gross production (column 5);⁶⁹ controlling for quartiles of COVID-19 excess mortality interacted with year (column 6)⁷⁰; and including only municipality and year fixed effects, without the additional interacted fixed effects (column 7).

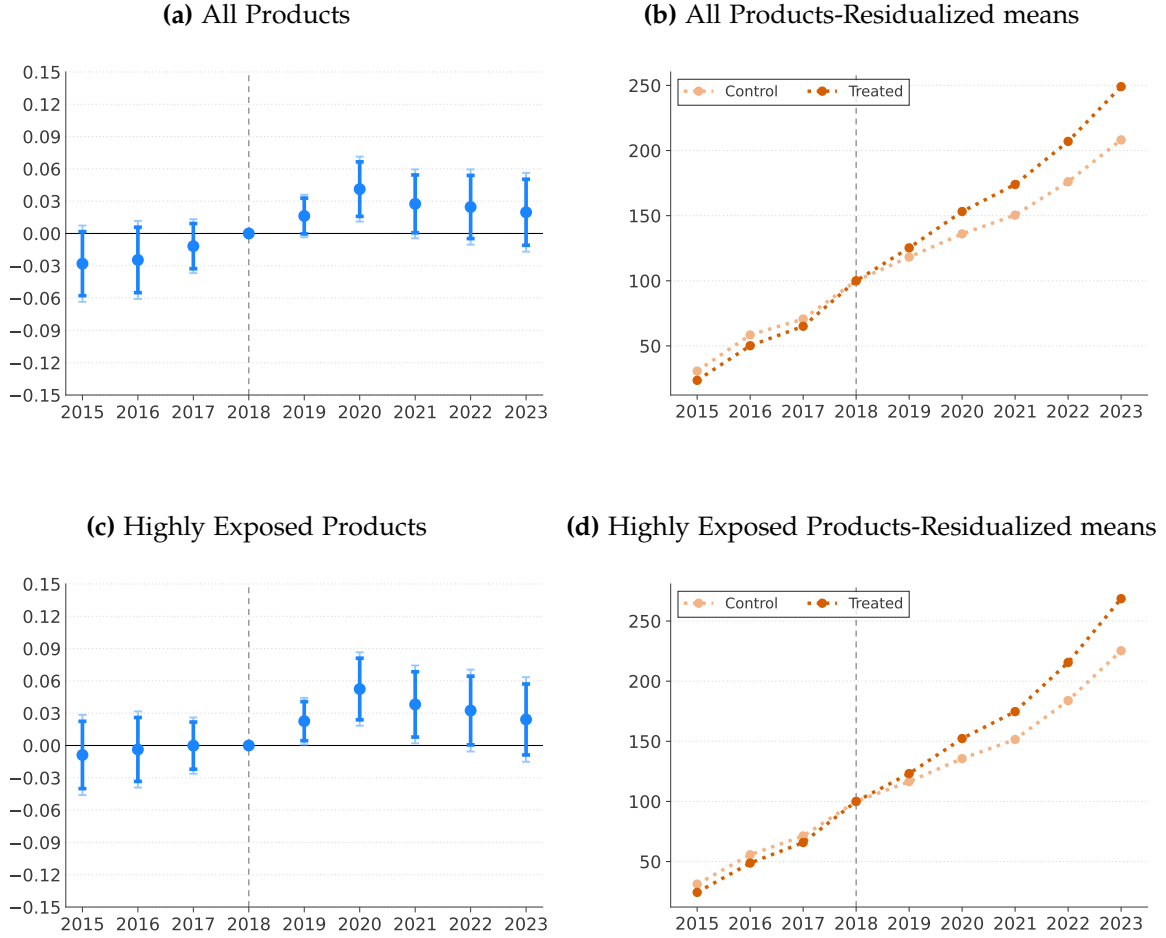
⁶⁷These include products from the food, personal-expense, and cleaning-product categories. Table B.22 reports the exposure classification and provides product examples.

⁶⁸Table B.14 reports estimates using the realized change in transfers between 2018 and 2019 rather than the predicted change. This is the closest analogue to the realized-change treatment used in the Census that avoids assigning future transfer realizations to earlier outcomes. The results are again similar.

⁶⁹These are the characteristics with the largest differences between the two treatment groups before reweighting.

⁷⁰We measure excess mortality using death registers from INEGI. For each municipality, we estimate a linear trend in registered deaths over the pre-pandemic period 2014-2019 and project for the years forward to obtain expected deaths. Excess mortality is the difference between registered and expected deaths. Figure B.28 provides additional evidence that differential exposure to COVID-19 is unlikely to account for our results. We classify municipalities according to whether they experienced high or low excess mortality during the pandemic and estimate the post-reform differential in economic activity between the two groups. Panel (a) reports the differences in total, formal, and informal employment and sales using the Economic Census, while Panel (b) reports the corresponding differences in retailer sales. In both cases, we find no differential change associated with higher excess mortality, suggesting that cross-municipality differences in pandemic severity are unlikely to drive the estimated effects of pension exposure.

Figure 8: Effect on Retail Sales



Notes: This figure plots the coefficients θ_t from Equation (30) for log sales in panels (a) and (c), estimated on the balanced panel of stores of the retail chain with 2018 as the reference year; the dashed line marks the start of universalization. Panels (b) and (d) plot mean sales by treatment status, residualized with respect to state-by-year fixed effects and normalized to 100 in 2018. Panels (a) and (b) cover all products, and panels (c) and (d) the highly exposed product categories from Section 5.2.2; Table B.22 reports the classification and product examples. Treatment is an indicator for municipalities above the within-state median change in predicted pension transfers per capita. The event-study specifications include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality, state-by-month fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with month. Standard errors are clustered at the municipality level; bars show 90% (dark) and 95% (light) confidence intervals.

Finally, Table B.14 sheds light on the role of local price adjustment response. We first decompose the change in sales into quantities and prices. In the baseline specification, sales across all products increase by 4.6%, while quantities sold increase by 4.3%, thus the quantity response closely tracks the sales response. We then examine the price response directly.⁷¹ Price effects are substantially smaller. In the baseline median-split specification, prices increase by only 0.6%. The estimates become somewhat larger when treatment intensity increases: comparing the top and bottom treatment quartiles implies a price increase of 1.5%, while the continuous-treatment specification implies an increase of 1.4%. These responses remain considerably smaller than the corresponding increases in quantities, indicating that

⁷¹We construct product-level unit prices as sales divided by quantities sold and aggregate them using sales weights.

the demand shock was absorbed primarily through higher real sales, with a more modest adjustment in local prices. A positive price response is consistent with our model, in which local prices adjust to the increase in demand (see Equation (A.25)), and with existing evidence that local retail prices respond to local demand conditions (Stroebel and Vavra, 2019; Beraja et al., 2019; DellaVigna and Gentzkow, 2019).⁷²

Overall, the results are consistent with the demand-induced expansion of the formal sector described in Proposition 1. The observed reallocation toward formal activity is consistent with non-homothetic demand and positive average net formalization content $\mathcal{G}$ of the pension. In the model, this occurs when recipients are sufficiently high in the income distribution that their average marginal propensity to spend formally exceeds the average formal share of spending in the municipality. In the next section, we exploit variation in recipient composition across municipalities to provide further evidence for this mechanism.

6.5 Heterogeneity by formalization content of demand

In this section, we conduct two main heterogeneity analyses that the observed reallocation toward the formal sector is driven by non-homotheticities in the formalization content of demand. To do so, we turn to the second component of the decomposition of the effect on formal sector reallocation in Equation (18): the net formalization content ($\mathcal{G}$), which reflects how much of recipients' expenditure is directed toward the formal sector (as opposed to the informal sector). The increase in the formal employment share documented in the previous subsection implies that this net formalization content is positive on average. The model further predicts that $\mathcal{G}$ varies with recipient composition: all else equal, formalization is greater when recipients' marginal propensity to spend formally is higher relative to the municipality's average formal expenditure share. We now assess whether the heterogeneity in formalization responses across municipalities align with this prediction. The first analysis shows that reallocation toward the formal sector is greater when new recipients rank higher in their municipality's income distribution. The second shows that formalization is stronger in municipalities with lower pre-reform pension coverage, where greater exposure to the reform primarily reflected the extension of benefits to new high-income recipients rather than higher payments to existing lower-income beneficiaries.

Income of potential recipients. First, we rely on Proposition 2 which predicts that conditional on fiscal exposure, reallocation toward the formal sector is greater when the formal share of beneficiaries' marginal spending is higher relative to the municipality's average formal expenditure share.⁷³

$$g_{R,m} = \bar{\sigma}_{R,m} - \bar{s}_m, \quad (31)$$

⁷²Part of the measured price response could reflect within-category composition if households respond to the transfer by purchasing more expensive products rather than only larger quantities of the same products.

⁷³The full net formalization content $\mathcal{G}_m$ also incorporates the wage and price feedback summarized by g_W , $\mathcal{D}$, and the remaining equilibrium terms. We focus here on g_R because it is the component directly determined by the allocation of the fiscal transfer across households and therefore admits a transparent empirical counterpart.

We assess this prediction by estimating heterogeneity by the marginal formal consumption of beneficiaries relative to the rest of the municipality’s population. While do not observe $\bar{\sigma}_{R,m}$ directly, both the model and empirical evidence in Section 5.2.3 show that the marginal propensity to spend formally increases with resources. Therefore, municipalities where prospective recipients are richer relative to the population municipality should display a higher $g_{R,m}$, and hence a larger reallocation towards the formal sector post-reform.

Our baseline proxy for variation in $g_{R,m}$ is therefore new beneficiaries’ position within the local economic distribution. For this, we construct a household asset index using individual-level microdata from the 2015 Population Census (Ruggles et al., 2025). We rely on assets rather than labor income because the latter is a particularly noisy measure of economic resources for households with 65+ members, with approximately 50% reporting no labor income.⁷⁴ We then calculate each household’s percentile rank in the asset index distribution of their municipality and then average these ranks among households with a member aged 65 or older that did not receive government transfers before the reform.⁷⁵ A higher value of this measure indicates that likely new recipients were economically better off relative to other households in the same municipality. We divide municipalities into terciles according to the average asset index rank of prospective recipients and estimate the following specification:

$$y_{m,t} = \sum_{g=1}^3 \sum_{k \neq 2018} \theta_{g,k} \mathbb{1}\{G_m = g\} T_m \mathbb{1}\{t = k\} + \lambda_{g,t} + \nu_m + \lambda_{s,t} + \alpha_{Q_m^{pop},t} + \gamma_{Q_m^{inf},t} + \chi_{Q_m^{firm},t} + \varepsilon_{m,t}, \quad (32)$$

where G_m denotes the tercile of prospective recipients’ average asset index and $\lambda_{g,t}$ are tercile-by-year fixed effects. Each $\theta_{g,2023}$ therefore compares high- and low-treatment municipalities within the same recipient-resource tercile, net of tercile-specific aggregate trends. Importantly, the pre-reform share of potential recipients is similar across the three terciles, at 42%, 40%, and 41%. The difference in the increase in transfers per capita between treated and control municipalities is slightly higher for the lower tercile: 1082, 961, and 847 MXN for each tercile respectively. Therefore, if anything this would dampen the reallocation towards the formal sector for among municipalities with rich new recipients.

Panel (a) from Figure 9 reports the results. The treatment effect rises with the pre-reform resources of prospective recipients. In municipalities in the bottom tercile, the effect on the formal employment share is 0.7 percentage points and is not statistically significant. The effects increase in the middle tercile to 2 percentage points, and in the highest tercile to 3.7 percentage points. Panel (a) in Figure

⁷⁴The asset index includes 33 household assets. We extract the first principal component, with the corresponding loadings reported in Table B.21, and standardize the resulting index using the national household distribution. The index is negatively related to whether any household member experienced hunger during the previous month, which falls from 62% of households in the first decile to 42% in the fifth and 8% in the tenth. The index also has a correlation of 0.55 with household income among households reporting positive income. Before universalization, households with members aged 65 or older that did not receive targeted social programs were substantially better off than those already receiving them and somewhat better off than the average household, with average standardized asset indices of 0.34, -0.40, and 0.013, respectively.

⁷⁵The municipal-level correlation between the share of households aged 65 or older receiving a social program in the Census and the corresponding measure from our administrative data in 2018 is 0.55.

B.29 in the Appendix shows very similar patterns when the outcome is the formal share of sales. These results are consistent with Proposition 2: when recipients sit higher in the local resource distribution, and hence spend a larger share of their consumption formally than the average household in their municipality, the transfer generates a stronger formality shift in the composition of aggregate demand and a stronger reallocation of labor towards the formal sector.

Universalization vs. deepening. We next assess whether formalization varies with the composition of transfer recipients, drawing on Corollary 2. Starting from a means-tested program, the corollary predicts that extending coverage to higher-income households shifts demand toward the formal sector more than increasing benefits for existing low-income recipients. The pension reform combined both margins: it increased benefits for existing recipients (the *deepening margin*) and extended coverage to previously ineligible individuals aged 65 and above (the *universalization margin*). We therefore exploit the fact that for different municipalities these margins were differentially important depending on their share of elderly that were already recipients pre reform to assess whether, as the model predicts, formalization is stronger when the universalization margin is stronger.

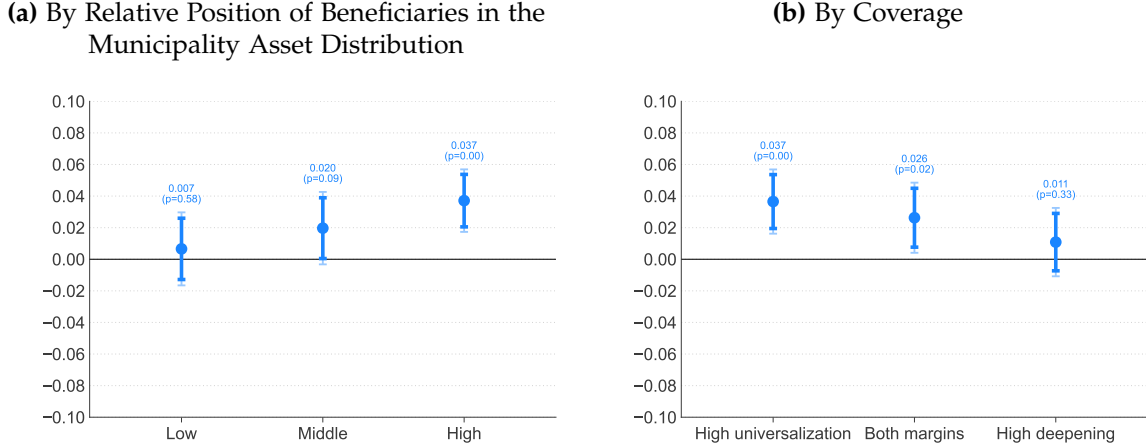
Specifically, we divide municipalities into terciles based on the pre-reform share of individuals aged 65 and older receiving a non-contributory pension. In the bottom tercile, where initial coverage was low, a larger share of the post-reform increase in transfers reflects the expansion of coverage to new, higher-income, individuals of age 65 and above. We therefore expect a larger reallocation toward the formal sector in these municipalities. In the top tercile, the increase operates relatively more through higher benefits to individuals already receiving the pension. As pre-reform recipients are mostly lower income households, we consequently expect less reallocation toward formal activity among these municipalities. We assess how the reform's effects on formalization vary across terciles of this measure by estimating Equation (32). The treated-control difference in the increase in transfers per capita is slightly larger in the top tercile. Thus, differences in transfer magnitude would, if anything, work against finding greater formal-sector reallocation in municipalities with low pre-reform coverage⁷⁶

Importantly, this measure captures a different dimension of recipient composition from our previous measure of prospective beneficiaries' position within the local resource distribution. The two are only weakly correlated: the correlation between the continuous measures is $\rho = -0.06$, while the correlation between their tercile classifications is $\rho = -0.03$. Thus, the extent to which the reform operated through broader coverage is largely independent of where the newly covered households sit relative to other households in their municipality.⁷⁷

⁷⁶Municipalities in which the transfer expansion operated primarily through broader coverage (universalization) experienced an average increase in pension transfers of approximately MXN 800 per capita, compared with MXN 870 in municipalities where both coverage and benefit increases contributed substantially, and MXN 1,080 in municipalities where the expansion operated primarily through higher benefits to existing recipients (deepening).

⁷⁷The distinction arises because low pre-reform pension coverage does not mechanically imply that prospective new beneficiaries are relatively well off within their municipality. For example, a municipality may have few elderly residents receiving the pension before the reform, so that universalization generates a large expansion in coverage, while the newly covered elderly are nevertheless located near or below the median of the municipality's resource distribution. Conversely, the relative position of prospective beneficiaries can be high even in municipalities where a larger share of elderly residents was already covered.

Figure 9: Heterogeneous Effects on Formal Employment



Notes: This figure plots the coefficients $\theta_{g,2023}$ from Equation (32), together with 90% (dark) and 95% (light) confidence intervals. The outcome is the employment formal share, constructed from the balanced panel of firms in the Economic Census. Within each group, municipalities above the median change in pension transfers per capita are compared with municipalities below the median, with 2018 as the reference year. In Panel (a), terciles are constructed from the average percentile rank of households with a member aged 65 or older that did not receive government transfers before the reform within their municipality's asset distribution. The low, middle, and high terciles therefore correspond to municipalities in which prospective beneficiaries were located progressively higher in the local asset distribution. In Panel (b), municipalities are grouped according to the pre-reform share of elderly individuals already receiving a non-contributory pension, which determines the relative importance of the two margins through which universalization increased transfers. "Higher benefits" denotes municipalities where the increase in transfers came relatively more from larger benefits to existing beneficiaries; "Both margins" denotes municipalities where increases in benefits and coverage both contributed; and "Broader coverage" denotes municipalities where the increase came more from the addition of new beneficiaries. All specifications include municipality fixed effects, state-by-year and group-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Panel (b) of Figure 9 presents the results. Consistent with Corollary 2, the increase in the formal share is largest in municipalities where transfer exposure rises primarily through the extension of benefits to previously ineligible households. The formal share increases by 3.7 percentage points in the lowest tercile of pre-reform pension coverage and 2.6 percentage points in the middle tercile, compared with a statistically insignificant 1.1 percentage points in the highest tercile. Panel (b) of Appendix Figure B.29 shows similar patterns for the formal share of sales. Panel (b) of Appendix Figure B.30 shows that this pattern reflects both a larger expansion of formal employment and a sharper contraction of informal employment in municipalities where the universalization margin is more prominent. The negative correlation between formal and informal employment responses also provides further evidence consistent with the expansion of the formal sector drawing workers from informal employment within municipalities.

Taken together, the municipal evidence is consistent with Proposition 2 and maps closely to Equation (18). Municipalities receiving a larger fiscal shock reallocate activity toward the formal sector. For a similar fiscal shock, this reallocation is larger where new recipients are located higher in their local resource distribution, and where the increase in municipal level transfers is driven mostly by the extension of the pension to higher-income households. These patterns suggest that formalization

depends not only on the size of the fiscal injection but also on the net formalization content of the marginal transfer in the municipality. These findings also highlight the importance of targeting for the aggregate effects of cash transfers: both the magnitude of the transfer and the composition of recipients shape the resulting reallocation toward the formal sector. Having established both the direction of the labor reallocation and its relation to the demand-side objects predicted by the model, we next quantify the implications of this reallocation for allocative efficiency.

6.6 Aggregate Productivity Effects

The results presented so far show that the pension caused a reallocation of labor toward formal firms. As formal firms have a higher marginal revenue product of labor than informal firms, this reallocation implies improvements in allocative efficiency in the economy. In this section, we use the structure of the model to quantify the resulting effect of this reallocation on aggregate productivity, through improvements in the allocation of labor across firms. Specifically, we estimate the effect on the Solow residual of treated municipalities using data from the Economic Census and Equation (3) from the model:

$$\Delta Solow_m = \sum_{j \in m} \lambda_j \alpha_j^L \frac{\tau_j}{(1 + \tau_j)} \Delta \log(L_j) \quad (33)$$

where j indexes firms located in treated municipality m . λ_j is the ratio of firm j 's sales to total value added in municipality m , α_j^L is the elasticity of firm output with respect to labor inputs, τ_j is the firm-specific labor wedge, and $\Delta \log(L_j)$ is the change in firm j 's log employment. As discussed in Section 3, this equation highlights the rise in aggregate productivity due to a rise in labor inputs for firms that suffer from high distortions (τ), or decrease for firms with negative wedges.

Estimation of the Solow residual. To estimate (33), we empirically estimate each of its components using data from the Economic Census. We compute λ_j as firm revenue divided by total value added in its municipality. We recover α_j^L from a production-function estimation. We follow [Levinsohn and Petrin \(2003\)](#), allowing the elasticity of firm output with respect to labor to differ across 2-digit industries. The average estimate is 0.56.⁷⁸ To estimate the initial wedges τ , we follow the conventional approach used in [Bau and Matray \(2023\)](#). Specifically, we use the following relationship implied by firms' first-order conditions: $(1 + \tau) = \alpha_j^L \cdot \frac{pY_j}{wL_j}$. We can empirically measure each element on the right-hand side of this expression. As above, α^L is recovered from the production function estimation. pY_j is the firm's annual revenue and wL_j is its annual wage bill, both measured using the 2018 Economic Census.⁷⁹ Panel (a) of Figure B.32 in the Appendix shows the distribution of the estimated $(1 + \tau)$ across formal

⁷⁸Figure B.31 in the Appendix shows the distribution of the estimated α^L across the 2-digit sectors in our sample. Industries that rely more heavily on labor inputs, such as Retail, Finance, and Transport, have higher estimated labor elasticities, while more capital-intensive industries, such as Manufacturing and Utilities, have lower estimated elasticities.

⁷⁹Worker benefits and contributions associated with formal employment are part of τ , as they are only paid by formal firms. Therefore, when constructing firms' wage bills, we use wages excluding social security contributions and other benefits associated with formal-sector employment. For formal firms, we also remove the portion of the wage corresponding to ISR withholdings. Finally, for firms that do not report net wages paid to workers, we impute the average wage using means within fine cells defined by formality status $\times$ municipality $\times$ 2-digit NAICS sector $\times$ firm-size group.

and informal firms. As expected, the distribution of wedges for formal firms is substantially shifted to the right, indicating that these firms face larger distortions.⁸⁰

Finally, we obtain $\Delta \log L_j$ from the establishment-level difference-in-differences estimates reported in Column 2 of Table B.9, allowing the employment response to differ between formal and informal establishments. Since control municipalities are also affected by the pension, this Solow residual estimation should be interpreted as the relative gain in aggregate productivity across treated municipalities relative to control municipalities.⁸¹

Results. Using the empirical estimates of each component of (33), we estimate the average increase in the Solow residual across treated municipalities relative to control municipalities. Table 3 presents the results. In our main specification, the Solow residual increases by approximately 0.32% on average in treated municipalities relative to control municipalities. This corresponds to an approximately 0.6% increase in productivity per USD 100 allocated to a municipality in cash transfers. The median increase in the Solow residual is very close to the mean, while the 25th and 75th percentiles are approximately 0.2% and 0.45%, respectively. In the second row, we restrict the sample to firms in sectors that were highly exposed to the rise in demand from the pension (defined in Section 6.3). As expected, the productivity gains in these sectors are larger: the average increase in the Solow residual is approximately 0.65% on average in treated municipalities, while the 75th percentile reaches 0.95%. Overall, the evidence indicates that the pension-induced reallocation of demand toward the formal sector improved allocative efficiency in treated municipalities, with the largest gains concentrated in the sectors that experienced the greatest increases in demand.

Table 3: Estimated increase in Solow residual (%)

	Mean	Median	p25	p75	Positive gains (%)
All firms	0.32	0.33	0.18	0.45	97.80
Highly exposed sectors	0.65	0.70	0.34	0.95	94.52

Notes: Statistics summarize estimated productivity gains across treated municipalities using Equation (33). Estimates use data from Mexico’s Economic Census for 2018 and 2023. Positive gains reports the percentage of municipalities with strictly positive estimated gains.

These estimates are in line with recent evidence on the productivity gains generated by policies that induced changes in allocative efficiency in low- and middle-income countries.⁸² In Mexico City, [Zárate \(2022\)](#) finds that the expansion of the subway induced a reallocation of employment toward

⁸⁰The average wedge τ is 3.21 among formal firms and 1.06 among informal firms. Almost 40% of informal firms have negative wedges.

⁸¹As $\Delta \log(L_j)$ varies with formality status, the average increase in the Solow residual across treated municipalities can be expressed as $[\Delta \log L]_F \cdot \hat{\tau}_F + [\Delta \log L]_I \cdot \hat{\tau}_I$ where N_T is the number of treated municipalities and $\hat{\tau}$ is a weighted average of $\frac{\tau}{1+\tau}$ across formal and informal firms. The relative interpretation of the change in the Solow residual requires this weighted average to be equal across treated and control municipalities. Empirically, for formal firms, the weighted average is 0.219 in treated municipalities and 0.203 in control municipalities, while for informal firms it is 0.056 and 0.057, respectively.

⁸²Our estimates are quantitatively smaller than the productivity gains estimated by [Bau and Matray \(2023\)](#), who find gains of 16% under the approach we follow above. This difference is likely explained by the fact that their setting examines a policy that *directly* reduced wedges by relaxing capital restrictions, whereas the pension reform affects allocative efficiency only indirectly, through the expansion in formal demand.

the formal sector, with improved allocative efficiency raising output by approximately 0.3%.⁸³ In Vietnam, [McCaig and Pavcnik \(2018\)](#) find that tariff-induced export expansion shifted workers from informal businesses toward formal firms, generating a 2.8% increase in aggregate labor productivity through this reallocation. Similarly, [Chakraborty et al. \(2024\)](#) shows that Chinese import competition in India increased the formal employment share by about 3.7 percentage points and generated aggregate productivity gains ranging from approximately 0.8% to 2.9%.

7 Conclusion

The relationship between the composition of cash transfer beneficiaries and the aggregate effects of these programs has remained difficult to study. Most cash transfer programs are concentrated among low-income households, and data linking household spending responses to firm and local labor-market outcomes are rarely available. Yet beneficiary composition can matter for aggregate outcomes: if households at different points of the income distribution direct their marginal spending toward sectors facing different wedges, the same amount of transfers can generate different effects on labor allocation and aggregate productivity.

Using a theoretical model and evidence from the universalization of Mexico's non-contributory pension, we show that this relationship operates through the composition of demand. Relatively richer households spend a larger share of the marginal peso at formal establishments. Expanding eligibility toward these households therefore shifts demand toward the formal sector, where firms face larger wedges on average. The resulting increase in formal demand reallocates labor toward these firms and raises aggregate productivity. The results show that the aggregate effects of cash transfers depend not only on the size of the program, but also on who receives the marginal extra dollar.

This paper leaves several questions open for future research. One is how to evaluate the trade-off between distributional objectives and allocative efficiency. Targeting transfers toward poorer households places greater weight on redistribution, while extending benefits farther up the income distribution can generate larger productivity gains when richer households direct more of their marginal spending toward highly distorted sectors. Understanding how these forces should be weighed in program design is an important question that we do not address here.

A second question concerns dynamics. On the household side, our model is static and households live for a single period. Allowing households to anticipate future eligibility or smooth the transfer across periods could alter both the magnitude and the composition of the consumption response. On the firm side, entry and exit, transitions between the formal and informal sectors, investment responses, or expectations of future taxes could attenuate or amplify the allocative gains we estimate.

⁸³ [Zárate \(2022\)](#) estimates total output gains of approximately 0.85%, about one-third of which are attributable to improvements in allocative efficiency.

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A Model Appendix

This appendix derives the demand system, the firm relationships, the price index, the equilibrium response to the transfer, and the existence and uniqueness of the equilibrium in the model of Section 3, and proves the results stated there. Equation numbers without the A prefix refer to the main text.

A.1 Household demand

Roy's identity. Indirect utility is given by (4). The marginal utility of expenditure is

$$\frac{\partial V}{\partial e_i} = e_i^{\varepsilon-1} P^{-\varepsilon}. \quad (\text{A.1})$$

Since $P = p_F^\omega p_I^{1-\omega}$, we have $\partial P / \partial p_F = \omega P / p_F$, so

$$\frac{\partial V}{\partial p_F} = \left(\frac{e_i}{P}\right)^{\varepsilon-1} \left(-\frac{e_i}{P^2}\right) \frac{\omega P}{p_F} + \frac{\nu}{p_F} = -\frac{1}{p_F} \left[\omega \left(\frac{e_i}{P}\right)^{\varepsilon} - \nu \right]. \quad (\text{A.2})$$

Roy's identity gives the quantity of the formal good, $c_F^i = -(\partial V / \partial p_F) / (\partial V / \partial e_i)$, so formal spending is

$$D_F^i = p_F c_F^i = \frac{\omega (e_i/P)^{\varepsilon} - \nu}{e_i^{\varepsilon-1} P^{-\varepsilon}} = \omega e_i - \nu P^{\varepsilon} e_i^{1-\varepsilon}. \quad (\text{A.3})$$

Dividing by e_i gives the formal spending share in (5), $s_F(e_i) = \omega - \nu (e_i/P)^{-\varepsilon}$. Informal spending is the rest of the budget, $D_I^i = e_i - D_F^i$.

Nonnegativity of formal spending. Under Assumption 1, the share $s_F(e_i)$ rises with e_i and approaches ω from below as e_i grows. It reaches zero at

$$\underline{e} = P \left(\frac{\nu}{\omega} \right)^{1/\varepsilon}. \quad (\text{A.4})$$

For $e_i < \underline{e}$, the formula in (5) would give a negative share. The unconstrained solution then demands negative formal spending, so the constrained optimum is the corner, and the household spends its whole budget on the informal good with $D_F^i = 0$. Section 3.2 restricts attention to equilibria in which all households have $e_i > \underline{e}$. In this region (5) holds, and all the aggregation and comparative statics below are taken there.

Proof of Lemma 1. For part (i), subtract (5) from (6):

$$\sigma(e_i) - s_F(e_i) = \varepsilon \nu \left(\frac{e_i}{P} \right)^{-\varepsilon} = \varepsilon [\omega - s_F(e_i)], \quad (\text{A.5})$$

where the second equality uses $\omega - s_F(e_i) = \nu (e_i/P)^{-\varepsilon}$ from (5). Under Assumption 1, $s_F(e_i) < \omega$ for every e_i , so the difference is positive. For part (ii), differentiate (6) with respect to e_i ,

$$\sigma'(e_i) = -(1 - \varepsilon) \nu P^{\varepsilon} (-\varepsilon) e_i^{-\varepsilon-1} = \varepsilon(1 - \varepsilon) \nu P^{\varepsilon} e_i^{-\varepsilon-1}, \quad (\text{A.6})$$

which is positive under Assumption 1. □

Remark 1 (Household spending on tradables). In the model, households spend their whole budget on the two local goods, and the tradable good enters the economy only as a production input. Suppose instead that households spend a constant share $1 - \chi$ of every marginal peso on tradables directly, and split the remaining share χ between the two local goods according to (5). Then the direct demand response to the transfer is multiplied by χ , and its formal-informal composition is unchanged. Under this structure, with a leakage share that is constant across households and does not respond to prices, the leakage also weakens the wage feedback, since every round of the loop between spending and wage income leaks a share $1 - \chi$ to tradables. Richer substitution structures between tradables and local goods would let the leakage share respond to prices and to resources, with ambiguous effects on the feedback. The main text sets $\chi = 1$.

A.2 Product-level foundation of the demand system

The two-good demand system in (5) can be generated exactly by aggregation over a continuum of product categories. This foundation connects the model to the measurement of formal spending in Sections 2 and 5, which classifies spending by the place of purchase. Let $n \in [0, 1]$ index product categories, with fixed budget shares θ_n and $\int \theta_n dn = 1$. Let $\lambda_n(e) \in [0, 1]$ be the formal share of spending in category n , the fraction that goes to formal firms, and suppose that it varies with resources,

$$\lambda_n(e) = \omega_n - v_n \left(\frac{e}{p} \right)^{-\varepsilon}, \quad (\text{A.7})$$

where ω_n is the asymptotic formal share of category n . A positive v_n means that as households get richer, a larger fraction of the peso spent on category n goes to formal sellers. Adding across categories, the formal spending share is

$$s_F(e) = \int \lambda_n(e) \theta_n dn = \underbrace{\int \omega_n \theta_n dn}_{\omega} - \left(\frac{e}{p} \right)^{-\varepsilon} \underbrace{\int v_n \theta_n dn}_{\nu}, \quad (\text{A.8})$$

which is (5). So ν is the average of the within-category slopes v_n , weighted by the budget shares, and Assumption 1, which makes the formal good a luxury, requires this average to be positive.

This construction is one way to generate the system and it is the one consistent with the data. Figure B.7 and Table B.3 decompose the rise of the formal consumption share across the expenditure distribution into a within part, where households buy the same products from formal outlets more often, and an across part, where households buy a different mix of products, and show that most of the rise comes from within categories. Figure B.8 estimates (A.7) group by group: the slope v_n is positive in groups that account for 97% of spending, and ν , the average of the slopes weighted by budget shares, is 0.59 with a standard error of 0.01.

In the region where all $\lambda_n(e)$ are between zero and one, the category system collapses exactly to the two-good system in (5). The categories play no role in the equilibrium. We use them only to interpret ν and to connect the model to the data. For households poor enough that some $\lambda_n(e)$ would fall below zero, we work directly with the two-good system.

A.3 Aggregation across households

Integrating (A.3) over households,

$$D_F = \int D_F^i di = \omega E - \nu P^\varepsilon \int e_i^{1-\varepsilon} di. \quad (\text{A.9})$$

Dividing by E and using the definition of $\mathcal{A}$ gives (13). Because $e \mapsto e^{1-\varepsilon}$ is strictly concave and households have unit mass, Jensen's inequality gives $\int e_i^{1-\varepsilon} di \leq E^{1-\varepsilon}$, so $\mathcal{A} \in (0, 1]$, with equality only when all households have the same expenditure. Two identities from (A.9) and (13) are used repeatedly below:

$$\nu P^\varepsilon \int e_i^{1-\varepsilon} di = (\omega - \bar{s})E, \quad \omega - \bar{s} = \nu \mathcal{A} \left(\frac{E}{P} \right)^{-\varepsilon} > 0. \quad (\text{A.10})$$

A.4 Firms

Firm revenue. The first-order conditions in (10) give $l_j = \alpha_s R_j / [(1 + \tau_j)w]$ and $x_j = \beta_s R_j$. Substituting them into the production function (8),

$$R_j = p_s A_j l_j^{\alpha_s} x_j^{\beta_s} = p_s A_j \alpha_s^{\alpha_s} \beta_s^{\beta_s} ((1 + \tau_j)w)^{-\alpha_s} R_j^{\gamma_s}, \quad (\text{A.11})$$

so firm revenue has the closed form

$$R_j = \left[p_s A_j \alpha_s^{\alpha_s} \beta_s^{\beta_s} ((1 + \tau_j)w)^{-\alpha_s} \right]^{\frac{1}{1-\gamma_s}}. \quad (\text{A.12})$$

Lemma 2 (Proportional firm responses within a sector). *Firm employment factors as $l_j = \phi_j \Psi_s(p_s, w)$, where*

$$\phi_j \equiv A_j^{\frac{1}{1-\gamma_s}} (1 + \tau_j)^{-\frac{1-\beta_s}{1-\gamma_s}} \quad (\text{A.13})$$

depends only on the firm's primitives and Ψ_s is common to all firms in sector s . As a result, within-sector employment shares l_j/L_s don't depend on (p_s, w) , and for any marginal change,

$$d \log l_j = \frac{1}{1-\gamma_s} d \log p_s - \frac{1-\beta_s}{1-\gamma_s} d \log w = d \log L_s \quad \forall j \in \mathcal{F}_s. \quad (\text{A.14})$$

Proof. Combining $l_j = \alpha_s R_j / [(1 + \tau_j)w]$ with (A.12),

$$l_j = \alpha_s \left[p_s \alpha_s^{\alpha_s} \beta_s^{\beta_s} \right]^{\frac{1}{1-\gamma_s}} w^{-\frac{1-\beta_s}{1-\gamma_s}} \cdot A_j^{\frac{1}{1-\gamma_s}} (1 + \tau_j)^{-\frac{1-\beta_s}{1-\gamma_s}}. \quad (\text{A.15})$$

The exponent on $(1 + \tau_j)$ collects the direct term and the term coming from revenue, $1 + \alpha_s/(1 - \gamma_s) = (1 - \beta_s)/(1 - \gamma_s)$, and the same holds for w . The first factor, including the constant $\alpha_s [\alpha_s^{\alpha_s} \beta_s^{\beta_s}]^{1/(1-\gamma_s)}$, is $\Psi_s(p_s, w)$, and the second factor is ϕ_j . Since ϕ_j is fixed, $l_j/L_s = \phi_j / \int_{\mathcal{F}_s} \phi_{j'} dj'$ doesn't depend on (p_s, w) . Taking logs and differentiating l_j gives (A.14). The expression is the same for every firm in the sector, so it equals $d \log L_s$. $\square$

Because the employment weights never move, the employment-weighted average wedge $\bar{\tau}_s$ in (11) is a constant of the sector.

Sector employment, imports, and profits. Integrating the labor condition $(1 + \tau_j) w l_j = \alpha_s R_j$ over the sector, and using goods market clearing in value terms, $D_s = \int_{\mathcal{F}_s} R_j dj$, and the definition of $\bar{\tau}_s$ in (11),

$$(1 + \bar{\tau}_s) w L_s = \alpha_s D_s \quad \implies \quad L_s = \kappa_s \frac{D_s}{w}, \quad \kappa_s \equiv \frac{\alpha_s}{1 + \bar{\tau}_s}, \quad (\text{A.16})$$

which is (12). The input condition $x_j = \beta_s R_j$ gives sector imports and profits,

$$X_s = \beta_s D_s, \quad \Pi_s = (1 - \gamma_s) D_s. \quad (\text{A.17})$$

Each peso spent in sector s splits into wages κ_s , wedge payments $\alpha_s \bar{\tau}_s / (1 + \bar{\tau}_s)$, imported inputs β_s , and profits $1 - \gamma_s$. The four shares add up to one. In the model, profits and wedge payments don't enter local household resources. Rebating them locally would add one more round of spending without changing the reallocation identities.

A.5 Prices and the price index

Integrating (A.12) over sector s gives sector supply,

$$D_s = C_s p_s^{\frac{1}{1-\gamma_s}} w^{-\frac{\alpha_s}{1-\gamma_s}}, \quad (\text{A.18})$$

where $C_s > 0$ depends on the joint distribution of (A_j, τ_j) in the sector. Taking logs, differentiating, and solving for the price,

$$d \log p_s = (1 - \gamma_s) d \log D_s + \alpha_s d \log w. \quad (\text{A.19})$$

The sector employment rule (A.16) with fixed κ_s gives $d \log D_s = d \log w + d \log L_s$. Substituting,

$$d \log p_s = (1 - \beta_s) d \log w + (1 - \gamma_s) d \log L_s. \quad (\text{A.20})$$

Since $P = p_F^\omega p_I^{1-\omega}$, and since fixed total labor implies $d \log L_F = d \ell_F / \ell_F$ and $d \log L_I = -d \ell_F / (1 - \ell_F)$,

$$d \log P = \omega d \log p_F + (1 - \omega) d \log p_I = \varphi_w d \log w + \varphi_\ell d \ell_F, \quad (\text{A.21})$$

which is the price index response stated in Section 3.4, with coefficients

$$\varphi_w \equiv \omega(1 - \beta_F) + (1 - \omega)(1 - \beta_I), \quad \varphi_\ell \equiv \frac{\omega(1 - \gamma_F)}{\ell_F} - \frac{(1 - \omega)(1 - \gamma_I)}{1 - \ell_F}. \quad (\text{A.22})$$

Equation (A.20) shows that a larger imported input share β_s lowers the pass-through of the wage into the sector price.

A.6 The equilibrium response to the transfer

Feedback between the formal composition of spending and prices. Key to the definition of the equilibrium is the feedback between the composition of spending and both wages and prices. The wage feedback operates as follows. When the formal spending share rises, the wage bill changes, because an additional peso of aggregate demand in each sector is translated into wages at different rates (i.e. $\kappa_F \neq \kappa_I$). The new wage changes household resources. Since the formal share of the marginal wage peso differs from that of the average peso (by Lemma 1), the change in resources changes the composition of spending, which in turn changes the wage bill again. We define the average marginal propensity to spend formally out of wage income,

$$\bar{\sigma}_W \equiv \frac{\int \sigma(e_i) z_i di}{\bar{L}}, \quad (\text{A.23})$$

which weights households by the labor they supply. The wage feedback works through the corresponding gap for marginal wage income,

$$g_W \equiv \bar{\sigma}_W - \bar{s} - \varepsilon(\omega - \bar{s}) \frac{\varphi_w}{\bar{\kappa}}, \quad (\text{A.24})$$

which measures how much more formal the marginal wage peso is than the average local peso, net of the local inflation that the higher wage creates, as higher local prices lower every household's real resources and shift spending away from the formal good proportionally to the distance $\omega - \bar{s}$. Each peso of extra wage income raises local prices by enough to erode $\varphi_w/\bar{\kappa}$ pesos of real resources, which gives the correction term in (A.24).

The price feedback works through the employment reallocation itself. Moving labor across sectors moves local prices through the price index response above, and the change in real resources moves the formal share again. The equilibrium responses depend on the two channels through

$$\mathcal{D} \equiv (1 - \bar{\kappa}) \Lambda_P - \Delta \kappa g_W, \quad \Lambda_P \equiv 1 + \varepsilon(\omega - \bar{s}) \varphi_\ell \mathcal{H}, \quad \Delta \kappa \equiv \kappa_F - \kappa_I, \quad (\text{A.25})$$

where Λ_P collects the price channel in the form in which it enters the equilibrium system below.

Assumption 3 (Stability). $\mathcal{D} > 0$ at every equilibrium.

The assumption bounds the feedback below one for one. The induced change in the formal spending share from the wage and price feedback is smaller than the original change. Section A.7 derives the slope of the equilibrium map, $\mathcal{B}$, shows that $\mathcal{D} = (1 - \bar{\kappa})(1 - \mathcal{B})$, proves that an equilibrium exists, and shows that under the assumption the equilibrium is unique.

Formal employment share. The sector employment rule (A.16) and labor market clearing (14) give $\ell_F = \kappa_F \bar{s}/\bar{\kappa}(\bar{s})$ and $1 - \ell_F = \kappa_I(1 - \bar{s})/\bar{\kappa}(\bar{s})$. Differentiating the first expression,

$$\frac{d\ell_F}{d\bar{s}} = \frac{\kappa_F \bar{\kappa} - \kappa_F \bar{s} \Delta \kappa}{\bar{\kappa}^2} = \frac{\kappa_F \kappa_I}{\bar{\kappa}^2} = \mathcal{H}, \quad (\text{A.26})$$

using $\bar{\kappa} - \bar{s} \Delta \kappa = \kappa_I$. This is the derivative $\mathcal{H}$ stated in Section 3.4. Multiplying the two share expressions gives the alternative form of the employment pass-through used in Section 3.5.1,

$$\mathcal{H} = \frac{\kappa_F \kappa_I}{\bar{\kappa}^2} = \frac{\ell_F(1 - \ell_F)}{\bar{s}(1 - \bar{s})}. \quad (\text{A.27})$$

Demand composition. Differentiating (A.9),

$$dD_F = \omega dE - \nu P^\varepsilon(1 - \varepsilon) \int e_i^{-\varepsilon} de_i di - \varepsilon \nu P^\varepsilon \int e_i^{1-\varepsilon} di \cdot d \log P. \quad (\text{A.28})$$

By (6), $-\nu P^\varepsilon(1 - \varepsilon) e_i^{-\varepsilon} = \sigma(e_i) - \omega$, so the second term is $\int [\sigma(e_i) - \omega] de_i di$. Using $de_i = z_i dw + dT_i$ from Section 3.1, the definitions in (15) and (A.23), and $\int de_i di = dE$, the first two terms of (A.28) add up to $\bar{\sigma}_R dT + \bar{\sigma}_W \bar{L} dw$. The third term equals $\varepsilon(\bar{s} - \omega)E d \log P$ by (A.10). Then

$$dD_F = \bar{\sigma}_R dT + \bar{\sigma}_W \bar{L} dw - \varepsilon(\omega - \bar{s})E d \log P. \quad (\text{A.29})$$

The last term is the local inflation effect described above. With $dE = dT + \bar{L} dw$ and $E d\bar{s} = dD_F - \bar{s} dE$,

$$E d\bar{s} = (\bar{\sigma}_R - \bar{s}) dT + (\bar{\sigma}_W - \bar{s}) \bar{L} dw - \varepsilon(\omega - \bar{s})E d \log P. \quad (\text{A.30})$$

Now substitute the price index response (A.21). For the wage term, $E d \log w = (E/w) dw = (\bar{L}/\bar{\kappa}) dw$ by (14). For the reallocation term, $d\ell_F = \mathcal{H} d\bar{s}$ by (A.26). Moving this term to the left side gives

$$E \left[1 + \varepsilon(\omega - \bar{s}) \varphi_\ell \mathcal{H} \right] d\bar{s} = (\bar{\sigma}_R - \bar{s}) dT + \left[\bar{\sigma}_W - \bar{s} - \varepsilon(\omega - \bar{s}) \frac{\varphi_w}{\bar{\kappa}} \right] \bar{L} dw, \quad (\text{A.31})$$

which, with the definitions in (15), (A.24), and (A.25), is the demand-composition equation of the system referenced in Section 3.5.

Labor market clearing. Differentiating labor market clearing (14),

$$\bar{L} dw = \bar{\kappa} dE + E \Delta \kappa d\bar{s}, \quad (\text{A.32})$$

and substituting $dE = dT + \bar{L} dw$ gives the wage block of the system:

$$(1 - \bar{\kappa}) \bar{L} dw = \bar{\kappa} dT + E \Delta \kappa d\bar{s}. \quad (\text{A.33})$$

Proof of Proposition 1. Substitute (A.33) into (A.31):

$$E \Lambda_P d\bar{s} = g_R dT + \frac{g_W}{1 - \bar{\kappa}} (\bar{\kappa} dT + E \Delta \kappa d\bar{s}). \quad (\text{A.34})$$

Collecting the $d\bar{s}$ terms,

$$E \left[(1 - \bar{\kappa}) \Lambda_P - \Delta \kappa g_W \right] d\bar{s} = \left[(1 - \bar{\kappa}) g_R + \bar{\kappa} g_W \right] dT. \quad (\text{A.35})$$

Dividing by $\mathcal{D} = (1 - \bar{\kappa}) \Lambda_P - \Delta \kappa g_W$, which is positive under Assumption 3, gives $d\bar{s} = \mathcal{G} dT/E$. For the wage,

substitute this solution back into (A.33):

$$(1 - \bar{\kappa}) \bar{L} \frac{dw}{dT} = \bar{\kappa} + \Delta \kappa \mathcal{G} = \frac{\bar{\kappa} \mathcal{D} + \Delta \kappa [(1 - \bar{\kappa}) g_R + \bar{\kappa} g_W]}{\mathcal{D}} = \frac{(1 - \bar{\kappa}) [\bar{\kappa} \Lambda_P + \Delta \kappa g_R]}{\mathcal{D}}, \quad (\text{A.36})$$

where the last equality expands $\bar{\kappa} \mathcal{D} = \bar{\kappa}(1 - \bar{\kappa}) \Lambda_P - \bar{\kappa} \Delta \kappa g_W$ and cancels the $\bar{\kappa} \Delta \kappa g_W$ terms. Dividing by $(1 - \bar{\kappa}) \bar{L}$ gives the wage response in (16). For employment, $d\ell_F = \mathcal{H} d\bar{s}$ by (A.26), so $dL_F = \bar{L} \mathcal{H} d\bar{s} = \mathcal{H} \mathcal{G} \bar{L} dT/E$ with $\mathcal{H} = \kappa_F \kappa_I / \bar{\kappa}^2$, and $dL_I = -dL_F$ by (1). $\square$

Remark 2 (Stability condition). Every correction term in $\mathcal{D}$ is of order ν . By (A.10), $\omega - \bar{s} = O(\nu)$, so $\Lambda_P = 1 + O(\nu)$. Both $\bar{\sigma}_W$ and $\bar{s}$ equal ω plus a term proportional to ν , by (6) and (13), so $g_W = O(\nu)$. Then $\mathcal{D} = (1 - \bar{\kappa}) + O(\nu)$. Since $\kappa_s < 1$ implies $\bar{\kappa} < 1$, Assumption 3 holds when ν is small enough.

Proof of Corollary 1. With $\nu = 0$, equation (5) gives $s_F(e) = \omega$ for every e , so every household splits every peso in the same proportion. Then $D_F/D_I = \omega/(1 - \omega)$ in any equilibrium, whatever the level and distribution of resources. By (13), $\bar{s} = \omega$. By (6), $\sigma(e) = \omega$ for every e , so $\bar{\sigma}_R = \bar{\sigma}_W = \omega$, and by (15), (A.24), and (A.10), $g_R = g_W = 0$ and $\omega - \bar{s} = 0$.

The result holds for a transfer of any size. By the sector employment rule (A.16), $L_F/L_I = (\kappa_F/\kappa_I)(D_F/D_I) = \kappa_F \omega / [\kappa_I(1 - \omega)]$ is constant. Together with $L_F + L_I = \bar{L}$, this pins down sector employment independently of the transfer. By Lemma 2, within-sector employment shares don't depend on (p_s, w) , so the employment of every firm is unchanged. With every $dl_j = 0$, the first-order change in real local value added is zero by (A.45) in Appendix A.8. The derivation of (A.45) doesn't use this corollary.

For the level responses, clearing (14) with $\bar{s} = \omega$ gives $w = \bar{\kappa}(\omega) E / \bar{L}$, and the resource identity (3) then gives $E = (Y_0 + T)/(1 - \bar{\kappa}(\omega))$ exactly, so E and w rise with T . Prices rise by (A.20) with $d \log L_s = 0$, and imports $M = X_F + X_I = [\omega \beta_F + (1 - \omega) \beta_I] E$ rise with E . $\square$

Proof of Proposition 2. (i) By (15), $\bar{\sigma}_{R,m}$ enters the solution (16) only through $g_R = \bar{\sigma}_{R,m} - \bar{s}_m$. It enters linearly, with coefficient $(1 - \bar{\kappa}_m)$ in the numerator of $\mathcal{G}_m$, and the denominator $\mathcal{D}_m$ doesn't contain g_R . Then $\partial \mathcal{G}_m / \partial \bar{\sigma}_{R,m} = (1 - \bar{\kappa}_m) / \mathcal{D}_m > 0$.

(ii) By (15), the solution (16) depends on $\bar{\sigma}_{R,m}$ only through $g_{R,m}$, so the argument in (i) applies, and the employment response follows from (17). $\square$

Proof of Corollary 2. Write $\bar{\sigma}_R = \int \sigma(e) d\mu(e)$, where μ is the distribution of recipients' pre-transfer resources weighted by transfer pesos, $d\mu(e) = dT_i / dT$ over recipients with resources e . By Lemma 1, σ is strictly increasing in the interior region. If μ^A first-order stochastically dominates μ^B , then $\int \sigma d\mu^A \geq \int \sigma d\mu^B$, with strict inequality when the dominance is strict, which is the standard ranking of expectations of increasing functions under first-order stochastic dominance. $\square$

A.7 Existence and uniqueness of the equilibrium

An equilibrium is given by Definition 1 in the main text. This section reduces the equilibrium to a single equation in the formal spending share. We use this equation to prove existence and to give a condition for uniqueness. The proof of uniqueness repeats the derivations of Appendix A.6.

Candidate prices. We assume $Y_0 + T > 0$, so the candidate expenditure defined below is positive. Fix a conjectured formal spending share $\bar{s} \in [0, 1]$. If households split aggregate spending in this way, labor market clearing (14) and the resource identity (3) pin down expenditure and the wage in closed form,

$$E(\bar{s}) = \frac{Y_0 + T}{1 - \bar{\kappa}(\bar{s})}, \quad w(\bar{s}) = \frac{\bar{\kappa}(\bar{s}) E(\bar{s})}{\bar{L}}, \quad (\text{A.37})$$

with household resources $e_i(\bar{s}) = w(\bar{s})z_i + y_i + T_i$ and sector spending $D_F(\bar{s}) = \bar{s} E(\bar{s})$ and $D_I(\bar{s}) = (1 - \bar{s}) E(\bar{s})$. Inverting the sector supply curve (A.18) gives the price at which each sector supplies exactly the conjectured spending,

$$p_s(\bar{s}) = \left(\frac{D_s(\bar{s})}{C_s} \right)^{1-\gamma_s} w(\bar{s})^{\alpha_s}, \quad s \in \{F, I\}, \quad (\text{A.38})$$

and the price index $P(\bar{s}) = p_F(\bar{s})^\omega p_I(\bar{s})^{1-\omega}$. At the candidate prices, firms are on their supply curves, the labor market clears, and each sector produces the conjectured demand. The supply curve (A.18) is in value terms, and at a common sector price clearing in value is the same as clearing in quantity. The only condition not yet imposed is that households actually choose the conjectured split at these prices and resources.

The share households demand. Let $\Phi(\bar{s})$ denote the formal share that households demand at the candidate prices,

$$\Phi(\bar{s}) \equiv \omega - \nu P(\bar{s})^\varepsilon \frac{\int e_i(\bar{s})^{1-\varepsilon} di}{E(\bar{s})}. \quad (\text{A.39})$$

A conjecture is an equilibrium if and only if the share households demand equals the conjectured share, $\Phi(\bar{s}) = \bar{s}$. When $\Phi(\bar{s}) > \bar{s}$, households want to spend more on the formal good than the candidate output, so there is excess demand for the formal good. Since households spend their whole budget, excess demand for the formal good equals excess supply of the informal good in value. So this single equation clears both goods markets. Define the gap $h(\bar{s}) \equiv \Phi(\bar{s}) - \bar{s}$, so an equilibrium is a zero of h .⁸⁴

Proposition 4 (Existence). *An equilibrium exists, and every equilibrium share lies in $(0, \omega)$.*

Proof. On $[0, 1]$, $\bar{\kappa}(\bar{s})$ lies between κ_F and κ_I , which are both in $(0, 1)$ by the restriction $1 + \bar{\tau}_s > \alpha_s$, so $E(\bar{s})$, $w(\bar{s})$, and $\int e_i(\bar{s})^{1-\varepsilon} di$ are continuous and bounded, and $p_s(\bar{s})$ and $P(\bar{s})$ are continuous. Then Φ is continuous on $[0, 1]$. As $\bar{s} \rightarrow 0$, $D_F(\bar{s}) \rightarrow 0$, so $p_F(\bar{s}) \rightarrow 0$ by (A.38) and $P(\bar{s}) \rightarrow 0$. The correction term in (A.39) vanishes and $\Phi(0) = \omega$. For $\bar{s} \in (0, 1)$ the correction term is positive, so $\Phi(\bar{s}) < \omega$. Then $h(0) = \omega > 0$ and

⁸⁴Equation (A.39) uses the interior demand system, so it is valid when all households have $e_i(\bar{s}) > \underline{e}(\bar{s})$ along the candidate path. If some households were at the corner $D_F^i = 0$, the share households demand would be $\Phi(\bar{s}) = \int \max\{0, \omega e_i - \nu P^\varepsilon e_i^{1-\varepsilon}\} di / E$, which is still continuous, and the existence argument below is unchanged. The corner value follows from the nonnegativity discussion in Appendix A.1.

$h(\omega) = \Phi(\omega) - \omega < 0$, so by the intermediate value theorem h has a zero in $(0, \omega)$, which is an equilibrium. Any equilibrium in $(0, 1)$ satisfies $\bar{s} = \Phi(\bar{s}) < \omega$. The endpoints are not equilibria: the same limit argument gives $\Phi(1) = \omega < 1$, and $\Phi(0) = \omega > 0$. $\square$

If nobody bought the formal good, formal firms would sell at a price near zero. At that price the price index is close to zero, so real resources are high. Then every household wants to buy some formal good, and the formal share cannot be zero in equilibrium.

The slope of the equilibrium map. The share households demand responds to the conjectured share through wages and prices, the two channels described above. Adding them, a rise in the conjectured formal share moves it, at an equilibrium, at the rate

$$\mathcal{B} \equiv \underbrace{\frac{\Delta\kappa g_W}{1 - \bar{\kappa}}}_{\text{wage feedback}} - \underbrace{\varepsilon(\omega - \bar{s}) \varphi_\ell \mathcal{H}}_{\text{price feedback}}, \quad (\text{A.40})$$

and both channels are in the same units, pesos of demanded formal spending per peso of the original shift. Since $\Lambda_P - 1 = \varepsilon(\omega - \bar{s}) \varphi_\ell \mathcal{H}$, the definitions in (A.25) give $\mathcal{D} = (1 - \bar{\kappa})(1 - \mathcal{B})$. Assumption 3 is the condition $\mathcal{B} < 1$, not $|\mathcal{B}| < 1$, so it guarantees a unique crossing rather than convergence of discrete adjustment.

For uniqueness we use the interior demand system (5). As in Section 3.2, we restrict attention to the case in which all households have $e_i > \underline{e}$ at every equilibrium, so Φ is differentiable there.

Proposition 5 (Uniqueness). *At any equilibrium,*

$$\Phi'(\bar{s}) = \mathcal{B}, \quad 1 - \Phi'(\bar{s}) = \frac{\mathcal{D}}{1 - \bar{\kappa}}, \quad (\text{A.41})$$

with $\mathcal{B}$ and $\mathcal{D}$ evaluated at that equilibrium, so their terms g_W , Λ_P , and $\bar{\kappa}$ take the values of the equilibrium at hand. Under Assumption 3, the equilibrium is unique.

Proof. Differentiate Φ along the candidate prices. By construction, $w = \bar{\kappa}E/\bar{L}$ and $\ell_F = \kappa_F \bar{s}/\bar{\kappa}$ hold at every conjecture, so $E d \log w = (\bar{L}/\bar{\kappa}) dw$ and $d\ell_F = \mathcal{H} d\bar{s}$, and the identity $\nu P^\varepsilon \int e_i^{1-\varepsilon} di = (\omega - \Phi)E$ holds by the definition of Φ . Differentiating (A.37) gives $\bar{L} dw = E \Delta\kappa d\bar{s}/(1 - \bar{\kappa})$. Repeating the steps of (A.29) with $dT = 0$, and evaluating the coefficients at an equilibrium, where $\Phi(\bar{s}) = \bar{s}$, gives

$$E d\Phi = g_W \bar{L} dw - (\Lambda_P - 1) E d\bar{s}. \quad (\text{A.42})$$

Substituting the wage response,

$$\Phi'(\bar{s}) = \frac{g_W \Delta\kappa}{1 - \bar{\kappa}} - \Lambda_P + 1, \quad 1 - \Phi'(\bar{s}) = \frac{(1 - \bar{\kappa}) \Lambda_P - \Delta\kappa g_W}{1 - \bar{\kappa}} = \frac{\mathcal{D}}{1 - \bar{\kappa}}, \quad (\text{A.43})$$

The first expression equals $\mathcal{B}$ in (A.40). This gives (A.41). Now suppose there are two equilibria $\bar{s}_1 < \bar{s}_2$. Under Assumption 3, $h'(\bar{s}) = \Phi'(\bar{s}) - 1 < 0$ at both, so there are points a just above $\bar{s}_1$ and b just below $\bar{s}_2$

with $h(a) < 0$ and $h(b) > 0$. The set of zeros of h in $[a, b]$ is nonempty by the intermediate value theorem and closed by continuity. Let $\bar{s}_3$ be its largest element. Then $h > 0$ on $(\bar{s}_3, b]$, since a negative value there would give a zero above $\bar{s}_3$. So $h'(\bar{s}_3) \geq 0$. But $\bar{s}_3$ is an equilibrium, and Assumption 3 gives $h'(\bar{s}_3) < 0$, a contradiction. So the equilibrium is unique. $\square$

A.8 Productivity

Change in real local value added. Real local value added at pre-reform prices is defined in (20). The second equality there follows from $D_s = \int R_j dj$ and $x_j = \beta_s R_j$. For a marginal change, holding pre-reform prices fixed as the weights of real output,

$$dY^L = \sum_s \int_{j \in \mathcal{F}_s} \left[p_s \frac{\partial q_j}{\partial l_j} dl_j + \left(p_s \frac{\partial q_j}{\partial x_j} - 1 \right) dx_j \right] dj. \quad (\text{A.44})$$

The input condition in (10) sets $p_s \partial q_j / \partial x_j = \beta_s R_j / x_j = 1$, so every dx_j term cancels. Additional imported inputs raise gross output but have zero net value at the optimal margin, to first order. What remains is the labor margin, where $p_s \partial q_j / \partial l_j = \alpha_s R_j / l_j = \text{MRPL}_j = (1 + \tau_j)w$:

$$dY^L = \sum_s \int_{j \in \mathcal{F}_s} \text{MRPL}_j dl_j dj = w \sum_s \int_{j \in \mathcal{F}_s} (1 + \tau_j) dl_j dj = w \sum_s \int_{j \in \mathcal{F}_s} \tau_j dl_j dj, \quad (\text{A.45})$$

where the last equality uses fixed total labor, $\sum_s \int dl_j dj = 0$, to cancel the common wage component. The productivity effect comes entirely from moving the fixed local factor across firms with different wedges.

Proof of Proposition 3. For the firm-level formula, use $\text{MRPL}_j = (1 + \tau_j)w$ and $\alpha_s R_j = (1 + \tau_j)w l_j$ to write each term in (A.45) as

$$w \tau_j dl_j = \frac{\tau_j}{1 + \tau_j} (1 + \tau_j) w l_j d \log l_j = \alpha_s R_j \frac{\tau_j}{1 + \tau_j} d \log l_j = Y^L \lambda_j \alpha_s \frac{\tau_j}{1 + \tau_j} d \log l_j, \quad (\text{A.46})$$

using $\lambda_j = R_j / Y^L$. Dividing (A.45) by Y^L gives (21). The Domar weights λ_j don't need to add up to one, because gross sales include imported inputs.

For the sector formula, Lemma 2 gives $dl_j = l_j d \log L_s$ for all $j \in \mathcal{F}_s$, so

$$\int_{j \in \mathcal{F}_s} \tau_j dl_j dj = d \log L_s \int_{j \in \mathcal{F}_s} \tau_j l_j dj = d \log L_s \bar{\tau}_s L_s = \bar{\tau}_s dL_s, \quad (\text{A.47})$$

using (11). Then $dY^L = w(\bar{\tau}_F dL_F + \bar{\tau}_I dL_I)$, and $dL_I = -dL_F$ gives (22). Equivalently, since $\alpha_s D_s = (1 + \bar{\tau}_s) w L_s$ by (A.16), each sector term satisfies $w \bar{\tau}_s dL_s / Y^L = \lambda_s \alpha_s [\bar{\tau}_s / (1 + \bar{\tau}_s)] d \log L_s$ with sector Domar weights $\lambda_s \equiv D_s / Y^L$, so the firm formula collapses to

$$d \log \text{TFP} = \lambda_F \alpha_F \frac{\bar{\tau}_F}{1 + \bar{\tau}_F} d \log L_F + \lambda_I \alpha_I \frac{\bar{\tau}_I}{1 + \bar{\tau}_I} d \log L_I, \quad (\text{A.48})$$

which equals $w(\bar{\tau}_F - \bar{\tau}_I) dL_F / Y^L$. Since $w > 0$, $Y^L > 0$, and the employment pass-through $\kappa_F \kappa_I / \bar{\kappa}^2$ is positive,

under Assumption 2 the sign of $d \log \text{TFP}$ is the sign of $\mathcal{G}$. □

A.9 Notation

$s \in \{F, I\}$	sector	$i; j; n$	household; firm; product category
ε, ν, ω	PIGL curvature, non-homotheticity parameter, asymptotic share	$P = p_F^\omega p_I^{1-\omega}$	local price index
e_i, z_i, y_i, T_i	resources (expenditure), labor, nonlabor income, transfer	E, Y_0, T	municipal aggregates
$s_F(e), \sigma(e)$	formal spending share; marginal propensity to spend formally	$\underline{e}$	minimum resources for positive formal spending
$\theta_n, \omega_n, \nu_n$	category budget share; asymptotic formal share; within-category slope	$\mathcal{A}$	distributional term
$\bar{s}, D_F, D_I$	municipal formal share and spending	$\bar{\sigma}_R, \bar{\sigma}_W$	marginal propensity to spend formally out of the
$A_j, \tau_j, l_j, x_j, R_j$	firm productivity, wedge, inputs, revenue	$\alpha_s, \beta_s, \gamma_s$	elasticities; $\gamma_s = \alpha_s + \beta_s$
$\bar{\tau}_s, \kappa_s = \frac{\alpha_s}{1+\bar{\tau}_s}$	sector wedge; share of a peso paid as local wages	$\bar{\kappa}(\bar{s}), \Delta\kappa$	its average across sectors; $\kappa_F - \kappa_I$
$L_s, \ell_F, \bar{L}$	employment, formal share, labor supply	$\mathcal{H} = \kappa_F \kappa_I / \bar{\kappa}^2$	employment pass-through
φ_w, φ_ℓ	price index coefficients	$\mathcal{B}, \Lambda_P$	slope of the equilibrium map; one plus the price
g_R, g_W	Engel gaps of the transfer and of wage income	$\mathcal{D}, \mathcal{G}$	denominator of the equilibrium responses; net fo
$\Phi(\bar{s}), h(\bar{s})$	formal share households demand; $\Phi(\bar{s}) - \bar{s}$	C_s	sector supply constant
X_s, Π_s, M, Y^L	sector imports, profits; total imports; local value added	λ_j, λ_s	firm and sector Domar weights

B Data Appendix

This appendix documents, for each data source, its source, access form, sampling design and level of representativeness, the sample restrictions we impose, and the construction of key variables. The municipality is the main unit of analysis that links the sources we use. Data across sources are merged using the harmonized municipality codes from INEGI’s *Marco Geoestadístico* (the standard 5 digit state-municipality identifier). All monetary values are deflated to constant 2018 pesos using INEGI’s INPC, and per capita variables use population counts from INEGI’s Population Census and CONAPO municipal population projections.

A. Administrative pension records. Monthly administrative payment records from *Secretaría de Bienestar*, reporting for each municipality and month, the list of beneficiaries and the amounts they received. This is aggregated to have the number of beneficiaries and the total pesos received under the non-contributory pension program. The data is retrieved from the *Padron de Beneficios Unicos* which was made public in 2025.

Our baseline treatment intensity is **the change in per capita transfers** between the pre and post-universalization period. We benchmark every year against 2018, the last full pre-reform year. We treat the two non-contributory pension programs, *65 y Más* (pre-2019) and *Pensión para el Bienestar* (post-2019), as a single non-contributory pension program, with the only difference being in eligibility and transfer generosity. We use this continuous measure directly and also discretize it into within-state median splits and top/bottom quartiles for the binary specifications reported in the main text.

B. Household surveys (ENIGH) The *Encuesta Nacional de Ingresos y Gastos de los Hogares* is INEGI’s biennial household income and expenditure survey, with a probabilistic, multi stage, stratified, clustered design drawn from INEGI’s master sampling frame. ENIGH is representative at the national level and by state. The sample includes approximately 88,000 to 106,000 dwellings per wave.

Some key variables from ENIGH. The income module reports benefits from specific federal programs as distinct income sources. The non-contributory elderly pension appears under an elderly pension code (for

both "65 y Más" and *Pensión para el Bienestar*). The expenditure module reports place of purchase under 19 categories: e.g. supermarkets and department stores versus street vendors and street markets. We use this place of purchase to classify expenditure by the formality of the seller and to construct the household formal budget share following Bachas et al. (2024). For socioeconomic stratification, we use the ENIGH's socioeconomic stratum, which classifies dwellings into four groups (low, middle-low, middle-high, and high) using 24 indicators of the socioeconomic characteristics of residents and the physical and equipment characteristics of the dwelling, combined through multivariate statistical methods.⁸⁵ We pool the top two strata into a single high group. We complement this classification with an asset-based measure constructed from 33 household assets. Specifically, we extract the first principal component from a principal component analysis (PCA) and standardize the resulting index at the national-year level.

C. Establishment census (Economic Census) INEGI's *Censos Económicos* enumerates the universe of economic establishments every five years, excluding agriculture and ambulant activities. We use two waves: the 2019 Census (reference year 2018) and the 2024 Census (reference year 2023). Establishment-level microdata are confidential and can only be accessed on site through INEGI's *Laboratorio de Microdatos*.

For each establishment, the census reports gross output, employment, gross value added, intermediate inputs, fixed assets, as well as the municipality and 6-digit industry. We classify formality by registration with the tax authority.

We match establishments across the 2018 and 2023 waves to build a balanced panel of roughly two million continuing establishments, which we use for the intensive margin and misallocation analyses (firm level MRPL and within industry productivity dispersion, following Hsieh and Klenow (2009)).

D. Private Retail Data We use proprietary transaction data from one of the largest retail chains in Mexico. The data cover more than 3,000 stores across more than 700 municipalities over the period 2015–2023, at a monthly frequency. For each store, we observe sales, physical quantities sold, and total revenue at the product level. The chain sells primarily food and beverages, along with some financial services (e.g., bill payment, ATM access, etc) and health and personal care products. Because the data report both sales and quantities, they allow us to separate changes in nominal sales into price and quantity components. We aggregate the data to the municipality × month level and merge them with our treatment measurements

Linkage across datasets. We combine the sources at two levels of aggregation. At the municipality level we merge pension treatment intensity (A), census establishment outcomes (C) and private retail data (D) on the harmonized municipality identifier from INEGI. We also merge baseline municipal controls like 2015 poverty levels, pre-reform population projections with the data coming from CONEVAL and CONAPO, excess deaths data and income-asset data at the household level from the 2015 Inter-Census. For the local labor market analysis we aggregate municipalities into the commuting-based markets of Aldeco et al. (2024). Household

⁸⁵The indicators are constructed from the 2010 Census of Population and Housing. Because the stratum is anchored to predetermined dwelling and locality characteristics rather than to contemporaneous income, it is independent of the household's pension history and of any changes occurring during our sample period.

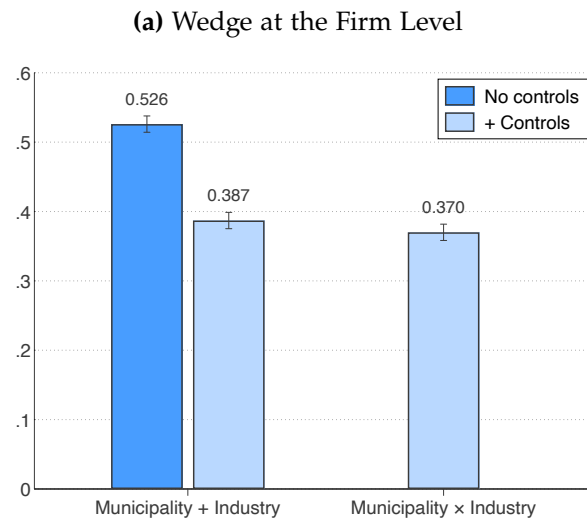
data (B) is used to characterized the household side mechanisms.

C Figures and Tables Appendix

C.1 Figures Appendix

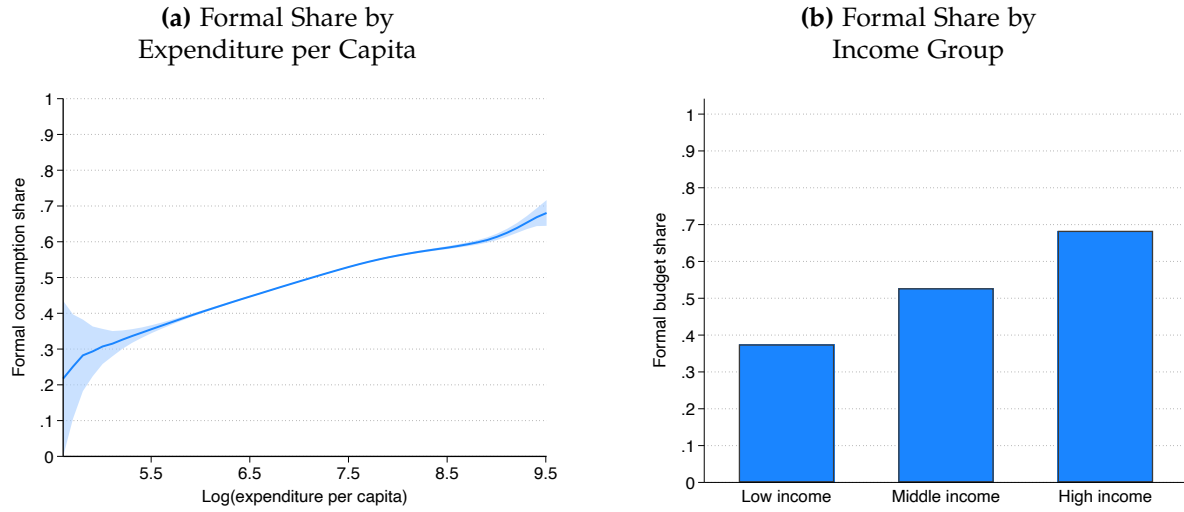
C.1.1 Descriptive

Figure B.1: Formality Wedge Across Specifications



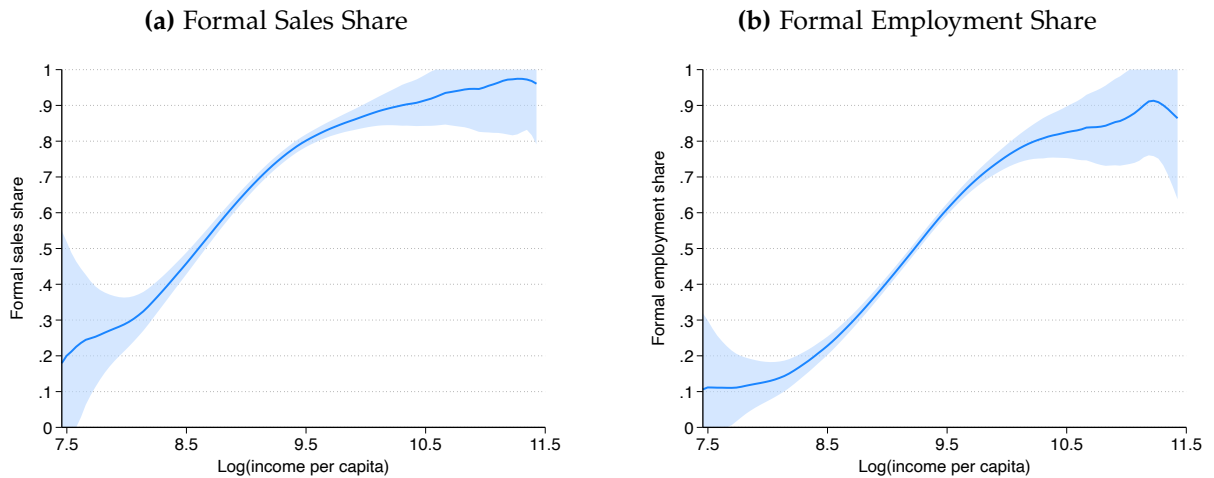
Notes: This figure plots point estimates and 95% confidence intervals of the formal wedge gap at the firm level. Panel (a) regresses the log sales-to-worker ratio, a proxy for the MRPL, on a formality indicator using the 2018 wave of the Economic Census, with the reported fixed effects, where Industry FE denotes 4-digit SCIAN codes, and (where indicated) firm-level controls (firm age, firm size category, and form of ownership). A firm is defined as formal if it is registered with the Mexican tax authority (SAT). Standard errors are clustered at the municipality level.

Figure B.2: Formal Consumption Share Across the Income Distribution



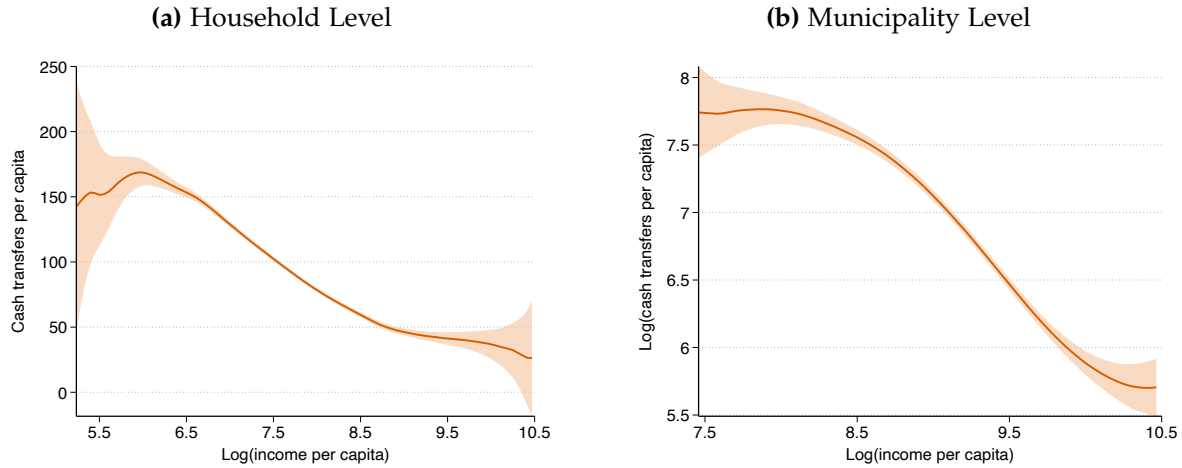
Notes: This figure documents the non-homotheticity of formal consumption. Panel (a) plots the share of formal expenditure in total classifiable expenditure against log expenditure per capita at the household level, where expenditures are classified as formal or informal following [Bachas et al. \(2024\)](#). The solid line is a local polynomial estimate of degree two; the shaded area denotes the 95% confidence interval. Panel (b) shows the formal consumption share across the three socio-economic household strata defined by INEGI.

Figure B.3: Formal Non-Homotheticity Across Municipalities



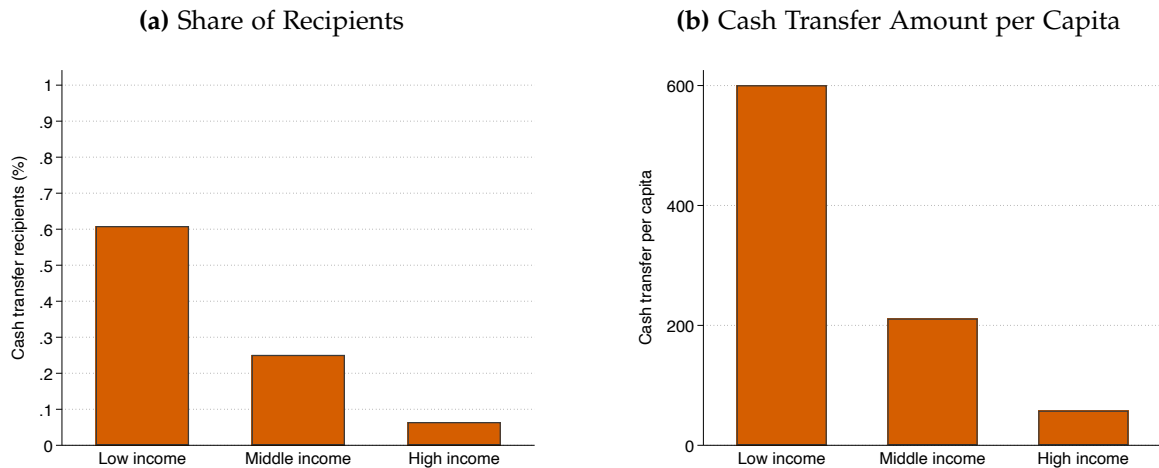
Notes: This figure documents the non-homotheticity of formal sales and employment at municipality level. Panel (a) plots the share of formal sales against log income per capita at the municipality level. Panel (b) plots the share of formal employment against log income per capita. The solid lines are local polynomial estimate of degree two; the shaded area denotes the 95% confidence interval.

Figure B.4: Relationship Between Income and Cash Transfers



Notes: This figure plots the relationship between income and pre-reform cash transfer receipt, showing that transfers were concentrated among poorer households and municipalities. Panel (a) plots a degree-two local polynomial of per capita cash transfers received against log per capita income at the household level, using the 2014, 2016, and 2018 waves of the ENIGH; the transfer amount is netted out of the income measure. Panel (b) plots the same relationship at the municipality level, with both per capita cash transfers and per capita income in logs; transfer data are for 2017 and cover *Prospera* and *Pensión para Adultos Mayores*, and municipal income per capita is from the ICMM. Shaded areas denote 95% confidence intervals.

Figure B.5: Relationship Between Income and Cash Transfers at the Household Level



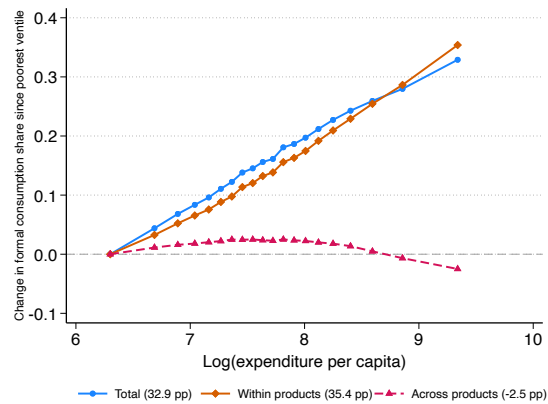
Notes: This figure shows pre-reform cash transfer receipt by socioeconomic stratum, using the ENIGH socioeconomic classification and pooling the 2014, 2016, and 2018 waves. Households are grouped into low, middle, and high-income strata. Panel (a) plots the share of households in each stratum that receive any cash transfer: 60% among low-income households, compared with 25% for middle-income and 4% for high-income households. Panel (b) plots the average per capita transfer amount received in each stratum, in pesos.

Figure B.6: Marginal Formal Share Across the Income Distribution



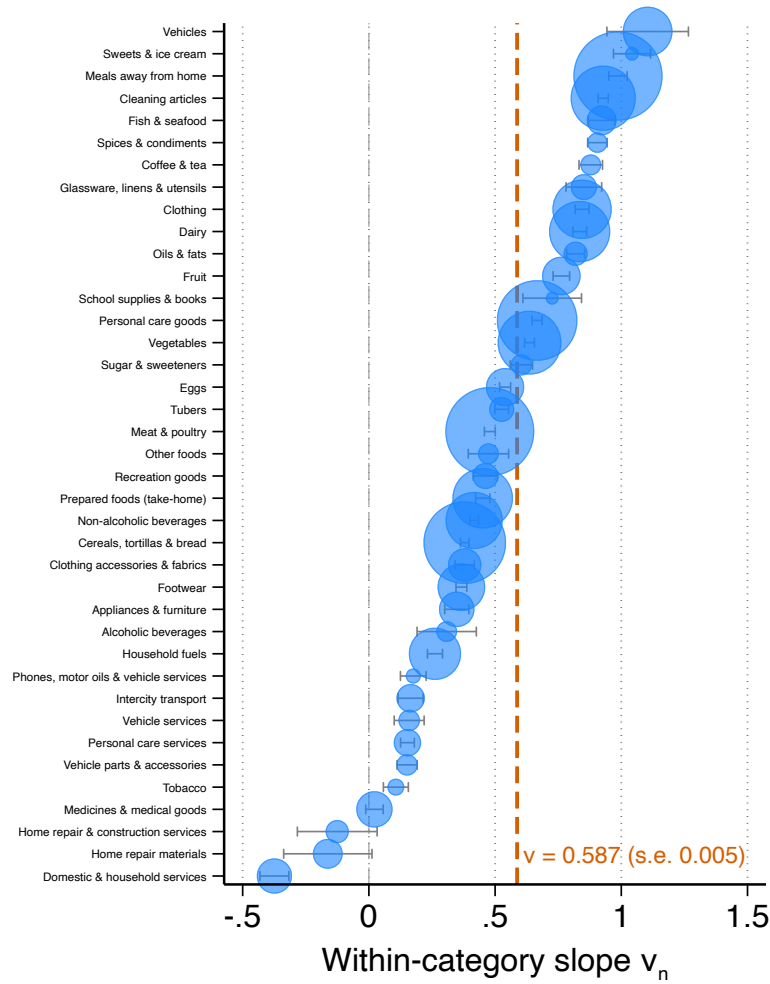
Notes: This figure plots a nonparametric estimate of the marginal formal share $\mu(e)$ across the income distribution in the pre-reform period. Households in the 2014–2018 ENIGH waves are grouped into brackets of monthly per capita income of MXN 2,500. Each point reports the ratio of the change in mean per capita formal expenditure to the change in mean per capita total expenditure between consecutive brackets, that is, the share of each additional peso of spending allocated to formal outlets as household resources rise; expenditure covers the categories we can assign to formal or informal outlets (food, beverages, and cigarettes).

Figure B.7: Decomposition of the Formal Consumption Share Across the Expenditure Distribution



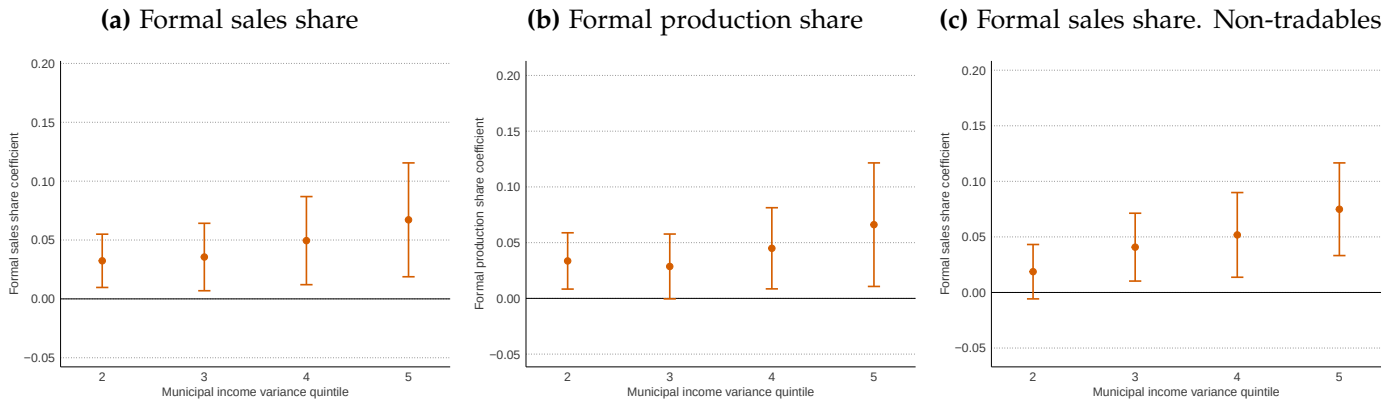
Notes: This figure plots the change in the formal share of consumption relative to the poorest ventile of per capita expenditure (Total), and how much of it comes from households buying the same products in formal outlets more often (Within products) or from buying a different mix of products (Across products). Households in the 2014–2018 ENIGH waves are grouped into ventiles of monthly per capita expenditure (2018 pesos), after trimming the top and bottom 1% of per capita expenditure. The horizontal axis is the mean log expenditure of each ventile. Between two neighboring ventiles, the within part is the change in each product's formal share, weighted by its budget share, and the across part is the change in each product's budget share, weighted by its formal share. Weights are averages over the two ventiles, so the two parts add up to the total. The legend reports the value of each line at the richest ventile, in percentage points. The formal share is 34.9% in the poorest ventile and 67.8% in the richest. Products are the 647 ENIGH product codes (for example, pasteurized milk, powdered milk, fresh cheese, Oaxaca cheese, cream, or butter within the dairy group). The share of formal expenditure is classified based on the place of purchase. Statistics do not use survey weights.

Figure B.8: Within-Category Formal Consumption Slopes by Spending Group



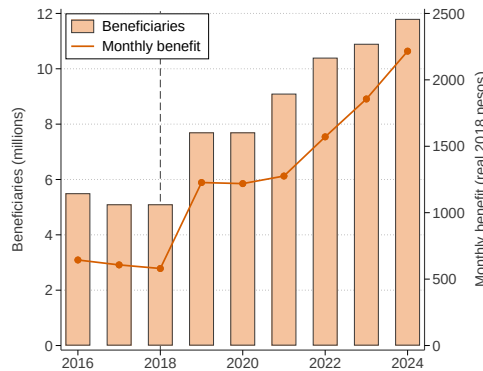
Notes: This figure plots the estimated within-category slope v_n for each spending group, from equation (A.7). For each group we regress the formal share of a household's spending in the group on $-(e/\bar{e})^{-\varepsilon}$ and year fixed effects, using the households that buy the group and weighting each by its spending in the group, where e is monthly per capita expenditure (2018 pesos) and $\bar{e}$ its mean. Households are from the 2014–2018 ENIGH waves, after trimming the top and bottom 1% of per capita expenditure. The curvature parameter ε is common to all groups and is estimated from the aggregate informal share of consumption under the restriction that it goes to zero as resources grow ($\omega = 1$), which gives $\varepsilon = 0.21$. Since expenditure is normalized by its mean, v_n is the gap between the asymptotic formal share of the group and its formal share at mean expenditure. Groups are sorted by the size of the estimate. Horizontal bars are 95% confidence intervals from a bootstrap that resamples primary sampling units. The size of each marker is proportional to the group's budget share. The dashed line is v , the average of the slopes weighted by budget shares, equal to 0.59 (standard error 0.01). Groups with a positive slope account for 97% of spending. The share of formal expenditure is classified based on the place of purchase. Statistics do not use survey weights.

Figure B.9: Relationship Between Municipal Variance in Household Income and Formal Sector



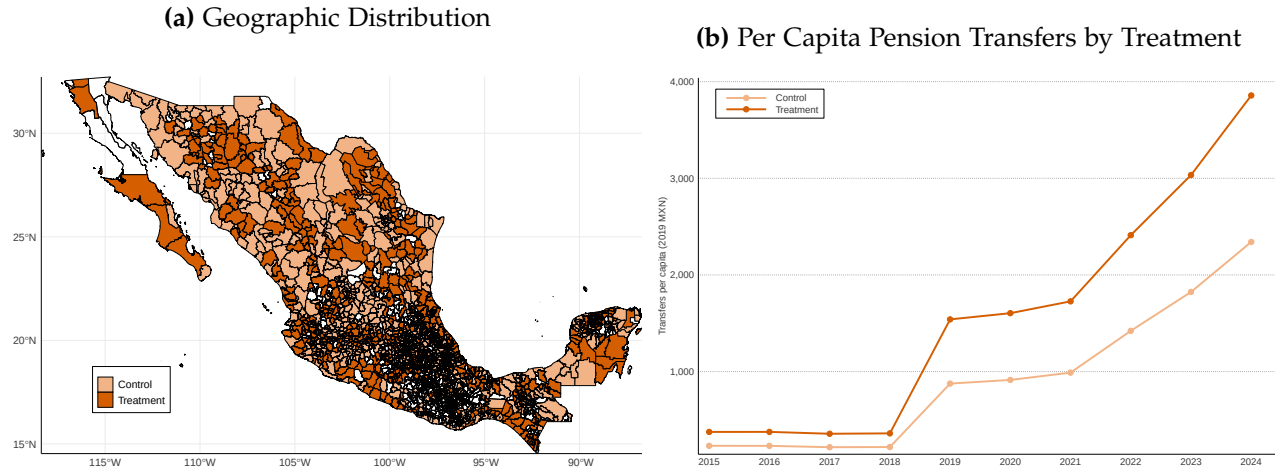
Notes: This figure plots the relationship between within-municipality income dispersion and the formal share of local economic activity. Panels A and B use all establishments and report the formal shares of total establishment sales and gross output. Panel C reports the formal share of establishment sales in non-tradable sectors. Each point is the coefficient on an indicator for the corresponding quintile of normalized income variance, defined as municipal income variance divided by mean income. The omitted category is the first quintile. All regressions control for mean-income quintile indicators. Household income measures come from the 2015 IPUMS data; economic activity measures come from the 2018 Economic Census.

Figure B.10: Evolution of the non-contributory pension, 2018–2024



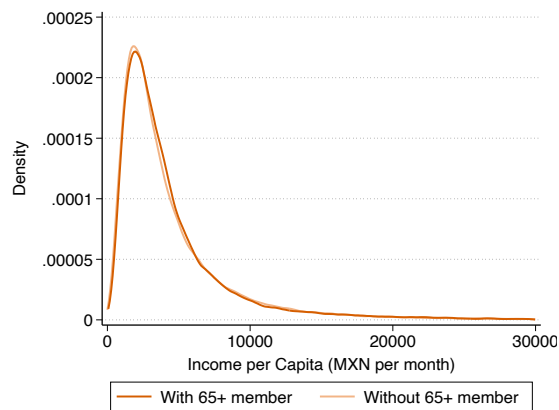
Notes: The figure plots the evolution of the non-contributory pension between 2016 and 2024. Bars report the number of beneficiaries, in millions, measured on the left axis. The line reports the monthly benefit deflated to real 2018 pesos using the INEGI INPC price index, measured on the right axis. The dashed vertical line marks 2018, the last year before the universalization reform. The 2016–2018 values correspond to the pre-reform means-tested program, whose benefit was flat at 580 pesos per month.

Figure B.11: Treatment Intensity and Geographical Distribution



Notes: Panel (a) shows the municipality geographical distribution by treatment group. Dark orange denotes high-treatment municipalities, those where the change in per capita pension transfers between the pre and post-reform periods exceeds the within-state median. Light orange denotes low-treatment municipalities. White areas are excluded from the sample due to population or formal-sector restrictions described in the text. Panel (b) plots the evolution of per capita pension transfers in real 2019 MXN for each group.

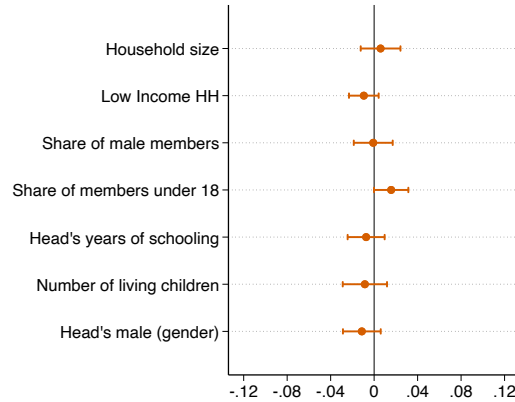
Figure B.12: Income Distribution for Recipient and Non-Recipient Households



Notes: This figure plots kernel densities of household income per capita for households with at least one member aged 65 or older (potential recipients) and households with no members aged 65 or older, pooling the 2014, 2016, and 2018 waves of ENIGH. Income per capita is total household income divided by household size

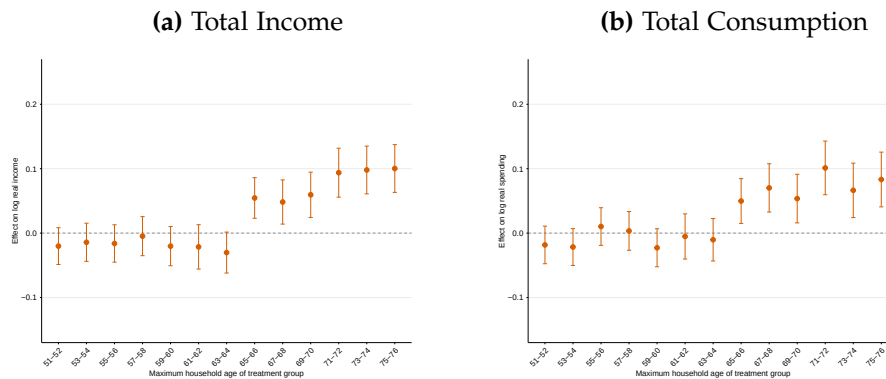
C.2 Household Results Section

Figure B.13: Effects of Pension on the Household Composition



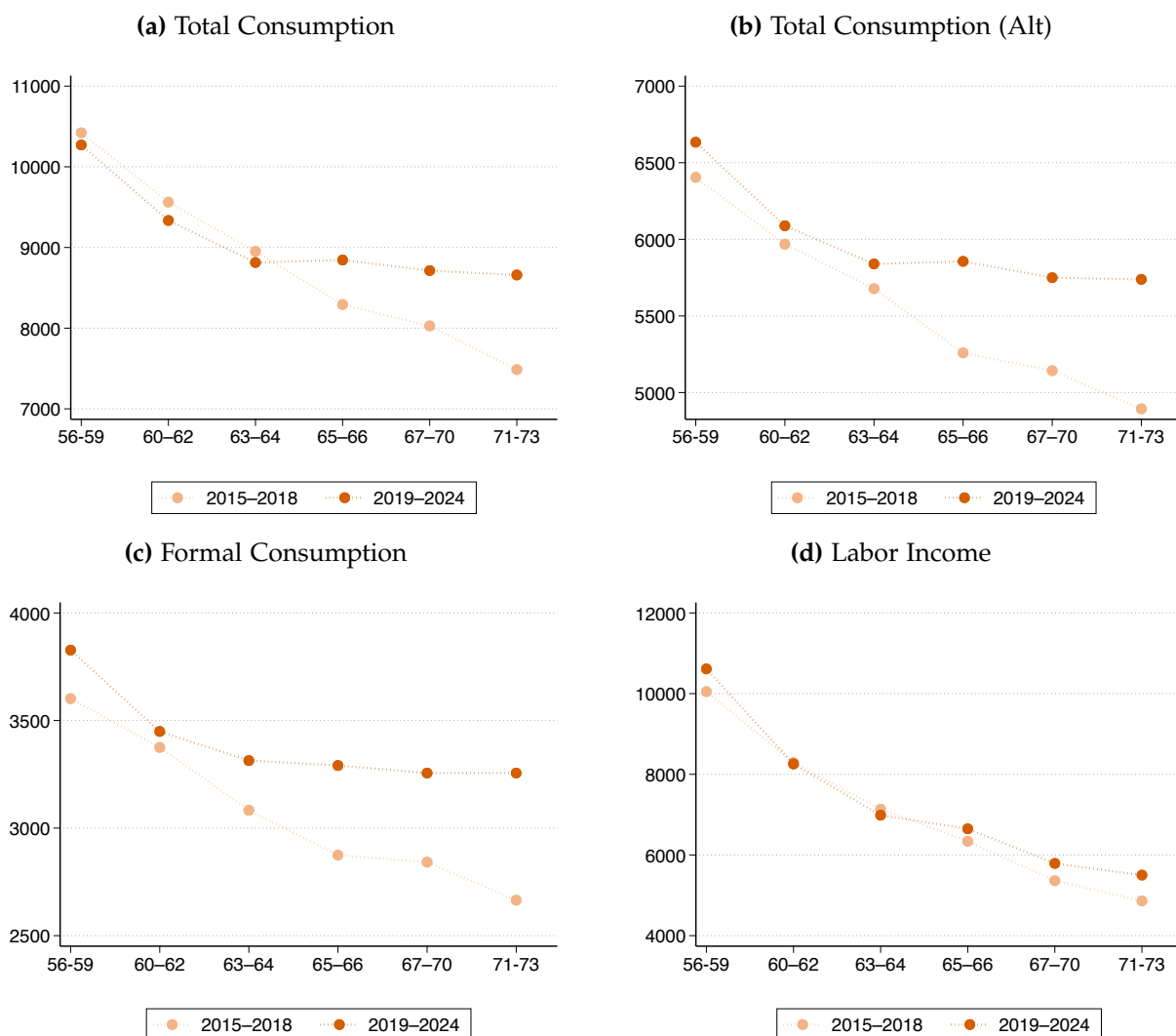
Notes: This figure reports difference-in-differences estimates and 95% confidence intervals from a static version of specification 23, where the vector of year-specific coefficients is replaced by a single interaction between household eligibility and a post-reform indicator ($t \geq 2019$). Each row uses a different (standardized) household characteristic as the dependent variable, and the plotted point is the coefficient on that eligibility-by-post interaction. The only controls are state-by-year and municipality-size-by-year fixed effects. All outcomes are standardized. The outcomes are: number of household members; an indicator for low-income households; share of male members; share of members under 18; household head's years of schooling; an indicator if the head is male; number of living children; an indicator for whether the head is married or cohabiting; and an indicator for whether any member has social security.

Figure B.14: Difference-in-Differences Estimates for Different Treatment Group Definitions



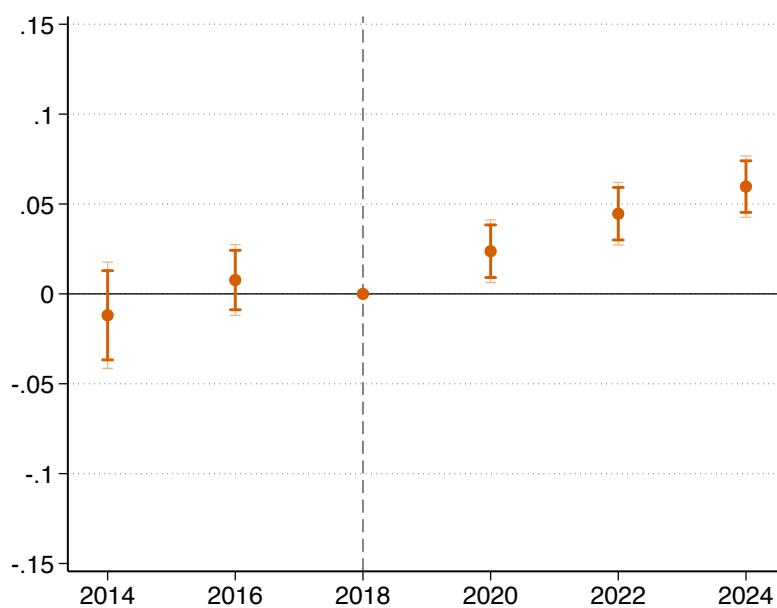
Notes: This figure reports difference-in-differences estimates and 95% confidence intervals from a static version of specification 23, where the vector of year-specific coefficients is replaced by a single interaction between household eligibility and a post-reform indicator ($t \geq 2019$). Each coefficient is the result of a separate regression where the treated group is defined as households where the eldest member is between a specific age range. Control group is households where the eldest member is 50 in all specifications. Regressions estimated on household level data from ENIGH 2014-2024.

Figure B.15: Consumption and Labor Income by Age Group, Pre and Post-Universalization



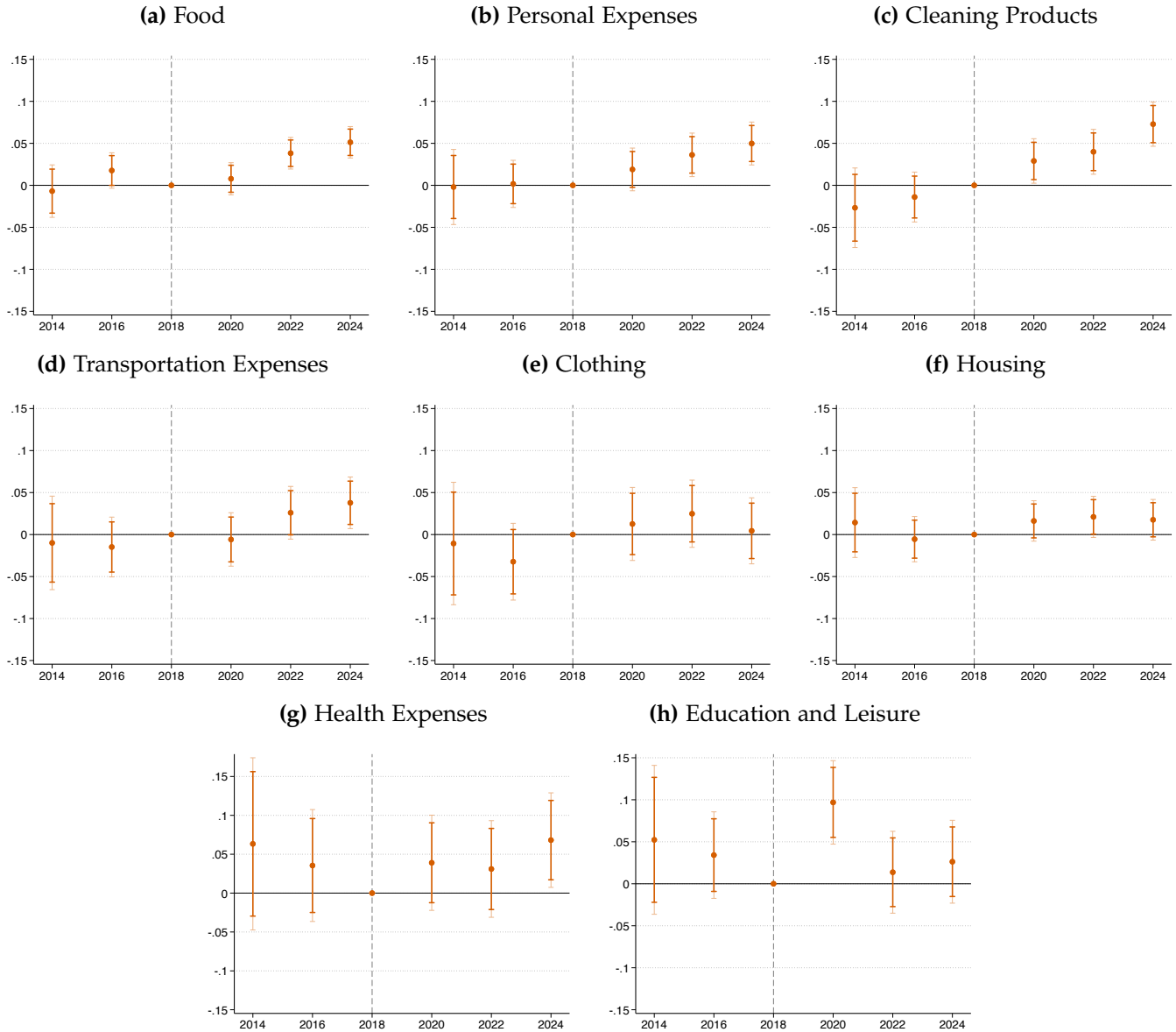
Notes: This figure plots average total consumption per capita (Panel a), average consumption in categories that can be classified as formal or informal (food, beverages, and tobacco) (Panel b), average formal consumption per capita (Panel c), and average labor income per capita (Panel d), by age cohort, separately for the pre-universalization period (2015–2018, gray) and the post-universalization period (2019–2024, black). Each point represents the mean across households in the indicated age bin. Households are assigned to a bin according to the age of their eldest member. The dashed lines connecting the points illustrate the age gradient in each period. All outcomes are expressed in 2018 pesos and in per capita terms, dividing by the number of household members.

Figure B.16: Effects on Total Consumption (Alternative Specification)



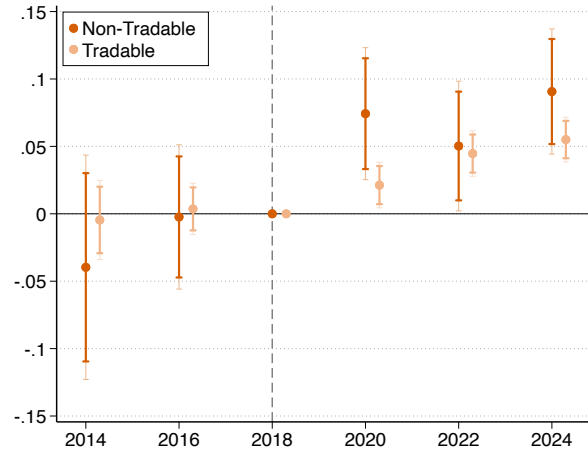
Notes: This figure presents the effect on household consumption based on Equation 23, restricting the outcome to the subset of products (food, beverages and cigarettes) that report a place of purchase, which is the information we use to classify consumption as formal or informal. This subset accounts for 68% of total household expenditure on average. Vertical bars represent 95% confidence intervals, with standard errors clustered at the municipality level.

Figure B.17: Effect on Consumption by Category



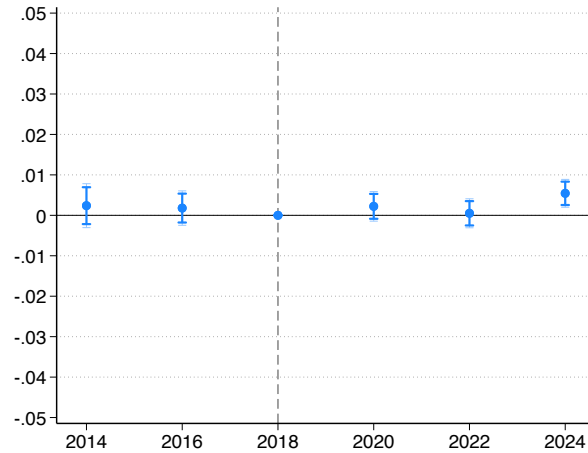
Notes: Each panel plots event-study coefficients from the difference-in-differences specification in equation 23, estimated separately for the main consumption categories in the ENIGH. All outcomes are in logs. (a) Food: expenditures on food and beverages. (b) Personal Expenses: spending on personal care products and services (e.g., haircuts, cosmetics, hygiene items). (c) Cleaning Products: cleaning supplies and services, household care items, domestic appliances, furniture, utensils, etc. (d) Transportation: public transit fares, vehicle acquisition and maintenance and communications services including telephone and internet. (e) Clothing: purchases of garments, footwear, and related accessories for all household members. (f) Housing: dwelling maintenance and repairs, water services, electricity, cooking gas, and other household fuels. (g) Health Expenses: out-of-pocket spending on medical consultations, hospitalizations, medicines, lab tests, and medical devices. (h) Education and Leisure: tuition, textbooks and recreational spending on entertainment. The dashed vertical line marks the onset of treatment. Shaded areas denote 95% confidence intervals. [Back to Section 5.2.2]

Figure B.18: Effects on Tradable and Non-Tradable Consumption



Notes: This figure presents the effect on household consumption of tradable and non-tradable goods and services (in logs), based on Equation 23. Each product is classified as tradable or non-tradable according to whether the good or service can be produced away from where it is consumed; Table B.17 in the Appendix reports the full classification. Vertical bars represent 95% confidence intervals, with robust standard errors. All variables are expressed in 2018 MXN.

Figure B.19: Effects on the Extensive Margin of Formal Consumption



Notes: This figure plots the effect on an indicator equal to one if the household reports any expenditure in formal establishments, based on Equation 23. Each purchase is classified as formal or informal by its place of purchase following Bachas et al. (2024), and the indicator is defined over the products (food, beverages and cigarettes) for which the place of purchase is reported. Before the reform, 98% of treated households reported some formal purchase. Vertical bars represent 95% confidence intervals, with standard errors clustered at the municipality level.

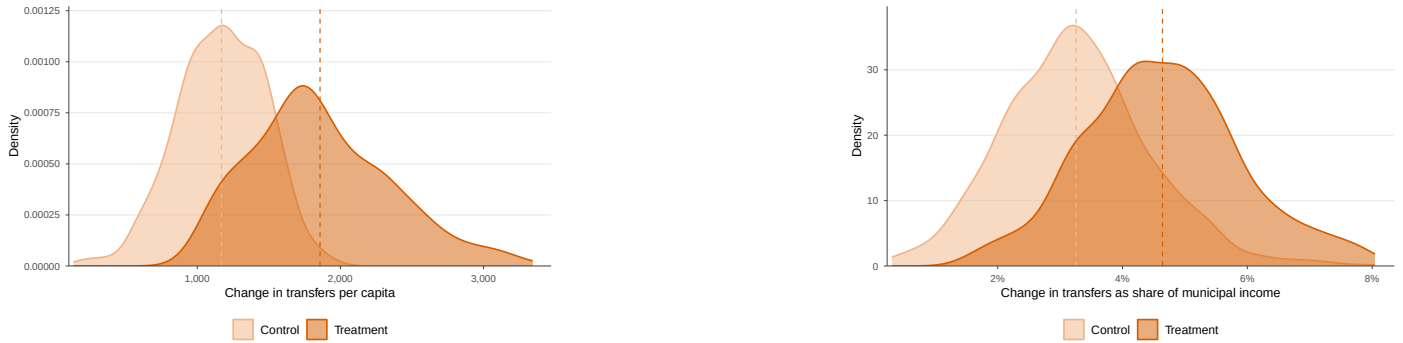
Figure B.20: Effects on Labor Supply



Notes: This figure plots the effect on labor supply households using equations 23. Panel (a) plots the pensions effect in household-level labor earnings along the extensive margin (whether any member received work-related income) while Panel (b) plots the intensive margin (log amount). Vertical bars represent 95% confidence intervals.

C.2.1 Firms and Municipality Results Section

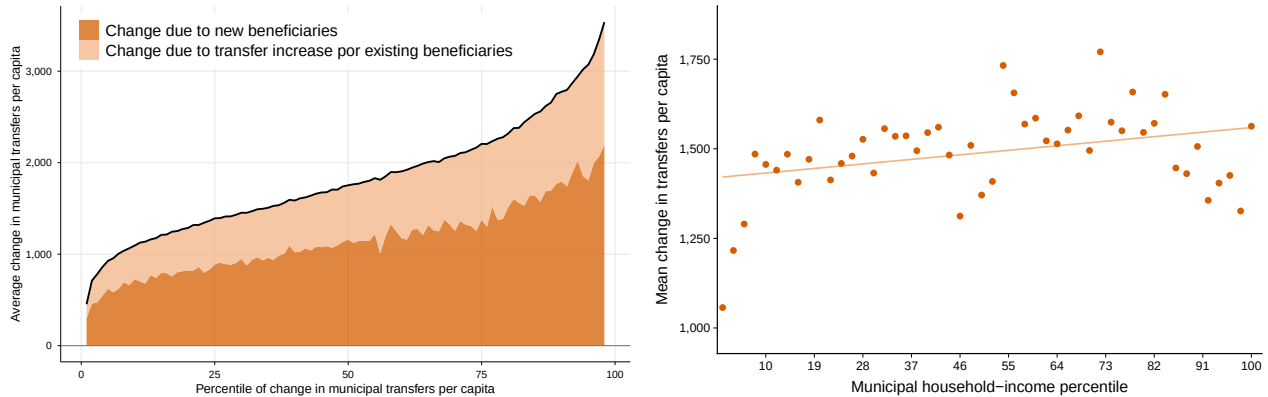
Figure B.21: Distribution of continuous municipality level treatment variable



Notes: Panel (a) plots the distribution of the change in transfers per capita between the pre and post period by treatment group. Panel (b) plots the distribution of the change total transfers between the pre and post period, as a share of total municipal income in 2020. [Back to Section 6]

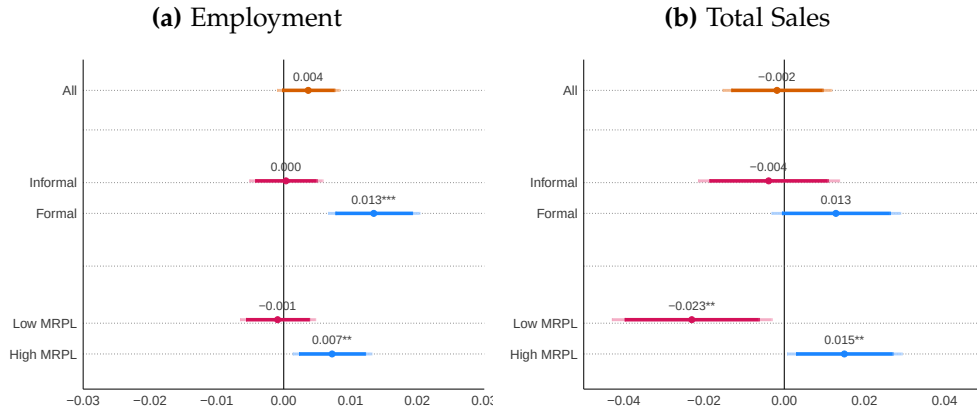
Figure B.22: Municipal level continuous treatment exposure descriptives

(a) Changes in beneficiaries vs transfers per beneficiary (b) Municipality treatment and average municipal income



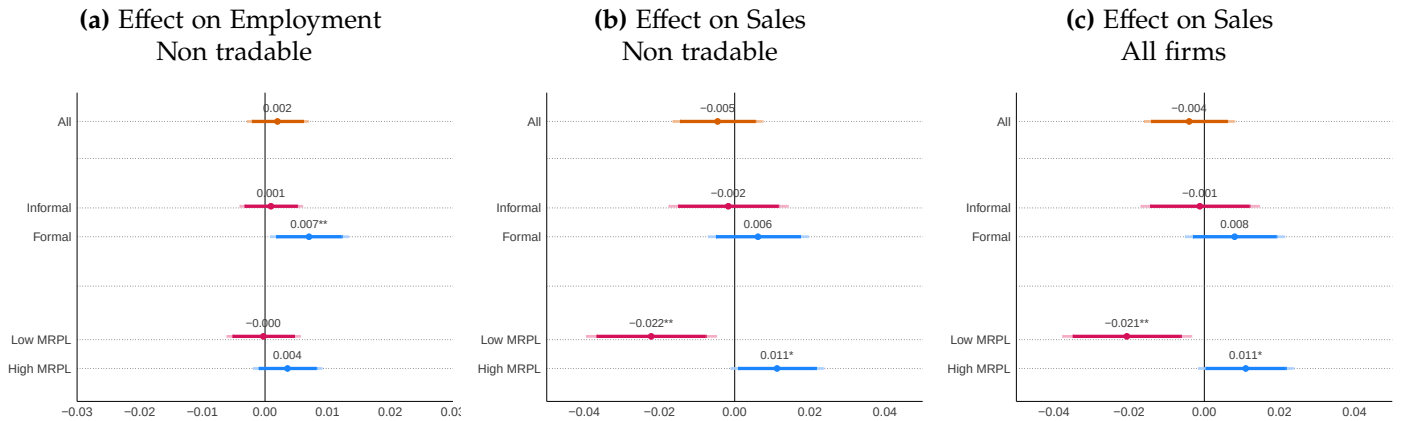
Notes: Panel (a) decomposes variation in treatment exposure across municipalities into the part explained from the rise in transfers given to new beneficiaries ($\Delta \text{beneficiaries} * \text{avg transfers}_{post}$) and the rise in transfers given to individuals who were already beneficiaries in the means-tested non-contributory pension pre reform ($\Delta \text{transfers per beneficiary} * \text{beneficiaries}_{pre}$). Municipalities are grouped into percentiles according to their pre-post change in transfers per capita. The black line shows the average change within each percentile. The shaded areas decompose this change into the contribution of changes in beneficiary coverage (dark grey) and changes in transfers per beneficiary (light grey). The 100th percentile is excluded for visual purposes. Panel (b) the average change in pension transfers per capita across percentiles of average real quarterly household income at the municipality. [Back to Section 6]

Figure B.23: Effect on total sales and employment of high-exposed firms



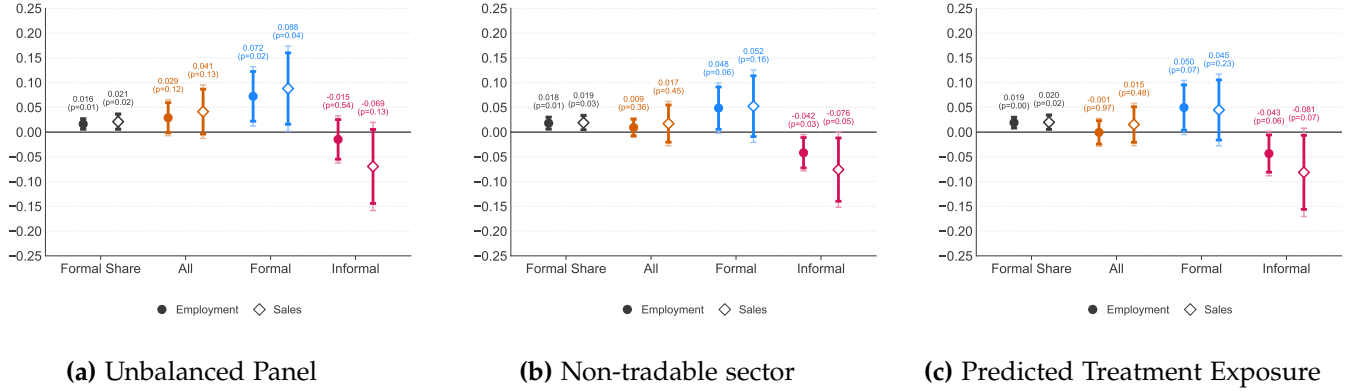
Notes: This figure plots the estimated effects of the pension on firm employment and sales of firms in high-demand sectors and highly exposed municipalities. The 'All' coefficient corresponds to the estimated θ^{dd} from Equation 28. The Informal-Formal coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm is classified as formal. The Low-High MRPL coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm's MRPL was above the 2-digits NAICS sector median in 2018. Regressions are estimated on a balance sample of firms from the Economic Census 2018 and 2023 with at least 2 workers, and which belong to high-demand sectors. Standard errors clustered at the municipality x year level. Wide lines correspond to 90% CI while narrow lines correspond to 95% CI.

Figure B.24: Effect on Total Employment and Sales at the Firm Level



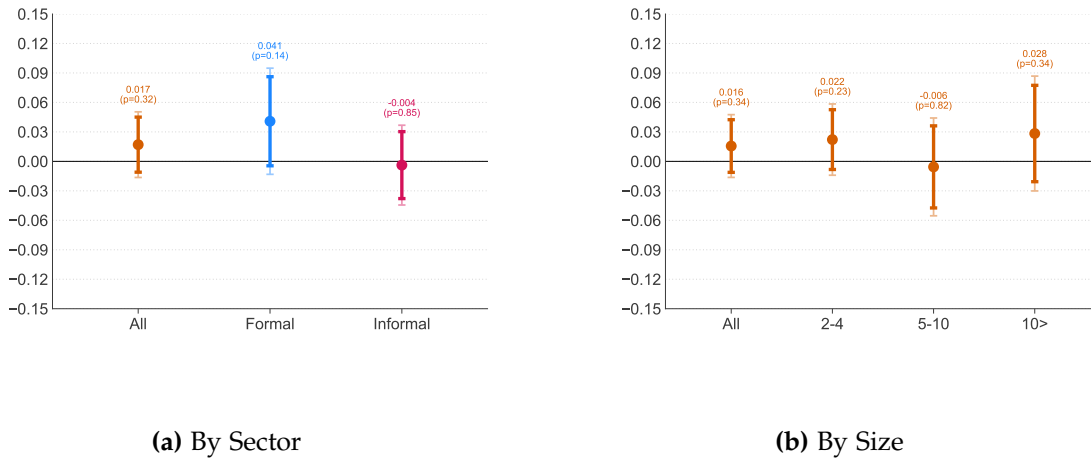
Notes: This figure plots the estimated effects of the pension on log total firm employment (panel a) and sales (panels b and c) of firms in highly exposed municipalities. The 'All' coefficient corresponds to the estimated θ^{dd} from Equation 28. The Informal-Formal coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm is classified as formal. The Low-High MRPL coefficients correspond to the estimated θ_0^{dd} and θ_1^{dd} from Equation (29), where $g = 1$ if the firm's MRPL was above the 2-digits NAICS sector median in 2018. Regressions are estimated on a balance sample of firms from the Economic Census 2018 and 2023 with at least 2 workers. Panels (a) and (b) restrict the sample to firms in the retail sector. Panel (b) restricts the sample to firms in non-tradable sectors. Standard errors clustered at the municipality x year level. Wide lines correspond to 90% CI while narrow lines correspond to 95% CI. [Back to Section 6.4]

Figure B.25: Effect on Employment and Output at the Municipal Level



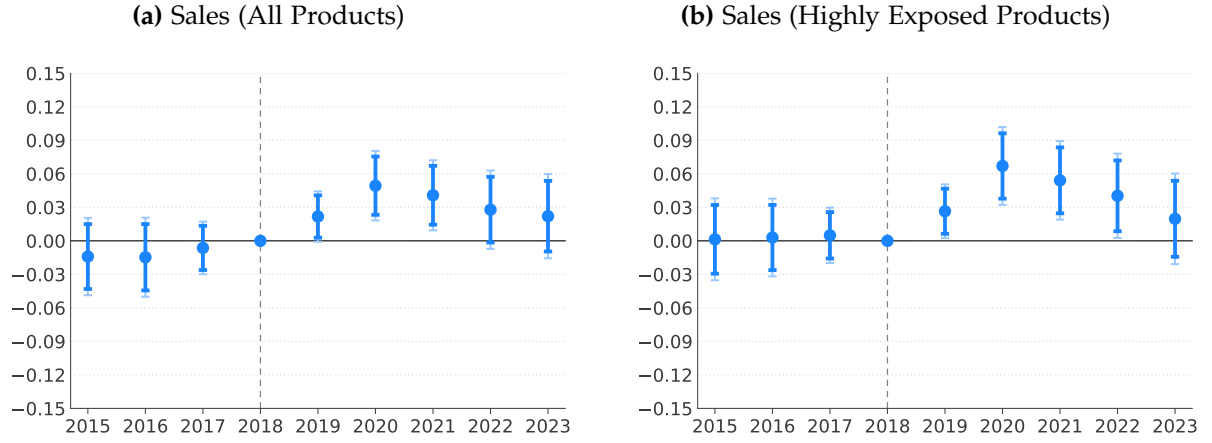
Notes: This figure plots the coefficient θ_{2023} from equation (30) and 90% (dark) and 95% (light) confidence intervals. Panel (a) is estimated on the unbalanced panel, allowing for firms' entry and exit between 2018 and 2023. Panel (b) is estimated on the balanced panel of firms in non-tradable sectors. In both panels treatment is an indicator for municipalities above the median change in pension transfers per capita between 2018 and 2023, and 2018 is the reference year. In Panel (c) Treatment is an indicator for municipalities above the median change in predicted pension transfers per capita between 2018 and 2023, constructed from the shares of new and existing beneficiaries and the change in benefit amounts. Filled circles correspond to employment and hollow diamonds to gross output. The first group of estimates uses the formal share of employment and output as the dependent variable, in levels; the remaining groups use the log of total, formal, and informal employment and output. A firm is formal if it reports being registered with the fiscal authority. All specifications include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Figure B.26: Effect on Number of Firms



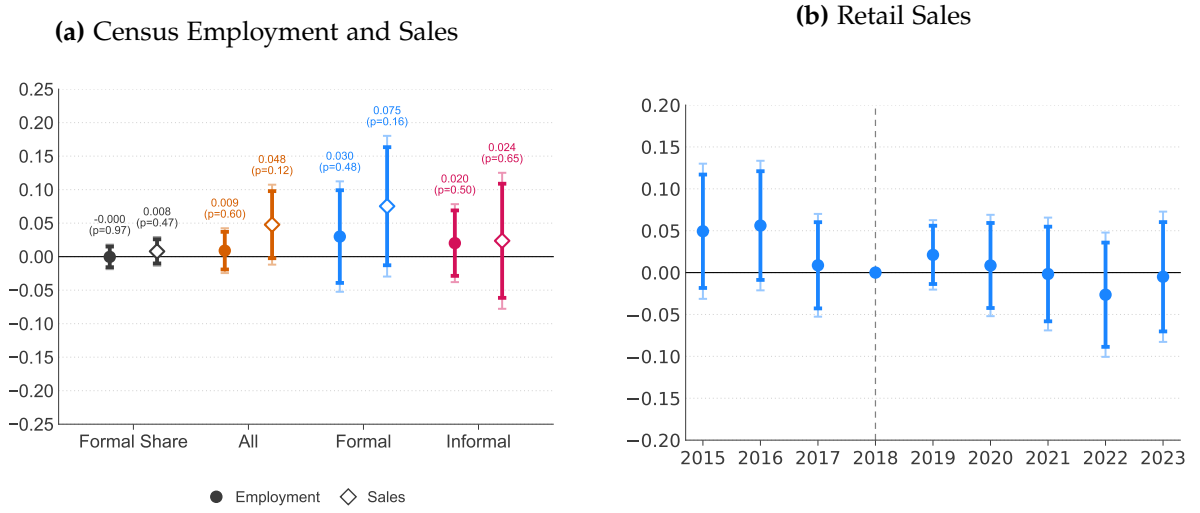
Notes: This figure plots the coefficient θ_{2023} and 90% (dark) and 95% (light) confidence intervals from equation (30) five years after universalization, using the log number of firms in the municipality as the dependent variable. Panel (a) splits firms by formality status and panel (b) by employment size; both panels use the universe of firms in the Economic Census, without balancing. Treatment is an indicator for municipalities above the median change in pension transfers per capita between 2018 and 2023, and 2018 is the reference year. A firm is formal if it reports being registered with the fiscal authority. All specifications include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Figure B.27: Effect on Retail Sales and Quantities of Goods Sold (Unbalanced Panel)



Notes: This figure plots the coefficients θ_t from equation (30), estimated on all the stores of the retail chain—allowing for stores entry and exit—with 2018 as the reference year; the dashed line marks the start of the universalization. Panels (a) plot the effect on sales for all products, and panels (b) for the highly exposed product categories from Section 5.2.2; Table B.22 details the classification of categories with product examples. Treatment is an indicator for municipalities above the median change in predicted pension transfers per capita. All specifications include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality, state-by-month fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with month. Standard errors are clustered at the municipality level; bars show 90% (dark) and 95% (light) confidence intervals.

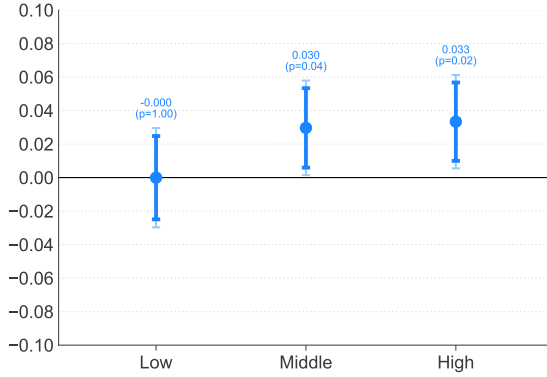
Figure B.28: COVID-19 Treatment Placebo



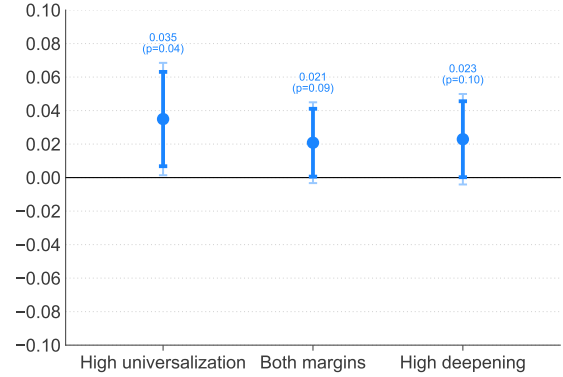
Notes: This figure reports a placebo version of Equation (30) in which the treatment indicator T_m is defined using municipalities' COVID-19 excess mortality rather than their exposure to the pension reform. Specifically, municipalities are divided according to whether their excess mortality during the pandemic, measured as a percentage of baseline mortality, is above or below the sample median. Panel (a) plots the coefficient θ_{2023} and 90% (dark) and 95% (light) confidence intervals using data from the 2018 and 2023 Economic Censuses, with 2018 as the reference year. Filled circles correspond to employment and hollow diamonds to sales. The first group of estimates uses the formal share of employment and sales as the dependent variable, in levels; the remaining groups use the log of total, formal, and informal employment and sales. As in the baseline specification, all regressions include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Panel (b) plots the coefficients θ_t from the same placebo specification using log retailer sales, estimated on the balanced panel of stores with 2018 as the reference year. The regression includes municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality, state-by-month fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with month. Standard errors are clustered at the municipality level; bars show 90% (dark) and 95% (light) confidence intervals.

Figure B.29: Heterogeneous Effects on Formal Sales

(a) By Relative Position of Beneficiaries in the Municipality Asset Distribution



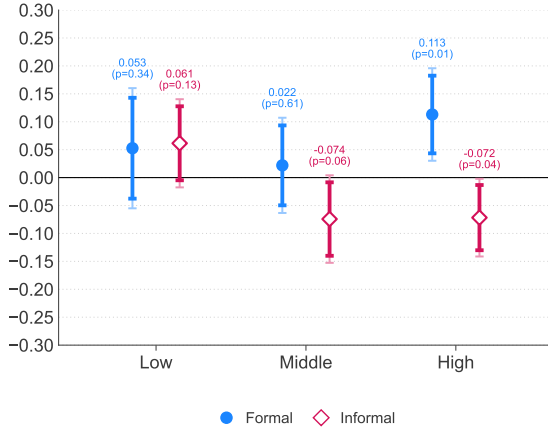
(b) By Coverage



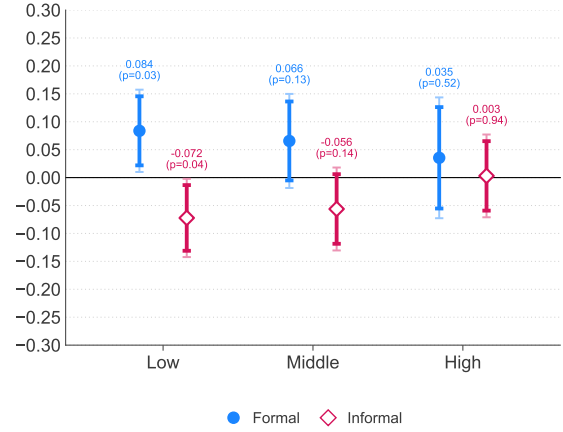
Notes: This figure plots the coefficients $\theta_{g,2023}$ from Equation (32), together with 90% (dark) and 95% (light) confidence intervals. The outcome is the sales formal share, constructed from the balanced panel of firms in the Economic Census. Within each group, municipalities above the median change in pension transfers per capita are compared with municipalities below the median, with 2018 as the reference year. In Panel (a), terciles are constructed from the average percentile rank of the local asset distribution of households with a member aged 65 or older that did not receive government transfers before the reform within their municipality's asset distribution. In Panel (b), municipalities are grouped according to the pre-reform share of elderly individuals already receiving a non-contributory pension. "Higher benefits" denotes municipalities where the increase in transfers came relatively more from larger benefits to existing beneficiaries; "Both margins" denotes municipalities where increases in benefits and coverage both contributed; and "Broader coverage" denotes municipalities where the increase came more from the addition of new beneficiaries. All specifications include municipality fixed effects, state-by-year and group-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Figure B.30: Heterogeneous Effects on Formal Employment (logs)

(a) By Relative Position of Beneficiaries in the Municipality Asset Distribution

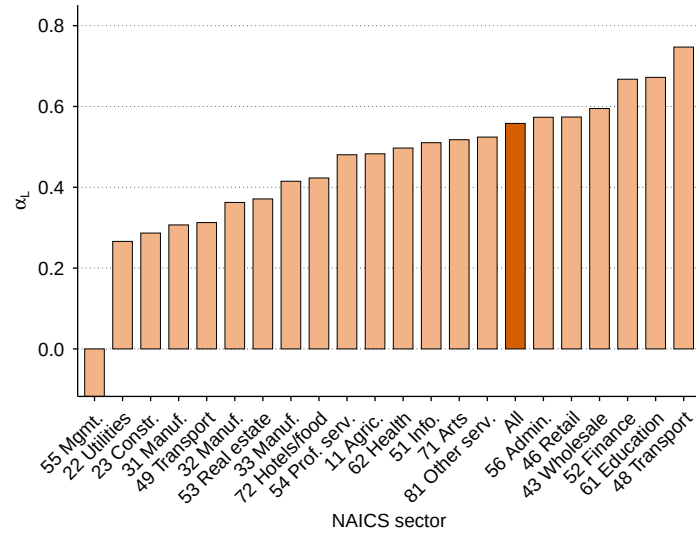


(b) By Coverage



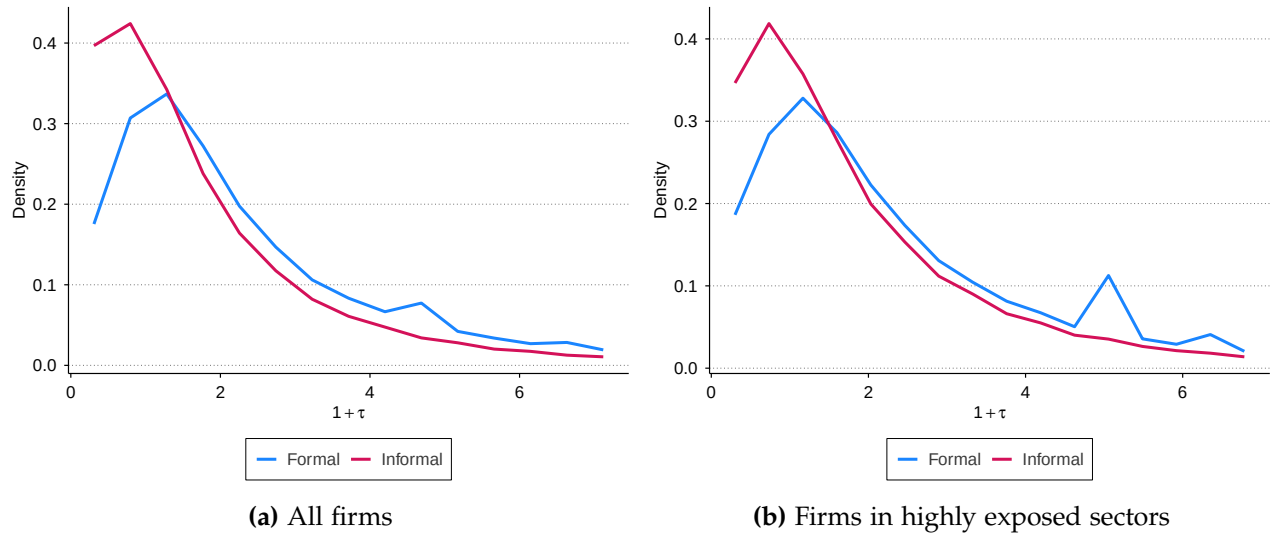
Notes: This figure plots the coefficients $\theta_{g,2023}$ from Equation (32), together with 90% (dark) and 95% (light) confidence intervals. The outcomes are the formal and informal employment (both in logs), constructed from the balanced panel of firms in the Economic Census. Within each group, municipalities above the median change in pension transfers per capita are compared with municipalities below the median, with 2018 as the reference year. In Panel (a), terciles are constructed from the average percentile rank of households with a member aged 65 or older that did not receive government transfers before the reform within their municipality's asset distribution. The low, middle, and high terciles therefore correspond to municipalities in which prospective beneficiaries were located progressively higher in the local asset distribution. In Panel (b), municipalities are grouped according to the pre-reform share of elderly individuals already receiving a non-contributory pension, which determines the relative importance of the two margins through which universalization increased transfers. "Higher benefits" denotes municipalities where the increase in transfers came relatively more from larger benefits to existing beneficiaries; "Both margins" denotes municipalities where increases in benefits and coverage both contributed; and "Broader coverage" denotes municipalities where the increase came more from the addition of new beneficiaries. All specifications include municipality fixed effects, state-by-year and group-by-year fixed effects, and quartiles of baseline informality, number of firms, and population, each interacted with year. Standard errors are clustered at the municipality level.

Figure B.31: Estimated output labor elasticities across sectors



Notes: This figure plots the estimated elasticity of firm output with respect to labor (α^L) across 2-digit NAICS industries. The elasticities are estimated using data from the Economic Census and the methodology of [Levinsohn and Petrin \(2003\)](#).

Figure B.32: Distribution of wedges ($1 + \tau$) across formal and informal firms



Notes: This figure plots the distribution of the estimated $(1 + \tau_j)$ across formal and informal firms in the 2018 Economic Census. $(1 + \tau_j)$ is estimated as $\alpha^L \cdot \frac{p \cdot Y_j}{w \cdot L_j}$ using firm level data from the 2018 Economic Census and estimates of α^L from Figure B.31. Firms are defined as formal if they are registered in the tax authority (SAT) in 2018, and zero otherwise.

C.3 Tables Appendix

C.3.1 Descriptive

Table B.1: Evolution of the non contributory pension, 2016–2024

Year	Program	Eligibility age	Means-tested	Monthly benefit (MXN)		Beneficiaries (millions)	Coverage (% of 65+)
				Nominal Pesos	Real 2018 pesos		
2016	65 y Más	65	Yes	580	644	5.5 ^d	63.1
2017	65 y Más	65	Yes	580	607	5.1 ^d	57.3
2018	65 y Más	65	Yes	580	580	5.1 ^d	55.1
2019	Pensión Bienestar	68 ^a	No	1,275	1,227	7.7	79.9
2020	Pensión Bienestar	68	No	1,310	1,219	7.7	78.1
2021	Pensión Bienestar	65 ^b	No	1,350–1,550 ^c	1,276	9.1	90.5
2022	Pensión Bienestar	65	No	1,925	1,571	10.4	100.9
2023	Pensión Bienestar	65	No	2,400	1,856	10.9	101.0
2024	Pensión Bienestar	65	No	3,000	2,216	11.8	104.8

Notes: Benefit amounts are monthly, computed as one half of the official bimonthly transfer. Real values are deflated to 2018 pesos using the INEGI INPC price index, with base 2018 equal to 100. The pre-2019 “65 y Más” program was means-tested. The 2019 reform eliminated means-testing and universalized the benefit. Beneficiary counts for 2019–2024 are computed from the program’s administrative records (padrón de beneficiarios) and correspond to the number of individuals paid in the last bimester of each year. Coverage is the ratio of beneficiaries to CONAPO’s projected national population aged 65 and older; values above 100 percent reflect projection error and lags in removing deceased beneficiaries from the rolls. In 2019–2020 the eligibility age was 68, so coverage relative to the 65+ population mechanically understates coverage of the statutorily eligible population. ^a The 2019 program set the eligibility age at 68 nationally, with an exception for residents of Indigenous communities, who remained eligible at 65. Beneficiaries already enrolled under “65 y Más” retained their pensions, so the higher threshold bound only new entrants. ^b The national eligibility age was lowered to 65 beginning in August 2021. ^c The nominal benefit rose mid-year, from 1,350 pesos per month in January–June to 1,550 pesos per month in July–December; the real number is the annual average of the two periods. ^d Administrative records begin in 2019; the 2016–2018 figures are the population served (*población atendida*) reported by CONEVAL in its *Fichas de Monitoreo y Evaluación* (FMyE) for S176: FMyE 2016–2017 for 2016, FMyE 2017–2018 for 2017, and FMyE 2020–2021 for 2018. *Sources:* Authors’ calculations using Secretaría de Bienestar administrative records and CONAPO population projections; Diario Oficial de la Federación, CONEVAL, INEGI.

Table B.2: Summary Statistics of Main Samples

	Mean	SD	N
<i>Panel A: Individuals (ENOE, 2015–2018)</i>			
Age	65.48	3.94	494,453
Employed	0.393	0.488	494,453
Years of schooling	7.15	6.61	494,453
Informal (among employed)	0.686	0.464	194,083
Monthly labor income (among employed)	3,480	5,923	194,083
Weekly hours worked (among employed)	38.34	20.04	194,083
<i>Panel B: Households (ENIGH, 2014–2018)</i>			
Household size	3.09	1.65	26,115
Per-capita current income	5,555	16,293	26,115
Per-capita labor income	2,379	4,229	26,115
Per-capita pension income	67.18	157.60	26,115
Per-capita monetary expenditure	3,031	4,105	26,115
Share of formal expenditure	0.501	0.253	26,063
<i>Panel C: Firms (Economic Census, 2018)</i>			
Log total production (Y)	5.68	1.49	1,410,970
Log total workers (L)	1.17	0.64	1,410,970
Log fixed assets (K)	4.46	1.91	1,410,970
Log total income	6.17	1.46	1,410,970
<i>Panel D: Municipalities (2018)</i>			
Δ pension transfers per capita (treatment)	1,000	1,330	2,447
Beneficiaries per capita	0.076	0.042	2,447
Share elderly 65+ (%)	10.92	4.84	2,447
Share active workers (%)	55.24	4.31	2,441
Share informal workers	0.801	0.169	2,441

Notes: This table reports pre-reform summary statistics for the main samples used in the analysis. Panel A uses ENOE individual data for 2015–2018, restricted to individuals ages 60–73. Labor income and hours are conditional on employment. Panel B uses ENIGH household level data for 2014–2018, restricted to households with at least one member ages 60–73; monetary values are in pesos per capita. Panel C reports establishment level statistics from the 2019 Economic Census (reference year 2018). Panel D reports municipality level statistics for 2018. Pensions records come from *Secretaría del Bienestar*, and the employment shares are from INEGI's ILMM.

Table B.3: Decomposition of the Formal Consumption Share Across the Expenditure Distribution

Expenditure ventiles	Formal consumption share			Group level		Product-code level	
	Start (1)	End (2)	Change (3)	Across (4)	Within (5)	Across (6)	Within (7)
1 → 5	34.9	44.5	9.6	1.1	8.5	2.0	7.6
5 → 10	44.5	50.5	6.0	0.1	5.9	0.3	5.7
10 → 15	50.5	56.1	5.6	-0.3	5.9	-0.4	6.0
15 → 20	56.1	67.8	11.7	-4.4	16.1	-4.5	16.2
All (1 → 20)	34.9	67.8	32.9	-3.4	36.3	-2.5	35.4

Notes: This table reports the change in the formal share of consumption between ventiles of per capita expenditure, and how much of it comes from households buying the same products in formal outlets more often (Within) or from buying a different mix of products (Across). Households in the 2014–2018 ENIGH waves are grouped into ventiles of monthly per capita expenditure (2018 pesos), after trimming the top and bottom 1% of per capita expenditure. Each row goes from the ventile on the left of the arrow to the ventile on the right. Columns (1) and (2) report the formal share (%) in the first and last ventile of each row, and column (3) the change in percentage points. Between two neighboring ventiles, the within part is the change in each product’s formal share, weighted by its budget share, and the across part is the change in each product’s budget share, weighted by its formal share. Weights are averages over the two ventiles. Each row adds up these parts over the ventiles it covers, so across and within add up to column (3). Columns (4) and (5) treat each of the 39 spending groups as a product (for example, dairy), so the within part also picks up changes in the mix of products inside a group. Columns (6) and (7) use the 647 ENIGH product codes (for example, pasteurized milk, powdered milk, fresh cheese, Oaxaca cheese, cream, or butter within the dairy group). The share of formal expenditure is classified based on the place of purchase. Statistics do not use survey weights.

C.3.2 Households Results Section

Table B.4: Household Characteristics Comparison (Treated and Control)

	Control			Treatment		
	Mean	SD	Median	Mean	SD	Median
<i>Panel A: Demographics</i>						
Household size	3.277	1.641	3.000	3.043	1.654	3.000
Share of male members (%)	46.88	25.20	50.00	45.43	26.91	50.00
Share of members under 18 (%)	7.68	14.18	0.00	6.12	12.88	0.00
Head's years of schooling	5.19	2.73	4.00	4.57	2.77	4.00
Number of living children	3.72	2.63	3.00	4.32	3.20	4.00
Has Social Security (%)	32.64	46.89	0.00	23.98	42.70	0.00
Observations	12,734			13,381		
<i>Panel B: Earnings</i>						
Income per capita (MXN)	6,370	25,431	3,787	5,613	9,333	3,470
Share active workers (%)	54.83	31.74	50.00	45.59	33.82	50.00
Share formal workers (%)	28.70	39.69	0.00	24.32	38.39	0.00
Monetary spending per capita (MXN)	3,412	4,045	2,231	3,183	4,921	2,022
Food and Beverage spending share (%)	66.03	22.74	66.77	67.88	18.82	69.15
Formal spending share (%)	51.10	24.05	51.16	49.52	24.32	48.81
Observations	12,734			13,381		
<i>Panel C: Assets</i>						
Owns car (%)	46.68	49.89	0.00	40.67	49.12	0.00
Owns refrigerator (%)	89.66	30.45	100.00	88.16	32.31	100.00
Owns washing machine (%)	71.62	45.09	100.00	69.59	46.00	100.00
Owns microwave (%)	47.22	49.92	0.00	45.71	49.82	0.00
Owns computer (%)	28.45	45.12	0.00	22.95	42.05	0.00
Observations	12,734			13,381		

Notes: This table reports summary statistics for the treated (households with a member above the 65 age-eligibility threshold,) and control (households with members near the eligibility threshold) groups, using household ENIGH data for the pre-reform waves (2016–2018). Panel A reports demographic characteristics, Panel B earnings and spending behavior, and Panel C durable-asset ownership. Shares and ownership indicators are expressed in percent. Observations denote the number of households in each group.

Table B.5: Household Characteristics by Socioeconomic Status

	Low Income	Middle Income	High Income
<i>Panel A: Demographics</i>			
Household size	3.236 (1.685)	3.274 (1.712)	2.964 (1.532)
Share of male members	0.477 (0.245)	0.465 (0.257)	0.449 (0.272)
Share of members under 18	0.078 (0.145)	0.077 (0.141)	0.053 (0.121)
Head's years of schooling	2.892 (1.740)	4.300 (2.321)	6.572 (2.772)
Head's age	64.290 (8.253)	63.147 (8.710)	63.108 (8.506)
Head is male	0.742 (0.438)	0.662 (0.473)	0.638 (0.481)
Number of living children	5.358 (3.591)	4.273 (2.931)	3.078 (2.264)
Head is married or cohabiting	0.582 (0.493)	0.627 (0.484)	0.651 (0.477)
Has Social Security	0.070 (0.256)	0.273 (0.445)	0.394 (0.489)
<i>Panel B: Earnings and Spending</i>			
Per-capita current income	2403.505 (3235.040)	4365.430 (7625.289)	9842.522 (30220.870)
Per-capita labor income	1166.268 (2243.503)	2116.676 (2876.245)	4091.572 (7887.767)
I(labor income)	0.880 (0.324)	0.843 (0.364)	0.769 (0.421)
Share of Active Workers	0.597 (0.315)	0.502 (0.326)	0.455 (0.337)
Share of Formal Workers	0.047 (0.180)	0.230 (0.364)	0.440 (0.437)
Per-capita pension income	99.139 (196.247)	60.251 (146.576)	32.342 (106.707)
Per-capita monetary expenditure	1549.175 (1458.787)	2563.880 (2729.976)	5098.128 (6400.388)
Share of formal expenditure	0.347 (0.233)	0.479 (0.221)	0.607 (0.225)
<i>Panel C: Durable Assets</i>			
Owens a car	0.227 (0.419)	0.385 (0.487)	0.603 (0.489)
Owens a refrigerator	0.663 (0.473)	0.905 (0.293)	0.972 (0.165)
Owens a washing machine	0.393 (0.488)	0.687 (0.464)	0.877 (0.329)
Owens a microwave	0.143 (0.350)	0.402 (0.490)	0.699 (0.459)
Owens a computer	0.035 (0.184)	0.184 (0.387)	0.458 (0.498)

Notes: This table reports pre-reform means and standard deviations (in parentheses) of household characteristics by income class for households with at least one member aged 60–73. Income classes follow the INEGI classification. Panel A reports demographic characteristics; Panel B reports earnings and spending variables; Panel C reports ownership of durable assets (indicators equal to one if the household owns at least one of the item). All monetary values are in pesos per capita. The share of formal expenditure is classified based on the place of purchase following [Bachas et al. \(2024\)](#).

Table B.6: Marginal Expenditure Response by Asset Quartiles

	Formal (1)	Informal (2)	Classifiable (3)	Total (4)
<i>IV by Asset-index Quartile</i>				
Q1: Lowest assets	0.032 (0.029)	0.084*** (0.026)	0.118*** (0.041)	0.101 (0.065)
Q2	0.066* (0.040)	0.044* (0.026)	0.105** (0.049)	0.314*** (0.088)
Q3	0.167*** (0.060)	0.029 (0.030)	0.196*** (0.069)	0.265** (0.124)
Q4: Highest assets	0.177 (0.120)	0.043 (0.038)	0.226* (0.128)	0.284 (0.254)
$p(Q4 > Q1)$, 1-tail	0.121	0.813	0.211	0.243
First-stage F	1279.8	1279.8	1279.8	1279.9
Observations	94,683	94,683	94,683	94,685

Notes: This table reports two-stage least squares estimates of marginal expenditure per peso of pension income by estimating Equation (25) with pension income interacted with quartiles of the asset-index distribution. Column (1) reports expenditure at formal establishments, column (2) expenditure at informal establishments, column (3) total expenditure on the same set of goods for which place of purchase can be classified (see Table B.19), and column (4) total monetary expenditure across all categories. All expenditure measures are expressed in 2018 MXN. For each asset quartile, pension income is instrumented with the corresponding quartile-specific interaction between pension eligibility and the post-reform period. The specification allows the pension response to vary across quartiles and therefore does not impose the linear gradient used in Equation (49). All specifications include asset-quartile-by-state, asset-quartile-by-year, and state-by-year fixed effects, together with household controls. The row $p(Q4 > Q1)$ reports the one-sided test that the marginal expenditure response in the highest asset quartile exceeds that in the lowest quartile. The first-stage F -statistic is the Kleibergen–Paap Wald statistic. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3.3 Firms and Municipality Results Section

Table B.7: Firms Characteristics 2018 (Economic Census)

	Bottom 50%			Top 50%		
	Mean	SD	Median	Mean	SD	Median
Log Total production (Y)	5.73	1.53	5.66	5.58	1.41	5.52
Log Total income	6.23	1.50	6.07	6.07	1.39	5.95
Log Total workers (L)	1.19	0.67	1.10	1.13	0.59	1.10
Log Fixed assets (K)	4.54	1.93	4.50	4.32	1.87	4.32
Log Average labor costs per worker	3.55	0.77	3.60	3.46	0.74	3.52
Observations	915 471			495 499		

Notes: The table reports pre-reform (2018) firm-level summary statistics by municipality-level exposure group. We restrict the sample to firms present in the 2018 and 2023 Economic Census, and with above 1 worker. Control and Treated refer to firms in municipalities below and above the state median of the treatment exposure variable, respectively. [Back to Section 6]

Table B.8: Balance regressions for firm outcomes

	(1) Log Total production (Y)	(2) Log Total income	(3) Log Total workers (L)	(4) Log Fixed assets (K)	(5) Log Average labor costs per worker
Treated Municipality	0.0164 (0.0825)	-0.0050 (0.0709)	-0.0077 (0.0263)	0.0038 (0.0324)	-0.0172 (0.0405)
State fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	62	62	62	62	62
R ²	0.8941	0.9061	0.8949	0.9761	0.8551

Notes: Each column reports a separate weighted regression using state-by-treatment-cell means. Regressions are weighted by the number of establishments in each cell and include state fixed effects. Treated Municipality equals one for municipalities above the treatment cutoff. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [Back to Section 6]

Table B.9: Effects on firm outcomes: All firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Exposure Q1 vs Q4		Control for Y_{2018}		Predicted Treatment Exposure					
	All	Formality	MRPL $\left(\frac{Y}{L}\right)$	MRPL $\left(\frac{VA}{L}\right)$	Formality	MRPL $\left(\frac{Y}{L}\right)$	Formality	MRPL	Formality	MRPL $\left(\frac{Y}{L}\right)$
<i>Panel A: Labor</i>										
Post $\times$ Treat	0.002 (0.002)									
Post $\times$ Treat $\times$ Informal		0.001 (0.002)			0.001 (0.003)		0.000 (0.002)		0.001 (0.002)	
Post $\times$ Treat $\times$ Formal		0.008** (0.003)			0.013*** (0.004)		0.003 (0.003)		0.007** (0.003)	
Post $\times$ Treat $\times$ Low Productivity			0.000 (0.003)	-0.000 (0.003)		0.000 (0.004)		0.000 (0.003)		-0.001 (0.003)
Post $\times$ Treat $\times$ High Productivity			0.003 (0.003)	0.004 (0.003)		0.007** (0.003)		0.004 (0.003)		0.006** (0.003)
Observations	2,082,378	2,082,378	2,082,378	2,026,874	1,113,798	1,113,798	2,082,378	2,082,378	2,082,378	2,082,378
<i>Panel B: Total sales</i>										
Post $\times$ Treat	-0.004 (0.006)									
Post $\times$ Treat $\times$ Informal		-0.001 (0.008)			-0.017 (0.011)		-0.009 (0.011)		-0.005 (0.008)	
Post $\times$ Treat $\times$ Formal		0.008 (0.007)			0.004 (0.009)		-0.016* (0.009)		-0.005 (0.008)	
Post $\times$ Treat $\times$ Low Productivity			-0.021** (0.009)	-0.023*** (0.008)		-0.037*** (0.012)		-0.023** (0.010)		-0.021** (0.009)
Post $\times$ Treat $\times$ High Productivity			0.011* (0.006)	0.011* (0.006)		0.006 (0.008)		0.018** (0.009)		0.002 (0.007)
Observations	2,082,322	2,082,322	2,082,322	2,026,820	1,113,774	1,113,774	2,082,322	2,082,322	2,082,322	2,082,322
<i>Panel C: Total production</i>										
Post $\times$ Treat	-0.004 (0.007)									
Post $\times$ Treat $\times$ Informal		0.000 (0.009)			-0.013 (0.012)		-0.009 (0.012)		-0.004 (0.009)	
Post $\times$ Treat $\times$ Formal		0.007 (0.007)			0.008 (0.009)		-0.019* (0.010)		-0.006 (0.008)	
Post $\times$ Treat $\times$ Low Productivity			-0.021** (0.010)	-0.024*** (0.009)		-0.032** (0.013)		-0.025** (0.011)		-0.020** (0.010)
Post $\times$ Treat $\times$ High Productivity			0.012* (0.007)	0.011 (0.007)		0.010 (0.008)		0.022** (0.009)		0.002 (0.007)
Observations	2,082,378	2,082,378	2,082,378	2,026,874	1,113,798	1,113,798	2,082,378	2,082,378	2,082,378	2,082,378
Heterogeneity variable		Formality status	Productivity $\left(\frac{Y}{L}\right)$ median	Productivity $\left(\frac{VA}{L}\right)$ median	Formality status	Productivity $\left(\frac{Y}{L}\right)$ median	Formality status	Productivity $\left(\frac{Y}{L}\right)$ median	Formality status	Productivity $\left(\frac{Y}{L}\right)$ median

Notes: This table reports estimates from firm-level regressions using a balanced panel of firms observed in both the 2018 and 2023 Economic Census. Standard errors, clustered at the municipality $\times$ year level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. [Back to Section 6.4]

Table B.10: Municipality Characteristics

	Bottom 50%			Top 50%		
	Mean	SD	Median	Mean	SD	Median
<i>Panel A: Demographics</i>						
Total population	72,737	163,773	24,150	38,113	127,229	11,874
Share 65 and older (%)	7.75	1.99	7.80	12.44	3.60	11.99
Share women (%)	50.81	1.19	50.92	50.74	1.50	50.88
Share 65+ receiving pension, pre-reform (%)	38.72	9.48	39.01	42.62	8.92	42.70
Share 65+ receiving pension, post-reform (2021) (%)	72.15	9.10	72.70	78.63	72.89	74.60
Excess mortality 2018 (%)	-3.28	12.93	-1.95	-3.22	16.04	-2.47
Excess mortality 2021, COVID (%)	59.29	73.81	50.29	56.38	114.91	46.08
Observations	1,024			1,046		
<i>Panel B: Local market</i>						
Gross output (thousands MXN)	12,508	48,116	431	4,608	27,807	108
Number of firms	1,521	3,792	432	874	3,473	162
Number of employees	13,774	48,272	1,865	6,017	30,959	610
Share formal firms (%)	29.67	20.15	26.31	32.81	20.28	29.92
Share formal employment (%)	46.91	27.50	47.11	45.28	25.14	46.20
Share formal sales (%)	67.78	28.69	77.61	65.76	29.42	76.86
Share non-tradable output (%)	59.21	28.87	68.14	65.70	26.02	74.48
Share non-tradable formal sales (%)	67.05	27.81	77.76	65.67	28.79	77.14
Observations	1,024			1,046		
<i>Panel C: Income distribution</i>						
Income per capita (MXN)	4,189	1,893	4,035	3,917	1,546	3,833
Standard Deviation	5,118	6,951	3,675	4,535	6,061	3,533
Observations	1,024			1,046		

Notes: This table reports pre-reform summary statistics for municipalities in the sample, split by treatment intensity. “Bottom 50” and “Top 50%” denote municipalities below and above the within-state median of the change in per capita pension transfers between the pre- and post-reform periods. Panel A reports demographic characteristics; Panel B reports local labor market characteristics. Panel C reports moments of the income distribution. All characteristics are measured in 2018, except where a different year is explicitly indicated

Table B.11: Effect on Employment and Output at the Municipal Level (Census)

	Top 50% vs Bottom 50% (1)	Top 25% vs Bottom 25% (2)	Treatment intensity (3)	Population weights (4)	Entropy balancing (5)	COVID Shock (6)	No FE (7)
Panel A: Employment							
Share Formal	0.024*** (0.007)	0.027** (0.011)	0.012* (0.007)	0.023*** (0.006)	0.028*** (0.008)	0.024*** (0.007)	0.010 (0.006)
All	0.022 (0.015)	0.000 (0.025)	-0.011 (0.015)	0.003 (0.010)	0.013 (0.018)	0.021 (0.015)	0.027** (0.014)
Formal	0.063** (0.029)	0.052 (0.049)	0.026 (0.027)	0.034** (0.017)	0.065* (0.036)	0.059** (0.029)	0.035 (0.026)
Informal	-0.038 (0.023)	-0.062 (0.038)	-0.055** (0.027)	-0.086*** (0.029)	-0.052* (0.027)	-0.039* (0.023)	0.004 (0.022)
Panel B: Sales							
Share Formal	0.025*** (0.009)	0.010 (0.015)	0.013 (0.008)	0.021 (0.013)	0.026*** (0.010)	0.024*** (0.009)	0.010 (0.008)
All	0.061*** (0.022)	0.011 (0.038)	-0.002 (0.021)	0.031 (0.019)	0.075*** (0.026)	0.059*** (0.022)	0.059*** (0.021)
Formal	0.100*** (0.039)	-0.018 (0.064)	0.022 (0.032)	0.053 (0.033)	0.111** (0.047)	0.095** (0.039)	0.071** (0.034)
Informal	-0.070 (0.047)	-0.079 (0.077)	-0.117*** (0.041)	-0.175* (0.097)	-0.045 (0.047)	-0.071 (0.047)	0.019 (0.041)
Observations	4,085	2,057	4,085	4,085	4,085	4,085	4,085

Notes: This table reports alternative specifications for the municipality-level balanced panel of firms analysis in equation (30). Panel A reports effects on employment and Panel B on sales; within each panel, the outcomes are the formal share and total, formal, and informal activity. All outcomes are in logs except for the formal shares. Column (1) reproduces the baseline specification, splitting municipalities at the median of the change in per capita transfers. Column (2) restricts the comparison to municipalities in the top and bottom quartiles. Column (3) uses the change in per capita transfers (per \$1,000) as a continuous treatment variable. Column (4) weights municipalities by population. Column (5) reweights low-treatment municipalities via entropy balancing to match high-treatment municipalities on pre-reform population, number of firms, and gross output. Column (6) adds quartiles of COVID-19 excess mortality during 2021 interacted with year fixed effects. Column (7) includes only municipality and state-by-year fixed effects and drops the additional controls. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.12: Effect on Sales, Quantities Sold and Prices at the Municipal Level (Retail)

	Top 50% vs Bottom 50% (1)	Top 25% vs Bottom 25% (2)	Treatment intensity (3)	Population weights (4)	Entropy balancing (5)	COVID Shock (6)	No FE (7)
Panel A: All							
Sales	0.040*** (0.013)	0.049** (0.022)	0.066*** (0.022)	0.012 (0.011)	0.053*** (0.019)	0.040*** (0.013)	0.066*** (0.012)
Quantity	0.039*** (0.013)	0.037* (0.022)	0.055** (0.022)	0.014 (0.011)	0.044** (0.019)	0.039*** (0.013)	0.065*** (0.012)
Price	0.004 (0.003)	0.016*** (0.005)	0.014*** (0.005)	-0.001 (0.003)	0.014** (0.005)	0.004 (0.003)	0.004 (0.003)
Panel B: Highly Exposed Products							
Sales	0.037** (0.014)	0.041* (0.023)	0.060** (0.023)	0.005 (0.012)	0.050** (0.021)	0.037** (0.014)	0.061*** (0.014)
Quantity	0.034** (0.014)	0.029 (0.022)	0.049** (0.023)	0.007 (0.011)	0.045** (0.020)	0.035** (0.014)	0.060*** (0.013)
Price	0.004 (0.003)	0.013*** (0.005)	0.014*** (0.005)	-0.003 (0.003)	0.009** (0.004)	0.004 (0.003)	0.003 (0.003)
Observations	82,752	41,419	82,752	82,752	82,752	82,752	82,752

Notes: This table reports alternative specifications for the retail analysis in equation (30) for the balanced stores that were open before 2018 up to 2023. Because the retail data goes from 2018-2023 at the municipality-by-month level, the reported coefficient is on the treatment indicator interacted with a post-universalization dummy, so it captures the average effect over the post period relative to the pre-reform period. Panel A includes all products and Panel B the highly exposed product categories; within each panel, the outcomes are sales, quantities sold, and the sales-weighted average price, all in logs. Treatment is an indicator for municipalities above the median predicted change in pension transfers per capita (see Section 6.1.3). Column (1) reproduces the baseline specification. Column (2) restricts the comparison to municipalities in the top and bottom quartiles of predicted treatment. Column (3) uses the predicted change in per capita transfers (per \$1,000) as a continuous treatment variable. Column (4) weights municipalities by population. Column (5) reweights low-treatment municipalities via entropy balancing to match high-treatment municipalities on pre-reform population, number of firms, and gross output. Column (6) adds quartiles of COVID-19 excess mortality during 2021 interacted with year fixed effects. Column (7) includes only municipality, municipality-by-calendar-month, and state-by-year fixed effects, dropping the additional controls. All specifications include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $p < 0.1$.

Table B.13: Effect on Sales, Quantities Sold and Prices at the Municipal Level (Retail-Unbalanced)

	Top 50% vs Bottom 50% (1)	Top 25% vs Bottom 25% (2)	Treatment intensity (3)	Population weights (4)	Entropy balancing (5)	COVID Shock (6)	No FE (7)
Panel A: All							
Sales	0.040*** (0.013)	0.051** (0.021)	0.058*** (0.022)	0.011 (0.012)	0.055*** (0.019)	0.041*** (0.013)	0.064*** (0.012)
Quantity	0.040*** (0.013)	0.042** (0.021)	0.047** (0.022)	0.013 (0.011)	0.049*** (0.019)	0.040*** (0.013)	0.065*** (0.012)
Price	0.003 (0.003)	0.014*** (0.005)	0.015*** (0.005)	-0.002 (0.003)	0.011** (0.006)	0.003 (0.003)	0.004 (0.003)
Panel B: Highly Exposed Products							
Sales	0.040*** (0.014)	0.046** (0.022)	0.054** (0.024)	0.005 (0.012)	0.058*** (0.021)	0.040*** (0.014)	0.063*** (0.013)
Quantity	0.037*** (0.014)	0.036* (0.022)	0.040* (0.023)	0.007 (0.011)	0.053*** (0.020)	0.037*** (0.014)	0.061*** (0.013)
Price	0.004 (0.003)	0.011** (0.005)	0.016*** (0.005)	-0.003 (0.003)	0.009** (0.005)	0.004 (0.003)	0.003 (0.003)
Observations	91,816	45,580	91,816	83,480	83,480	91,816	91,816

This table reports alternative specifications for the retail analysis in equation (30), using all stores of the retail chain and thus allowing for store entry and exit within the sample period. Because the retail data goes from 2018-2023 at the municipality-by-month level, the reported coefficient is on the treatment indicator interacted with a post-universalization dummy, so it captures the average effect over the post period relative to the pre-reform period. Panel A includes all products and Panel B the highly exposed product categories; within each panel, the outcomes are sales, quantities sold, and the sales-weighted average price, all in logs. Treatment is an indicator for municipalities above the median predicted change in pension transfers per capita (see Section 6.1.3). Column (1) reproduces the baseline specification. Column (2) restricts the comparison to municipalities in the top and bottom quartiles of predicted treatment. Column (3) uses the predicted change in per capita transfers (per \$1,000) as a continuous treatment variable. Column (4) weights municipalities by population. Column (5) reweights low-treatment municipalities via entropy balancing to match high-treatment municipalities on pre-reform population, number of firms, and gross output. Column (6) adds quartiles of COVID-19 excess mortality during 2021 interacted with year fixed effects. Column (7) includes only municipality, municipality-by-calendar-month, and state-by-year fixed effects, dropping the additional controls. All specifications include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $p < 0.1$.

Table B.14: Effect on Sales, Quantities Sold and Prices at the Municipal Level (Retail-Realized Transfers Change)

	Top 50% vs Bottom 50% (1)	Top 25% vs Bottom 25% (2)	Treatment intensity (3)	Population weights (4)	Entropy balancing (5)	COVID Shock (6)	No FE (7)
Panel A: All							
Sales	0.046*** (0.014)	0.071*** (0.023)	0.096*** (0.022)	0.027** (0.013)	0.025 (0.026)	0.046*** (0.014)	0.073*** (0.013)
Quantity	0.043*** (0.014)	0.061*** (0.023)	0.086*** (0.022)	0.028** (0.012)	0.030 (0.025)	0.044*** (0.014)	0.071*** (0.013)
Price	0.006* (0.003)	0.015*** (0.006)	0.014** (0.006)	0.000 (0.003)	-0.001 (0.006)	0.005 (0.003)	0.006* (0.003)
Panel B: Highly Exposed Products							
Sales	0.040** (0.015)	0.059** (0.026)	0.084*** (0.025)	0.018 (0.014)	0.026 (0.027)	0.040*** (0.016)	0.065*** (0.014)
Quantity	0.035** (0.015)	0.049** (0.025)	0.074*** (0.024)	0.017 (0.013)	0.024 (0.025)	0.036** (0.015)	0.063*** (0.013)
Price	0.006* (0.003)	0.010* (0.005)	0.013** (0.006)	0.001 (0.003)	0.003 (0.005)	0.006* (0.003)	0.004 (0.003)
Observations	82,752	42,021	82,752	82,752	82,752	82,752	82,752

Notes: This table reports alternative specifications for the retail analysis in equation (30) for the balanced stores that were open before 2018 up to 2023. Because the retail data goes from 2018-2023 at the municipality-by-month level, the reported coefficient is on the treatment indicator interacted with a post-universalization dummy, so it captures the average effect over the post period relative to the pre-reform period. Panel A includes all products and Panel B the highly exposed product categories; within each panel, the outcomes are sales, quantities sold, and the sales-weighted average price, all in logs. Treatment is an indicator for municipalities above the median realized change in pension transfers per capita between 2018 and 2019, instead of the predicted change used in the main specification. Column (1) reproduces the baseline specification. Column (2) restricts the comparison to municipalities in the top and bottom quartiles of predicted treatment. Column (3) uses the predicted change in per capita transfers (per \$1,000) as a continuous treatment variable. Column (4) weights municipalities by population. Column (5) reweights low-treatment municipalities via entropy balancing to match high-treatment municipalities on pre-reform population, number of firms, and gross output. Column (6) adds quartiles of COVID-19 excess mortality during 2021 interacted with year fixed effects. Column (7) includes only municipality, municipality-by-calendar-month, and state-by-year fixed effects, dropping the additional controls. All specifications include municipality-by-calendar-month fixed effects to absorb municipality-specific seasonality. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $p < 0.1$.

Table B.15: Effect on Formal Employment and Sales across Municipalities by Recipients' Average Economic Status

	Marginal Recipient (1)	65+ Household (2)	All Household (3)	Income per Capita (4)
Panel A: Employment				
Low	0.011 (0.011)	0.005 (0.011)	0.003 (0.011)	-0.003 (0.011)
Middle	0.015 (0.011)	0.026** (0.011)	0.028*** (0.011)	0.036*** (0.011)
High	0.039*** (0.011)	0.037*** (0.012)	0.036*** (0.012)	0.034*** (0.012)
Observations	3,820	3,842	3,844	3,842
Panel B: Sales				
Low	0.008 (0.016)	0.005 (0.015)	0.007 (0.015)	0.004 (0.015)
Middle	0.030** (0.013)	0.031** (0.014)	0.029** (0.014)	0.033*** (0.012)
High	0.023* (0.014)	0.030** (0.014)	0.027* (0.014)	0.033** (0.016)
Observations	3,820	3,842	3,844	3,842

Notes: This table reports estimates of the treatment effect across terciles of alternative measures of recipients' pre-reform economic status. The dependent variables are the formal shares of employment (Panel A) and sales (Panel B), constructed from the balanced panel of firms in the Economic Census. Within each column, municipalities are divided into terciles of the corresponding pre-reform measure, and the rows report the estimated effect of high pension-transfer exposure in municipalities in the low, middle, and high terciles, respectively. Column (1) uses the average asset index of marginal-recipient households, defined as households with individuals age 65 or older who did not receive government transfers before the reform. Column (2) uses the average asset index among all households with individuals age 65 or older, column (3) the average asset index among all households, and column (4) municipal income per capita. All specifications include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, population, and number of firms, each interacted with year. We additionally include tercile-by-year fixed effects for the economic-status measure used in each column. Standard errors clustered at the municipality level are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.16: Effect on Formal Employment and Sales across Municipalities by Recipients' Average Economic Status

	Marginal Recipient (1)	65+ Household (2)	All Household (3)	Income per Capita (4)
Panel A: Employment				
High transfers $\times$ Post	0.022*** (0.007)	0.022*** (0.007)	0.022*** (0.007)	0.023*** (0.007)
High transfers $\times$ Post $\times$ Economic Status	0.015** (0.006)	0.016*** (0.006)	0.016*** (0.006)	0.018*** (0.006)
Observations	3,820	3,842	3,844	3,842
Panel B: Sales				
High transfers $\times$ Post	0.021** (0.009)	0.022** (0.009)	0.021** (0.009)	0.024*** (0.009)
High transfers $\times$ Post $\times$ Economic Status	0.011 (0.008)	0.009 (0.008)	0.007 (0.008)	0.017* (0.009)
Observations	3,820	3,842	3,844	3,842

Notes: This table reports estimates of the treatment effect and its gradient with respect to alternative measures of pre-reform economic status. The dependent variables are the formal shares of employment (Panel A) and sales (Panel B), constructed from the balanced panel of firms in the Economic Census. The row *High transfers $\times$ Post* reports the treatment effect when the corresponding economic-status measure equals zero, while *High transfers $\times$ Post $\times$ Economic Status* reports how the treatment effect changes as economic status increases. Columns (1)–(3) use averages of a household asset index that is standardized using the national pre-reform household distribution before aggregation to the municipality level. Thus, a one-unit increase in these measures corresponds to a one-standard-deviation increase in household assets relative to the national household distribution. Column (1) uses the average asset index of marginal-recipient households, defined as households with individuals age 65 or older who did not receive government transfers before the reform. Column (2) uses the average asset index among households with individuals age 65 or older, and column (3) uses the average asset index among all households. Column (4) uses standardized municipal income per capita. All specifications include municipality fixed effects, state-by-year fixed effects, and quartiles of baseline informality, population, and number of firms, each interacted with year. They additionally include year fixed effects interacted with the continuous economic-status measure and tercile-by-year fixed effects for the corresponding measure. Standard errors clustered at the municipality level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3.4 Others

Table B.17: Classification of expenditure into tradable and non-tradable, by consumption category (ENIGH)

Category	Tradable	Non-tradable
Food & beverages	Groceries: cereals, tortillas and bread, meat, fish, dairy and eggs, oils, fruit and vegetables, sugar, packaged and prepared foods, soft drinks, alcohol and tobacco.	Meals away from home: breakfasts, lunches, dinners, snacks and other out of home food.
Transport & vehicles	Purchase of vehicles (cars, pickups, motorcycles, bicycles), spare parts, tires, batteries and accessories; fuels (gasoline, diesel, gas, oils).	Urban and road public-transport fares (metro, bus, taxi, intercity bus, ferry, toll) and vehicle servicing (tuning, alignment, balancing, tire repair, parking, washing).
Communication	Cell phones and accessories, phone cards and airtime	Private telephone line installation and fax services.
Household cleaning & supplies	Cleaning products and supplies: detergents, soap, bleach, softeners, cleaners, paper goods, brooms and mops, matches, batteries, light bulbs, insecticide, containers.	Home services: domestic help, laundry, dry cleaning, gardening, fumigation and pool cleaning.
Personal care	Personal care goods: soap, perfumes, toothpaste, shampoo, deodorant, creams, cosmetics, razors, sanitary products and diapers.	Personal care services: haircuts and styling, baths and massages, perms and dyeing, manicures, shaving and waxing.
Education	School supplies and books: textbooks, school equipment (e.g. calculators) and reading or coloring materials.	Tuition at all levels (preschool to graduate), technical and special education, childcare, school transport, recurring school fees and equipment repair.
Recreation & culture	Recreation goods: TVs, radios and stereos, computers and tablets, DVD/Blu-ray players, cameras, video games, books, newspapers, magazines, music and films.	Recreation services: cinema, theater and concerts, sporting events, lottery, club and gym memberships, equipment rental, photo developing and repairs.
Housing & utilities	Household fuels for cooking and heating: LP gas, kerosene, diesel, charcoal, firewood and candles.	Rent paid and dwelling charges: rent, employer or other housing fees, land rental, garbage collection, surveillance and administration fees.
Clothing & footwear	Garments and footwear for men, women and children: trousers, shirts, dresses, underwear, socks, shoes, fabrics and accessories.	Clothing and footwear services: shoe repair and cleaning, tailoring and mending.
Kitchenware & furnishings	Glassware and utensils (dishes, glasses, cutlery, pots and pans, tools) and home textiles (mattresses, bedding, linens, towels).	Repair and maintenance of these items.
Appliances, furniture & home	Appliances and furniture: refrigerators, stoves, washers, blenders, microwaves, fans, water heaters, sewing machines and furniture.	Repair, maintenance, installation, construction and remodeling services.
Health	Health goods: prescription and over the counter medicines, vitamins, curative material.	Health services: general, specialist and dental consultations, surgery, hospitalization, midwife and ambulance services, and traditional healers.

Table B.18: Products for which the place of purchase is observed (ENIGH)

Products	% of total household expenditure
<i>Panel A. Place of purchase recorded</i>	
Food, beverages and tobacco	39.9
Personal care goods	6.2
Clothing, footwear and accessories	5.2
Cleaning and household care articles	5.2
Vehicles: cars, pickups, motorcycles and bicycles	4.0
Appliances, furniture and home maintenance materials	2.1
Recreation goods	0.6
Glassware, linens and kitchen utensils	0.6
Subtotal	63.9
<i>Panel B. Recorded only for the goods within the category</i>	
Household fuels: LP gas, kerosene, diesel, charcoal, firewood and candles (<i>within housing and utilities</i>)	2.5
Medicines, vitamins, curative material and glasses (<i>within health</i>)	1.4
Motor oils, lubricants and additives (<i>within fuels and vehicle services</i>)	0.2
School supplies, textbooks and reading material (<i>within education and recreation</i>)	0.1
Subtotal	4.1
<i>Panel C. Place of purchase not recorded</i>	
Gasoline, vehicle services and communications (<i>excl. motor oils</i>)	9.0
Capital and financial outlays	8.3
Other expenses and transfers	4.4
Rent, water, electricity and dwelling charges (<i>excl. fuels</i>)	4.2
Tuition, school fees and recreation services (<i>excl. school supplies</i>)	2.8
Health services: consultations, hospitalization, surgery and dental (<i>excl. medicines</i>)	2.1
Transfers in kind	1.2
Public transport fares	0.0
Item non-response	0.1
Subtotal	32.0
Place of purchase recorded	68.0
Total	100.0

Notes: This table reports the share of household expenditure for which the ENIGH records the place of purchase, the variable used to classify each purchase as formal or informal. The survey asks where the item was bought for each of the goods acquired by the household, and does not ask it for services, utility payments or institutional transfers. Panel B has the categories in which only the goods carries the question. Panels A, B and C are mutually exclusive and exhaust household expenditure. Shares are weighed by expenditure across the 2014–2022 waves. Shares may not add up to subtotals because of rounding.

Table B.19: Classification of places of purchase into formal and informal establishments (ENIGH)

Code	ENIGH label (<i>lugar de compra</i>)	Description
<i>Panel A. Informal establishments</i>		
01	Mercado	Public market
02	Tianguis o mercados sobre ruedas	Street market
03	Vendedores ambulantes	Street vendors
17	Persona particular	Purchases from private individuals
04	Tiendas de abarrotes	Corner grocery stores
12	Loncherías, fondas, torterías, cocinas económicas, cenadurías	Small eateries
<i>Panel B. Formal establishments</i>		
05	Tiendas específicas del ramo	Specialized stores (bakeries, butchers, pharmacies, hardware)
06	Supermercados	Supermarkets
07	Tiendas departamentales	Department stores
09	Tiendas con membresía	Membership warehouse clubs
10	Tiendas de conveniencia	Convenience stores
15	Diconsa	Government grocery stores
16	Lechería Liconsa	Government milk outlets
08	Compras fuera del país	Purchases abroad
11	Restaurantes	Restaurants
<i>Panel C. Not classified</i>		
13	Cafeterías	Coffee shops
14	Pulquería, cantina o bar	Bars
18	Internet	Online purchases
00	No aplica	Not applicable or not recorded

Notes: This table lists the categories of the ENIGH place of purchase variable and their assignment to formal and informal establishments. The second column reproduces the official INEGI value labels, which are identical across the 2014 to 2024 waves. Purchases with no store front (markets, *tianguis*, street vendors and private individuals), in corner shops, and in small eateries are classified as informal. Purchases in specialized shops, large stores (supermarkets, department stores and membership clubs), convenience stores, institutions and restaurants are classified as formal. Purchases in coffee shops and bars, online purchases and items without a recorded place of purchase are not assigned to either group.

Table B.20: Principal Component Analysis Household Level Asset Loadings (ENIGH)

	Loading
Vehicles	0.186
Computers	0.193
Fans	0.102
Refrigerator	0.188
Washing machine	0.197
Microwave	0.208
Stove	0.189
Printer	0.160
Vacuum cleaner	0.132
Cement floor	0.119
Finished floor	0.224
Solid walls	0.145
Solid roof	0.213
Piped water inside	0.252
Public sewer	0.200
Flush toilet	0.104
Electricity	0.068
Owns home	0.003
Rooms per capita	0.134
Bedrooms per capita	0.113
Light bulbs	0.244
Clean cooking fuel	0.228
Garbage collection	0.159
Electricity meter	0.112
Shower	0.268
Water tank (roof)	0.192
Cistern	0.142
Water pump	0.149
Water heater	0.233
Kitchen sink	0.258
Laundry sink	0.050
Air conditioning	0.107
Heating	0.073

Notes: This table reports the loadings of the first principal component of the full asset index for households in the ENIGH, estimated by principal component analysis over the 33 household assets and dwelling characteristics listed. The PCA is estimated on the pre-reform waves (2014–2018) using survey sampling weights, and the resulting loadings are projected onto all later waves to construct a time-consistent index. The index is oriented so that higher values denote greater household wealth. Loadings are the correlations between each item and the first component; larger absolute values indicate items that contribute more to the wealth ordering.

Table B.21: Principal Component Analysis Household Level Asset Loadings (Census)

	Loading
Vehicles	0.186
Computers	0.198
Refrigerator	0.198
Washing machine	0.205
Microwave	0.214
Television	0.153
Flat-screen television	0.181
Radio	0.117
Air conditioning	0.102
Cable television	0.160
Cement floor	0.129
Finished floor	0.220
Solid walls	0.160
Solid roof	0.214
Piped water inside	0.233
Public sewer	0.202
Flush toilet	0.267
Exclusive toilet	0.118
Electricity	0.086
Solar panel	0.008
Owns home	-0.005
Owns land	-0.087
Rooms per capita	0.147
Bedrooms per capita	0.119
Light bulbs	0.234
Clean cooking fuel	0.226
Garbage collection	0.146
Shower	0.271
Water tank (roof)	0.154
Cistern	0.111
Water pump	0.140
Water heater	0.241
Kitchen	0.110

Notes: This table reports the loadings of the first principal component of the full asset index for households in the Census 2015, estimated by principal component analysis over the 33 household assets and dwelling characteristics listed. The index is oriented so that higher values denote greater household wealth. Loadings are the correlations between each item and the first component; larger absolute values indicate items that contribute more to the wealth ordering.

Table B.22: Product categories included in the highly exposed group (Retail)

Category	Example products	Products
<i>Panel A. Food and beverages (ENIGH: food and beverages consumed at home)</i>		
Cereals and cereal products	Packaged whole-wheat sliced bread (4-piece bag); vanilla wafer cookies (117 g pack)	9,021
Meat	Cooked turkey ham (500 g package); boneless fresh beef chuck (sold by the kg)	2,718
Fish and seafood	Canned tuna in water (140 g can); smoked tuna (100 g pouch)	210
Milk and dairy products	Drinkable fruit yogurt (150 g bottle); fresh panela cheese (250 g package)	4,540
Eggs	White eggs (18-piece carton); eggs sold by the piece (bulk)	263
Oils and fats	Corn cooking oil (946 ml bottle); pork lard (250 g block)	191
Tubers	White potatoes (1 kg bag); pre-cut jicama (ready-to-eat cup)	280
Vegetables, legumes, and seeds	Dried árbol chili (1 kg bag); chili-coated peanuts (60 g bag)	4,008
Fruits	Gala apples (sold by the kg); pre-cut pineapple (ready-to-eat cup)	2,001
Sugar and honey	Bee honey (350 g jar); strawberry jam (510 g jar)	440
Coffee, tea, and chocolate	Instant decaffeinated coffee (275 g jar); chamomile tea (25-bag box)	811
Spices and dressings	Bottled fresh-chili hot sauce (220 g); ground chili with salt and lime (100 g shaker)	2,633
Other food products	Chamoy-covered strawberries (120 g cup); milk-chocolate bar (125 g)	14,836
Non-alcoholic beverages	Purified still water (1.38 l bottle); mango juice drink (400 ml bottle)	6,623
Alcoholic beverages	Lager beer (355 ml can); reposado tequila (750 ml bottle)	3,031
<i>Panel B. Personal expenses (ENIGH: personal care and health)</i>		
Personal care and health	Children's shampoo (265 ml bottle); adhesive pain-relief patches (10-count box)	6,522
<i>Panel C. Cleaning products (ENIGH: housekeeping articles)</i>		
Household cleaning	Liquid dish detergent (375 ml bottle); disinfectant wipes (15-count pack)	3,014
Total		61,142

Notes: The table lists the product categories included in the highly exposed group, mapped from the retailer's product catalog to the expenditure classification of Mexico's household income and expenditure survey (ENIGH): rubric *alimentos y bebidas consumidas dentro del hogar* for Panel A and the corresponding personal care and household cleaning rubrics for Panels B and C. Example products are actual items from the retailer's catalog with brand names removed. The last column reports the number of distinct products (SKUs) assigned to each category..