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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) an octagonal front-of-package warning label on processed and ultra-processed food and beverage products high in sugar, sodium, saturated fat, or calories. Using nationally representative labor force data and three complementary identification strategies—interrupted time series, difference-in-differences, and synthetic control—we evaluate effects on employment, income, hours worked, and informality in directly and indirectly affected sectors.

Results show no statistically significant short-run changes in employment, income, or informality. The only robust adjustment occurs along the intensive margin: workers in the non-alcoholic beverage sector increased their weekly hours by roughly one hour on average. This effect is concentrated among white-collar, rural, and female workers, suggesting that firms adapted to the new regulations by adjusting workloads rather than employment levels.

Overall, the evidence indicates that Colombia's fiscal and labeling policies did not disrupt labor markets during their initial implementation. The results align with findings from Mexico, Chile, and Peru, supporting the view that well-designed "high-in" food policies can advance public health objectives without undermining employment or income stability. These findings contribute to the growing evidence base on the economic effects of food-related fiscal and informational measures in middle-income countries, offering reassurance to policymakers balancing health and labor market goals.

Keywords: Employment; Labor income; Colombia; Food policy; Labeling; Taxes; Processed foods.

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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 and sugar-sweetened beverages (SSBs), together with front-of-package warning labels (FOPWL), aim to reduce consumption of products high in sugar, sodium, saturated fat, or calories. 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 ultra-processed sugary drinks and ultra-processed foods, while Resolution 2492 of 2022 established an octagonal front-of-package warning label for processed and ultra-processed food and beverage products high in sugar, sodium, saturated fat, or calories. 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 three empirical strategies—interrupted time-series, difference-in-differences, and synthetic control—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.

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 responses: instead of broad employment effects, the reforms are associated with a modest but significant increase in weekly hours worked among beverage-sector employees—particularly white-collar, rural, and female workers—indicating adjustment along the intensive rather than extensive margin.

2 Policy Context and Timeline

Colombia's food tax and front-of-package labeling (FOPL) 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 sugar-sweetened beverages (SSBs) following the World Health Organization's recommendations. Although the first legislative attempt 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 ultra-processed foods (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 a new front-of-package labeling (FOPL) standard 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 FOPL 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 exploit these stages in our difference-in-differences and event study designs. 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.

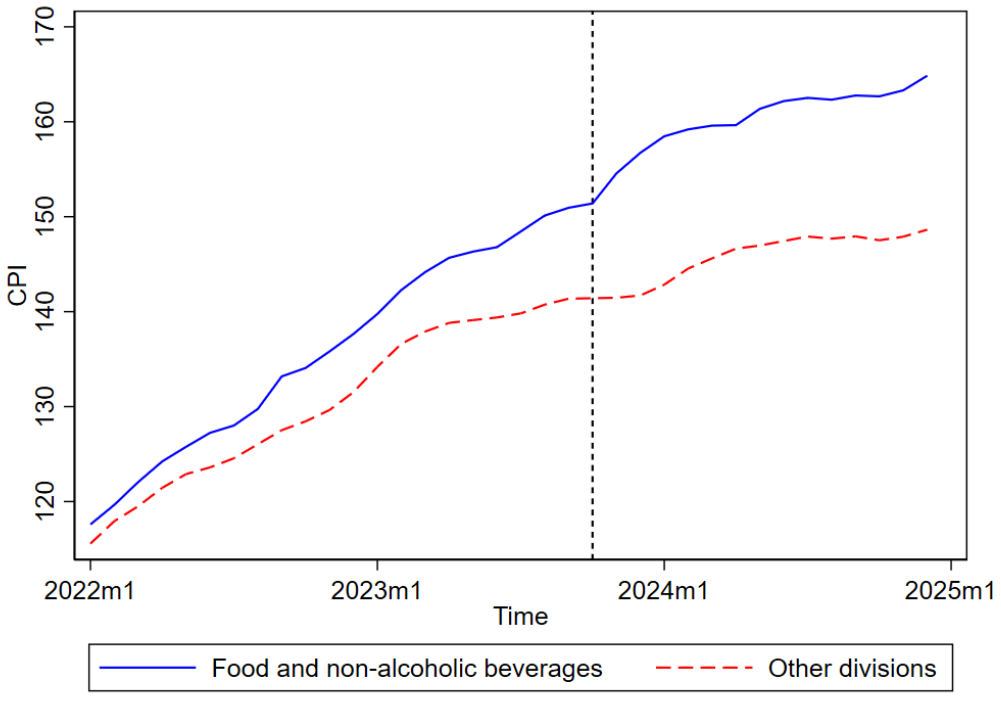
Figure 1 shows the Consumer Price Index (CPI) for food and non-alcoholic beverages relative to other divisions of the economy. A modest post-implementation divergence from the general CPI suggests partial pass-through of the tax to consumer prices, consistent with limited inflationary effects and gradual adjustment in production and retail markets.

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.

Figure 1: Price indexes for food and non-food classes



Note: Information compiled from DANE CPI index.

3 Methodology

We estimate the effects of Colombia's food taxes and FOPL on labor outcomes using three complementary approaches that share the same identification logic. The core idea is difference-in-differences (DiD): compare the evolution of outcomes in *treated* sectors (directly exposed to the reform) with *control* sectors (similar food sectors with potential indirect exposure and unrelated sectors unlikely to be affected). The

key assumption is *parallel trends*: absent the reform, treated and control sectors would have followed similar trajectories. Throughout, the **post-treatment period** begins in **2023Q4 (T2 onward)**, when the tax and labeling mandates became binding (see Section 2).

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.

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 three groups of sectors: (i) **Group A:** non-alcoholic beverage manufacturing and distribution; (ii) **Group B:** food manufacturing and commercialization; and (iii) **Group C:** sectors unrelated to the food industry. This classification supports the difference-in-differences design by comparing labor outcomes across segments with varying exposure to the reform.

Groups A and B correspond to activities typically considered part of the food-and-beverages complex in both policy evaluation and industrial statistics. Prior research defines the affected space broadly, including beverage and processed-food production as well as downstream commercialization channels (Paraje et al., 2021). Consistent with this view, Colombia's *Encuesta Anual Manufacturera* (EAM) identifies beverage manufacturing (ISIC 1103–1104), other food manufacturing (ISIC 1089), and food and beverage wholesale and retail trade (ISIC 4631, 4724–4729) as core nodes in the national food-industry chain, spanning production, packaging, distribution, and point-of-sale stages (DANE, 2025). The World Trade Organization applies a comparable definition, covering the manufacturing, processing, preservation, warehousing, and distribution of food and beverage products—corresponding to ISIC Divisions 15–16 in Revision 3.1 and to Divisions 10–11 in Revision 4 (World Trade Organization, 2024).

Panel C includes sectors with no direct role in this chain. This selection are based on the sectors that had the highest weights in the SC and SDiD main results (see appendix A.2) and that were not related to the food value chain. While each SC and SDiD analysis computes its own set of weights to reconstruct the counterfactual levels of employment and income from sectors in groups A and B, we use the panel C set as a robustness check of the basic DiD results.

Table 2 summarizes pre- and post-reform averages for key outcomes. Columns on the left present individual-level indicators (income, hours worked, informality, firm size, education, gender, location), while those on the right report sector-level employment and average earnings by ISIC code. Asterisks denote significant mean

differences between periods.

Overall, descriptive statistics reveal notable post-reform patterns. In food and beverage sectors, informality declines and average income rises, though retail employment contracts slightly. Unrelated sectors display heterogeneous changes, likely reflecting broader labor-market dynamics rather than the policy itself. These findings motivate the econometric identification strategy presented next.

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								
Labor income	1,051,599	1,269,286	Production of malt beverages	1103	6,113	6,454**	1,575,784	1,999,094**
Hours worked	44.8	45.6*	Production of non-alcoholic beverages	1104	20,633	17,267***	1,377,742	1,613,378
Informality	60.4	54.9***	Wholesale of beverages and tobacco	4632	14,531	9,189	1,682,162	1,837,250
Small firm	80.8	77.8	Retail of beverages and tobacco	4724	64,529	36,993	745,091	881,894*
Medium firm	13.5	15.6	Packaging and bottling	8292	4,648	2,704	1,016,110	1,157,482
Large firm	5.6	6.6		-	-	-	-	-
Urban	85.8	85.7		-	-	-	-	-
Rural	14.2	14.3		-	-	-	-	-
Female	48.3	45.3		-	-	-	-	-
Male	51.7	54.7		-	-	-	-	-
Primary	19.0	17.5		-	-	-	-	-
Secondary	53.8	55.3		-	-	-	-	-
Tertiary	27.2	27.2		-	-	-	-	-
Total (N)	110,454	72,607	*Total (sector)*	-	XXX	XXX	-	-
Panel B: Other food industry								
Labor income	1,316,027	1,591,587	Manufacture of other food products	1089	40,889	29,026	1,540,777	1,817,485***
Hours worked	46.3	46.1**	Wholesale of food	4631	74,838	53,995	1,543,250	1,811,581***
Informality	47.1	41.5***	Retail of other food	4729	44,151	25,317	739,646	839,312*
Small firm	57.3	53.2		-	-	-	-	-
Medium firm	28.3	32.1		-	-	-	-	-
Large firm	14.4	14.7		-	-	-	-	-
Urban	88.4	90.1		-	-	-	-	-
Rural	11.6	9.9		-	-	-	-	-
Female	44.9	42.3		-	-	-	-	-
Male	55.1	57.7		-	-	-	-	-
Primary	13.8	12.6**		-	-	-	-	-
Secondary	54.5	54.5		-	-	-	-	-
Tertiary	31.7	32.9		-	-	-	-	-
Total (N)	159,879	108,337	*Total (sector)*	-	XXX	XXX	-	-
Panel C: Unrelated to food industry								
Labor income	1,904,362	2,271,953	Lignite mining	0520	78	177*	3,537,444	2,980,000
Hours worked	44.9	44.3	Wood veneer	1620	658	643	1,801,430	1,844,800
Informality	43.5	40.2***	Cement, lime & plaster	2394	8,756	7,183**	2,559,229	2,826,448
Small firm	61.3	60.8	Wiring devices	2732	398	465	2,283,636	1,966,667
Medium firm	23.8	24.3	Residential construction	4111	544,520	395,238***	1,072,146	1,263,654***
Large firm	14.9	14.9	Fund distribution	6494	301	188	2,066,159	2,360,000
Urban	86.2	86.3	Brokerage activities	6612	1,299	1,255	3,677,949	5,089,032
Rural	13.8	13.7	Office admin services	8211	2,274	1,153	1,755,892	2,300,212
Female	29.7	30.3	Public admin exec.	8412	156,054	113,840***	2,916,890	3,258,501***
Male	70.3	69.7	Combined education	8530	293,644	228,630***	2,703,278	3,274,953***
Primary	17.8	16.6	Theatrical creation	9003	214	471***	2,541,429	6,020,000
Secondary	38.2	39.0		-	-	-	-	-
Tertiary	43.9	44.4		-	-	-	-	-
Total (N)	1,008,195	749,243	*Total (sector)*	-	XXX	XXX	-	-

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. Asterisks indicate statistically significant differences in means between pre- and post-periods. The sectors included in Panel C are based on the sectors that had the highest weights in the SC and SDiD main results (see appendix A.2) and that were not related to the food value chain. Appendix A.1 shows that missing values in the income variable are generally low and balanced across periods.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. .
Source: Own calculations based on GEIH 2022–2024.

3.2 Difference-in-Differences (DiD)

We estimate DiD models with repeated cross-sections of workers at quarterly frequency (2022Q1–2024Q4). Our preferred specification is an event-study that recovers dynamic effects and allows a visual assessment of pre-trends:

$$\log(\text{Income}_{ist}) = \alpha_s + \delta_t + \sum_{k \neq 0} \beta_k \mathbb{1}\{t = k\} \times \text{Treat}_s + \gamma^\top X_{ist} + \varepsilon_{ist}, \qquad (1)$$

where i indexes workers, s ISIC 4-digit sectors, and t quarters. Sector fixed effects α_s capture time-invariant sector characteristics and time fixed effects δ_t absorb aggregate shocks. Treat_s indicates that sector s is directly exposed. Event time is normalized so that $t = 0$ corresponds to 2023Q4. The coefficients β_k trace quarter-specific treatment effects relative to $t = 0$.

The vector X_{ist} includes worker-level covariates (gender, education, informality), firm-size dummies (small/medium/large), and urban/rural status. Standard errors are clustered at the sector level to account for serial correlation within sectors. As a summary, we also report a two-period DiD estimator that collapses the post-period (2023Q4–2024Q4).

3.3 Interrupted Time Series with Controls (ITSA)

As a complementary specification at the *sector* level, we estimate a comparative ITSA that models potential changes in both *levels* and *slopes* around the intervention:

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

where Y_{st} is a sector-level outcome (e.g., log number of workers 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. When analyzing trends, we also report specifications that include a post-policy slope change and its interaction with treatment. We use Newey–West standard errors to correct for serial correlation and present fitted versus counterfactual series for visualization, following the formulation in Linden (2015).

3.4 Synthetic Difference-in-Differences (SDiD)

To strengthen identification with few treated units and multiple pre-periods, we apply the Synthetic DiD estimator, which combines features of synthetic control and DiD (**?**). The estimator chooses unit weights that best match treated sectors' pre-policy outcomes while incorporating unit and time fixed effects to absorb unobserved heterogeneity. We implement SDiD using the open-source `sdid` package (Pailanir et al., 2023). Our approach follows the event-study SDiD extensions of Ciccia (2024) and implementation guidance from Clarke et al. (2023).

Estimation details and robustness. Across all methods we (i) use the same treated/control definitions from Section 3.1, (ii) keep the post period fixed at 2023Q4–2024Q4, (iii) report heteroskedasticity-robust standard errors clustered at the sector level

(worker-level models) or Newey–West (sector-level time series), and (iv) examine sensitivity to alternative control sets (similar-food vs. unrelated). Full robustness and alternative estimators are summarized in Appendix A.

4 Results

This section presents the estimated effects of Colombia's new food and beverage taxes and front-of-package labeling (FOPL) 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 most exposed sectors (non-alcoholic beverages and processed foods) during the first year after implementation. Point estimates are generally small and often 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 2 shows the estimated effects on (log) employment using three alternative approaches — Difference-in-Differences (DiD), Synthetic Control (SC), and Synthetic Difference-in-Differences (SDiD) — for (i) non-alcoholic beverages and distribution, and (ii) other food manufacturing and commercialization.

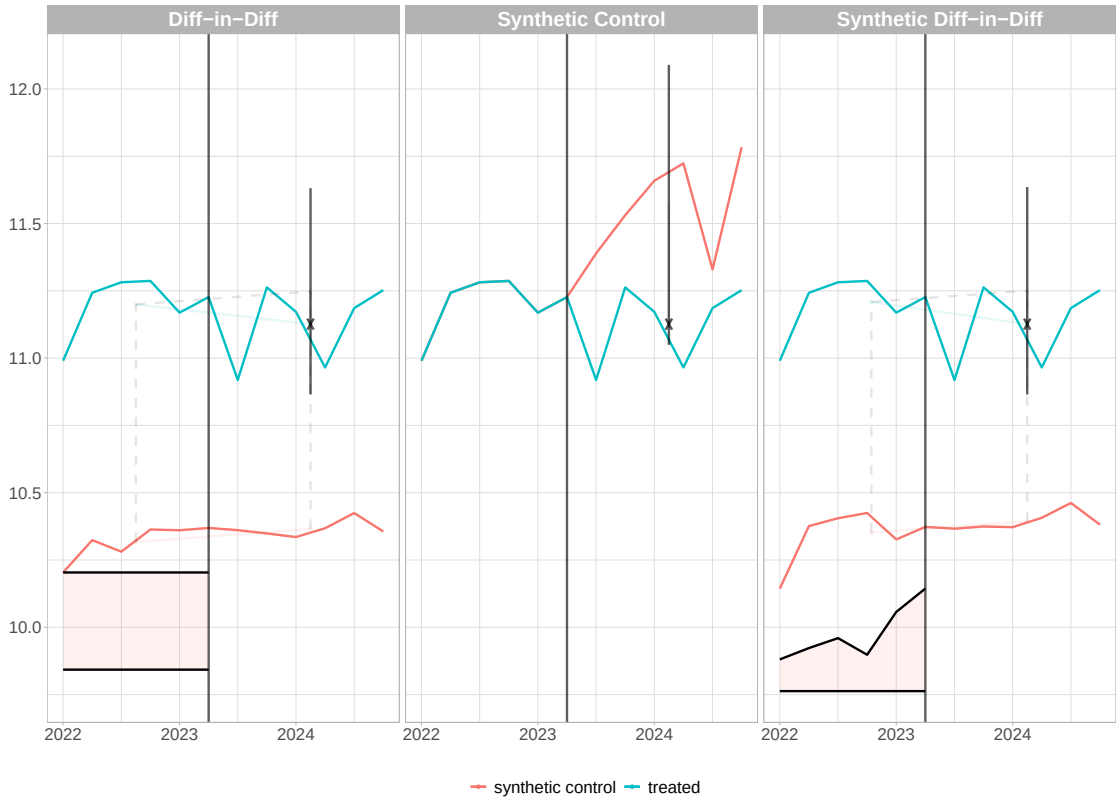
Table 3 reports the post-reform treatment effects. For non-alcoholic beverages, the SDiD estimate is -0.12 with a standard error of 0.19, and the simple DiD estimate is also -0.12 (s.e. 0.19). For the other food group, the SDiD estimate is -0.08 (s.e. 0.24), and the DiD estimate is -0.09 (s.e. 0.25). None of these coefficients are statistically different from zero at conventional confidence levels.

The synthetic control model produces somewhat larger negative point estimates (for example, -0.44 for non-alcoholic beverages and -0.37 for other food), but the corresponding 95% confidence intervals include zero. In other words, even under the most pessimistic specification we do not obtain a statistically significant drop in total employment.

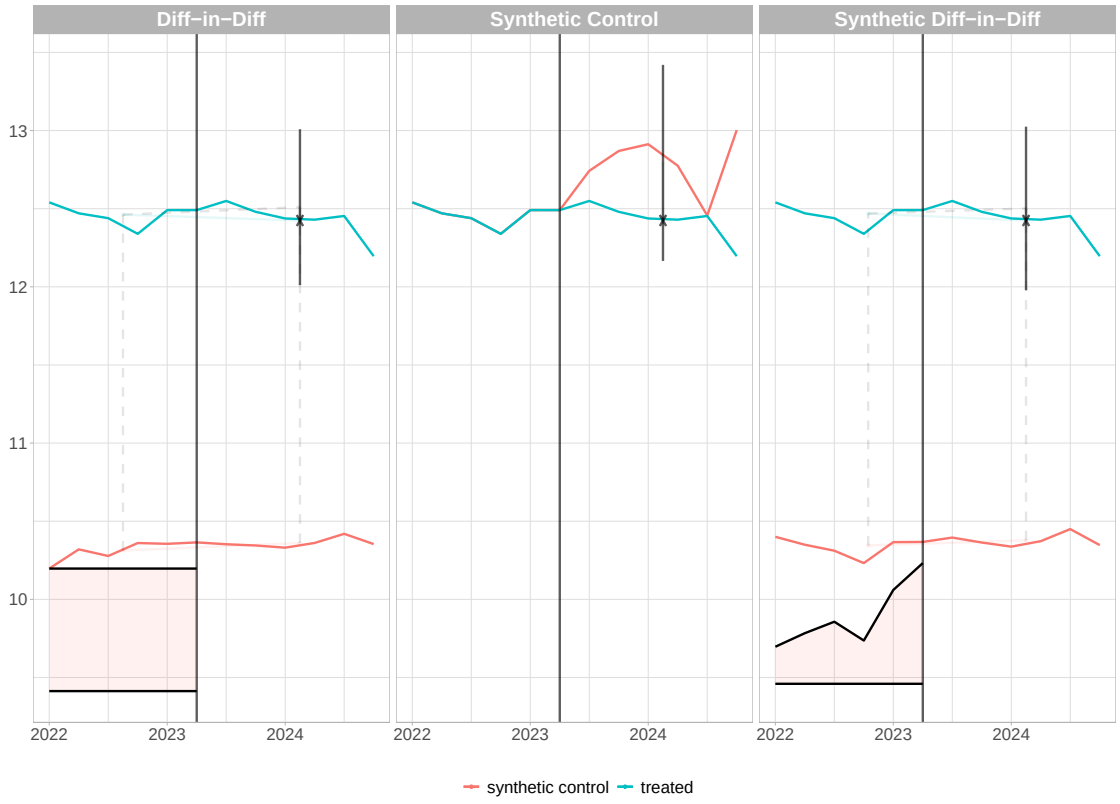
Results using ITSA are qualitatively similar (Appendix A.3.1). Taken together, these findings indicate no clear evidence of an immediate reduction in headcount in beverage or food-related sectors following the policy.

Figure 2: Estimates of the impacts on the number of workers in directly affected sectors

Panel A. Non alcoholic beverages



Panel B. Other food



Note: the calculations are based on data from the GEIH 2022–2024.

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

Model	Non-alcoholic beverage				Other food			
	Estimate	Std. Error	CI Lower	CI Upper	Estimate	Std. Error	CI Lower	CI Upper
Synthetic DiD	-0.12	0.19	-0.50	0.25	-0.08	0.24	-0.55	0.40
Synthetic Control	-0.44	0.26	-0.95	0.06	-0.37	0.33	-1.02	0.28
Diff-in-Diff (DiD)	-0.12	0.19	-0.50	0.26	-0.09	0.25	-0.57	0.40
Observations	Control: 5,574 Treatment: 30				Control: 5,586 Treatment: 18			
Periods	Control: 12 Treatment: 6				Control: 12 Treatment: 6			
Sectors	Control: 467 Treatment: 5				Control: 467 Treatment: 3			

Note: The calculations are based on data from the GEIH 2022–2024. 95% confidence intervals were computed using standard errors based on the placebo variance–covariance method, applying the normal approximation $\hat{\tau} \pm 1.96 \times SE$.

4.2 Income: Log Labor Income

We next study earnings. Figure 3 shows the results for the logarithm of sector-level income under the same three strategies (DiD, SC, SDiD) for non-alcoholic beverages (Panel A) and other food (Panel B). Table 4 summarizes the corresponding estimates. The weights given to the control sectos in the SC and SDiD are presented in appendix A.2.

For most models, we again detect no statistically significant change. The DiD estimates for average income are -0.18 (s.e. 0.40) for non-alcoholic beverages and -0.04 (s.e. 0.41) for other food. The SDiD results are similarly close to zero and imprecisely estimated.

One exception appears in the synthetic control specification for non-alcoholic beverages: the point estimate is -0.92 with a standard error of 0.36, and the reported 95% confidence interval lies entirely below zero. While this result suggests a possible short-run decline in average labor income in beverage-related sectors, it should be interpreted with caution. The synthetic control estimator assigns relatively high weight to a narrow subset of comparison sectors that most closely replicate pre-treatment levels, which can amplify shocks specific to those sectors. By contrast, the synthetic difference-in-differences (SDiD) model incorporates a broader set of comparison sectors while still enforcing parallel-trend alignment in the pre-policy period. Consequently, we view the SC estimate as potentially overstating the magnitude of the post-treatment decline. For other food sectors, the synthetic control estimate (about -0.55 with a standard error of 0.60) is smaller and not statistically significant.

To test whether similar patterns appear at the worker level, Table 5 turns to individual log income. The coefficient on the *Post* $\times$ *Treatment* interaction is statistically insignificant in all specifications, both for non-alcoholic beverages and for other food. For example, in the beverage sectors the estimate is -0.21 (s.e. 0.28)

using all other sectors as controls, and −0.24 (s.e. 0.32) using unrelated sectors. For the food sectors, the corresponding estimates are 0.20 (s.e. 0.15) and 0.17 (s.e. 0.25). None of these are precisely estimated.

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. This provides an internal validity check on the individual-level regression.

Figure 4 shows an event-study version of these estimates. The coefficients hover around zero in both the pre- and post-policy periods, with wide confidence intervals and no visible structural break after implementation. This is consistent with the regression results: we do not observe broad, immediate wage shocks at the individual level in treated sectors.

Figure 3: Synthetic Difference-in-Difference - income (Sector Level)

Panel A. Non alcoholic beverages

Panel B. Other food

Note: the calculations are based on data from the GEIH 2022–2024.

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

Model	Non-alcoholic beverage				Other food			
	Estimate	Std. Error	CI Lower	CI Upper	Estimate	Std. Error	CI Lower	CI Upper
Synthetic DiD	-0.15	0.36	-0.85	0.55	-0.03	0.47	-0.95	0.88
Synthetic Control	-0.92	0.36	-1.62	-0.22	-0.55	0.60	-1.72	0.63
Diff-in-Diff (DiD)	-0.18	0.40	-0.97	0.62	-0.04	0.41	-0.85	0.78
Observations	Control: 5.574 Treatment: 30				Control: 5.586 Treatment: 18			
Periods	Control: 12 Treatment: 12				Control: 12 Treatment: 12			
Sectors	Control: 467 Treatment: 5				Control: 467 Treatment: 3			

Note: The calculations are based on data from the GEIH 2022–2024. 95% confidence intervals were computed using standard errors based on the placebo variance–covariance method, applying the normal approximation $\hat{\tau} \pm 1.96 \times SE$.

Table 5 shifts the analysis to individual workers rather than sectors. The coefficient on the *Post* × *Treatment* interaction is statistically insignificant in all specifications, both for non-alcoholic beverages and for other food. While the point estimates differ slightly depending on the control group, there is no consistent evidence of an effect on workers' earnings.

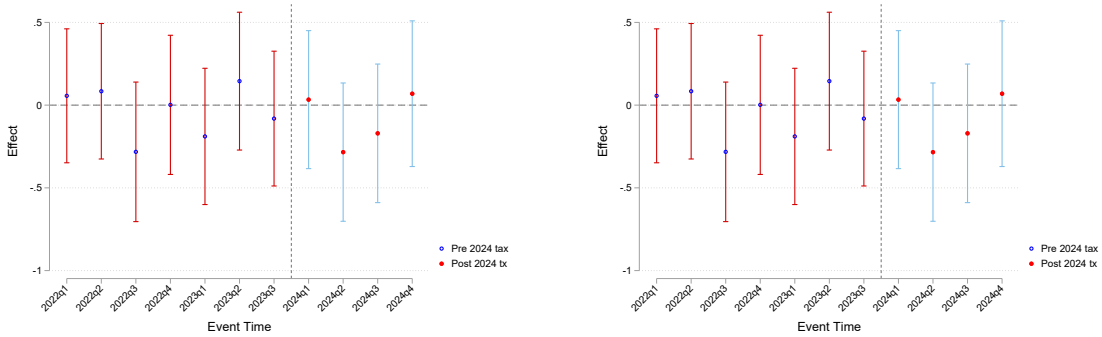
The remaining covariates behave as expected. Workers in medium and large firms earn more than those in small firms; wages are higher in urban areas; and there are systematic differences by gender and education. This pattern supports internal consistency of the specification.

Table 5: Effect on log income by type of control group (Individual level)

	Non-A beverages		Other food	
	All other sectors	Unrelated sectors	All other sectors	Unrelated sectors
Post × Treatment	-0.21	-0.24	0.20	0.17
	(0.28.)	(0.32)	(0.15)	(0.25)
Small firm [B]				
Medium firm	1.39***	0.48***	1.40***	0.64***
	(0.13)	(0.09)	(0.13)	(0.09)
Large firm	1.52***	0.78***	1.52***	0.97***
	(0.15)	(0.12)	(0.14)	(0.11)
Urban [B]				
Rural	-0.55***	-0.16***	-0.55***	-0.22***
	(0.09)	(0.07)	(0.09)	(0.07)
Male [B]				
Female	-1.01***	-0.52***	-1.01***	-0.51***
	(0.21)	(0.13)	(0.21)	(0.12)
Primary [B]				
Secondary	-0.23***	-0.04	-0.23***	0.01
	(0.07)	(0.06)	(0.07)	(0.06)
Tertiary	-0.42***	-0.07***	-0.43***	-0.26***
	(0.11)	(0.11)	(0.11)	(0.11)
Constant	12.69***	13.21***	12.69***	13.15***
	(0.11)	(0.06)	(0.13)	(0.06)
Units	846,843	67,711	850,328	71,196
Periods	12	12	12	12
Clusters	495	.	493	.
Observations	846,843	67,711	850,328	71,196

Note: the calculations are based on data from the GEIH 2022–2024. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. For the regression with the "All other sectors" control group, we cluster standard errors at the economic-activity level; for the "Unrelated sectors" specification we report robust standard errors.

Figure 4: Event study: Log income in treated versus control sectors (individual level)



(a) Non alcoholic beverage

(b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. The control group corresponds to all the other economic sectors. Coefficients are displayed as points and 95% confidence intervals, using standard errors clustered at the sector level, are shown.

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 and/or (ii) the composition of employment between formal and informal arrangements. We use weekly hours and informality as proxies for these margins of adjustment.

4.3.1 Hours Worked

Table 6 reports difference-in-differences estimates for weekly hours worked. In the non-alcoholic beverage group, the *Post* × *Treatment* coefficient is positive and statistically significant across specifications (around 1 additional hour per week). This suggests that, in beverage-related activities, the adjustment margin after the reform may have been intensive: slightly more hours per worker rather than fewer workers.

For the other food group, the estimated effects on hours are small, negative, and in some cases statistically significant when using all other sectors as the control group, but they are not robust when using unrelated sectors as the control. We interpret these food-sector results with more caution because they are sensitive to the control definition.

Table 6: Effect on hours worked per week by type of control group

	Non-A beverages		Other food	
	All categories	Unrelated control	All categories	Unrelated control
Post × Treatment	0.989***	0.928**	-0.372***	-0.469
	(0.148)	(0.443)	(0.111)	(0.285)
Small	-4.163***	-3.815***	-4.157***	-3.747***
	(0.431)	(0.126)	(0.426)	(0.120)
Medium	-0.549***	-0.749***	-0.548***	-0.736***
	(0.160)	(0.108)	(0.157)	(0.100)
Urban	2.055***	1.256***	2.042***	1.111***
	(0.270)	(0.146)	(0.269)	(0.141)
Male	4.987***	3.377***	4.949***	2.837***
	(0.417)	(0.147)	(0.415)	(0.127)
Primary [B]				
Secondary	-0.0622	0.300***	-0.0810	0.0886
	(0.153)	(0.103)	(0.153)	(0.0979)
Tertiary	-1.316***	-0.907***	-1.341***	-1.279***
	(0.289)	(0.153)	(0.287)	(0.141)
Age 30–44	2.183***	1.212***	2.174***	1.171***
	(0.266)	(0.103)	(0.264)	(0.0969)
Age 45–59	1.723***	0.865***	1.717***	0.855***
	(0.302)	(0.111)	(0.301)	(0.106)
Age 60+	-1.664***	-1.513***	-1.658***	-1.378***
	(0.346)	(0.160)	(0.346)	(0.152)
Constant	42.25***	44.00***	42.31***	44.86***
	(0.482)	(0.236)	(0.479)	(0.221)
Units	846,834	67,710	850,320	71,196
Periods	12	12	12	12
Cluster	495	.	495	.
Observations	846,834	67,710	850,320	71,196

Note: the calculations are based on data from the GEIH 2022–2024. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3.2 Informality

Table 7 examines labor informality (working without social security or formal benefits). The *Post* × *Treatment* coefficients are very small and statistically indistinguishable from zero for both the non-alcoholic beverage group and the other food group, regardless of the control definition. This indicates that the reform did not trigger an immediate shift toward informality (or formalization) in these sectors.

By contrast, the covariates line up with standard patterns in the Colombian labor market: informality is much higher in small firms than in large firms, lower in urban areas, and strongly decreasing with education. These patterns confirm that the measure of informality is behaving as expected.

Overall, hours and contract type provide no evidence of large-scale labor restructuring. If firms adjusted in response to the policy, that adjustment does not appear to have taken the form of cutting hours, shedding formal jobs, or replacing formal with informal work.

Table 7: Effect on informality by type of control group

	Non-A beverages		Other food	
	All categories	Unrelated control	All categories	Unrelated control
Post × Treatment	-0.00879*	-0.0101	-0.00188	-0.00171
	(0.00526)	(0.00901)	(0.00612)	(0.00802)
Small	0.633***	0.717***	0.633***	0.714***
	(0.0227)	(0.00477)	(0.0224)	(0.00448)
Medium	0.189***	0.177***	0.189***	0.176***
	(0.0185)	(0.00397)	(0.0182)	(0.00375)
Urban	-0.0186***	-0.0202***	-0.0186***	-0.0204***
	(0.00496)	(0.00335)	(0.00496)	(0.00349)
Male	-0.0225***	0.0134***	-0.0221***	0.0148***
	(0.00625)	(0.00385)	(0.00620)	(0.00383)
Secondary	-0.0529***	-0.0396***	-0.0533***	-0.0446***
	(0.00468)	(0.00261)	(0.00474)	(0.00267)
Tertiary	-0.168***	-0.187***	-0.169***	-0.194***
	(0.00859)	(0.00481)	(0.00861)	(0.00468)
Constant	0.372***	0.295***	0.371***	0.296***
	(0.0212)	(0.00675)	(0.0210)	(0.00667)
Units	846,843	67,711	850,328	71,196
Periods	12	12	12	12
Clusters	495	.	493	.
Observations	846,843	67,711	850,328	71,196

Note: the calculations are based on data from the GEIH 2022–2024. Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The coefficients on the treatment–post interaction terms are small and statistically insignificant across all specifications. This suggests that the reform did not induce formalization or a shift toward informality in the short run. In contrast, covariates behave as expected: informality is significantly lower among medium and large firms, and higher in rural areas and among workers with lower education levels. Taken together, the results indicate that the policy did not meaningfully alter the structure of formal versus informal employment in treated sectors.

Our analysis reveals no significant changes in these indicators post-policy, which suggests that firms may have adjusted along other margins such as pricing or product reformulation rather than labor restructuring. Furthermore, the phased implementation likely eased abrupt shifts in workforce demand.

4.4 Heterogeneous effects

Finally, we assess whether the policy is associated with distributional or subgroup-specific patterns that are not visible in the averages. Given that most estimates did not differ across the main methods, we keep the analysis to simple DID.

Figures 5–9 report Difference-in-Differences estimates by gender, location, occupation type, age group, and education for three outcomes: (i) log income, (ii) total hours worked, (iii) hourly wages, and (iv) total employment.

Most subgroup estimates are close to zero and statistically insignificant, consistent with the aggregate results. Still, three patterns stand out:

- **Gender.** In several cases, female workers in treated sectors exhibit statistically significant post-policy effects (for example, in income and hours worked), while the corresponding estimates for male workers are smaller and less precisely estimated. . This suggests that part of the within-sector adjustment may have been absorbed by women.

- **Occupation.** Among white-collar workers in the non-alcoholic beverage group, total hours worked increase significantly after the reform. By contrast, estimated effects for blue-collar workers are smaller and often indistinguishable from zero. This points to an adjustment margin in administrative, supervisory, or coordination roles rather than in production headcount.

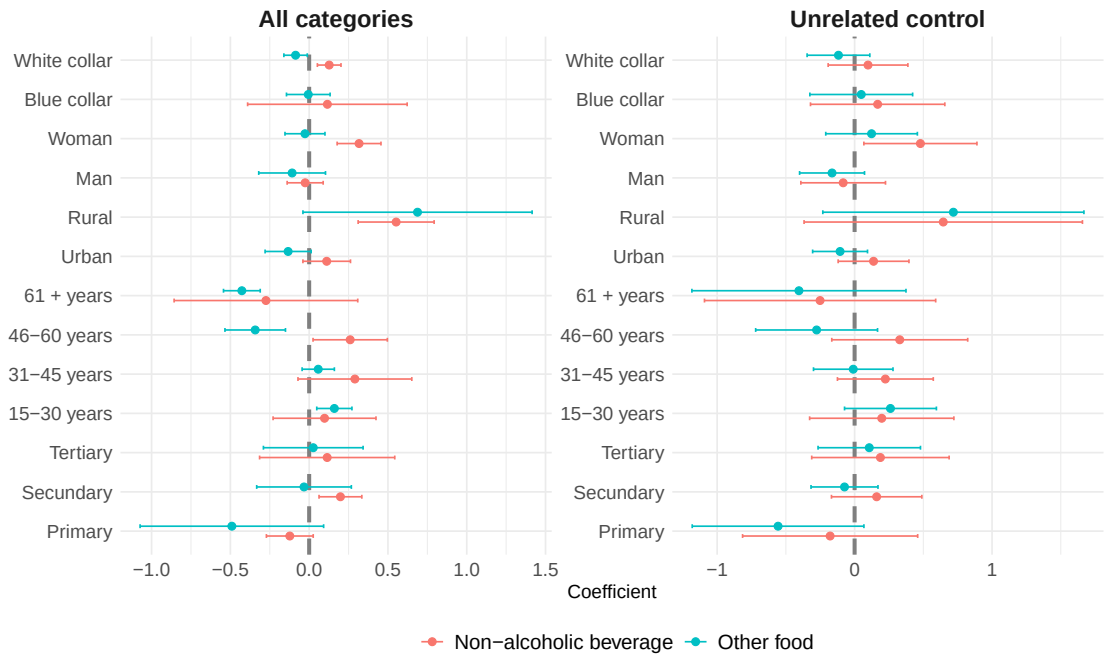
- **Age.** There is a reduction in the 'other food' sectors on the total number of workers of the younger groups (15-30, 31-45). This is not reflected in the total income.

- **Location.** Rural workers show statistically significant positive changes in some specifications (especially in total hours), while urban workers do not. This is consistent with a mild expansion of labor activity in rural or distribution-linked parts of the supply chain.

Outside of these specific cases, we do not detect robust heterogeneous effects by education level or age that would indicate systematic reallocation of labor.

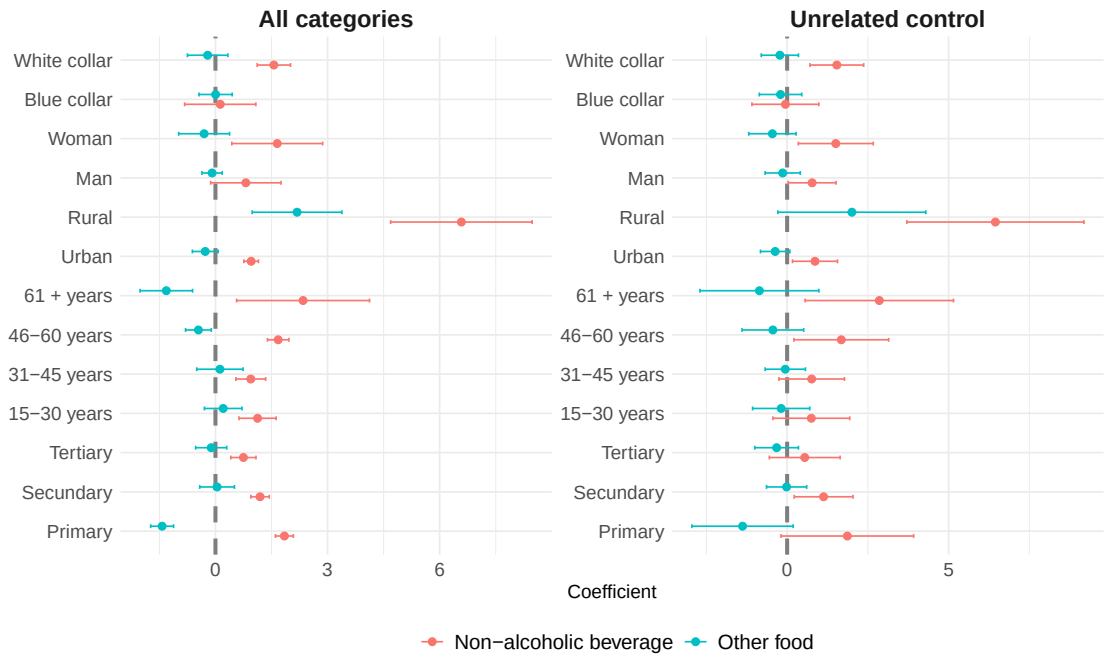
In summary, the early adjustments we observe are not mass layoffs or broad wage cuts. Instead, they appear in specific margins: slightly higher hours per worker in beverage-related activities, and subgroup differences concentrated among women, rural workers, and white-collar occupations.

Figure 5: DiD impact on log-income by worker characteristics



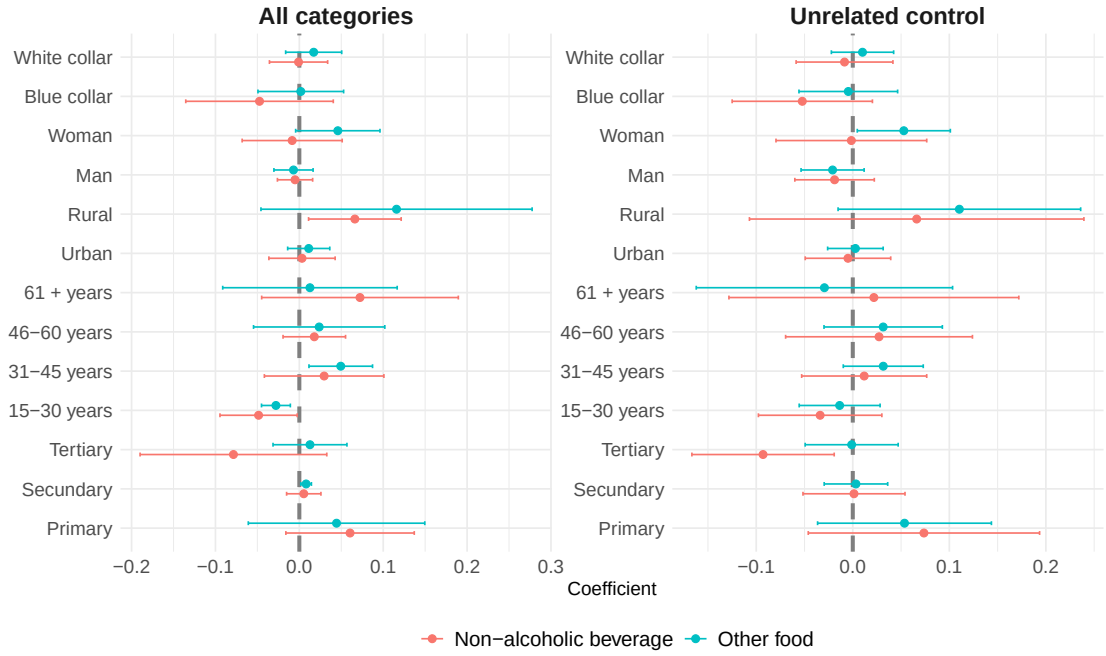
Note: the calculations are based on data from the GEIH 2022–2024. For the regressions with the "All other sectors" control group, we cluster standard errors at the economic-activity level while for the "unrelated control" we use robust standard errors.

Figure 6: DiD impact on total hours by worker characteristics



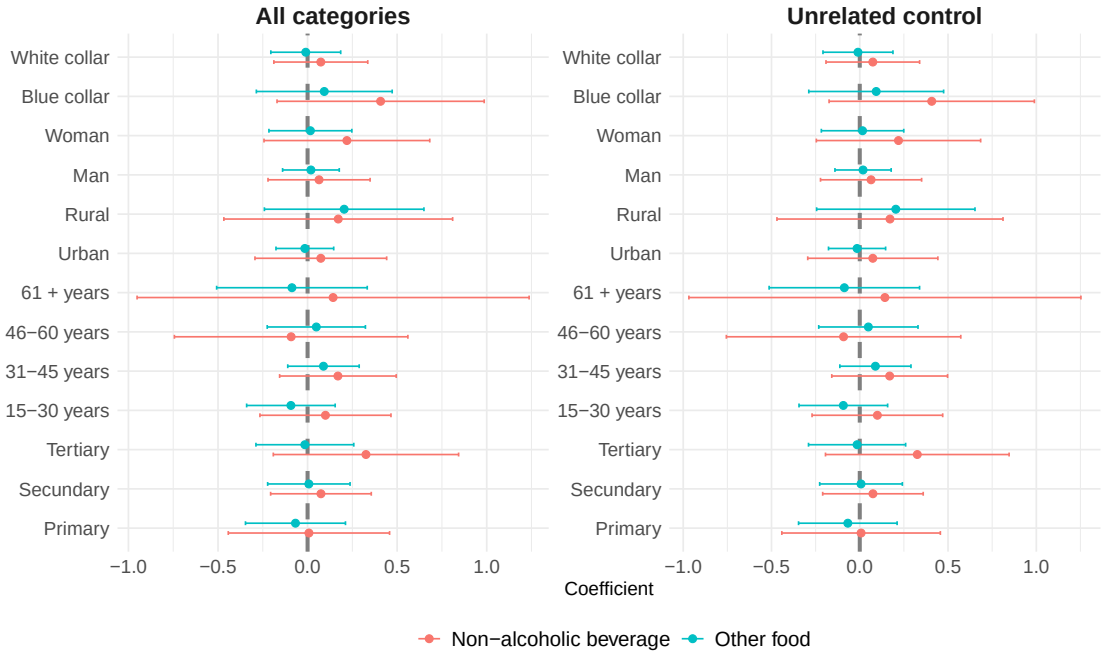
Note: the calculations are based on data from the GEIH 2022–2024. For the regressions with the "All other sectors" control group, we cluster standard errors at the economic-activity level while for the "unrelated control" we use robust standard errors.

Figure 7: DiD impact on wage per hours by worker characteristics



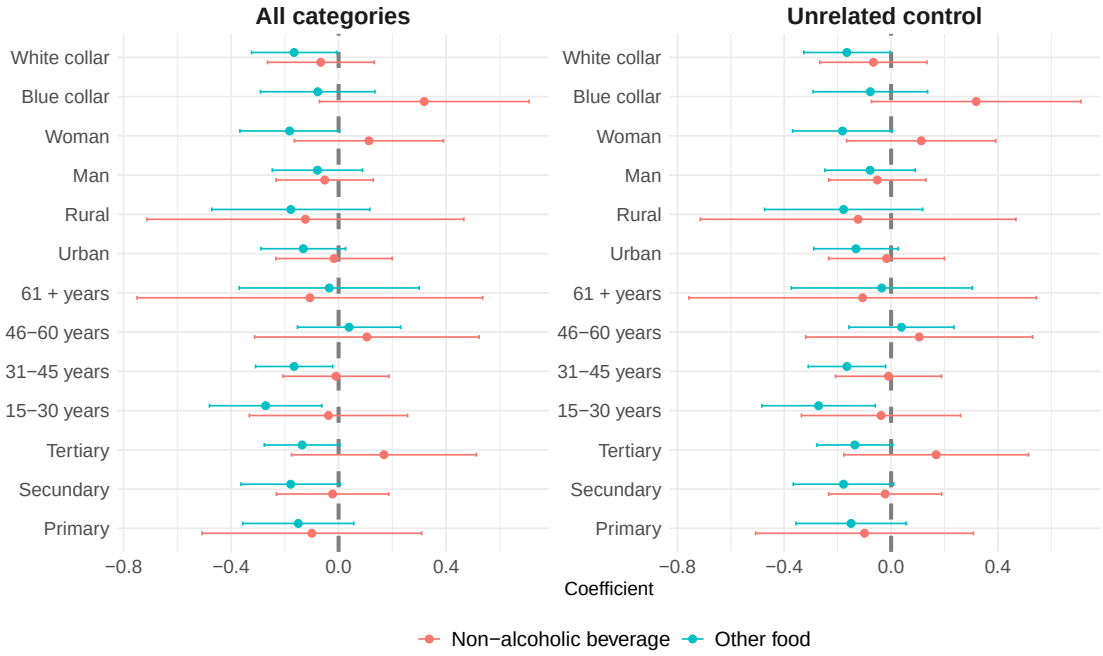
Note: the calculations are based on data from the GEIH 2022–2024. For the regressions with the "All other sectors" control group, we cluster standard errors at the economic-activity level while for the "unrelated control" we use robust standard errors.

Figure 8: DiD impact on log-income by sector characteristics



Note: the calculations are based on data from the GEIH 2022–2024. For the regressions we use robust standard errors.

Figure 9: DiD impact on log-employment by sector characteristics



Note: the calculations are based on data from the GEIH 2022–2024. For the regressions we use robust standard errors.

5 Discussion

This study provides early empirical evidence on the labor market effects of Colombia's recent fiscal and regulatory policies on ultra-processed foods (UPFs) and sugar-sweetened beverages (SSBs). Implemented between 2023 and 2024, these measures combined an excise tax on SSBs with the phased introduction of front-of-package labeling (FOPL) for products exceeding thresholds of sugar, sodium, or saturated fat. Using nationally representative labor force data and a combination of synthetic control, synthetic difference-in-differences, and traditional DiD models, we find no statistically significant short-term effects on aggregate employment, earnings, informality, or hours worked in the sectors directly affected by the reform.

These results suggest that the policy, at least during its initial implementation phase, did not disrupt labor demand in beverage and food manufacturing or related industries. This finding is consistent with the experience of other Latin American countries that have adopted comparable measures. In Mexico, Guerrero-López et al. (2017) found no significant employment losses following the introduction of SSB and high-calorie food taxes. Similarly, Paraje et al. (2021) reported no significant changes in wages or employment after Chile's 2016 FOPL reform, and Díaz et al. (2023) documented neutral effects in Peru's beverage and food industries after analogous policies. Together, these findings reinforce the notion that fiscal and labeling interventions aimed at improving nutrition can be implemented without jeopardizing jobs in affected sectors.

While aggregate employment and income remained stable, our results reveal a modest but statistically significant increase in total hours worked among employees in the non-alcoholic beverage sector. This pattern, concentrated among white-collar, rural, and female workers, suggests an adjustment along the intensive margin rather than through layoffs or wage reductions. In practice, firms may have temporarily increased workloads to adapt to reformulation or marketing changes associated with the new regulatory environment, consistent with evidence from other contexts where firms reallocate tasks instead of reducing staff (Paraje et al., 2021; Guerrero-López et al., 2017).

The stability of aggregate employment and earnings likely reflects the way firms adjusted to the reform. Evidence from other settings shows that manufacturers often respond through non-labor margins, particularly by reformulating products or substituting taxed ingredients with untaxed alternatives such as non-caloric sweeteners (Colchero et al., 2017; Ng et al., 2019). In general, sweeteners are cheaper than sugarcane-based inputs (Sharma et al., 2014), therefore such reformulation may have allowed beverage producers to maintain output and employment levels. Moreover, the gradual rollout of the tax and FOPL policy likely smoothed the adjustment process, avoiding sudden changes in production or labor demand.

While aggregate results show limited disruption, our heterogeneous analyses reveal meaningful differences across worker groups. First, the reform appears associated with increased hours worked among *white-collar* workers in the non-alcoholic beverage sector, possibly reflecting short-term adjustments in managerial and supervisory roles related to compliance, reformulation, and logistical changes. Second, *rural workers* show modest positive effects, suggesting that peripheral supply-chain activities—such as ingredient sourcing, packaging, or distribution—may have experienced a temporary expansion. Third, we detect small but statistically significant

differences for *female workers*, whose labor outcomes exhibit distinct adjustment patterns relative to men. These results are consistent with gendered segmentation in the Colombian labor market, where women are more concentrated in administrative and service roles and thus may have been more directly exposed to compliance-related or demand-side shifts.

Beyond these differences, there is no evidence of systematic effects by education level, firm size, or urban location. However, smaller firms could still face longer-term challenges, as they operate with thinner margins and limited capacity to absorb regulatory or demand shocks. Future research using administrative data sources such as the *Encuesta Anual Manufacturera* (EAM) and *Encuesta Anual de Servicios* (EAS) could provide insight into these dynamics, including firm entry and exit, reallocation, and employment transitions.

Finally, understanding how these policies shape consumption and production patterns across the broader food system remains critical. In Mexico, households shifted from taxed SSBs toward bottled water and untaxed beverages (Colchero et al., 2017). In Colombia, substitution could extend toward minimally processed foods such as fruits, legumes, and starchy staples—products already widely consumed according to the 2015 National Nutrition Survey (Herrán, 2024). Such a shift would reinforce the public health goals of the reform while supporting employment in traditional food markets and local supply chains.

In summary, our results suggest that Colombia's combined tax and FOPL policies did not generate immediate employment or wage losses in affected sectors. Instead, firms and workers appear to have adapted through compositional changes in production and labor allocation. These findings align with regional evidence showing that well-designed health-related fiscal policies can coexist with stable labor markets when implemented gradually and supported by industry adaptation and consumer substitution.

6 Conclusions

This study provides the first empirical evidence on the short-term labor market effects of Colombia's recent taxes and front-of-package labeling on unhealthy foods and beverages. Using complementary empirical strategies and nationally representative data, we find no significant short-run changes in total employment, labor income, or informality across affected sectors.

The only robust adjustment appears in the non-alcoholic beverage industry, where workers increased their weekly hours by about one hour on average. This effect—more pronounced among women, rural workers, and white-collar occupations—suggests an adaptation along the intensive margin rather than workforce contraction.

These results have two key policy implications. First, they reinforce evidence from other countries indicating that well-designed public health regulations can achieve health objectives without harming employment or earnings. Second, they highlight the importance of monitoring subtle distributional effects across worker groups, as adjustment may occur unevenly even when aggregate indicators remain stable.

Future research should use firm-level and administrative data to examine whether these labor adjustments correspond to changes in productivity, reformulation efforts,

or shifts in task composition within firms. Longer-term evaluation will also be needed as the policy reaches full implementation.

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A Appendix

A.1 Income Missing Values by Sector (ISIC Rev. 4)

Table 8 in this appendix reports missing observations for the income variable at the sector level, using the same ISIC Rev. 4 (4-digit) classification and panel structure as the main descriptive table. For each sector, we present the number of missing observations in the pre- and post-periods and their corresponding shares (% Missing Pre / % Missing Post) relative to the sector's sample in each period. Panel totals at the end of each block match the group totals in the main table. Details on the construction of the income measure are provided in the Methodology section.

Table 8: Missing values by sector

		Income			
Sector	**Code**	**Missing Pre**	**Missing Post**	**% Missing Pre**	**% Missing Post**
Panel A: Non Alcoholic Beverages					
Production of malt beverages	1103	8	8	2.6	3.2
Production of non-alcoholic beverages	1104	23	13	2.0	1.7
Wholesale of beverages and tobacco	4632	35	25	4.4	4.6
Retail of beverages and tobacco	4724	249	144	7.1	7.2
Packaging and bottling	8292	4	1	2.0	0.9
Total (sector)		**319**	**191**	**5.3**	**5.2**
Panel B: Other food					
Manufacture of other food products	1089	57	37	3.4	3.4
Wholesale of food	4631	150	121	3.6	3.9
Retail of other food	4729	192	104	8.0	8.1
Total (sector)		**399**	**262**	**4.8**	**4.8**
Panel C: Unrelated					
Lignite mining	0520	0	1	0.0	16.7
Wood veneer	1620	0	0	0.0	0.0
Cement, lime & plaster	2394	9	3	2.6	1.3
Wiring devices	2732	0	1	0.0	14.3
Residential construction	4111	698	424	2.4	2.2
Fund distribution	6494	0	0	0.0	0.0
Brokerage activities	6612	6	0**	13.3	0.0**
Office admin services	8211	1	1	1.9	2.9
Public admin exec.	8412	567	355	5.3	5.0
Combined education	8530	667	514	3.8	4.0
Theatrical creation	9003	0	1	0.0	16.7
Total (sector)		**1,948**	**1,300**	**3.3**	**3.3**

Notes: The columns "Missing" and "% Missing" refer to the **Income** variable. Source: GEIH 2022–2024. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.2 Weights after synthetic methods and construction of the unrelated control

Table 9 displays the economic activities that received the highest weights in constructing the synthetic controls. Results are shown for the two treatment groups analyzed—non-alcoholic beverages and other food—across two dependent variables: income and employment. Activities with higher weights are those that best replicate the pre-intervention behavior of treated sectors and thus form the basis of the unrelated synthetic control group.

Table 9: Sector Weights in Synthetic Control Groups

Sector		Non-Alcoholic Beverages		Other Food	
Code	**Description**	Income	Empl.	Income	Empl.
Panel A: Synthetic Control					
0520	Mining of coal and lignite	–	0.171	–	0.162
1620	Manufacture of veneer sheets and wood-based panels	–	0.068	–	–
2732	Manufacture of wiring devices	0.039	0.071	–	–
2816	Manufacture of lifting and handling equipment	–	–	–	0.050
3822	Treatment and disposal of hazardous waste	0.028	–	–	–
4111	Construction of residential buildings	0.203	0.526	0.197	0.707
5920	Sound recording and music publishing activities	–	–	0.046	–
6494	Other fund distribution activities	0.075	–	–	–
6612	Security and commodity contracts brokerage	0.050	–	–	–
6615	Activities of currency exchange professionals	–	0.041	–	–
8211	Combined office administrative service activities	0.097	0.056	0.031	–
8412	Executive activities of the public administration	0.214	–	0.326	–
8530	Establishments combining different levels of education	0.123	–	0.191	–
9003	Theatrical creation	0.091	–	0.122	–
Panel B: Synthetic Difference-in-Differences[a]					
0114	Growing of tobacco	–	0.004	–	–
1083	Manufacture of macaroni, noodles, couscous and similar farinaceous products	–	0.004	–	–
1620	Manufacture of veneer sheets and wood-based panels	–	0.004	–	–
1921	Manufacture of refined petroleum products	–	0.004	–	–
1922	Fuel blending activities	–	0.004	–	–
2212	Retreading and rebuilding of rubber tires	0.004	–	–	–
2732	Manufacture of wiring devices	–	0.004	–	–
2816	Manufacture of lifting and handling equipment	–	0.004	–	–
6431	Trusts, funds and similar financial entities	–	–	0.004	0.007
6493	Factoring activities	0.004	–	–	–
6494	Other fund distribution activities	–	–	0.004	–
6521	Social health insurance services	–	0.004	–	–

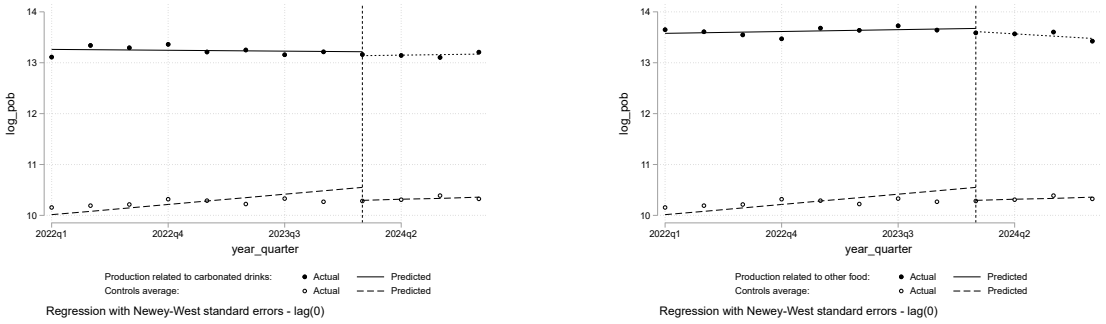
Notes:
[a]For Synthetic DiD, only sectors with maximum weight (0.4
Weights represent each control sector's contribution to the synthetic control group. Sector codes and descriptions follow the Colombian adaptation of ISIC Rev. 4 (CIIU Rev. 4 A.C. 2022).
Empl. = Employment. "–" indicates the sector was not selected for that specification.
Source: Authors' calculations using GEIH 2022-2024.

A.3 Alternative identification strategies

A.3.1 Number of workers

We next examine whether the reform affected aggregate employment levels in treated sectors using interrupted time series analysis (ITSA). Figures 10 and 11 shows observed and predicted values for sector-level employment. The predicted line represents the expected employment trajectory in the absence of the policy, based on pre-reform trends. Visually, there is no notable deviation between the actual and counterfactual series after the policy was introduced in 2023Q4, suggesting little immediate disruption in employment levels.

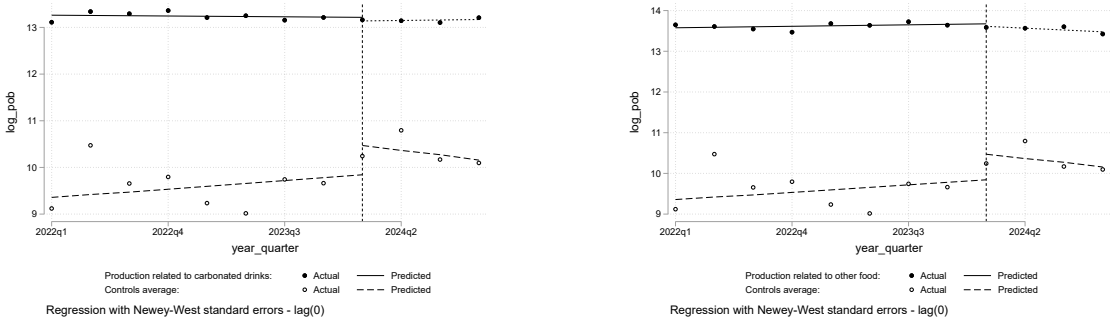
Figure 10: ITSA: log number of workers vs all categories, by sector



(a) Non alcoholic beverage (b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. For these estimates we use sector level aggregates, and the "all sectors" control group.

Figure 11: ITSA: log number of workers vs unrelated controls, by sector



(a) Non alcoholic beverage (b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. For these estimates we use sector level aggregates, and the "all sectors" control group.

Table 10 presents the corresponding ITSA regression estimates. The coefficients for the post-policy level and trend changes are small and not statistically significant. This indicates that the reform did not cause an observable break in employment patterns. These findings reinforce the conclusion that sector-level employment remained stable following implementation.

Table 10: ITSA Regression Results: Log of Population (Sector Level)

	Non-A beverages		Other food	
	All categories	Unrelated control	All categories	Unrelated control
Time	0.07***	0.06	0.07***	0.06
	(0.01)	(0.16)	(0.01)	(0.16)
Treatment-Control Level Diff	3.25***	3.90***	3.56***	4.22***
	(0.09)	(0.68)	(0.07)	(0.68)
Treatment-Control Trend Diff	-0.07***	-0.07	-0.06***	-0.05
	(0.02)	(0.16)	(0.02)	(0.16)
Control Immediate Change	-0.26**	0.63	-0.26**	0.63
	(0.11)	(1.10)	(0.11)	(1.10)
Control Trend Change	-0.04	-0.17	-0.04	-0.17
	(0.04)	(0.43)	(0.04)	(0.43)
Level Effect (Post)	0.18	-0.71	0.20*	-0.69
	(0.12)	(1.11)	(0.11)	(1.11)
Trend Effect (Post)	0.06	0.18	-0.01	0.11
	(0.05)	(0.43)	(0.05)	(0.43)
Constant	10.01***	9.36***	10.01***	9.36***
	(0.06)	(0.67)	(0.06)	(0.67)

Note: the calculations are based on data from the GEIH 2022–2024. For these estimates we use sector level aggregates, and the "all sectors" control group. Newey–West standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11 reports difference-in-differences estimates using alternative control groups. Across all specifications, the interaction between treatment and the post-policy period yields coefficients that are close to zero and not statistically significant. When using unrelated sectors as the comparison group, the estimated effect is slightly positive; when using sectors with potential spillovers, it becomes slightly negative. However, none of these differences are meaningful in magnitude or precision.

Table 11: Difference-in-Differences Estimates: Log Number of workers per sector

Analysis group:	Non-A beverages		Other food	
Control group:	All	Unrelated	All	Unrelated
Treatment × Post	-0.03	-0.26	-0.22	-0.45
	(0.18)	(0.26)	(0.19)	(0.30)
Medium Firm	-0.52***	0.06	-0.52***	0.29*
	(0.03)	(0.16)	(0.03)	(0.17)
Large Firm	-0.03	1.11***	-0.03	1.08***
	(0.03)	(0.18)	(0.03)	(0.19)
Constant	9.29***	8.95***	9.30***	9.26***
	(0.02)	(0.14)	(0.02)	(0.15)
Units				
Periods				
Clusters				
Observations	14,562	457	14,495	390

Note: The calculations are based on data from the GEIH 2022–2024. The table reports difference-in-differences estimates comparing treated sectors (non-alcoholic beverages and other food manufacturing) with control sectors (all other or unrelated sectors) across twelve quarterly periods (2022 Q1–2024 Q4). Robust standard errors for the "unrelated control" version, and clustered at the sector level for the "all others" are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 12 displays an event study of the logarithm of the number of employees by sector, centered around the implementation of the policy. Each point reflects the estimated difference in log employment between treated and control sectors in a given quarter, relative to the quarter just before the reform. The pre-treatment estimates are statistically close to zero, supporting the assumption of parallel trends. Following the introduction of the policy, the coefficients remain stable over time, with no visible changes in magnitude or direction. The series does not exhibit any abrupt shifts, suggesting continuity in employment dynamics across treated and control groups.

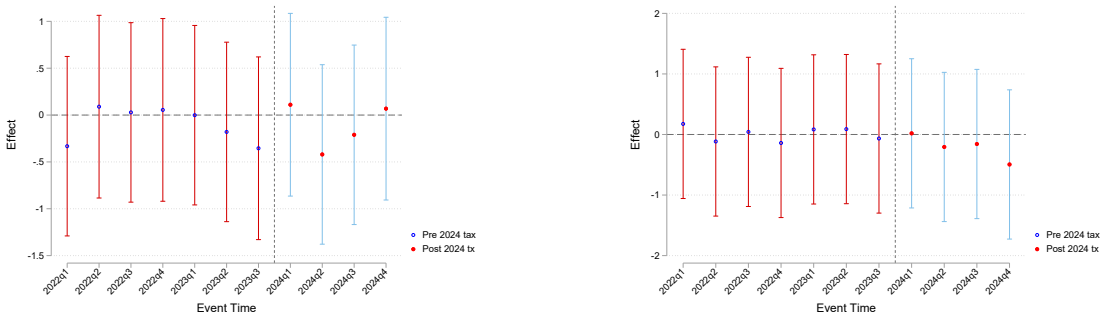
This visual pattern aligns closely with the estimates from the ITSA and DiD models, showing no marked deviations in employment levels after the policy took effect. The series remains relatively stable across quarters, with no evident change in magnitude or direction in the post-reform period.

A.3.2 Log income

We next examine whether the reform affected aggregate income in treated sectors. Figures 13 and 14 show observed and predicted values for sector-level income using interrupted time series analysis (ITSA). The predicted line represents the expected income trajectory in the absence of the policy, based on pre-reform trends. Visually, both the pre- and post-intervention periods exhibit an upward trend; however, the post-intervention slope is markedly steeper compared to the pre-intervention trend

Table 12 presents the results of the ITSA regression for (log) income by economic sector. In contrast to the analysis by individual workers, the income regression shows

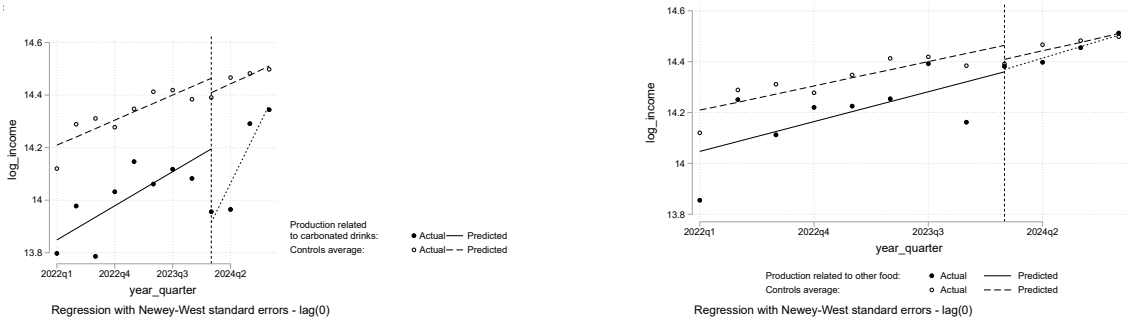
Figure 12: Event study: Log number of employees (sector level)



(a) Non alcoholic beverage (b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. The control group corresponds to all the other economic sectors. Coefficients are displayed as points and 95% confidence intervals, using standard errors clustered at the sector level, are shown.

Figure 13: ITSA: log income vs all categories, by sector

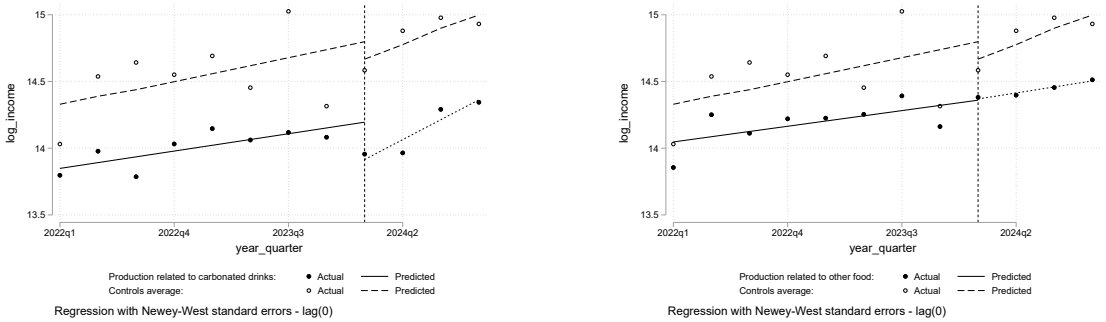


(a) Non alcoholic beverage (b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. For these estimates we use sector level aggregates, and the "all sectors" control group.

a statistically significant positive trend effect for the treatment group. Specifically, the post-treatment slope increased from 1.17 % to 10.24 %, indicating a substantial acceleration in income growth following the implementation of the policy.

Figure 14: ITSA: log income vs unrelated controls, by sector



(a) Non alcoholic beverage (b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. For these estimates we use sector level aggregates, and the "all sectors" control group.

Table 12: Regression results: Log income (sector level)

	Non-A beverages		Other food	
	All categories	Unrelated control	All categories	Unrelated control
Time	0.03***	0.06	0.03***	0.06
	(0.00)	(0.05)	(0.00)	(0.05)
Treatment-Control Level Diff	-0.36***	-0.48**	-0.16	-0.28
	(0.06)	(0.22)	(0.10)	(0.24)
Treatment-Control Trend Diff	0.01	-0.02	0.01	-0.02
	(0.01)	(0.06)	(0.02)	(0.06)
Control Immediate Change	-0.06	-0.13	-0.06	-0.13
	(0.04)	(0.30)	(0.04)	(0.30)
Control Trend Change	0.00	0.05	0.00	0.05
	(0.02)	(0.11)	(0.02)	(0.11)
Level Effect (Post)	-0.22***	-0.15	0.06	0.14
	(0.08)	(0.31)	(0.11)	(0.32)
Trend Effect (Post)	0.10***	0.06	0.00	-0.04
	(0.03)	(0.11)	(0.03)	(0.11)
Constant	14.21***	14.33***	14.21***	14.33***
	(0.02)	(0.22)	(0.02)	(0.22)

Note: The calculations are based on data from the GEIH 2022–2024. Standard errors clustered at the procedure level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13 presents difference-in-differences estimates for the logarithm of the income at the sector level, using three alternative control groups. The treatment–post interaction terms are consistently small and not statistically significant across specifications. While the sign of the estimates varies slightly, positive with unrelated controls and negative with spillover controls, none of the differences reach significance levels. These results suggest that the reform did not affect average income levels at the sector level in the short run.

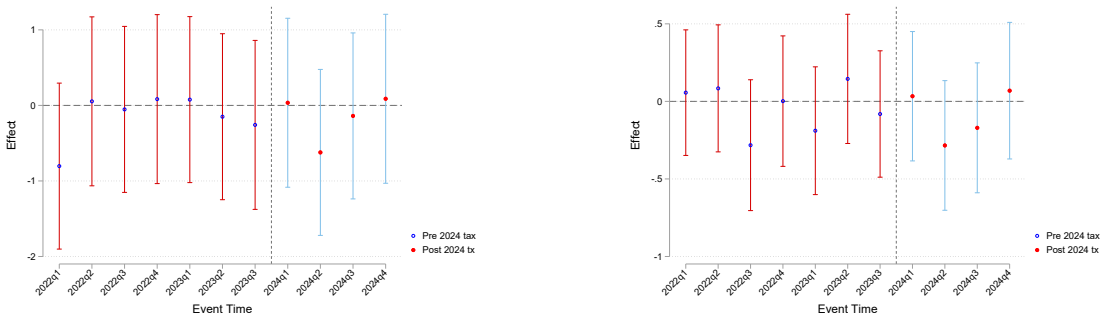
Table 13: Difference-in-Differences Estimates: Log Labor Income (Sector Level)

Analysis group:	Non-A beverages		Other food	
Control group:	All	Unrelated	All	Unrelated
Treatment × Post	-0.03	-0.48	-0.16	-0.59*
	(0.23)	(0.32)	(0.20)	(0.33)
Medium Firm	-0.30***	0.40*	-0.31***	0.50**
	(0.03)	(0.22)	(0.03)	(0.22)
Large Firm	0.45***	1.79***	0.44***	1.60***
	(0.03)	(0.22)	(0.03)	(0.23)
Constant	23.36***	22.94***	23.37***	23.41***
	(0.03)	(0.20)	(0.03)	(0.19)
Units				
Periods				
Clusters				
Observations	14,508	455	14,441	388

Note: the calculations are based on data from the GEIH 2022–2024. Standard errors clustered at the procedure level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 15 visualizes the sector-level DiD results. The plot shows estimated effects for each quarter surrounding the reform, with confidence intervals. The coefficients remain close to zero across the post-treatment period, reinforcing the finding that the reform did not trigger visible changes in average sectoral earnings.

Figure 15: Event study: Log income (sector level)



(a) Non alcoholic beverage

(b) Other food

Note: the calculations are based on data from the GEIH 2022–2024. The control group corresponds to all the other economic sectors. Coefficients are displayed as points and 95% confidence intervals, using standard errors clustered at the sector level, are shown.