Wages and Employment changes associated with the introduction of food taxes and Front-of Package Labeling in Colombia *

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Abstract

This paper examines the short-term labor market effects of Colombia's 2022–2024 health-related reforms, which combined (i) an excise tax on ultra-processed sugary drinks and ultra-processed foods and (ii) the introduction of octagonal front-of-package warning labels on processed and ultra-processed food and beverage. Using nationally representative labor force data and a synthetic difference-in-differences method, we evaluate effects on employment, income, hours worked, and informality in directly and indirectly affected sectors. We find no evidence of statistically significant short-run changes in aggregate employment, labor income, or informality following the implementation of the reform. Estimates for hours worked are generally small and not statistically distinguishable from zero across specifications. However, some results suggest modest increases in weekly hours worked in the non-alcoholic beverage sector, particularly among white-collar, rural, and female workers, pointing to limited adjustment along the intensive margin rather than changes in employment levels. Overall, the evidence indicates that Colombia's fiscal and labeling policies did not generate immediate disruptions in labor market outcomes during their initial implementation. These findings are consistent with evidence from Mexico, Chile, and Peru, and support the view that well-designed taxes and labeling policies targeting unhealthy foods and beverages can advance public health objectives without undermining employment or income stability. The results contribute to the growing evidence base on the economic effects of food-related fiscal and informational measures in middle-income countries, providing relevant guidance for policymakers seeking to balance health and labor market considerations.

Keywords: Food policy; Sugar-sweetened beverages; Ultra-processed foods; Employment; Informality; Colombia

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Abstract

Cambios en salarios y empleo asociados con la introducción de impuestos a alimentos y etiquetado frontal en Colombia

Este artículo examina los efectos de corto plazo en el mercado laboral de las reformas de salud implementadas en Colombia entre 2022 y 2024, las cuales combinaron: (i) un impuesto específico a bebidas azucaradas ultraprocesadas y a ciertos alimentos ultraprocesados, y (ii) la introducción de sellos de advertencia octogonales en el frente del empaque para productos con exceso de nutrientes críticos o con presencia de edulcorantes no calóricos. Utilizando datos representativos a nivel nacional de la fuerza laboral y un método de diferencias en diferencias sintético (SDiD), evaluamos los efectos sobre el empleo, los ingresos laborales, las horas trabajadas y la informalidad en sectores directa e indirectamente afectados.

No encontramos evidencia de cambios estadísticamente significativos en el corto plazo en el empleo agregado, los ingresos laborales o la informalidad tras la implementación de la reforma. Las estimaciones para horas trabajadas son, en general, pequeñas y no estadísticamente diferentes de cero en la mayoría de las especificaciones. No obstante, algunos resultados sugieren incrementos modestos en las horas trabajadas semanales en el sector de bebidas no alcohólicas, particularmente entre trabajadores de cuello blanco, en zonas rurales y entre mujeres, lo que apunta a ajustes limitados en el margen intensivo más que a cambios en los niveles de empleo.

En conjunto, la evidencia indica que las políticas fiscales y de etiquetado implementadas en Colombia no generaron disrupciones inmediatas en los resultados del mercado laboral durante su fase inicial. Estos hallazgos son consistentes con la evidencia existente para México, Chile y Perú, y respaldan la idea de que impuestos y políticas de etiquetado bien diseñados, orientados a reducir el consumo de alimentos y bebidas no saludables, pueden avanzar objetivos de salud pública sin comprometer la estabilidad del empleo o los ingresos. Los resultados contribuyen a la creciente literatura sobre los efectos económicos de las políticas fiscales e informativas relacionadas con alimentos en países de ingreso medio, proporcionando evidencia relevante para formuladores de política interesados en equilibrar consideraciones de salud y mercado laboral.

Palabras clave: Política alimentaria; Bebidas azucaradas; Alimentos ultraprocesados; Empleo; Informalidad; Colombia

1 Introduction

Fiscal and informational policies targeting unhealthy foods and beverages have become increasingly common as governments seek to curb diet-related non-communicable diseases. Taxes on ultra-processed foods (UPFs) and sugar-sweetened beverages (SSBs), together with front-of-package warning labels (FOPWL), aim to reduce consumption of products high in sugar, sodium, or saturated fat. However, their broader economic implications, especially potential employment or wage effects in affected industries, remain a matter of debate. Theoretical models predict divergent outcomes depending on how firms and consumers adapt: without adaptation, reduced demand may lead to lower output, wages, and employment (Powell et al., 2014), whereas reformulation or substitution can mitigate negative impacts or even yield positive effects through innovation and product diversification (Guerrero-López et al., 2017).

Evidence from high-income countries generally shows limited or transitory employment effects from health-related food taxes (Powell et al., 2014). In Latin America, however, higher informality, smaller firm size, and closer ties between domestic consumption and local employment make it crucial to assess these policies' labor implications. Studies from Mexico and Chile report reductions in purchases of high-in products (Colchero et al., 2016; Batis et al., 2016; Caro et al., 2018; Batis et al., 2023) and no significant sectoral job losses (Guerrero-López et al., 2017; Paraje et al., 2021); evidence from Peru points in the same direction (Díaz et al., 2023). Yet, potential adjustments along subtler margins, such as hours worked, informality, or heterogeneous effects by gender, geography, or occupation, remain underexplored.

Colombia offers a relevant case. Law 2277 of 2022 introduced an excise tax on SSBs and UPFs that exceed the thresholds for sugar, sodium, saturated fats, while in accordance with Law 2120 of 2021, Resolution 2492 of 2022 established a black octagonal FOPWL for processed and ultra-processed foods and beverages that exceed the thresholds for sugar, sodium, saturated fats, trans fats, or the presence of non-caloric sweeteners. Implementation between 2023 and 2024 unfolded in a segmented labor market dominated by micro- and small enterprises, creating a unique opportunity to assess whether such "high-in" product regulations affect employment or income.

This paper examines the short-run labor market consequences of these reforms. We address three questions. First, did the policies alter employment and income in directly exposed sectors (non-alcoholic beverages and processed foods)? Second, did firms adjust along other margins such as hours worked or informality? Third, are any effects concentrated among specific worker groups, women, rural workers, or white-collar occupations, rather than representing broad sectoral shocks?

Using monthly and quarterly data from 2022–2024 and a synthetic difference-in-differences (SDiD), we estimate short-run effects on employment, income, hours worked, and informality across sectors with varying exposure. Including hours worked and informality is particularly relevant in Colombia, where adjustments often occur along intensive rather than extensive labor margins. There results are robust to alternative estimation strategies.

Our contribution is threefold. First, we provide the first evidence on the short-term labor market effects of Colombia's comprehensive fiscal and labeling package. Second, we extend the regional literature on the economic consequences of "high-

in" food policies (Batis et al., 2016, 2023; Guerrero-López et al., 2017; Paraje et al., 2021; Caro et al., 2018; Díaz et al., 2023). Third, and new to this literature, we document heterogeneous adjustment patterns: rather than broad employment or wage effects, the reforms are associated in some specifications with modest changes in hours worked in the non-alcoholic beverage sector, particularly among white-collar, rural, and female workers, suggesting limited adjustment along the intensive rather than extensive margin.

2 Policy Context and Timeline

Colombia's food tax and FOPWL reforms emerged from public health and fiscal discussions that began in 2016. Early proposals by the Ministry of Health, supported by civil society and academic organizations, sought to reduce the consumption of SSBs following the World Health Organization's recommendations. Although the first legislative attempt for an SSBs tax failed in 2017, the initiative resurfaced in 2022 when the Ministry of Finance reframed it as part of a broader tax reform package (Congreso de Colombia, 2022), expanding its scope to include UPFs.

The policy process was politically contentious. Business associations such as ANDI (National Business Association of Colombia) and Fenalco (National Federation of Merchants) argued that taxing SSBs and UPFs would disproportionately harm low-income households and threaten employment in manufacturing, retail, and distribution. Media coverage amplified these claims, with headlines such as "Business groups warn that the beverage tax will destroy jobs" (Portafolio, 2022) and "Fenalco: 'The beverage tax will put thousands of jobs at risk'" (El Tiempo, 2022). Similar concerns were voiced in Chile and Mexico during their respective labeling and taxation reforms (Paraje et al., 2021; Batis et al., 2016), though ex-post evaluations found minimal employment effects.

Despite industry opposition, Congress approved the reform in December 2022. The policy introduced both excise taxes on SSBs and UPFs, and it coincided with a new FOPWL norm requiring black octagonal warning symbols alongside transitional round icons, depending on the implementation phase. This phased rollout allowed firms and consumers to adapt gradually. Table 1 summarizes the stages from early announcements (T1) to enforcement (T2–T4), with increasing tax thresholds and progressive adoption of the full octagonal labeling system.

In parallel, the FOPWL scheme evolved from the earlier circular icons to mandatory black octagonal warnings, aligning Colombia's approach with international standards for nutritional warning labels based on scientific evidence.

This phased approach allows for careful identification of timing effects in the econometric analysis. We leverage of these stages in our empirical strategy. For empirical analysis, we define **T2 (November 2023 onward)** as the start of the treatment period, when both the taxes and labeling rules became binding. Later stages (T5, from January 2025), which include higher tax rates and full enforcement of the octagonal system, fall outside our observation window.

Table 1: Timeline of Tax and Labeling Policy Phases in Colombia

Period	Dates	SSB Tax	UPF Tax	FOPWL
T0	Jan 2022 – Oct 2022	No	No	Round
T1	Dec 2022 – Oct 2023	Announcement	Announcement	Round
T2	Nov 2023 – Dec 2023	$< 6g$: no tax $[6, 10)g$: \$18 per 100ml $10g+$: \$35 per 100ml	10% UPF	Round + Octagon
T3	Jan 2024 – Jun 2024	$< 6g$: no tax $[6, 10)g$: \$28 per 100ml $10g+$: \$55 per 100ml	15% UPF	Round + Octagon
T4	Jun 2024 – Dec 2024	$< 6g$: no tax $[6, 10)g$: \$28 per 100ml $10g+$: \$55 per 100ml	15% UPF	Octagon – Full enforcement
T5	Jan 2025 onwards	$< 5g$: no tax $[5, 9)g$: \$38 per 100ml $9g+$: \$65 per 100ml	20% UPF	Octagon

Note: Information compiled from Law 2277 of 2022, Resolution 810 of 2021, and Resolution 2492 of 2022.

3 Methodology

Below, we first describe the data and sample construction. We then estimate the effects of Colombia's food taxes and FOPWL on labor outcomes using a sector-by-quarter SDiD as our primary empirical strategy. The SDiD approach combines the advantages of synthetic control and conventional difference-in-differences methods, improving pre-treatment balance while retaining the panel structure of the data. To assess the robustness of our findings, we complement the baseline SDiD estimates with three alternative approaches: a conventional difference-in-differences (DiD) specification, the synthetic control method (SCM), and interrupted time-series (ITSA) models.

3.1 Data

We use microdata from the *Gran Encuesta Integrada de Hogares* (GEIH), of the Colombian National Administrative Department of Statistics (DANE), a nationally representative labor force survey widely employed to study employment and income dynamics (DANE, 2015). The GEIH provides monthly information on employment status, earnings, hours worked, education, formality, gender, and geographic location at both individual and establishment levels. All individual-level regressions apply GEIH sampling weights; standard errors are clustered at the ISIC 4-digit level unless otherwise noted.

Our sample spans January 2022 to December 2024 (twelve quarters), covering nearly two years before the reform (*pre-treatment:* 2022Q1–2023Q3) and one year after its implementation (*post-treatment:* 2023Q4–2024Q4). Data are aggregated to quarterly averages to align labor-market indicators with the timing of the tax and labeling rollout.

Economic activity is coded using the International Standard Industrial Classification (ISIC) Revision 4 at the four-digit level. The analysis distinguishes five groups of sectors according to their position in the food and beverage value chain and their exposure to the reform: (i) Group A, non-alcoholic beverage production;

(ii) Group B, other food manufacturing; (iii) Group C, wholesale commerce of food and beverages; (iv) Group D, retail commerce of food and beverages; and (v) Group E, mom-and-pop stores. This classification supports the DiD design by comparing labor market outcomes across segments with varying degrees of direct and indirect exposure to the tax and labeling policies.

Groups A and B correspond to the production segment of the food-and-beverage complex, while Groups C and D capture downstream commercialization activities. This broad definition of exposure is consistent with prior evaluations of food taxes and FOPWL, which consider both manufacturing and distribution channels as potentially affected by demand and compliance responses (Paraje et al., 2021). In line with this approach, Colombia's Encuesta Anual Manufacturera (EAM) identifies beverage manufacturing (ISIC 1103, 1104, 8292), other food manufacturing (ISIC 1011, 1012, 1040, 1071, 1089), and food and beverage wholesale and retail commerce (ISIC 4631, 4632, 4724, 4729) as core nodes of the national food-industry chain, spanning production, packaging, distribution, and point-of-sale stages (DANE, 2025). A comparable scope is adopted by the World Trade Organization, which defines the food and beverage sector to include manufacturing, processing, preservation, warehousing, and distribution activities corresponding to ISIC Divisions 15-16 in Revision 3.1 and to Divisions 10-11 in Revision 4 (World Trade Organization, 2024).

We also separately analyze mom-and-pop stores (tiendas de barrio), a subset of retail commerce, given their policy relevance, commonly referred to in Colombia as *tiendas de barrio*. Public debate surrounding the reform frequently emphasized the potential vulnerability of these businesses. These establishments are small, family-run retail outlets that typically sell a limited set of frequently consumed food and beverage products and rely heavily on local foot traffic. The argument used to highligth their vulnerability is that these businesses operate with narrow margins and limited capacity to adjust prices, product mix, or compliance strategies, they were often perceived as particularly exposed to the reforms. We identify shopkeepers based on three criteria: (i) employment in food-related retail ISIC Rev.4 codes (4724, 4729, 4711, and 4755); (ii) establishments with ten or fewer workers; and (iii) workplaces corresponding to fixed or semi-fixed retail locations such as small shops, kiosks, stalls, or dwellings. This definition captures neighborhood-based retail outlets while excluding supermarkets and large retail chains.

Table 2 reports pre- and post-reform descriptive statistics. The left-hand columns summarize individual-level outcomes and characteristics, including income, hours worked, informality, firm size, education, gender, and location, while the right-hand columns present sector-level employment and average earnings by ISIC code. Asterisks denote statistically significant differences in means between periods. Overall, the descriptive patterns suggest rising average income and declining informality in food-related sectors, alongside modest contractions in retail employment. In contrast, unrelated sectors display heterogeneous changes, likely reflecting broader labor-market dynamics rather than the reform itself. These patterns motivate the econometric identification strategy developed in the next section.

Compared to Panel D (retail), Panel E isolates "mom-and-pop" stores, which are substantially more informal, smaller in scale, and more rural. Informality rates are markedly higher and increase further in the post-reform period, while hours worked are higher on average and slightly decline after the reform. This highlights that this subgroup operates under more constrained and informal conditions than the broader

retail sector.

Table 2: Descriptive Statistics by Individual and Sector Characteristics

Individual Level			Sector Level					
Variable	**Mean Pre (1)**	**Mean Post (2)**	**Sector**	**Code**	**Count Pre (3)**	**Count Post (4)**	**Mean Pre (5)**	**Mean Post (6)**
Panel A: Non-Alcoholic Beverages Production								
Labor income	1,370,163	1,652,810	Production of malt beverages	1103	6,113	6,454	1,575,784	1,999,094
Hours worked	47.3	48.0	Production of non-alcoholic beverages	1104	20,633	17,267	1,377,742	1,613,378
Informality	23.4	17.6	Packaging and bottling	8292	4,648	2,704	1,016,110	1,157,482
Small firm	51.8	35.9		-	-	-	-	-
Medium firm	30.9	38.9		-	-	-	-	-
Large firm	17.3	25.2		-	-	-	-	-
Urban	93.2	89.7		-	-	-	-	-
Rural	6.8	10.3		-	-	-	-	-
Female	28.2	29.5		-	-	-	-	-
Male	71.8	70.5		-	-	-	-	-
Primary	9.7	8.4		-	-	-	-	-
Secondary	53.0	55.3		-	-	-	-	-
Tertiary	37.3	36.3		-	-	-	-	-
				-	-	-	-	-
Total (N)	31,394	26,425	*Total (sector)*	-	31,394	26,425	-	-
Panel B: Other Food Production								
Labor income	1,442,361	1,664,593	Meat processing	1011	24,157	19,465	1,446,107	1,593,341
Hours worked	45.9	45.2	Fish processing	1012	2,753	1,689	1,471,099	1,432,963
Informality	32.2	29.3	Dairy products	1040	42,115	31,535	1,255,762	1,470,244
Small firm	44.8	46.6	Sugar refining	1071	11,325	10,165	2,057,016	2,344,354
Medium firm	33.0	29.5	Manufacture of other food products	1089	40,889	29,026	1,540,777	1,817,485
Large firm	22.2	23.9		-	-	-	-	-
Urban	79.7	79.3		-	-	-	-	-
Rural	20.3	20.7		-	-	-	-	-
Female	34.4	33.5		-	-	-	-	-
Male	65.6	66.5		-	-	-	-	-
Primary	14.8	12.0		-	-	-	-	-
Secondary	53.0	53.4		-	-	-	-	-
Tertiary	32.2	34.6		-	-	-	-	-
				-	-	-	-	-
Total (N)	121,239	91,879	*Total (sector)*	-	121,239	91,879	-	-
Panel C: Wholesale Trade								
Labor income	1,565,168	1,815,398	Wholesale of tobacco and beverages	4632	14,531	9,189	1,682,161	1,837,250
Hours worked	48.7	48.0	Wholesale of other food products	4631	74,838	53,995	1,543,250	1,811,581
Informality	34.1	32.0		-	-	-	-	-
Small firm	44.9	42.4		-	-	-	-	-
Medium firm	37.5	40.9		-	-	-	-	-
Large firm	17.6	16.6		-	-	-	-	-
Urban	92.8	92.8		-	-	-	-	-
Rural	7.2	7.2		-	-	-	-	-
Female	33.1	33.3		-	-	-	-	-
Male	66.9	66.7		-	-	-	-	-
Primary	10.5	9.9		-	-	-	-	-
Secondary	54.9	55.7		-	-	-	-	-
Tertiary	34.6	34.4		-	-	-	-	-
				-	-	-	-	-
Total (N)	89,369	63,184	*Total (sector)*	-	89,369	63,184	-	-
Panel D: Retail Trade								
Labor income	742,891	865,359	Retail of tobacco and beverages	4724	64,529	36,993	745,091	881,894
Hours worked	42.4	42.8	Retail of other food products	4729	44,151	25,317	739,645	839,312
Informality	82.8	80.8		-	-	-	-	-
Small firm	89.9	89.2		-	-	-	-	-
Medium firm	7.9	9.5		-	-	-	-	-
Large firm	2.2	1.2		-	-	-	-	-
Urban	80.2	81.8		-	-	-	-	-
Rural	19.8	18.2		-	-	-	-	-
Female	63.4	60.9		-	-	-	-	-
Male	36.6	39.1		-	-	-	-	-
Primary	25.2	25.9		-	-	-	-	-
Secondary	54.3	55.7		-	-	-	-	-
Tertiary	20.6	18.4		-	-	-	-	-
				-	-	-	-	-
Total (N)	108,681	62,310	*Total (sector)*	-	108,681	62,310	-	-
Panel E: Mom and Pop Stores								
Labor income	1,116,745	1,231,816	Retail: non-specialized food stores	4711	8,725	7,057	1,111,921	1,229,216
Hours worked	65.4	63.9	Retail of non-alcoholic beverages	4724	1,934	1,776	599,271	876,044
Informality	91.1	96.0	Retail of other food products	4729	1,425	685	701,864	605,605
Small firm	98.6	93.4	Retail of household equipment	4755	113	242	1,286,667	1,300,000
Medium firm	1.4	6.6		-	-	-	-	-
Large firm	0.0	0.0		-	-	-	-	-
Urban	51.5	45.0		-	-	-	-	-
Rural	48.5	55.0		-	-	-	-	-
Female	66.8	67.0		-	-	-	-	-
Male	33.2	33.0		-	-	-	-	-
Primary	40.8	43.4		-	-	-	-	-
Secondary	43.6	47.8		-	-	-	-	-
Tertiary	15.6	8.8		-	-	-	-	-
				-	-	-	-	-
Total (N)	12,196	9,759	*Total (sector)*	-	12,196	9,759	-	-

Notes: Individual-level statistics (columns 1–2) report pre- and post-reform means for labor outcomes and worker characteristics. Sector-level statistics (columns 3–6) report total employment and average income at the ISIC 4-digit level. All statistics are calculated using sampling weights divided by 36 to obtain monthly averages, as the dataset pools 36 months of nationally representative survey data (2022–2024). Asterisks indicate statistically significant differences in means between pre- and post-periods based on mean comparison tests, applied only to columns reporting averages (columns 1, 2, 5, and 6).

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
Source: Authors' calculations based on GEIH 2022–2024.

3.2 Estimation details and robustness

The empirical strategies employed differ in how they construct the counterfactual trajectory for treated sectors after the reform. The DiD compares the evolution of outcomes in treated sectors (directly exposed to the reform) with that of control sectors (all other sectors of the economy) and identifies the policy effect under a parallel-trends assumption: absent the reform, treated and control sectors would have followed similar trajectories. Synthetic control approaches instead construct a weighted combination of control sectors that best reproduces the treated sectors' pre-treatment trajectory, effectively approximating the counterfactual path implied by parallel trends using a synthetic comparison group. In contrast, interrupted time-series analysis with controls (ITSA) constructs the counterfactual from the pre-intervention trend of the treated unit itself and tests for deviations in level and/or slope after the reform.

Throughout, the post-treatment period begins in 2023Q4 (T2 onward), when the tax and labeling mandates became binding (see Section 2). Across all methods we (i) use the same treated/control definitions, (ii) keep the post period fixed at 2023Q4–2024Q4, and (iii) estimate all main specifications using the same control universe (all other sectors).

Across estimators, inference is implemented using method-specific procedures. We report standard errors clustered at the ISIC 4-digit level for DiD specifications, placebo-based standard errors for SCM and SDiD, and Newey–West standard errors for ITSA models to account for serial correlation.

In addition, we explore heterogeneous effects across worker characteristics. Using the SDiD framework, we estimate specifications that allow treatment effects to vary across key demographic and labor-market dimensions, including gender, age group, education level, occupation type (blue- vs. white-collar), and urban versus rural location. These exercises follow the same identification strategy and treatment timing described above, but are implemented separately for each subgroup to assess whether the policy is associated with differential labor-market adjustments across worker categories.

3.2.1 Difference-in-Differences (DiD)

We begin by outlining the conventional DiD framework that underlies our identification strategy. In the main analysis, DiD models are estimated at the sector-by-quarter level using ISIC 4-digit sectors as observational units. The corresponding specification is:

$$Y_{st} = \alpha_s + \delta_t + \beta \left(\text{Post}_t \times \text{Treat}_s\right) + \varepsilon_{st}, \tag{1}$$

where s indexes ISIC 4-digit sectors and t denotes quarters. Sector fixed effects α_s capture time-invariant sector characteristics and time fixed effects δ_t absorb aggregate shocks common to all sectors. Treat_s indicates that sector s is directly exposed to the reform, while Post_t is an indicator equal to one from 2023Q4 onward. The coefficient β therefore captures the average post-reform difference between treated and control sectors, relative to the pre-treatment period.

Standard errors are clustered at the ISIC 4-digit level to account for serial correlation within sectors.

As an additional robustness exercise, Appendix A reports worker-level DiD estimates using repeated cross-sections of individuals from the GEIH survey over the same period (2022Q1–2024Q4). These models include worker-level covariates such as gender, education, informality status, firm size, and urban/rural location.

3.2.2 Synthetic Control Method (SCM)

Building on the DiD framework, we also consider the synthetic control method (SCM), which constructs a counterfactual for treated sectors by forming a weighted combination of control sectors that best reproduces the treated sectors' pre-treatment outcomes. Unlike conventional DiD, which implicitly compares treated units with an equally weighted set of controls, SCM allows the data to determine the weights assigned to each control sector.

Following Abadie et al. (2010), we use a data-driven algorithm to select weights that minimize the discrepancy between treated and control sectors in the pre-treatment period. Identification relies on the ability of the resulting synthetic control to closely match the treated sectors' pre-intervention trajectory and to provide a credible counterfactual path in the absence of the reform. Inference follows the placebo-based procedure described in Abadie et al. (2010), which reassigns treatment to sectors in the donor pool to generate a distribution of placebo effects.

3.2.3 Synthetic Difference-in-Differences (SDiD)

Our baseline specification implements the SDiD estimator (Arkhangelsky et al., 2021), which combines elements of the SCM weighting approach with the fixed-effects structure of DiD. The estimator constructs unit weights that best reproduce the treated sectors' pre-policy outcomes while incorporating unit and time fixed effects to control for unobserved heterogeneity. This approach improves pre-treatment balance relative to conventional DiD while preserving the panel structure of the data.

We implement SDiD using the open-source `sdid` package (Pailanir et al., 2023), following the implementation guidance in Clarke et al. (2023). Inference is based on placebo-based standard errors obtained by reassigning treatment to units in the donor pool.

3.2.4 Interrupted Time Series with Controls (ITSA)

As an additional robustness specification at the *sector* level, we estimate a comparative ITSA model that allows for potential changes in both *levels* and *trends* around the intervention:

$$Y_{st} = \beta_0 + \beta_1 t + \beta_2 \text{Treat}_s + \beta_3 (t \times \text{Treat}_s) + \beta_4 D_t + \beta_5 (D_t \times \text{Treat}_s) + u_{st}, \quad (2)$$

where Y_{st} denotes a sector-level outcome (e.g., log employment or log average income), t is a linear time trend, and D_t is a post-policy indicator equal to one from 2023Q4 onward. The coefficient β_4 captures the common post-policy level change, while β_5 identifies the differential post-policy shift for treated sectors relative to controls. In additional specifications, we allow for post-policy slope changes

by interacting D_t with t and with $t \times \text{Treat}_s$. Standard errors are computed using Newey–West corrections to account for serial correlation, and fitted versus counterfactual series are presented for visualization following Linden (2015).

4 Results

This section presents the estimated effects of Colombia's new food and beverage taxes and FOPWL on labor market outcomes. We study employment, earnings, hours worked, and informality, using both sector-level and worker-level data.

Overall, we find no evidence of large or systematic disruptions in employment or average earnings in the production sectors (non-alcoholic beverages and food manufacturing) or commerce sectors (wholesale and retail commerce) during the first year after implementation. Point estimates are generally small and, with rare exceptions, statistically indistinguishable from zero. Where we do detect significant changes, they tend to be concentrated in specific subgroups (for example, women, rural workers, and white-collar occupations) rather than reflecting broad sector-wide shocks.

4.1 Employment: Number of Workers

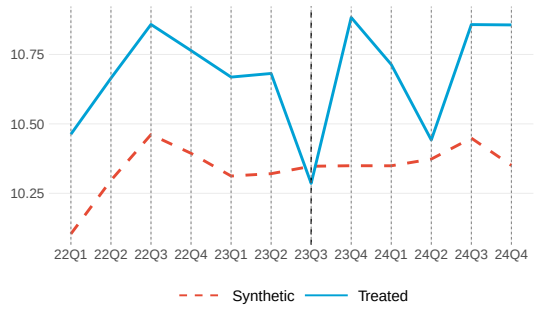
We begin by asking whether the reform is associated with changes in aggregate employment in treated sectors. Figure 1 presents the SDiD results by plotting, for each sector group, the quarterly average of log employment in treated sectors alongside the SDiD-constructed synthetic comparison group. The figure therefore provides a transparent visual assessment of pre-reform fit and post-reform divergence between treated sectors and their synthetic counterfactual.

Table 3 reports the corresponding post-reform treatment effects and compares estimates across three methods (SDiD, SCM, and DiD). For production sectors, the SDiD estimates are -0.06 (s.e. 0.21) for non-alcoholic beverages and -0.02 (s.e. 0.19) for other food manufacturing; the analogous DiD estimates are -0.06 (s.e. 0.23) and -0.01 (s.e. 0.21), respectively. For commerce sectors, SDiD estimates are -0.10 (s.e. 0.33) for wholesale commerce and -0.23 (s.e. 0.29) for retail commerce; DiD yields -0.07 (s.e. 0.32) and -0.26 (s.e. 0.28). None of these coefficients are statistically different from zero at conventional levels.

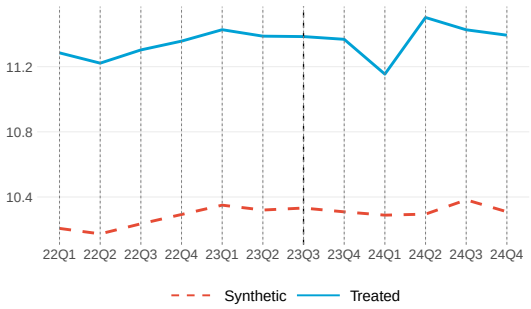
The synthetic control method yields larger negative point estimates in some cases (e.g., -0.48 for non-alcoholic beverages and -0.55 for retail commerce), but the corresponding confidence intervals remain wide and include zero. Overall, across methods and sector groups, we do not find evidence of a statistically significant short-run decline in employment following implementation.

Results from ITSA, reported in Appendix A.1, lead to the same qualitative conclusion: we do not observe a discrete level shift in employment trends in treated sectors relative to controls after 2023Q4.

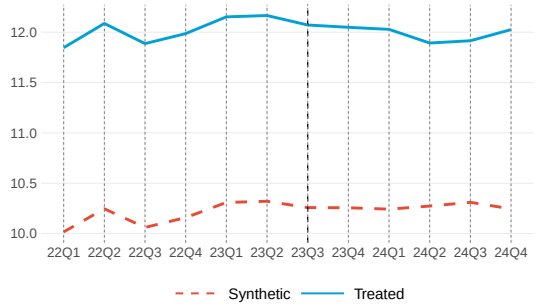
Figure 1: Estimates of the impacts on the number of workers in directly affected sectors



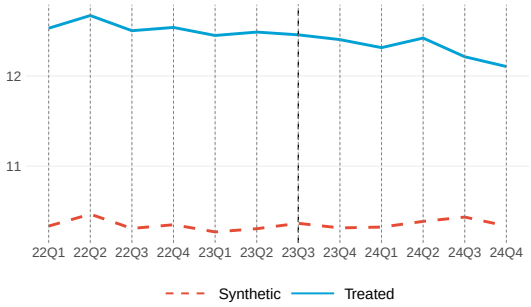
(a) Non-alcoholic beverages production



(b) Other food manufacturing



(c) Wholesale commerce of food and beverages



(d) Retail sale of food and beverages

Note: Based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. The figure plots treated and synthetic series constructed using the synthetic difference-in-differences (SDiD) method.

Table 3: Estimates of the impacts on the number of workers in directly affected sectors

	Production sectors		Commerce sectors	
	A. Non-alcoholic beverages	B. Other food	C. Wholesale commerce	D. Retail commerce
Synthetic DiD	-0.06	-0.02	-0.10	-0.23
	(0.21)	(0.19)	(0.33)	(0.29)
Synthetic Control	-0.48	-0.32	-0.29	-0.55
	(0.31)	(0.26)	(0.37)	(0.34)
DiD	-0.06	-0.01	-0.07	-0.26
	(0.23)	(0.21)	(0.32)	(0.28)
Observations (treated sectors)	18	30	12	12
Periods (post)	6	6	6	6
Sectors (treated)	3	5	2	2
Sectors (others)	455	455	455	455

Note: Estimates are based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. Each coefficient corresponds to the post-treatment effect from the corresponding model. Standard errors are reported in parentheses. For SDiD estimates, standard errors are computed via placebo distributions. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Income: Log Labor Income

We next study earnings. Figure 2 presents the evolution of sector-level log income using the SDiD estimator. The figure plots the quarterly average for treated sector groups alongside the corresponding synthetic comparison group constructed from unaffected sectors. Panel A shows results for production sectors (non-alcoholic beverages and other food manufacturing), while Panel B reports results for commerce sectors (wholesale and retail food and beverage trade).

Table 4 summarizes the estimated post-reform treatment effects using three complementary estimators: DiD, SC, and SDiD. Across all specifications, the estimated impacts on average sector-level income are small and statistically indistinguishable from zero. For example, the DiD estimates are −0.10 (s.e. 0.47) for non-alcoholic beverages, 0.02 (s.e. 0.36) for other food manufacturing, −0.17 (s.e. 0.60) for wholesale commerce, and −0.30 (s.e. 0.68) for retail commerce. The SDiD estimates are similarly close to zero. Although the synthetic control model yields somewhat larger negative point estimates in some sectors, the corresponding confidence intervals include zero.

Appendix A.2 reports additional worker-level specifications, which lead to the same qualitative conclusion.

Figure 2: Estimates of the impacts on log income in directly affected sectors

(a) Non-alcoholic beverages

(b) Other food

(c) Wholesale commerce of food and beverages

(d) Retail sale of food and beverages

Note: The figure plots treated and synthetic series based on GEIH 2022–2024 data. Estimates are obtained using the synthetic difference-in-differences (SDiD) method.

Table 4: Estimates of the impacts on log income in directly affected sectors

	Production sectors		Commerce sectors	
	A. Non-alcoholic beverages	B. Other food	C. Wholesale commerce	D. Retail commerce
Synthetic DiD	−0.09	0.05	−0.18	−0.27
	(0.43)	(0.33)	(0.68)	(0.46)
Synthetic Control	−0.88	−0.38	−0.60	−0.63
	(0.54)	(0.47)	(0.57)	(0.61)
DiD	−0.10	0.02	−0.17	−0.30
	(0.47)	(0.36)	(0.60)	(0.68)
Observations	18	30	12	12
Periods	6	6	6	6
Sectors (treated)	3	5	2	2
Sectors (others)	455	455	455	455

Note: Estimates based on GEIH 2022–2024 data. Each coefficient corresponds to the post-treatment effect from the specified model. Standard errors in parentheses. For Synthetic DiD, SDiD estimates, standard errors are computed via placebo distributions. Significance levels: * p < 0.10, ** p < 0.05, *** p < 0.01.

4.3 Adjustment Mechanisms

Firms can react to new taxes and labeling requirements without changing headcount or average pay by instead adjusting (i) hours worked per employee, (ii) informality, and (iii) hourly earnings.

Table 5 presents the estimates for each of these outcome variables. Below, we describe the results for each outcome.

Table 5: SDiD estimates for adjustment mechanisms

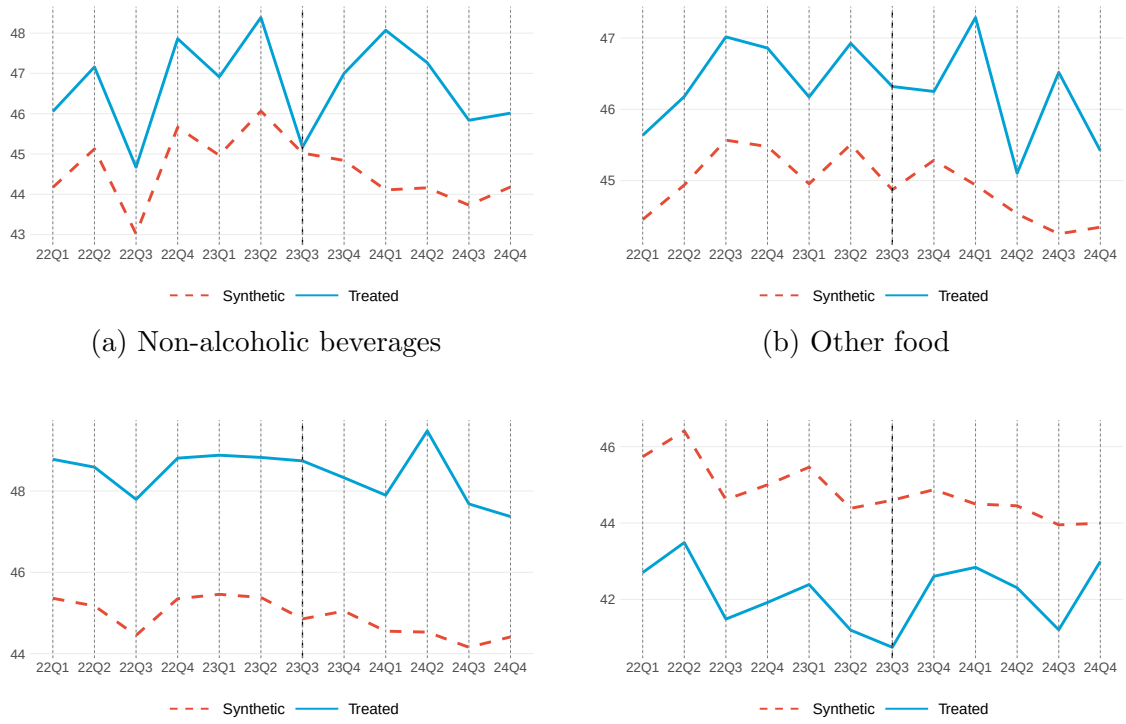
Outcome	Production sectors		Commerce sectors	
	A. Non-alcoholic beverages	B. Other food	C. Wholesale commerce	D. Retail commerce
Hours worked per week	0.179	0.096	0.242	0.830
	(1.047)	(0.815)	(1.365)	(1.347)
Informality	0.018	-0.007	0.017	0.022
	(0.050)	(0.042)	(0.063)	(0.064)
Log income per hour	-0.026	0.038	-0.026	0.009
	(0.101)	(0.081)	(0.116)	(0.151)

Note: Estimates are based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. Each coefficient corresponds to the post-treatment effect from the corresponding model. Standard errors are reported in parentheses. For SDiD estimates, standard errors are computed via placebo distributions. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3.1 Hours Worked

In this section, we analyze the impact on hours worked per week. In Figure 3, treated and synthetic series evolve similarly across all sectors, with no clear post-reform divergence. Consistent with Table 5, estimated effects are small, positive, and statistically insignificant, suggesting no meaningful adjustment along this margin.

Figure 3: Estimates of the impacts on mean hours worked per week in directly affected sectors



(a) Non-alcoholic beverages

(b) Other food

(c) Wholesale commerce

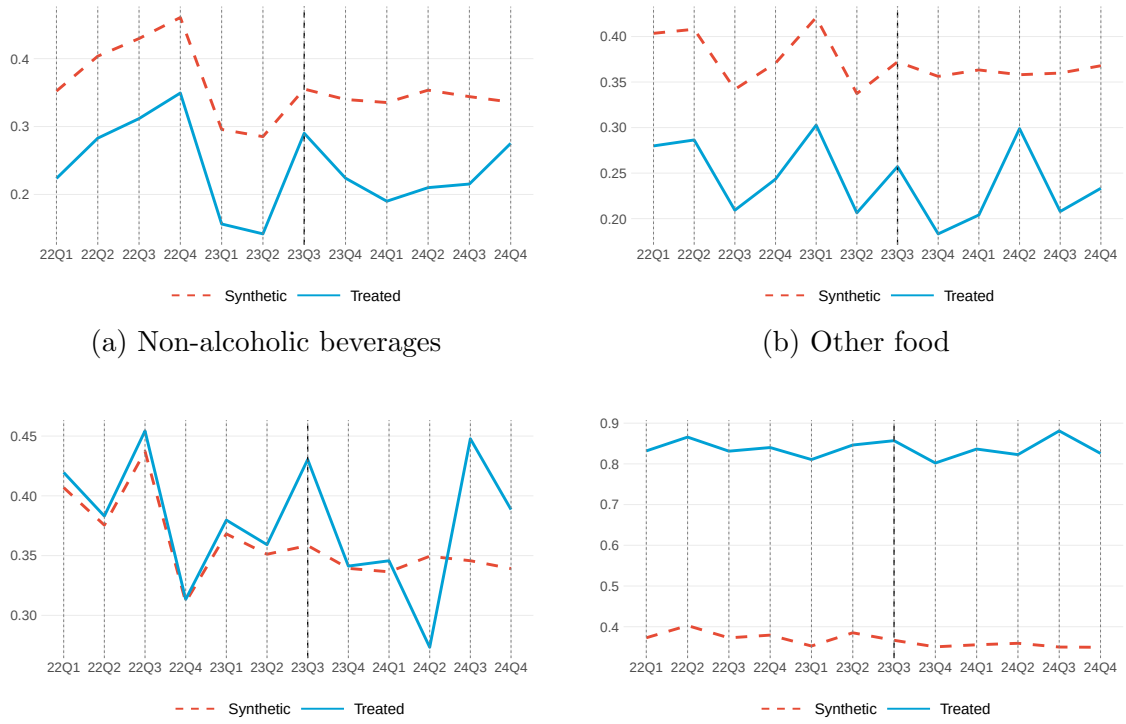
(d) Retail commerce

Note: Based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. The figure plots treated and synthetic series constructed using the synthetic difference-in-differences (SDiD) method.

4.3.2 Informality

We next analyze the impact on informality. In Figure 4, trends in treated and synthetic groups remain closely aligned after the reform. Table 5 confirms that estimated effects are close to zero and statistically insignificant across all sectors, indicating no evidence of shifts toward informal employment.

Figure 4: Estimates of the impacts on the share of informal workers in directly affected sectors



(a) Non-alcoholic beverages

(b) Other food

(c) Wholesale commerce

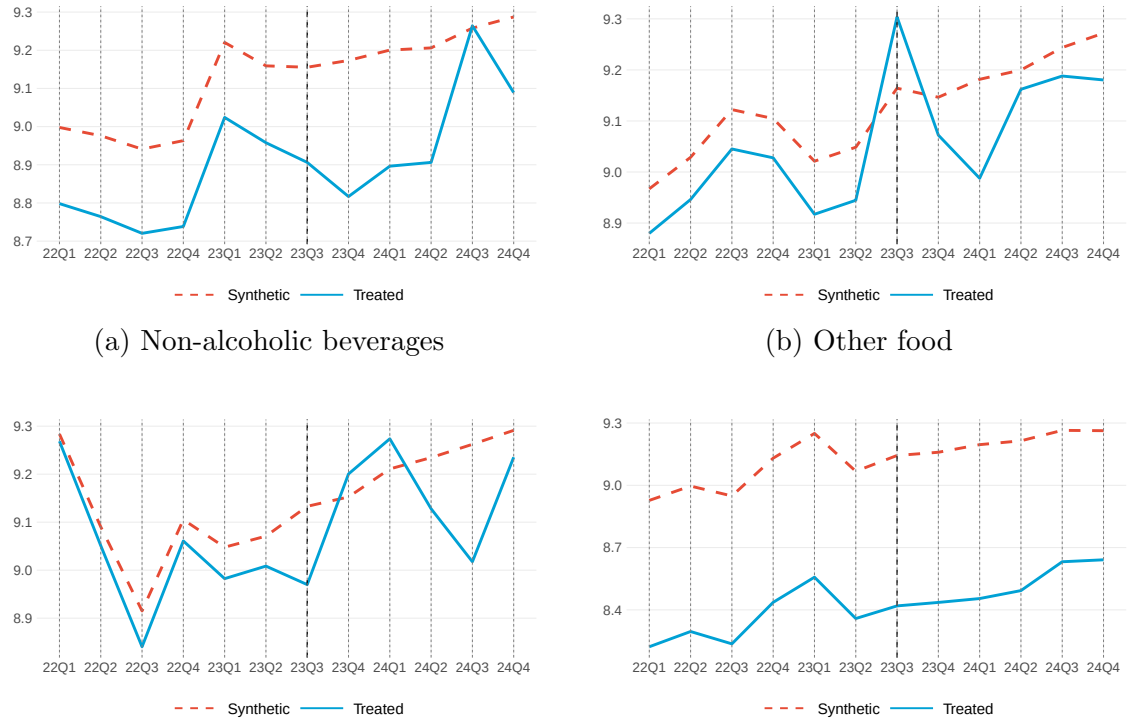
(d) Retail commerce

Note: Based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. The figure plots treated and synthetic series constructed using the synthetic difference-in-differences (SDiD) method.

4.3.3 Mean income per hour

Finally, we analyze the impact on mean income per hour. In Figure 5, the trajectories of treated and synthetic groups show no systematic divergence following the reform. In line with Table 5, estimated effects are small and statistically insignificant, suggesting no adjustment in hourly wages.

Figure 5: Estimates of the impacts on log income per hour in directly affected sectors



(a) Non-alcoholic beverages

(b) Other food

(c) Wholesale commerce

(d) Retail commerce

Note: Based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. The figure plots treated and synthetic series constructed using the synthetic difference-in-differences (SDiD) method.

4.4 Mom-and-pop store results

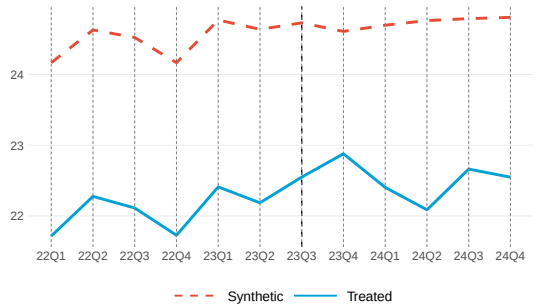
Figure 6 presents the SDiD trajectories for five labor outcomes among shopkeepers: log income, log employment, weekly hours worked, log income per hour, and informality. In each panel, the observed trajectory for shopkeepers is compared with the SDiD synthetic counterfactual constructed from the control sectors. Across outcomes, treated and synthetic trajectories remain broadly similar over the post-reform period, although some divergence emerges for weekly hours worked, hourly income, and informality.

Table 6 reports the corresponding SDiD post-treatment estimates. Consistent with the figure, estimated effects on log income and log employment are statistically indistinguishable from zero, suggesting no clear short-run change in overall earnings or headcount among shopkeepers. At the same time, the estimates point to adjustments along other margins: weekly hours worked decline by about 2.5 hours, log income per hour falls 15%, and informality increases in 9 pp. Taken together, these results suggest that neighborhood retailers did not experience a broad contraction in total income, but may have adjusted through reduced hours, lower hourly earnings, and a higher reliance on informal work arrangements.

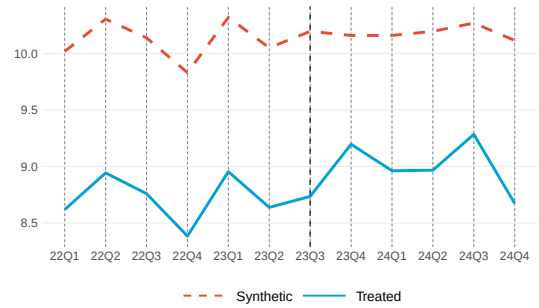
Overall, the evidence provides little support for the hypothesis that neighborhood retailers experienced substantial short-run disruptions in total employment or aggregate earnings following the reform. However, the results do suggest some ad-

justment along intensive and compositional margins, rather than complete stability across all labor outcomes.

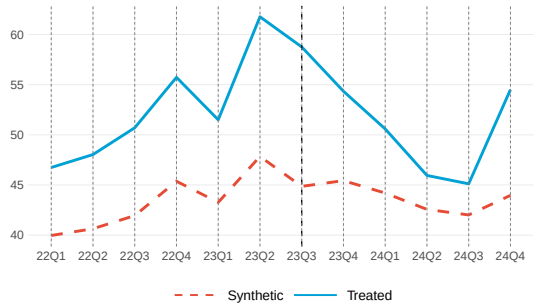
Figure 6: SDiD estimates for shopkeepers ("mom and pop" stores)



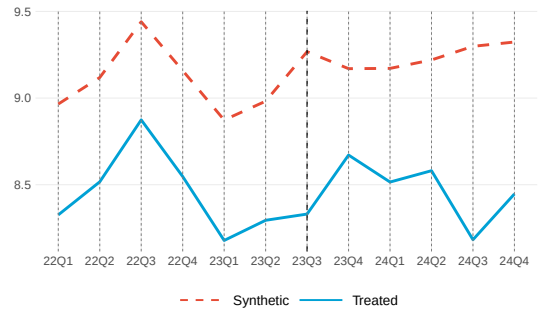
(a) Log income



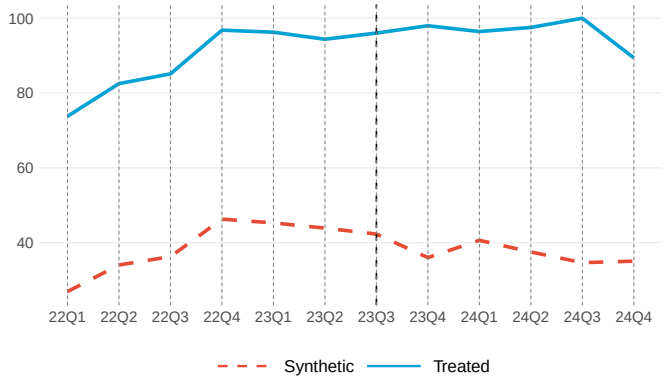
(b) Log employment



(c) Mean hours worked per week



(d) Mean income per hour



(e) Informality

Note: Based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. The figure plots treated and synthetic series constructed using the synthetic difference-in-differences (SDiD) method.

Table 6: SDiD estimates for Mom-and-pop stores (*tiendas de barrio*)

Outcome variable	SDiD
Log income	0.20
	(0.44)
Log employment	0.18
	(0.22)
Hours worked per week	-2.54***
	(0.90)
Informality	0.09**
	(0.04)
Log income per hour	-0.15*
	(0.09)

Note: Estimates are based on individual-level GEIH 2022–2024 data aggregated to quarterly averages at the CIIU Rev. 4 (4-digit) sector level. Each coefficient corresponds to the post-treatment effect from the corresponding model. Standard errors are reported in parentheses. Mom-and-pop stores (*tiendas de barrio*) are defined as workers in retail food-related ISIC Rev.4 activities (4724, 4729, 4711, and 4755) employed in establishments with ten or fewer workers and operating in fixed or semi-fixed retail locations. For SDiD estimates, standard errors are computed via placebo distributions. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Heterogeneous effects

Finally, we assess whether the policy generated heterogeneous labor-market effects across worker characteristics that may not be visible in sector-level averages. Figures 7–10 present SDiD estimates by gender, location, occupation type, age group, and education for four outcomes: (i) log income per hour, (ii) weekly hours worked, (iii) log total income, and (iv) informality. Each figure reports point estimates and 95% confidence intervals for the post-treatment effect across subgroups.

Across all outcomes, most estimates are small and statistically indistinguishable from zero. The confidence intervals generally overlap zero for nearly all demographic and occupational groups, indicating no clear evidence that the reform generated systematic distributional effects across worker characteristics.

Figure 7 reports heterogeneous effects for hourly wages. The estimates fluctuate around zero across gender, education, occupation type, age group, and location, and the corresponding confidence intervals typically include zero. This pattern suggests that the reform did not generate meaningful wage adjustments concentrated in specific worker groups.

Figure 8 shows results for weekly hours worked. Most subgroup estimates are again close to zero. However, two cases stand out. First, among workers aged 61 years and older in the wholesale sector, the SDiD estimate indicates a statistically significant reduction in weekly hours worked. Second, in the retail sector, rural workers exhibit a statistically significant increase in weekly hours worked after the reform. These effects are limited to specific subgroups and are not accompanied by consistent changes in other outcomes.

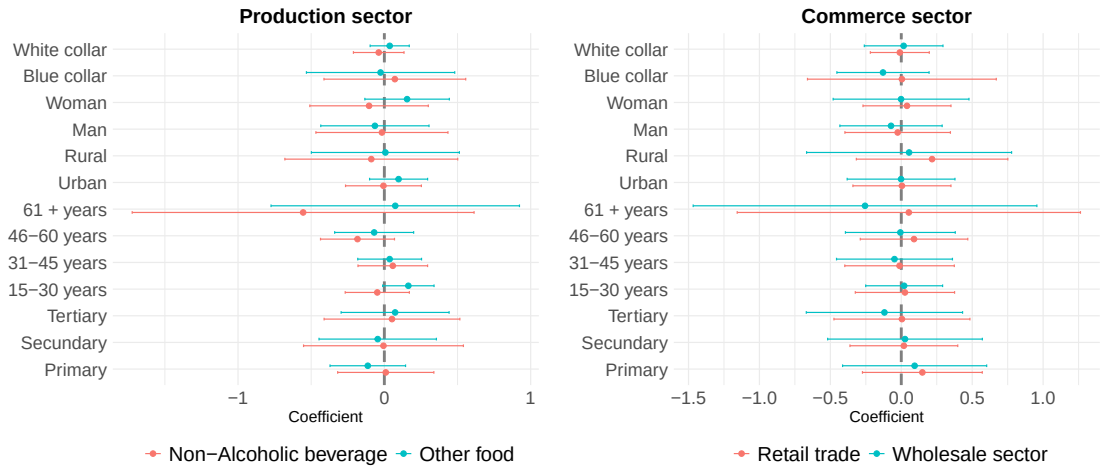
Figure 9 presents results for total income. Most coefficients are again close to zero and statistically insignificant. One exception appears for workers aged 61 years and older in the non-alcoholic beverages sector, where the SDiD estimate indicates a

statistically significant increase in log income. This effect does not appear for other age groups or sectors.

Finally, Figure 10 examines whether the policy affected informality rates differently across worker characteristics. The estimates again cluster around zero across groups, suggesting no detectable shift toward informal employment among specific demographic or occupational categories.

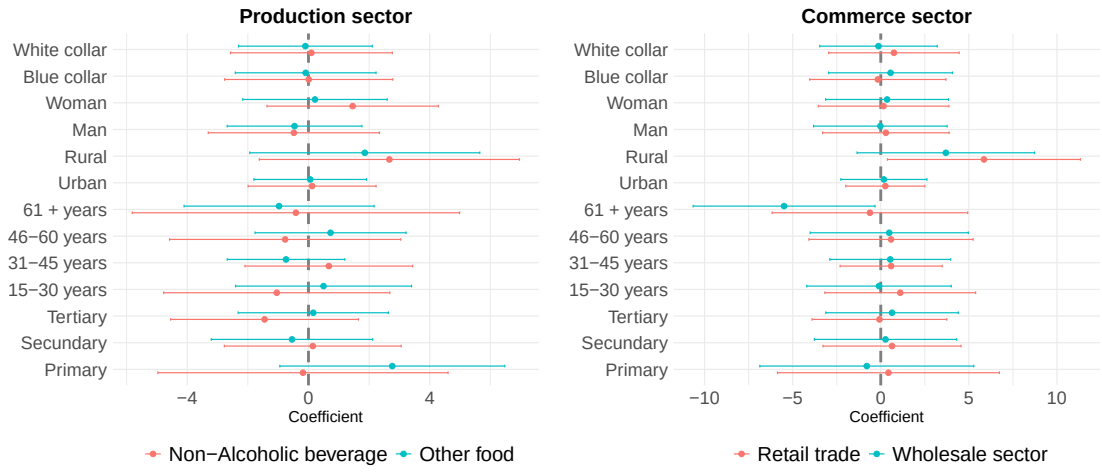
Overall, the heterogeneity analysis reinforces the main results of the paper. Although a few subgroup estimates reach statistical significance—such as the reduction in hours among older workers in wholesale activities and the increase in hours among rural workers in retail—the broader pattern indicates that the reform did not generate systematic labor-market adjustments across worker groups in the short run.

Figure 7: Synth-DiD impact on mean wage per hour by worker characteristics



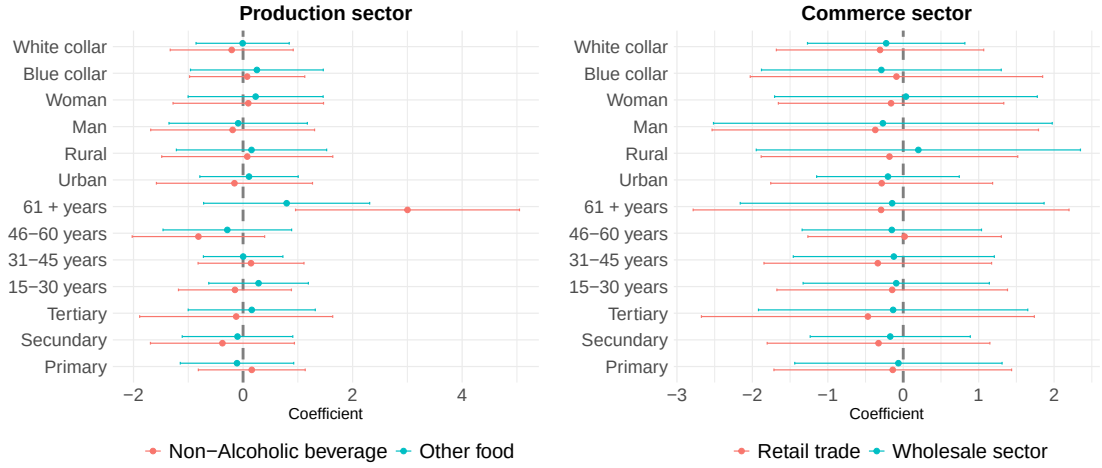
Note: the calculations are based on data from the GEIH 2022–2024. We cluster standard errors at the economic-activity level. 95% confidence intervals are presented.

Figure 8: Synth-DiD impact on hour worked per week by worker characteristics



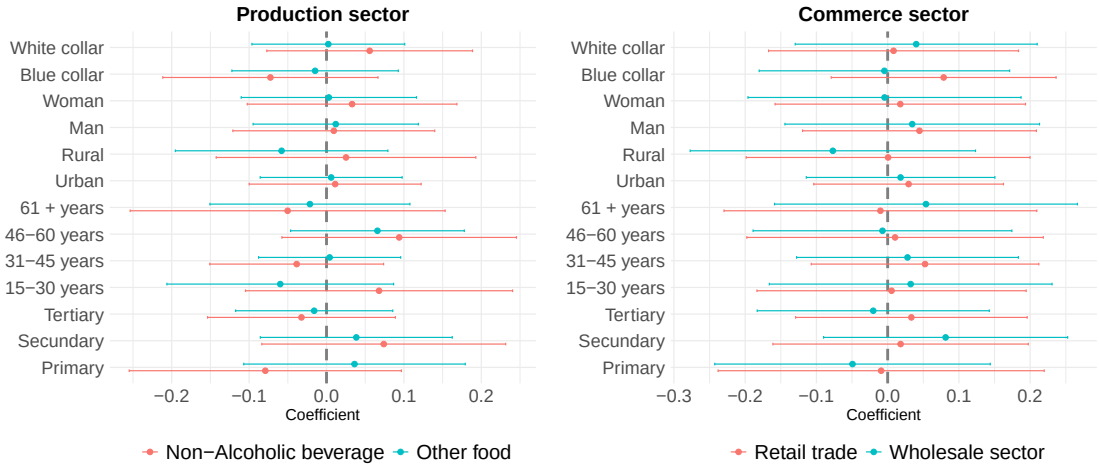
Note: the calculations are based on data from the GEIH 2022–2024. We cluster standard errors at the economic-activity level. 95% confidence intervals are presented.

Figure 9: Synth-DiD impact on log income by worker characteristics



Note: the calculations are based on data from the GEIH 2022–2024. We cluster standard errors at the economic-activity level. 95% confidence intervals are presented.

Figure 10: Synth-DiD impact on informality by worker characteristics



Note: the calculations are based on data from the GEIH 2022–2024. We cluster standard errors at the economic-activity level. 95% confidence intervals are presented.

5 Discussion

This study provides early empirical evidence on the labor market effects of Colombia's recent fiscal and regulatory policies targeting UPFs and SSBs. Implemented between 2023 and 2024, these measures combined an excise tax on SSBs with the phased introduction of FOPWL for products exceeding thresholds of sugar, sodium, saturated fat, or trans fat. Using nationally representative labor force data and a combination of SCM, SDiD, and DiD models, we find no evidence of short-run disruptions in aggregate employment, labor income, informality, or hours worked in the sectors most directly affected by the reform.

These findings suggest that, at least during the initial phase of implementation, the policy did not generate abrupt labor demand adjustments in beverage manufacturing, food processing, or related wholesale and retail activities. This pattern is consistent with evidence from other Latin American countries that have adopted similar fiscal or regulatory interventions. In Mexico, Guerrero-López et al. (2017) find no significant employment losses following the introduction of taxes on SSBs and high-calorie foods. Similarly, Paraje et al. (2021) report no significant changes in wages or employment after Chile's 2016 FOPWL reform, while Díaz et al. (2023) document neutral labor market effects in Peru's beverage and food industries after comparable policies. In the Colombian context, descriptive evidence suggests that firms in the beverage industry may adjust by reallocating costs across margins, such as marketing or product reformulation, rather than reducing employment (Villalba and Guarnizo, 2022). Our results provide causal evidence supporting the presence of such adjustment mechanisms. Together, this body of evidence indicates that health-oriented fiscal and labeling policies can be implemented without immediate adverse consequences for employment in affected sectors.

While aggregate employment and earnings remain broadly stable, the analysis of hours worked points to limited adjustment along the intensive margin in specific contexts. In particular, some specifications suggest modest increases in weekly hours worked among employees in the non-alcoholic beverage sector. These patterns are not uniform across all models or control groups, but they are consistent with the idea that firms may initially adjust workloads rather than headcount when responding to new regulatory requirements. In practice, such adjustments could reflect short-term reallocation of tasks associated with reformulation, compliance, labeling changes, or marketing strategies, without translating into layoffs or wage cuts.

The stability of aggregate labor outcomes likely reflects the mechanisms through which firms adapt to these policies. Evidence from other settings shows that manufacturers often respond to SSB taxes and labeling requirements by reformulating products or substituting taxed ingredients with untaxed alternatives, such as non-caloric sweeteners (Colchero et al., 2017; Ng et al., 2019). Because these inputs are typically cheaper than sugarcane-based ingredients (Sharma et al., 2014), reformulation may allow firms to maintain output levels and labor demand. Moreover, the gradual rollout of both the tax and FOPWL components of the reform likely smoothed the adjustment process, reducing the need for abrupt changes in production or employment.

Although average effects are limited, the heterogeneous analysis reveals differences across worker groups that are not visible in aggregate estimates. First, some specifications indicate increases in hours worked among *white-collar* workers in the

non-alcoholic beverage sector. This pattern is consistent with short-term adjustments in managerial, administrative, or supervisory roles related to regulatory compliance, reformulation processes, and coordination along the supply chain. Second, *rural workers* display modest positive responses in certain outcomes, suggesting that peripheral segments of the food and beverage supply chain—such as sourcing, packaging, or distribution—may have experienced temporary expansions following the reform. Third, labor outcomes for *female workers* differ from those of men in several specifications, pointing to distinct adjustment patterns by gender. These findings are consistent with well-documented gender segmentation in the Colombian labor market, where women are more concentrated in administrative and service occupations and may therefore be more directly exposed to compliance-related or demand-side adjustments.

Beyond these dimensions, we do not find systematic heterogeneous effects by education level, firm size, or urban location. Nonetheless, it remains possible that smaller firms face longer-term challenges, given their thinner margins and more limited capacity to absorb regulatory or demand shocks. Future research using firm-level administrative data—such as the *Encuesta Anual Manufacturera* (EAM) and the *Encuesta Anual de Servicios* (EAS), could provide deeper insight into these dynamics, including firm entry and exit, task reallocation, productivity adjustments, and employment transitions over time.

Finally, understanding how these policies reshape consumption and production patterns across the broader food system remains critical. Evidence from Mexico shows that households substituted away from taxed SSBs toward bottled water and other untaxed beverages following the introduction of excise taxes (Colchero et al., 2017). In Colombia, similar substitution could extend toward minimally processed foods—such as fruits, legumes, and starchy staples, which are already widely consumed according to the 2015 National Nutrition Survey (Herrán, 2024). Such shifts would reinforce the public health objectives of the reform while potentially supporting employment in traditional food markets and local supply chains.

In sum, Colombia's combined tax and FOPWL policies do not appear to have generated immediate employment or wage losses in affected sectors. Instead, where adjustments are observed, they are concentrated in specific worker groups and margins, consistent with gradual adaptation by firms and workers rather than broad labor market disruption.

6 Conclusions

This paper provides the first empirical assessment of the short-term labor market effects of Colombia's recent taxes on SSBs and UPFs, and FOPWL. Using nationally representative labor force data and complementary empirical strategies, we find no evidence of short-run changes in total employment, labor income, or informality in sectors directly or indirectly affected by the reform.

Across methods, estimated effects on earnings and hours worked are generally small and statistically indistinguishable from zero. Some specifications suggest modest increases in hours worked in the non-alcoholic beverage sector, particularly among women, rural workers, and white-collar occupations, but these patterns are not robust across specifications. As such, the results point to, at most, limited intensive-margin or compositional adjustments rather than broad changes in labor

demand.

These findings carry two main policy implications. First, they reinforce existing evidence that well-designed nutrition-oriented fiscal and informational policies can be implemented without immediate adverse effects on employment or earnings in affected industries. Second, they underscore the importance of monitoring distributional responses across worker groups, as adjustment may occur unevenly even when aggregate labor market indicators remain stable.

Future research should extend this analysis using firm-level and administrative data to examine whether observed labor adjustments are associated with changes in productivity, reformulation strategies, or task composition within firms. Longer-term evaluations will also be needed as Colombia's tax rates increase and labeling requirements reach full enforcement, potentially generating effects that are not yet visible in the short-run data.

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A Appendix

Table 7: Missing values by sector

		Income			
Sector	**Code**	**Missing Pre**	**Missing Post**	**% Missing Pre**	**% Missing Post**
Panel A: Non Alcoholic Beverages					
Malt beverage production	1103	8	8	2.6	3.2
Non-alcoholic beverage production	1104	23	13	2.0	1.7
Packaging and bottling	8292	4	1	2.0	0.9
Total (sector)		**35**	**22**	**2.1**	**1.9**
Panel B: Other food					
Meat processing	1011	16	11	1.6	1.5
Fish processing	1012	3	1	1.9	1.2
Dairy production	1040	88	57	5.1	5.1
Sugar refining	1071	5	10	2.2	5.5
Other food manufacturing	1089	57	37	3.4	3.4
Total (sector)		**169**	**116**	**3.6**	**3.7**
Panel C: Wholesale commerce					
Wholesale of food	4631	150	121	3.6	3.9
Wholesale of beverages and tobacco	4632	35	25	4.4	4.6
Total (sector)		**185**	**146**	**3.7**	**4.0**
Panel D: Retail commerce					
Retail of beverages and tobacco	4724	249	144	7.1	7.2
Retail of other food	4729	192	104	8.0	8.1
Total (sector)		**441**	**248**	**7.5**	**7.6**

Notes: The columns "Missing" and "% Missing" refer to the **Income** variable. Source: GEIH 2022–2024.

A.1 ITSA results

Table 8: ITSA: Log Income by Sector

	Production sectors		Commerce sectors	
	A. Non-alcoholic beverages	B. Other food	C. Wholesale commerce	D. Retail commerce
Time trend	0.032***	0.032***	0.032***	0.032***
	(0.004)	(0.004)	(0.004)	(0.004)
Treatment group (baseline)	−0.010	−0.127**	0.102	−0.730***
	(0.102)	(0.051)	(0.126)	(0.044)
Treatment × Time (pre-intervention)	−0.008	0.011	−0.019	0.015
	(0.023)	(0.014)	(0.024)	(0.011)
Level change (post-intervention)	−0.056	−0.056	−0.056	−0.056
	(0.036)	(0.036)	(0.036)	(0.036)
Slope change (post-intervention)	0.003	0.003	0.003	0.003
	(0.016)	(0.016)	(0.016)	(0.016)
Treatment effect: Level	−0.134	0.025	0.122	−0.329***
	(0.145)	(0.080)	(0.082)	(0.089)
Treatment effect: Slope	0.147***	−0.002	−0.022	0.112***
	(0.043)	(0.024)	(0.030)	(0.025)
Constant	14.210***	14.210***	14.210***	14.210***
	(0.019)	(0.019)	(0.019)	(0.019)
Observations	5516	5516	5516	5516

Standard errors in parentheses. Treatment period: Q4 2023 (quarter 256).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: ITSA: Log Employment by Sector

	Production sectors		Commerce sectors	
	A. Non-alcoholic beverages	B. Other food	C. Wholesale commerce	D. Retail commerce
Time trend	0.068***	0.068***	0.068***	0.068***
	(0.014)	(0.014)	(0.014)	(0.014)
Treatment group (baseline)	1.955***	3.310***	2.879***	3.294***
	(0.102)	(0.066)	(0.082)	(0.079)
Treatment × Time (pre-intervention)	−0.053*	−0.060***	−0.024	−0.092***
	(0.028)	(0.015)	(0.018)	(0.017)
Level change (post-intervention)	−0.257**	−0.257**	−0.257**	−0.257**
	(0.106)	(0.106)	(0.106)	(0.106)
Slope change (post-intervention)	−0.045	−0.045	−0.045	−0.045
	(0.043)	(0.043)	(0.043)	(0.043)
Treatment effect: Level	0.237	0.201*	0.016	0.233*
	(0.169)	(0.108)	(0.124)	(0.123)
Treatment effect: Slope	0.084*	0.091**	0.008	0.006
	(0.049)	(0.045)	(0.047)	(0.048)
Constant	10.010***	10.010***	10.010***	10.010***
	(0.061)	(0.061)	(0.061)	(0.061)
Observations	5524	5524	5524	5524

Standard errors in parentheses. Treatment period: Q4 2023 (quarter 256).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Worker-level DiD

We complement the sector-level analysis with worker-level DiD estimates using repeated cross-sections from the GEIH survey. Unlike the SDiD and synthetic control approaches, which explicitly construct a counterfactual that closely matches pre-treatment trends, the individual-level DiD relies on a comparison between treated and control sectors without imposing pre-treatment fit. As a result, these estimates may be more sensitive to violations of the parallel-trends assumption and to compositional changes in the workforce within sectors over time. We therefore interpret the worker-level results as a complementary robustness exercise, rather than as a primary identification strategy.

A.2.1 Income

Table 10 reports DiD estimates for labor income at the individual level. Across specifications, the coefficients on the *Post* × *Treatment* interaction are small and statistically indistinguishable from zero. This pattern holds across both production and commerce sectors and under alternative control definitions. Overall, we find no consistent evidence that the reform affected workers' total income at the individual level.

A.2.2 Income per hour

Table 11 presents estimates on income per hour. Across specifications, the coefficients on the *Post* × *Treatment* interaction are generally small and statistically indistinguishable from zero. This holds for both total income and income per hour, and across alternative control definitions. Overall, we find no consistent evidence that the reform affected workers' earnings at the individual level in either production or commerce sectors.

The remaining covariates behave as expected. Workers in medium and large firms earn more than those in small firms, wages are higher in urban areas, and there are systematic differences by gender and education. These patterns provide an internal validity check on the specification.

A.2.3 Informality

Table 12 reports DiD estimates for labor informality at the individual level.

The *Post* × *Treatment* coefficients are small across all sectors. In production sectors, estimates are close to zero and statistically insignificant. In commerce sectors, we observe a small negative and weakly significant coefficient for retail, but the magnitude is economically negligible.

Overall, we find no consistent evidence that the reform led to a systematic shift toward informality at the worker level.

As expected, informality is strongly associated with observable characteristics: it is higher in small firms, lower in urban areas, and decreases sharply with education. These patterns confirm that the measure of informality behaves in line with well-documented features of the Colombian labor market.

A.2.4 Hours worked

Table 13 reports DiD estimates for weekly hours worked at the individual level.

Across sectors, the *Post* × *Treatment* coefficients are small and imprecisely estimated. In production sectors, estimates for non-alcoholic beverages are positive but not statistically significant, suggesting at most a modest increase in hours worked. For other food manufacturing, estimated effects are close to zero and not robust to alternative control definitions.

In commerce sectors, estimated effects are similarly small and statistically indistinguishable from zero for both wholesale and retail activities. Overall, we do not find consistent evidence of systematic adjustments in hours worked at the individual level.

Taken together, the worker-level results reinforce the main findings of the paper. We do not observe broad changes in earnings, hours worked, or informality following the reform. Any potential adjustments appear limited in magnitude and are not robust across specifications.

Table 10: Effect on sectors on log income (Individual level)

	Production sectors		Commerce sectors	
	Non-A beverages	Other food	Wholesale commerce	Retail sector
Post × Treatment	-0.49*	0.13	0.05	-0.03
	(0.26)	(0.21)	(0.30)	(0.10)
Small firm [B]				
Medium firm	1.39***	1.39***	1.39***	1.39***
	(0.13)	(0.13)	(0.13)	(0.13)
Large firm	1.51***	1.51***	1.52***	1.51***
	(0.15)	(0.15)	(0.15)	(0.15)
Urban [B]				
Rural	-0.55***	-0.55***	-0.54***	-0.56***
	(0.10)	(0.10)	(0.10)	(0.10)
Male [B]				
Female	-1.01***	-1.01***	-1.01***	-1.01***
	(0.21)	(0.21)	(0.21)	(0.21)
Primary [B]				
Secondary	-0.23***	-0.23***	-0.23***	-0.23***
	(0.08)	(0.08)	(0.08)	(0.08)
Tertiary	-0.42***	-0.42***	-0.43***	-0.42***
	(0.11)	(0.11)	(0.11)	(0.11)
Constant	12.70***	12.70***	12.70***	12.69***
	(0.13)	(0.13)	(0.13)	(0.13)
Units	837,798	841,228	843,363	845,130
Periods	12	12	12	12
Clusters	489	491	488	488
Observations	837,798	841,228	843,363	845,130

Note: the calculations are based on data from the GEIH 2022–2024. [B] = baseline/reference category. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. For the regression with the control group, we cluster standard errors at the economic-activity level; for the specification we report robust standard errors.

Table 11: Effect on income per hour by sector (Individual level)

	Production sectors		Commerce sectors	
	Non-A beverages	Other food	Wholesale commerce	Retail sector
Post × Treatment	-0.02	0.04	-0.07***	0.03
	(0.08)	(0.06)	(0.02)	(0.06)
Small firm [B]				
Medium firm	-0.83***	-0.84***	-0.83***	-0.84***
	(0.11)	(0.11)	(0.11)	(0.11)
Large firm	-0.07*	-0.07*	-0.06*	-0.07*
	(0.04)	(0.04)	(0.04)	(0.04)
Rural [B]				
Urban	0.35***	0.36***	0.35***	0.36***
	(0.07)	(0.07)	(0.07)	(0.07)
Female [B]				
Male	0.55***	0.55***	0.55***	0.55***
	(0.10)	(0.10)	(0.10)	(0.10)
Primary [B]				
Secondary	-0.10***	-0.10***	-0.10***	-0.10***
	(0.04)	(0.04)	(0.04)	(0.04)
Tertiary	-0.12**	-0.12**	-0.13**	-0.12**
	(0.06)	(0.06)	(0.06)	(0.06)
15-30 years [B]				
31-45 years	0.32***	0.32***	0.32***	0.33***
	(0.06)	(0.06)	(0.06)	(0.06)
46-60 years	0.22***	0.22***	0.22***	0.22***
	(0.06)	(0.06)	(0.06)	(0.06)
61+ years	-0.10	-0.10	-0.10	-0.10
	(0.06)	(0.06)	(0.06)	(0.06)
Constant	7.74***	7.74***	7.74***	7.72***
	(0.09)	(0.09)	(0.09)	(0.09)
Units	837,798	841,228	843,363	845,130
Periods	12	12	12	12
Clusters	489	491	488	488
Observations	837,798	841,228	843,363	845,130

Note: the calculations are based on data from the GEIH 2022–2024. [B] = baseline/reference category. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. For the regression with the control group, we cluster standard errors at the economic-activity level

Table 12: Effect on informality by production sector (Individual level)

	Production sectors		Commerce sectors	
	Non-A beverages	Other food	Wholesale commerce	Retail sector
Post × Treatment	-0.03***	-0.00	0.00	-0.01*
	(0.01)	(0.01)	(0.00)	(0.00)
Small firm [B]				
Medium firm	0.63***	0.63***	0.63***	0.63***
	(0.02)	(0.02)	(0.02)	(0.02)
Large firm	0.19***	0.19***	0.19***	0.19***
	(0.02)	(0.02)	(0.02)	(0.02)
Rural [B]				
Urban	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
Female [B]				
Male	-0.02***	-0.02***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)
Primary [B]				
Secondary	-0.05***	-0.05***	-0.05***	-0.05***
	(0.00)	(0.00)	(0.00)	(0.00)
Tertiary	-0.17***	-0.17***	-0.17***	-0.17***
	(0.01)	(0.01)	(0.01)	(0.01)
15-30 years [B]				
31-45 years	-0.07***	-0.07***	-0.07***	-0.07***
	(0.00)	(0.00)	(0.00)	(0.00)
46-60 years	-0.10***	-0.10***	-0.10***	-0.10***
	(0.01)	(0.01)	(0.01)	(0.01)
61+ years	-0.10***	-0.10***	-0.10***	-0.10***
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	0.37***	0.37***	0.37***	0.37***
	(0.02)	(0.02)	(0.02)	(0.02)
Units	837,798	841,228	843,363	845,130
Periods	12	12	12	12
Clusters	489	491	488	488
Observations	837,798	841,228	843,363	845,130

Note: the calculations are based on data from the GEIH 2022–2024. [B] = baseline/reference category. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the economic-activity level.

Table 13: Effect on hours worked per week by production sector (Individual level)

	Production sectors		Commerce sectors	
	Non-A beverages	Other food	Wholesale commerce	Retail sector
Post × Treatment	0.57	0.24	-0.29	0.50
	(0.74)	(0.22)	(0.25)	(0.50)
Small firm [B]				
Medium firm	-4.22***	-4.26***	-4.17***	-4.21***
	(0.41)	(0.41)	(0.40)	(0.41)
Large firm	-0.56***	-0.57***	-0.54***	-0.54***
	(0.16)	(0.16)	(0.16)	(0.16)
Rural [B]				
Urban	1.98***	1.99***	1.98***	1.99***
	(0.27)	(0.27)	(0.27)	(0.27)
Female [B]				
Male	4.86***	4.87***	4.84***	4.88***
	(0.42)	(0.42)	(0.42)	(0.42)
Primary [B]				
Secondary	-0.07	-0.06	-0.07	-0.07
	(0.15)	(0.14)	(0.14)	(0.14)
Tertiary	-1.33***	-1.31***	-1.33***	-1.32***
	(0.28)	(0.28)	(0.27)	(0.27)
15-30 years [B]				
31-45 years	2.11***	2.10***	2.10***	2.13***
	(0.26)	(0.25)	(0.25)	(0.25)
46-60 years	1.64***	1.64***	1.64***	1.67***
	(0.29)	(0.29)	(0.29)	(0.29)
61+ years	-1.68***	-1.69***	-1.69***	-1.65***
	(0.35)	(0.35)	(0.35)	(0.34)
Constant	42.34***	42.34***	42.34***	42.28***
	(0.46)	(0.46)	(0.46)	(0.46)
Units	837,798	841,228	843,363	845,130
Periods	12	12	12	12
Clusters	489	491	488	488
Observations	837,798	841,228	843,363	845,130

Note: the calculations are based on data from the GEIH 2022–2024. [B] = baseline/reference category. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the economic-activity level.