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Tax Disincentives to Formal Employment in Latin America*

Abstract

Tax–benefit systems in Latin America have expanded alongside social protection, yet persistently high informality continues to constrain fiscal capacity and redistribution. This paper examines how tax policy changes affect formal employment in Bolivia, Colombia, and Ecuador over three periods (2008–2014/15-2019). The multi-country, multi-period design generates multiple quasi-experiments, enhancing external validity relative to studies focused on single reforms. We measure the implicit tax burden of moving from informal to formal work and estimate behavioral responses using grouped estimations robust to treatment heterogeneity. Higher tax burdens on formalization significantly reduce formal employment, with stronger responses concentrated among low-skilled, often self-employed workers facing high social contributions. Counterfactual simulations show that revenue-neutral reforms combining the removal of contribution floors with higher top taxation may simultaneously raise formalization and income tax progressivity, suggesting that expanding redistribution and limiting efficiency costs need not be in conflict in Latin American labor markets.

JEL classification

H24, H31, J24, J46

Keywords

informality, employment, self-employment, tax burden, social contributions, income tax, benefits

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1. Introduction

Tax-benefit systems in Latin America and the Caribbean (LAC) have developed over recent decades through the expansion of progressive income taxation and targeted cash transfer programs (Robles et al., 2015). Although the decline in income inequality since the early 2000s is largely explained by reduced wage inequality (Alvaredo and Gasparini, 2015), tax-benefit reforms have also contributed to redistribution (De la Torre et al., 2017). Nevertheless, LAC remains among the most unequal regions globally (Chancel et al., 2022). Strengthening redistributive capacity would require higher public spending, broader social programs, and more progressive taxation—objectives that depend on expanding the tax base. This is hindered by the region's large and persistent informal sector, which limits revenue collection and weakens redistribution. Moreover, greater tax progressivity or higher social contributions could incentivize shifts from formal to informal employment, further eroding the tax base.

This paper provides new evidence on this incentive-compatibility constraint for three countries with similar labor market structures and degrees of informality, namely Bolivia, Colombia, and Ecuador. We use harmonized cross-sectional microdata for three periods in each country (2008, 2014/15, and 2019), exploiting temporal and spatial variation in tax systems as multiple natural experiments. Financial incentives to formal employment are measured at the micro level through *formalization tax rates* (FTRs), defined as the implicit tax on moving from informal to formal work. FTRs are computed, for each household and period, using counterfactual disposable incomes obtained by country-specific tax-benefit microsimulations applied to the household surveys.

To exploit policy variation in the absence of panel data, we rely on grouped-data estimations of formal employment on FTRs. We construct country-specific pseudo-panels, with groups defined by gender x age x education x urban status. Behavioral responses are identified through group-level changes in formal–informal transitions, associated with changes in FTRs, following fiscal reforms. This approach builds on the classic grouped-estimation literature (Deaton, 1985; Angrist, 1991; Devereux, 2003, 2004, 2007). While it has been used mainly to study labor supply responses to financial incentives in Western countries (Blundell et al., 1998; Jäntti et al., 2015), we transpose this approach to transitions between formal and informal employment.[1] Our framework also relates to the Elasticity of Taxable Income (ETI) literature (Saez et al., 2012), where changes in marginal tax rates are evaluated holding taxable income constant to address endogeneity. Analogously, our preferred specification computes FTRs using constant earnings—set at the intertemporal group mean—so as to isolate pure policy effects. Two-way fixed-effects (TWFE) estimations, incorporating time and group fixed effects,

[1] Two recent studies apply grouped estimations in settings with substantial informality, though with important differences in focus and methodology. McKay et al. (2023) analyze African countries; it does not rely on counterfactual formalization tax rates (FTRs) or the constant-wage approach to isolate policy effects. Bargain and Silva (2023) examine transitions between formal employment, informal employment, and unemployment in LAC, identifying responses from long-run wage variation rather than policy-driven changes in tax-benefit systems.

also control for benefit levels, allowing us to measure employment responses to income effects arising from reforms to proxy-means-tested cash transfers.

The main findings are as follows. We document a quasi-systematic response of formal employment to changes in tax pressure, with FTR elasticities statistically significant for a majority of country–year pairs. This finding is notable insofar as the analysis does not center on a single major reform, common in the literature, but on repeated periods with ordinary policy variation in taxes and social security contributions. It reveals that disincentive effects are nevertheless detectable in most cases. Pooling all countries and years yields an international elasticity between 0.2 and 0.3 across specifications, in line with the average responsiveness reported in the broader literature on micro elasticities (e.g. Blundell et al, 1998; Bargain et al., 2014 ; Jäntti et al., 2015).

Results remain robust to alternative specifications, the use of actual vs. constant earnings, the incorporation of wage dynamics, alternative sample definitions and group constructions (e.g., addressing sample-size bias and adjustment along margins such as migration; Devereux, 2003, 2004), parallel-trend placebo estimations, and heterogeneity-robust estimation methods (de Chaisemartin et al., 2024). TWFE estimates actually represent a lower bound: heterogeneity-robust estimates are larger, indicating the presence of treatment effect heterogeneity.

Most importantly, this heterogeneity is central to our analysis: the feasibility of expanding tax systems depends critically on which workers respond most to the tax burden. Moving to heterogeneous estimations, we show that disincentives to formality are concentrated among low-wage (largely self-employed) workers. We derive key policy implications from this result. At low wage levels, workers face higher FTRs driven by minimum social security contributions (i.e. indexed on minimum-wage levels), while exhibiting higher elasticities and informality rates. Abolishing contribution floors can therefore encourage formal employment. Lower elasticities at higher income levels may indicate some scope for expanding tax progressivity. We simulate both policy changes and confirm the possibility to raise both formalization and redistribution.

This paper makes several contributions. *First*, it adds to the emerging literature on employment responses to financial incentives in LAC by exploiting exogenous variation in tax-benefit systems. By examining ordinary but repeated reforms, across three countries and three periods, the analysis complements quasi-experimental studies focused on single policy shocks, enhancing external validity. *Second*, the literature is rather concentrated on demand-side responses while we examine variation in workers' financial incentives following various changes in personal income tax and social security contribution parameters.[2] *Third*, we

[2] The literature mainly studies changes in employers' contributions and payroll taxation, including for Argentina (a payroll tax cut that had limited employment effect, cf. Cruces et al., 2010) and Colombia (the 1993 payroll tax increase that reduced formal employment: Kugler and Kugler, 2009; the 2012 payroll tax cut that boosted formal employment: Morales and Medina, 2017; Antón, 2014; Kugler et al., 2017; Fernández and Villar, 2017). A more

document substantial heterogeneity in behavioral responses across reforms and worker groups while addressing recent concerns regarding identification in TWFE frameworks under treatment heterogeneity (de Chaisemartin and d'Haultfoeuille, 2020). We specifically provide one of the rare applications where treatment is continuous (de Chaisemartin et al., 2024; Callaway et al., 2024). The results suggest that conventional estimates may understate responses to fiscal incentives, i.e. provide conservative lower-bound estimates of behavioral effects. *Fourth*, heterogeneous responses are not merely a source of bias but a central focus of our analysis. Accordingly, we show how revenue-neutral reforms may promote entry into formal employment while increasing tax progressivity. *Finally*, we can also isolate the role of universal, unconditional benefits in shaping formal employment, contributing to the limited evidence linking social protection design and labor-market formality.[3] Estimated income effects are generally negative but rarely statistically significant, again in line with the broader labor supply literature.

The rest of the paper is structured as follows. Section 2 presents the empirical strategy, including data, tax-benefit simulations and estimation methods. Section 3 provides background information on labor markets, informality and tax-benefit policies and reforms. Section 4 reports the main results together with sensitivity checks and heterogeneous estimates. Section 5 concludes.

2. Data and Empirical Strategy

2.1. Data and Tax-benefit Simulations

Surveys, Selection and Formality Measure. Our analysis relies on nationally representative household surveys from Bolivia (*Encuesta de Hogares*), Colombia (*Gran Encuesta Integrada de Hogares*), and Ecuador (*Encuesta Nacional de Empleo, Desempleo y Subempleo*), described in **Table A1**. These datasets have been widely used in labor market and welfare research (e.g., Pradhan and van Soest, 1997; Behrman et al., 2007; Levy and Schady, 2013; Busso et al., 2020). All surveys are cross-sectional and provide detailed information on employment, earnings, non-labor income, pensions, and individual and household characteristics.

Three survey waves are used in each country (2008, 2014/2015, and 2019), selected as regularly spaced points based on data availability and the objective of obtaining recent estimates of ordinary reforms (rather than focusing on specific policy shocks). We exclude data collected during the pandemic, as resulting estimates may be difficult to interpret. To ensure broad representativeness of potential tax disincentives affecting the labor force, we retain all the

limited literature examines social contributions paid by workers or whose economic incidence falls on workers (for instance the introduction of payroll contributions as part of the 1981 pension reform in Chile, leading to wage adjustments rather than changes in employment, cf. Edwards and Cox-Edwards, 2000).

[3] This literature describes situations in which access to social insurance for workers and their families is tied to formal jobs (e.g. Bergolo and Cruces, 2014) and, conversely, cases where holding a formal job is less necessary because healthcare or cash transfers are universal (e.g., Bosch and Campos-Vazquez, 2014).

households where the main adults are aged 18–65 and with positive labor income, whether from wage employment or self-employment. Each survey reports affiliation to social security, which we use to define formal employment—a standard identification in the literature that could not be implemented using administrative data (Perry et al., 2007; Bargain et al., 2025).

Tax–benefit simulations. We enrich the data by linking them to tax–benefit microsimulation models for Latin American countries: BOLMOD (Bolivia), COLMOD (Colombia), and ECUAMOD (Ecuador). These models apply country-specific policy rules to simulate the taxes and social insurance contributions paid, as well as the cash transfers received, by each household. Taxes and contributions are modeled as payable only by individuals affiliated with social security, i.e., those in formal employment. Part of the empirical work has consisted in extending tax–benefit simulations beyond those already available in the framework of the SOUTHMOD project (see details in **Appendix A**). **Table A2** summarizes the tax–benefit systems across countries and waves, highlighting the policy changes exploited in the analysis.

2.2 Formalization Tax Rates (FTRs)

Fiscal policies may alter the fiscal burden associated with formal employment. A rise in social contributions or income taxation, for instance, increases the cost of moving from informal to formal work. To summarize financial incentives, we compute *formalization tax rates* (FTRs), corresponding to the proportional loss in disposable income when moving from informal to formal employment (similar concepts can be found in Bergolo and Cruces, 2021; Jara et al., 2023; Fietz et al., 2025). The construction relies on microsimulated household disposable income, defined as market income minus direct taxes and social insurance contributions plus cash transfers and pensions. Thus, FTRs reflect the policy rules in force in each country and period (cf. **Tables A1–A2**). Formally, for each household i at time t, we simulate disposable income $D_{its}(X_{it}, Z_{it})$ in both informal status ($s = 0$) and formal status ($s = 1$), with X_{it} denoting market income and Z_{it} household characteristics relevant for tax–benefit rules. Household disposable income can be decomposed as:

$$D_{its}\,(X_{it}, Z_{it}) = X_{it} + Ben_{it}(Z_{it}) - s.Tax_{it}(X_{it}, Z_{it}) \qquad (1)$$

where $Ben_{it}(Z_{it})$ denotes cash transfers received by the household (categorical or proxy-means-tested) and $Tax_{it}(X_{it}, Z_{it})$ represents direct taxes and social contributions (which depend on earned income and apply only under formal employment, i.e. for s=1). The FTR is then calculated as:

$$T_{it}(X_{it}, Z_{it}) = -\frac{D_{it1}(X_{it},Z_{it}) - D_{it0}(X_{it},Z_{it})}{D_{it0}(X_{it},Z_{it})} = \frac{Tax_{it}(X_{it},Z_{it})}{D_{it0}(X_{it},Z_{it})}. \qquad (2)$$

This measure represents the share of disposable income lost due to taxes and social contributions upon entering formal employment.[4]

2.3 Two-way Fixed Effects (TWFE) Grouped Estimations

Principles. We now describe the estimation framework used to assess how tax policy changes affect formal employment in the countries under study. It is close in spirit to a difference-in-difference approach based on real panels. Yet, we do not focus here on clearly defined subgroups that would be differentially exposed to a specific tax reform (treated vs. untreated). Closer to Blundell et al. (1998), our analysis groups individuals along basic socio-demographic dimensions. It considers multiple tax parameters evolving over time that affect FTRs differentially across these pseudo-panel groups, i.e. varying the intensity of treatment across them, leading to differential changes in formalization rates across groups.

Implementation. For each country and year, individuals are grouped into 48 cells defined by gender, age groups (18-30, 30-40, 40-50 and 50-65 years old), education levels (primary education, lower/upper secondary, post-secondary to tertiary education), and location (urban/rural). We compute group means for all relevant variables. In particular, Y_{gt} denotes the formal employment rate in group g and year t, while T_{gt} is the group-average FTR. Grouped estimations rely on the following specification:

$$Y_{gt} = \alpha_g + \theta_t + \beta T_{gt} + \gamma Z'_{gt} + \delta B_{gt} + \varepsilon_{gt} \qquad (3)$$

$$\text{with } T_{gt} = \textstyle\sum_{i \in g} T_{it}(X_{it}, Z_{it}) / \sum_{i \in g} n_i,$$

estimated separately by country using pooled years and weighting group-year observations by their relative size $\sum_{i \in g} n_{it} \,/\, \sum_{g=1}^{G} \sum_{t=1}^{T} \sum_{i \in g} n_{it}$.[5] The continuous treatment T_{gt} captures the relative fiscal pressure faced by each group-year cell, allowing us to evaluate its effect on formal employment. Controls include group fixed effects α_g, capturing the persistent influence of socio-demographic characteristics on sector allocation, and time fixed effects θ_t, which absorb common changes in tax pressure across periods. We also account for time-varying group characteristics Z_{gt} (marital status and household size) and the group-level cash transfers B_{gt}, which are independent of formality status. Our primary parameter of interest is the treatment effect β, while δ captures income effects that may also shape the choice between formal and informal employment.

[4] It captures the *theoretical* fiscal pressure embedded in tax–benefit systems, hence avoiding endogeneity between sector choice and related behaviors (such as tax compliance in the formal sector). The approach also abstracts from future social insurance returns (e.g., contributory old-age pensions) by treating contributions as current costs, consistent with evidence on myopia and present bias in social security participation (Laibson et al., 1998; Andersen and Bhattacharya, 2011) and informality decisions (Perry et al., 2007).

[5] Note that such grouped estimations are exactly equivalent to an IV estimation in which individual FTRs are instrumented by group-level FTRs (Angrist, 1991).

To obtain an international summary estimate, while combining all the tax variation, we also estimate the model using a pooled sample across countries and years. The specification relates the formal employment rate of group g in year t and country c to the group-specific FTR:

$$Y_{gct} = \alpha_{gc} + \theta_{ct} + \beta T_{gct} + \gamma Z'_{gct} + \delta B_{gct} + \varepsilon_{gct}. \quad (4)$$

In this model, country–group fixed effects α_{gc} capture persistent labor-market characteristics specific to each demographic group within a country (e.g., being a low-educated rural young Bolivian woman), while country–time effects θ_{ct} absorb local fiscal conditions in each period. This specification exploits three years across three countries, yielding nine distinct policy environments. By increasing the number of observations, it also improves statistical precision.

Remarks on Identification. Groups are defined along socio-demographic dimensions that cannot be easily manipulated, making differential exposure to policy reforms plausibly more exogenous than in settings where groups are defined on the basis of earnings levels. The trade-off is that exposure contrasts may be smaller than in settings relying on sharp eligibility thresholds, since reforms affect all workers but with varying intensity across groups through their impact on formalization tax rates (FTRs). Identification therefore relies on reforms generating sufficient cross-group variation in FTR changes, a condition satisfied in the countries and periods studied, as documented below (**Figure 3**). It also requires that workers can adjust their sectoral allocation to some extent in response to fiscal incentives. This assumption is supported empirically and consistent with past evidence indicating limited labor market segmentation in LAC countries (e.g., Maloney, 2004; Pratap and Quintin, 2006; Bosch and Maloney, 2010; Diaz et al., 2018; Bargain and Kwenda, 2014). Finally, the degree of inertia in sectoral allocation may differ across groups, in particular with age and skills, and such persistent differences are captured by the group fixed effects α_{gc}.[6]

Extracting the Pure Policy Effect. In the basic specification (equation 3), FTRs depend on observed earnings X_{it}. Thus, they may evolve not only due to tax reforms but also in response to earnings dynamics (including labor supply responses to reforms) and non-fiscal policy changes (e.g., minimum wage uprating). To isolate the pure policy effect, our preferred specification computes FTRs using *fixed* earnings, in the spirit of the ETI literature. Precisely, all earnings are expressed in 2019 price levels and averaged over time at group level, i.e. yielding $\bar{X}_g = \frac{1}{T}\sum_{t=1}^{T}(1 + r_t)X_{gt}$ where r_t denoting the CPI-based adjustment factor. The empirical model becomes:

$$Y_{gt} = \alpha_g + \theta_t + \beta \tilde{T}_{gt} + \gamma Z'_{gt} + \delta B_{gt} + \varepsilon_{gt}, \quad (5)$$

$$\text{with } \tilde{T}_{gt} = \textstyle\sum_{i \in g} T_{it}(\bar{X}_g, Z_{it}) / \sum_{i \in g} n_i.$$

[6] Note that most reforms in our sample *increase* the tax burden and should therefore discourage formal employment; movements from formal to informal work are arguably less constrained by segmentation. If, conversely, reforms reduced tax pressure, our estimates would likely represent lower bounds on incentives to formalize.

By neutralizing earnings dynamics, variation in $\tilde{T}_{gt}$ reflects only differential exposure to policy parameters across groups and periods.

Heterogeneity-robust Estimations. TWFE estimations may not be robust to heterogeneous treatment effects across groups or over time (de Chaisemartin and d'Haultfoeuille, 2020). Recent contributions propose heterogeneity-robust estimators suited to settings with continuous treatments and mitigating bias from linearity assumptions in double differences (e.g. Callaway et al., 2024; de Chaisemartin et al., 2024, 2025). We adopt the Weighted Average of Switchers' Slopes (WAS) estimator for continuous treatments introduced by de Chaisemartin et al. (2024), which enables causal inference under heterogeneous treatment effects without requiring *stayers*, i.e., groups that remain untreated or are exposed to an unchanged treatment intensity. In our setting, all groups experience changes in FTRs across periods, implying the absence of stayers, although some *quasi-stayers* display only infinitesimal FTR variation (as we shall see in **Figure 3**) and can be used to test parallel trends.

Formally, for transitions between periods $t-1$ and t, the WAS estimator is written:

$$\tilde{\beta}_t^{WAS} = E\left(\frac{|T_t - T_{t-1}|}{E(|T_t - T_{t-1}|)} \cdot \frac{Y_t(T_t) - Y_t(T_{t-1})}{T_t - T_{t-1}}\right) = \frac{E(sgn(\Delta T_t)(Y_t(T_t) - Y_t(T_{t-1}))}{E(|\Delta T_t|)} \quad (6)$$

where $\Delta T_t = T_t - T_{t-1}$ denotes the FTR change and $Y_t(T_t) - Y_t(T_{t-1})$ the corresponding outcome change for switchers. The estimator is therefore a weighted average of switchers' marginal treatment effects, with weights proportional to the absolute treatment change. That is, groups experiencing larger fiscal-incentive variation receive greater weight, enhancing robustness to heterogeneous responses, while quasi-stayers, whose treatment changes approach zero, receive negligible weight. A multi-period extension is developed in de Chaisemartin et al. (2025, Section 6) in the presence of stayers, which we adapt to our three-period setting with quasi-stayers, as detailed in **Appendix B**.

Identification relies on the assumptions detailed in de Chaisemartin et al. (2024), included parallel trends, a bounded treatment support, and bounded-Lipschitz potential outcomes. The key parallel-trends assumption requires that, absent treatment variation, switchers and quasi-stayers would have followed similar outcome trajectories. We dedicate a full appendix to this issue (**Appendix B**), including quasi-stayer definition and details on the placebo test.

3. Labor Markets and Institutional Backgrounds

3.1 Formal and Informal Labor Markets

Informal employment has declined over recent decades in Latin America but remains high, constraining fiscal capacity and the expansion of redistributive systems (Maurizio and Monsalvo, 2021; ILO, 2023). This is particularly true in the three countries studied, with

informality rates of 81.5% in Bolivia, 62% in Colombia, and 63.5% in Ecuador.[7] Within countries, informality varies markedly across demographic groups. **Figure 1** plots formal employment rates by groups, ranked by mean earnings, while highlighting skill levels and indicating group size. It reveals a strong concentration of informality among low-skill workers in all countries. This pattern reflects both occupational structure (Perry et al., 2007) and potentially tax disincentives. Consistent with stylized facts for LAC, informality is particularly prevalent among youth, older workers, women, and the less educated (Gasparini et al., 2011; Ulyssea, 2020), though it affects most groups to varying degrees.

3.2 Tax-benefit Policies and FTRs

Tax-benefit Systems. Redistributive systems in LAC remain modest relative to high-income countries: the 2019 tax-to-GDP ratio averaged 22.9% in LAC versus 33.1% in the OECD (OECD et al., 2021). Bolivia, Colombia, and Ecuador lie close to the regional average. Personal income taxation plays a small redistributive role, due to limited progressivity: a flat 13 % rate applies in Bolivia while Colombia and Ecuador operate progressive schedules with high tax-free thresholds and deductions, which imply that only upper-income groups face substantial income taxation (see detailed descriptions in **Table A2**). By contrast, social insurance contributions represent a major source of fiscal pressure. This is particularly the case at low earnings due to contribution floors, i.e. minimum contributions set at minimum-wage earnings levels.[8] Direct cash transfers, mostly proxy-means-tested, remain small relative to GDP.[9] These programs are analyzed in many contributions, especially *Bono de Desarrollo Humano* in Ecuador (see for instance Carrillo and Ponce, 2009, Fernald and Hidrobo, 2011, Bosch and Schady, 2019), *Bono Juancito Pinto* in Bolivia (e.g. Bauchet et al., 2018) and *Familias en acción* in Colombia (e.g. Attanasio et al., 2015).

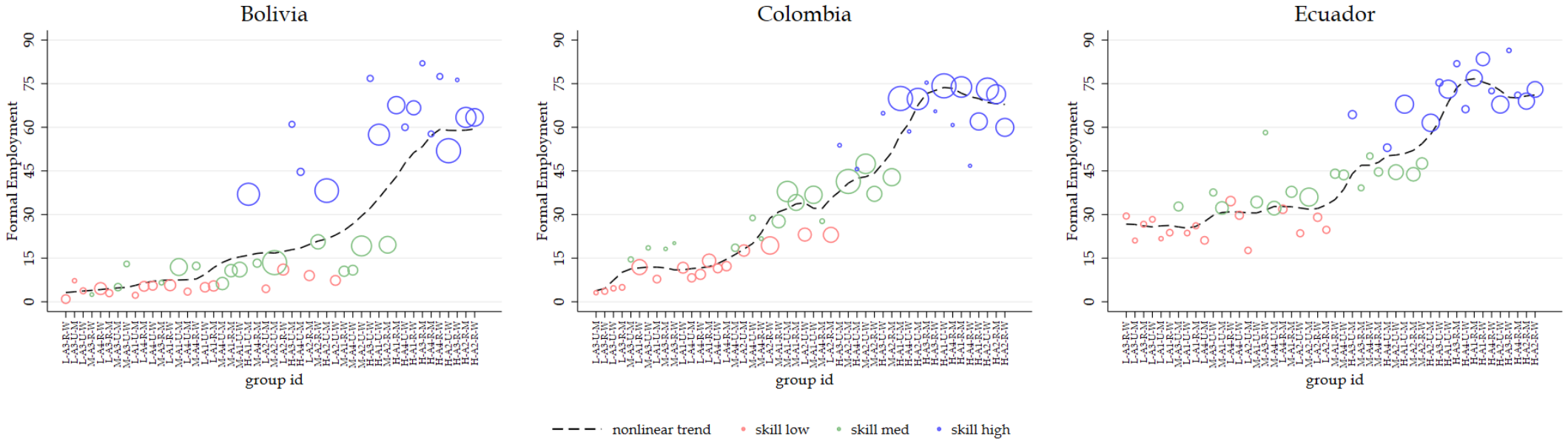
Distributions of FTRs. FTRs differ across countries but share a common feature: financial disincentives to formal employment are highest at low earnings levels. This largely reflects the presence of contribution floors, as mentioned. **Figure 2** illustrates this by reporting mean FTRs across groups ranked by their average earnings, revealing a broad negative relationship. This pattern would be U-shaped if personal income tax became effective at higher earnings levels. Yet, as noted, broad untaxed brackets and generous deductions limit the reach of income taxation in Colombia and Ecuador. Bolivia's single-rate tax above an exempt threshold proves somewhat more binding.

[7] See the ILO database: https://ilostat.ilo.org/topics/informality/

[8] Personal income tax accounts for only 9% of total tax revenues in LAC countries, and slightly less in the countries we examine (compared to 24% average in the OECD). Social insurance contributions represent a larger share in Bolivia and Ecuador (25% and 27% respectively), but only 9.5% in Colombia.

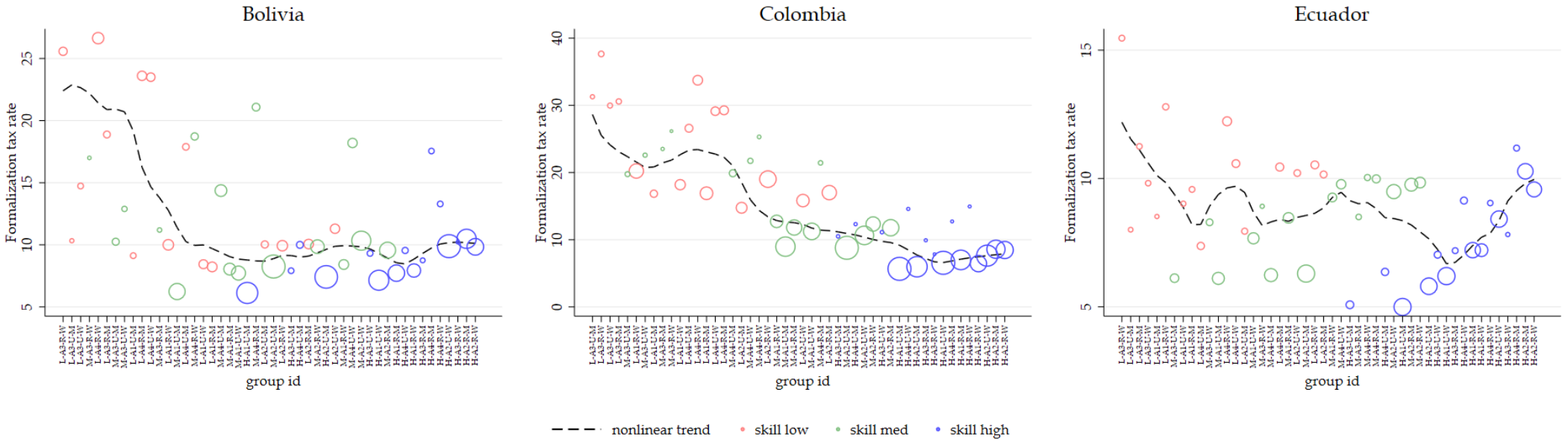
[9] Cash transfers represent 0.1% of GDP in Colombia, 0.2% in Bolivia and 0.6% in Ecuador.

Figure 1: Formal Employment Rates by Group, 2008-2019



Note: graphs plot the group-level formality rates. Groups are defined according to gender x age x education x region. Labels represent groups' education (e.g. Low-L), age groups (e.g. Young-A1), area (e.g. Urban-U) and gender (e.g. Men-M). Groups are ranked by group mean earnings.
The relative size of each group is indicated by the size of the hollow circle.

Figure 2: Formalization Tax Rates by Group, 2008-2019



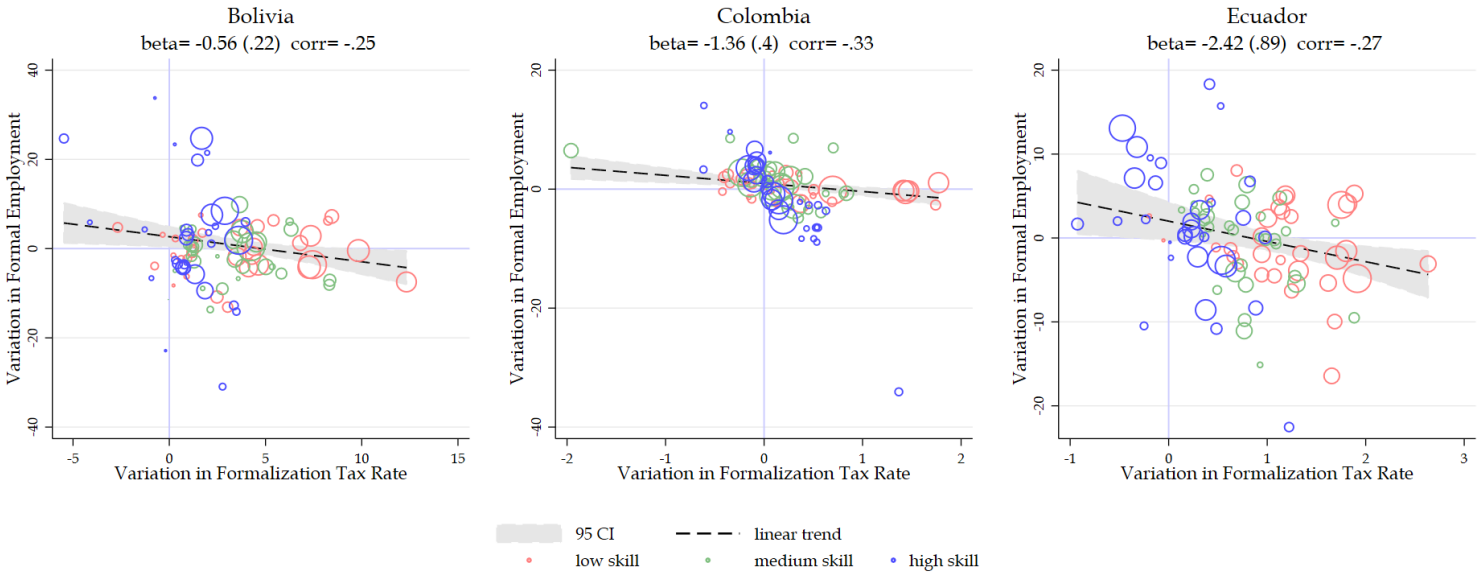
Note: graphs plot the group-level formalization tax rates. Groups are defined according to gender x age x education x region. Labels represent groups' education (e.g. Low-L), age groups (e.g. Young-A1), area (e.g. Urban-U) and gender (e.g. Men-M). Groups are ranked by group mean earnings.
The relative size of each group is indicated by the size of the hollow circle.

Recall that the FTR patterns do not represent *actual* tax levels but the relative change in disposable income incurred upon formalization. High FTRs at the bottom of the distribution therefore reflect the potentially large contribution burden faced by low-skill workers if they entered formal employment. Many such workers actually earn below the minimum wage, especially among the self-employed, who are overrepresented at low skill levels and are mainly informal (see **Appendix E**). Although the coexistence of high informality and high FTRs at low skill levels is suggestive, it is not causal. This motivates our empirical strategy, which exploits differential *changes* in FTRs across groups to identify behavioral responses.

3.3 Formalization Shift versus Change in FTRs

Preview. To provide a visual preview of our results, **Figure 3** plots changes in formal employment between 2008 and 2019 against corresponding changes in FTRs (computed using fixed earnings). These group-level changes are shown with regression lines and confidence intervals that account for group weights, while subtitles report slope estimates (with standard errors) and correlation coefficients. Despite substantial dispersion in both ΔY_g and $\Delta \tilde{T}_{gt}$, policy-driven increases in FTRs over the period are associated with declines in formal employment among affected groups, whereas occasional decreases in FTRs coincide with higher formalization. The correlation is negative, around −0.3 in all countries, and statistically different from zero, foreshadowing the main results presented below.

Figure 3: Change in Formal Employment
vs. Change in Formalization Tax Rates, 2008-2019



Note: graphs plot the group-level variation in formal employment rates against the variation in formalization tax rates (FTRs) over pairs of years for each country. FTRs capture the mean change in disposable income from moving from informal to formal work and are based on 'fixed earnings'. Groups are defined according to gender x age x education x region. The relative size of each group is indicated by the size of the hollow circle

Policy Changes. The country-specific patterns are consistent with the policy changes described in **Appendix A**. In all countries, increases in social contributions operate through rising rates and/or increases in contribution floors, reaching around half of mean earnings in

Bolivia and substantially higher levels in Colombia and Ecuador by 2019. These reforms are consistent with the concentration of low- and middle-skill groups (red and green markers in **Figure 3**) displaying both rising FTRs and declining formal employment (i.e. located in the south-west quadrant). At higher skill levels (blue markers), increases in the tax-free thresholds (relative to mean earnings) reduce tax liabilities for part of the distribution, while others experience the introduction of additional top marginal tax rates in Colombia. Finally, cash transfer systems and non-contributory pensions have expanded modestly over the period, which does not change FTRs but might contribute to the identification of income effects.

4. Results

4.1 Main Estimates

FTR-elasticities of Formal Employment. For each country, **Table 1** reports average formal employment rates, mean FTRs, and FTR elasticities derived from grouped-data estimations, using FTRs computed with either *actual* earnings (equation 3) or *fixed* earnings (equation 5). In the last panel, we also show the estimates of the pooled model (equation 4). Column (1) presents estimates without group fixed effects, which reflect both between- and within-group variation. The resulting elasticities are remarkably large, driven by the cross-sectional negative correlation discussed above: lower-skill workers face both higher fiscal pressure upon formalization, primarily due to contribution floors, and high rates of informality. To move toward a more causal interpretation, we focus on within-group variation driven by exogenous changes in policy parameters. Column (2) reports estimates of a baseline TWFE specification, i.e., a model including both time and group fixed effects. Column (3) presents the corresponding estimates with bootstrapped standard errors. Across countries and earnings definitions (actual versus fixed), elasticities remain consistently negative and statistically significant, indicating that higher fiscal pressure on formal work reduces formal employment.

Heterogeneity-robust Estimates. It is plausible that groups defined by gender, urban location, age, and education differ in their access to formal employment opportunities and therefore respond heterogeneously to financial (dis)incentives. In the absence of stayers—as in the present setting with a continuous treatment—the likelihood that TWFE estimates represent a non-convex combination of heterogeneous treatment effects is correspondingly higher. Thus, in column (4) of **Table 1**, we present results with the WAS estimator proposed by de Chaisemartin et al. (2024).

In Bolivia, the smaller number of underlying observations per pseudo-panel group generates greater measurement noise and therefore lower precision, a limitation that is further amplified under the WAS estimator. As a result, TWFE estimates are less significant than in other countries (column 3), despite similar orders of magnitude for point estimates, and WAS estimates are generally insignificant (column 4). By contrast, in Colombia and Ecuador, TWFE models appear to understate the true effects, as WAS estimates are significantly larger (column

5 reports coefficient differences using group-level bootstraps). This pattern is consistent with the fact that TWFE may assign small or negative weights to comparisons associated with large causal responses, whereas the WAS estimator forms a convex average of switchers' slopes weighted by the absolute change in FTR measures.

Table 1: Elasticities of Formal Employment (Three-wave Policy Changes)

	Formal empl. rate (%)	Mean FTR (%)	FTR-Elasticities (1)	(2)	(3)	(4)	(5)=(4)-(3)
Bolivia (years: 2008, 2015, 2019)							
Actual earnings	26	12	-0.89 ***	-0.34 *	-0.34 *	-0.36	-0.02
			(0.14)	(0.18)	(0.20)	(0.30)	(0.22)
Fixed earnings	25	8	-1.46 ***	-0.32 **	-0.32 **	-0.36	-0.04
			(0.15)	(0.16)	(0.15)	(0.44)	(0.35)
Colombia (years: 2008, 2014, 2019)							
Actual earnings	35	17	-1.37 ***	-0.45 ***	-0.45 ***	-0.88 ***	-0.43 **
			(0.12)	(0.12)	(0.13)	(0.23)	(0.17)
Fixed earnings	35	5	-1.14 ***	-0.60 ***	-0.60 ***	-1.04 ***	-0.44 ***
			(0.07)	(0.13)	(0.13)	(0.19)	(0.13)
Ecuador (years: 2008, 2014, 2019)							
Actual earnings	46	9	-1.10 ***	-0.31 ***	-0.31 ***	-0.23 **	0.09
			(0.11)	(0.10)	(0.11)	(0.11)	(0.08)
Fixed earnings	46	3	-0.47 ***	-0.23 ***	-0.23 ***	-0.49 ***	-0.26 **
			(0.06)	(0.07)	(0.08)	(0.16)	(0.12)
Pooled							
Actual earnings	36	13	-0.83 ***	-0.32 ***	-0.32 ***	-0.24 ***	0.08
			(0.05)	(0.10)	(0.10)	(0.08)	(0.07)
Fixed earnings	35	5	-0.84 ***	-0.20 ***	-0.20 ***	-0.29 ***	-0.09 *
			(0.05)	(0.06)	(0.07)	(0.07)	(0.05)
Group FE (gender x age x education x location)			NO	YES	YES	YES	
Year FE (Pooled model: Country x Year FE)			YES	YES	YES	YES	
Robust std. err.			YES	YES	YES	YES	
Bootstrapped std. err.			NO	NO	YES	YES	
Heterogeneity-robust			NO	NO	NO	YES	

Note: columns 1-4 report elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs). FTRs are computed as the relative change in disposable income associated with a virtual move from informal to formal sector. FTRs and other variables are averaged at group levels for the estimations, weighted by group size. Grouped fixed effects are controlled for as indicated, i.e. in two-way fixed effects (TWFE) models of columns 2-4. The heterogeneity-robust model (column 4) follows de Chaisemartin et al. (2024), using a Weighted Average on Switchers' Slopes (WAS) estimator. All specifications control for time dummies, time-varying controls (% of married individuals in the group, average household size) and unconditional benefit levels (i.e., benefits unrelated to formal or informal sector status). Model coefficients are semi-elasticities of formal employment, which are transformed in elasticities using mean FTRs and formal employment rates. Robust standard errors are clustered at group level and reported in parenthesis; bootstrapped std. err. are based on 100 replications. Column 5 reports the difference between TWFE and WAS estimates using a group-level bootstrap of the coefficient difference: equality is rejected in several specifications, indicating that treatment effect heterogeneity in TWFE estimates actually understate elasticity levels. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. For each country, n=144 (3 years × 48 groups); for pooled-country estimations, n=432.

Temporal heterogeneity. To inspect whether potential policy changes within subperiods suffice to generate detectable responses, we replicate the analysis separately for 2008–2014/15 and 2014/15–2019, focusing on FTRs computed using fixed earnings. This approach also

provides a direct assessment of time heterogeneity in behavioral responses. **Table 2** first shows that significant responses can still be detected within single transitions—at least for the countries with more precise estimates (Colombia and Ecuador). Then, differences in WAS estimates across transitions, capturing true time heterogeneity in causal effects, appear limited overall, as can be seen in the last column with a systematically insignificant (2′)-(1′) difference (bootstrapped standard errors). Orders of magnitude remain broadly similar across subperiods and relative to the three-wave estimates, with elasticities around 1 in Colombia and 0.5 in Ecuador. By contrast, the bias arising from heterogeneous treatment effects across groups remain important within subperiods: the gap between TWFE and WAS estimates is particularly pronounced during the first transition in Colombia, cf. the (1′)-(1) difference, and during the second in Ecuador and Bolivia, cf. (2′)-(2).

Parallel-Trend Placebo Tests. Following de Chaisemartin et al. (2024), we implement a placebo test adapted to continuous treatments in settings without stayers, an approach that has only rarely been applied in the literature and to which we contribute here. Specifically, we focus on quasi-stayers over the 2008–2014/15 period and compare their outcome trends during that interval. We test whether these trends are statistically indistinguishable between units that remain quasi-stayers and those that subsequently switch treatment status in the following period (2014/15–2019). The corresponding p-values, reported in the last column of Table 2, are large, indicating that the common trends assumption cannot be rejected. Further details on the implementation are provided in the second part of **Appendix B** (see **Figure B1**).

Table 2: Elasticities of Formal Employment (Two-wave Policy Changes)

	2008-14/15			2014/15 -19			Testing causal time heterogeneity	Parallel-trend equality test (p-value)
	TWFE	WAS		TWFE	WAS			
	(1)	(1')	(1')-(1)	(2)	(2')	(2')-(2)	(2')-(1')	(3)
Bolivia	-0.12	-0.22	-0.10	-0.54 ***	-0.05	0.49 *	0.17	0.97
	(0.11)	(0.35)	(0.26)	(0.20)	(0.37)	(0.30)	(0.46)	
Colombia	-0.38 ***	-0.86 ***	-0.48 ***	-1.04 ***	-1.26 ***	-0.21	-0.40	0.85
	(0.13)	(0.22)	(0.18)	(0.26)	(0.31)	(0.21)	(0.37)	
Ecuador	-0.51 ***	-0.42 *	0.09	-0.11	-0.46 ***	-0.35 **	-0.04	0.83
	(0.09)	(0.23)	(0.24)	(0.08)	(0.12)	(0.14)	(0.28)	
Pooled	-0.19 ***	-0.20 ***	-0.01	-0.11 *	-0.19 ***	-0.08	0.02	
	(0.07)	(0.06)	(0.08)	(0.06)	(0.06)	(0.07)	(0.07)	

Note: the table reports elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs, based here on fixed earnings). The estimation strategies are described in Table 1: the two-way fixed-effects model (TWFE) controls for time and group fixed effects; the Weighted Average on Switchers' Slopes (WAS) estimator of de Chaisemartin et al. (2024) is additionally heterogeneity-robust. For both, robust standard errors are clustered at group level and bootstrapped (100 replications); they are reported in parenthesis. The difference with Table 1 is that we focus here on pairs of years. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.1. For each country, n=96 (2 years × 48 groups); for pooled-country estimations, n=288. For parallel-trend placebo tests, see appendix B.

4.2 Additional Robustness Checks

We now present a series of sensitivity checks, reported in **Table 3**, addressing potential issues with grouped estimations as well as more specific aspects of our empirical strategy.

Accounting for Earnings Dynamics. FTRs based on fixed earnings allow us to address the potentially confounding role of earnings changes over time and the way these may interact with tax policy reforms. Nevertheless, the empirical model may still appear incompletely specified, as wage dynamics also matter for sector choice and may themselves reflect policy changes. To address this point, we first estimate individual-level earnings equations separately by sector (formal or informal), gender, and period, in order to obtain the most accurate predictions.[10] We compute group-level averages of predicted earnings, distinguishing mean predicted earnings in formal and informal employment, denoted $\hat{X}_{1,gt}$and $\hat{X}_{0,gt}$, respectively. We augment the WAS specification (based on FTRs with fixed earnings) with either the relative earnings differential, $(\hat{X}_{1,gt} - \hat{X}_{0,gt})/\hat{X}_{0,gt}$, or the level difference, $\hat{X}_{1,gt} - \hat{X}_{0,gt}$. Columns (1) and (2) show that formal employment elasticities remain broadly similar to the baseline estimates.

Small Sample Bias. A standard concern in pseudo-panel approaches is the risk of small-sample bias arising from the grouping of observations. In our framework, sampling error may lead to an underestimation of elasticities (Devereux, 2007). We have already noted this issue for Bolivia, where the number of observations per cell is relatively small (n = 225; see **Table A1**). For the other countries, the number of observations per cell appears broadly consistent with benchmarks from previous studies.[11] To further assess robustness, we follow Devereux (2004) and re-estimate the models under alternative sample restrictions on minimal group size.[12] WAS estimates (in columns 3 to 5) are broadly robust to these trimming procedures.

Migration. Our results could be biased if the composition of groups changed over the period (Deaton, 1985). The time span is likely too short for major shifts in the age or education structure to occur. However, one of the criteria used to construct groups is urban versus rural location, which may generate compositional change if migration is substantial. To mitigate this concern, or at least assess whether it materially affects the estimates, we restrict the sample to households reporting stable residence over the previous five years. Column (6) reports the corresponding estimates and the associated reduction in sample size. Elasticities remain sizeable, consistent with Devereux (2003), although the response in Colombia is somewhat smaller and closer in magnitude to that in Ecuador.

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[10] The estimations include region and education dummies, as well as a cubic polynomial in age, as key determinants. We deliberately avoid using group fixed effects, as this would effectively rely on cell-mean wages for formal or informal workers within each cell, which may represent poor counterfactuals if one of the two sectors is sparsely represented within the cell.

[11] Studies using grouped estimations report diverse settings, with an average of 142 observations per cell in Blundell et al. (1998), 2,048 in Devereux (2003) and 1,175 in Devereux (2004).

[12] In column (3), we drop the 5% smallest groups in each country. This ensures that each remaining cell has a minimum sample size of at least 14 individuals in Bolivia, 218 individuals in Colombia, and 68 individuals in Ecuador. However, this restriction does not guarantee a sufficient number of observations for formal and informal workers in each cell. Therefore, column (4) reports estimates obtained when dropping the 5% of groups with the fewest formal workers as well as all groups with fewer than 15 observations. Finally, in column (5), we drop the 5% of groups with the fewest formal workers and, in the remaining sample, the 5% smallest groups.

Table 3: Robustness Checks for Elasticities of Formal Employment (Three-wave Policy Changes)

	Baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bolivia	-0.36	-0.22	-0.16	-0.31	-0.43	-0.42	-0.18	-0.56 **
	(0.55)	(0.69)	(0.51)	(0.53)	(0.67)	(0.55)	(0.39)	(0.26)
Reduction in sample size				-5%	-10%	-10%	-5%	-15%
Colombia	-1.04 ***	-1.01 ***	-0.85 ***	-1.09 ***	-1.04 ***	-0.99 ***	-0.60 ***	-0.60 ***
	(0.19)	(0.20)	(0.23)	(0.21)	(0.18)	(0.18)	(0.17)	(0.21)
Reduction in sample size				-5%	-10%	-10%	-9%	-12%
Ecuador	-0.49 ***	-0.41 **	-0.53 ***	-0.54 ***	-0.66 ***	-0.61 ***	-0.57 ***	-0.24 ***
	(0.15)	(0.19)	(0.09)	(0.18)	(0.19)	(0.18)	(0.15)	(0.09)
Reduction in sample size				-5%	-10%	-10%	-3%	-10%
Pooled	-0.29 ***	-0.22 ***	-0.25 ***	-0.31 ***	-0.31 ***	-0.29 ***	-0.22 ***	-0.13 **
	(0.06)	(0.06)	(0.08)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)
Reduction in sample size				-5%	-10%	-10%	-6%	-13%
Robustness checks:		*Controlling for wage dynamics, alternatively:*		*Small sample checks*			*Group stability and definition*	
		(x1-x0)/x0	x1-x0 (in 2019 PPP)	trim 5% of smallest cells	trim 5% smallest formal and n<15	5% smallest formal and 5% smallest cells	dropping geogr. movers	using cohort rather than age to define groups

Note: the table report elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs, based here on fixed earnings). The estimation strategies are described in Table 1: the two-way fixed-effects model (TWFE) controls for time and group fixed effects; the Weighted Average on Switchers' Slopes (WAS) estimator of de Chaisemartin et al. (2024) is additionally heterogeneity-robust. For both, robust standard errors are clustered at group level and bootstrapped (100 replications); they are reported in parenthesis. Significance levels: *** p < 0.01, ** p < 0.05, and * p < 0.1. The differences with Table 1 consist of the following robustness checks:

■ Control for earnings differential between the formal sector (1) and informal sector (0), using two alternative measures, namely (1) (x1-x0)/x0 : the relative difference in (group-averaged) predicted earnings between sectors; (2) x1-x0 : the difference in (group-averaged) predicted earnings between sectors, in 2019 PPP USD.

■ Small sample issues: (3) dropping the 5% smallest cells in each country; (4) dropping the 5% of cells with the smallest number of formal workers and, among the remaining cells, those with fewer than 15 total observations; (5) dropping the 5% of cells with the smallest number of formal workers and, among the remaining cells, the 5% smallest cells.

■ Group stability: (6) restricting the sample to households reporting geographic stability over the past five years.

■ Group definition: (7) replacing age with cohort in group construction, using four cohort groups (born 1943–60, 1960–70, 1970–80, and 1980–90).

Age versus Cohort. In the baseline specification, age is one of the dimensions used to construct groups. Following the same age categories over time is sensible, particularly since age is an important determinant of informality. However, cohort effects may not be fully captured, for instance shocks affecting labor market attachment or education quality. Following Devereux (2003), we therefore consider an alternative, symmetrical grouping in which cohort categories replace age groups. This approach effectively makes pseudo-panels resemble true panels, as the same generations are tracked over time. Column (7) indicates smaller yet still sizeable elasticities in Colombia and Ecuador, along with a now significant elasticity in Bolivia.

4.3 Discussions, Heterogeneous Estimates and Simulations

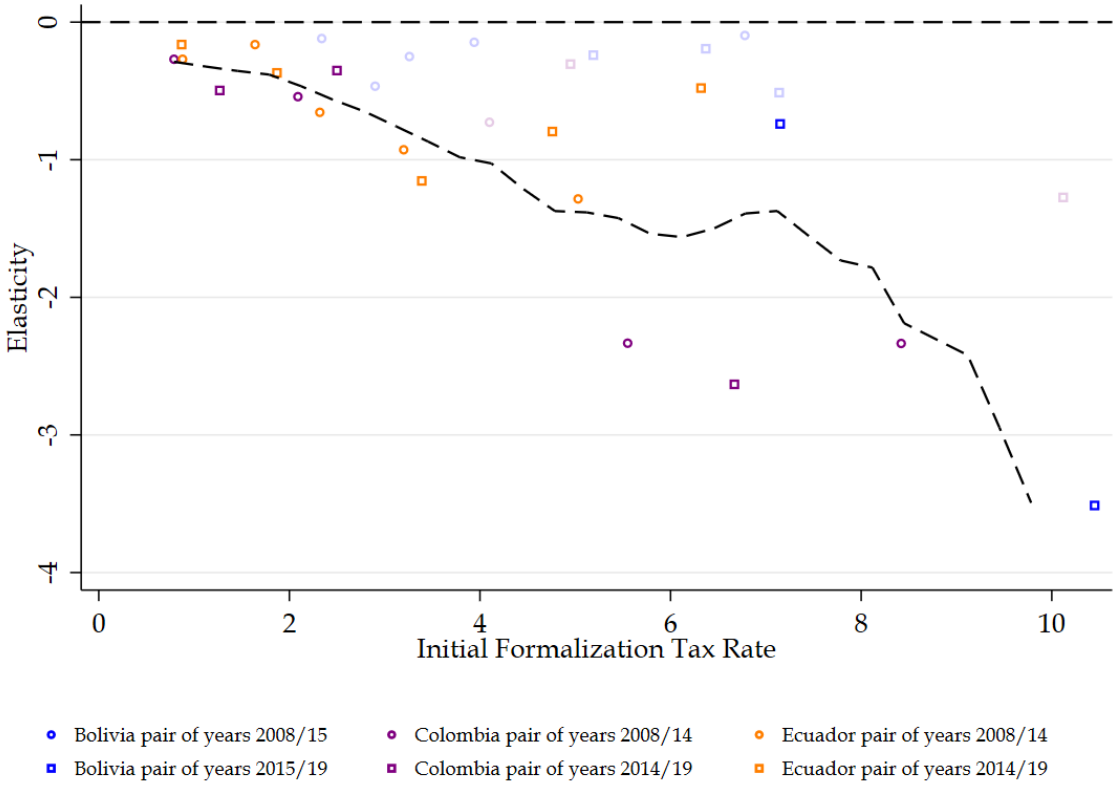
Magnitudes. In **Figure C1**, we present our baseline estimates (TWFE and WAS estimates based on three-wave policy changes, cf. **Table 1**) for comparison with studies examining policy

changes in LAC and other transition economies (see the detailed discussion in **Appendix C**). Overall, the magnitudes are broadly comparable to those obtained from single tax reforms, despite relying here on ordinary (but repeated) policy variation, thereby improving external validity. The ability to detect estimates of similar magnitudes across subperiods (**Table 2**) further strengthens this interpretation by effectively expanding the set of quasi-experimental variations underpinning the findings.

Figure C1 shows some dispersion across countries and studies, reflecting differing measures, empirical approaches and country-specific characteristics. We also find variation within our pool of countries, with WAS estimates spanning from 0 (insignificant in Bolivia) to 1 (Colombia). The mean elasticity in the literature reviewed in **Figure C1** (≈ -0.3) is close to our pooled TWFE (≈ -0.2) and WAS (≈ -0.3) estimates. WAS emphasizes the attenuation and weighting biases typical of TWFE, suggesting largest elasticities. The latter remain modest in the broader perspective of micro elasticities (i.e., compared to macro elasticities that typically reflect longer-run adjustments, cf. Chetty et al., 2011; Jäntti et al., 2015), but still translate into substantial disincentives in aggregate. For illustration, the WAS estimates reported in **Table 1** imply that a 10% increase in FTRs (i.e. an average 3.5-point increase) reduces formal employment by around 2% on average, which represents around 1 million workers overall for the three countries. Behavioral responses to tax pressure in these LAC economies therefore generate a significant erosion of the tax base and should be taken into account in policy design. The simulations presented below illustrate how reforms could potentially restore some degree of formalization while enhancing tax progressivity.

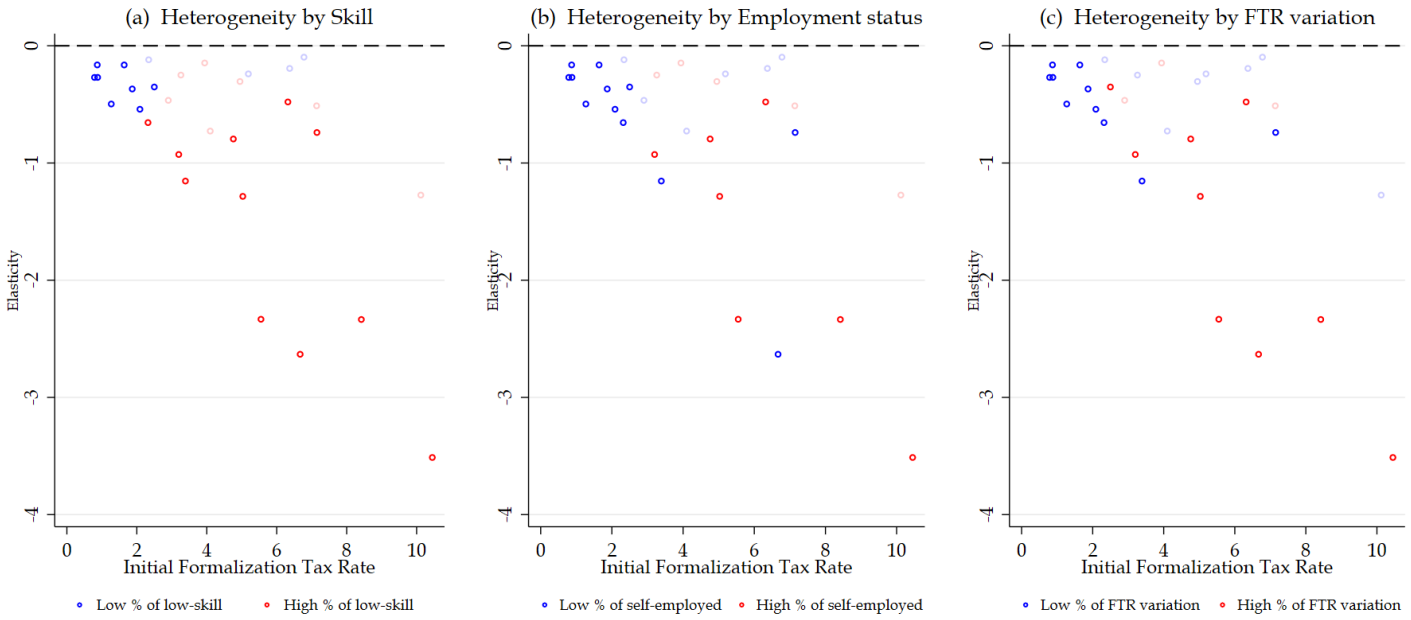
Exploiting Heterogeneous Estimates. Although heterogeneity in treatment effects is typically treated as a technical concern, i.e. a potential source of bias in mean elasticities, it is here central to assessing the scope for expanding fiscal capacity in LAC countries. To investigate this point, we re-run our grouped estimations using subperiod transitions and allowing for heterogeneous FTR coefficients, defined by five levels of pre-reform FTRs in each country-year pair. This quintile-based decomposition yields 30 coefficients (one per FTR quintile × country × subperiod). The resulting elasticities are plotted in **Figure 4** against the corresponding mean pre-reform FTR for each coefficient. The graph shows a pronounced negative gradient of elasticities with respect to pre-reform FTR levels: responses are small and often insignificant for low values of initial FTRs but become increasingly negative for large FTR levels. Importantly, the graph shows some overlap across countries, particularly between Colombia and Ecuador (as expected, heterogeneous estimates for Bolivia are frequently insignificant), implying that the gradient is not driven solely by cross-country differences in FTRs or responses. The overall pattern implies that behavioral distortions are concentrated among workers already facing high tax burden upon formalization. We have seen before that higher FTRs correspond to low-skilled, often self-employed workers, who consistently exhibit high informality rates. Contribution floors appeared to be the main driver.

Figure 4: Heterogeneous Elasticities of Formal Employment



Note: graphs plot heterogeneous elasticities of yearly variations in formal employment by levels of initial Formalization Tax Rates (FTRs), respectively 2008 and 2014/2015. Lighter hues represent insignficant elasticities. FTRs capture the mean change in disposable income from moving from informal to formal work. Each cell represents a quintile of initial FTR within each country and each subperiod (hence 30 cells).

Figure 5: Heterogeneous Elasticities of Formal Employment (Alternative Visualization)



Note: The graphs present the same elasticities as in the previous figure, highlighting the composing of each cell (country quintiles of initial FTRs by subperiod) regarding skill level (panel a), proportion of self-employed (panel b) or intensity of FTR variation (panel c). In each case, high (low) refers to a cell above (below) median average of the relevant characteristic.

We confirm this interpretation with alternative visualizations of our heterogeneous estimates in **Figure 5**, highlighting whether each quintile of initial FTRs is primarily composed of low- vs. high skilled workers (panel a), self-employed vs. salaried workers (panel b), or workers facing low vs. high changes in FTRs (panel c). The results confirm that the strongest responses are concentrated among low-skilled workers, who are often self-employed and experience larger *changes* in FTR, mainly driven by *increases* in contribution floors over the period under study (**Table A1**).

Simulations. These findings have clear policy implications. Abolishing contribution floors, for instance by decoupling them from minimum-wage references, could substantially reduce the tax burden on workers who currently face the highest formalization costs and exhibit the strongest behavioral responses. This reform is actually promoted by international organizations as an efficient way of reducing non-wage labor costs to encourage formal work (cf. OECD, 2025, Section 7.3) and diminish the regressivity implied by large contributions for those with the lowest incomes. At the same time, our finding of lower elasticities among higher-income groups suggest scope for increasing tax progressivity without triggering large adverse de-formalization effects. Such a combined reform can move LAC systems closer to the equity–efficiency frontier. We illustrate it by simulating both legs of the reform: (i) the elimination of contribution floors, assumed to affect the self-employed only,[13] and (ii) an increase in marginal income tax rates. The latter is calibrated in each country to ensure revenue neutrality once behavioral responses are taken into account (all marginal tax rates are raised by 2% in Bolivia and Ecuador and 1% in Colombia). Counterfactual FTRs are combined with the estimated heterogeneous elasticities to predict transitions across sectors.

Table 4: Revenue-Neutral Policy Simulation

	Baseline revenue			Static policy change		Behavioral response		End situation			Δ Formal Employ. (%)	Δ Formal Employ. (pp)	Δ Reynolds-Smolensky (%)
	Income tax	Soc. contrib.	Total	Δ Income tax	Δ Soc. contrib.	Δ Income tax	ΔSoc. contrib.	Income tax	Soc. contrib.	Total			
	(1)	(1')	(1'')	(2)	(2')	(3)	(3')	(4)	(4')	(4'')	(5)	(6)	(7)
Bolivia	49.2	50.8	100.0	0.98	-3.30	-0.14	2.45	50.1	49.9	100.0	7%	0.22	10%
Colombia	51.7	48.3	100.0	0.49	-3.24	-0.02	2.78	52.1	47.9	100.0	13%	0.76	10%
Ecuador	51.4	48.6	100.0	1.03	-3.32	-0.25	2.54	52.1	47.9	100.0	17%	0.90	9%

Note: the table shows results from counterfactual policy simulations using 2019 microdata for all countries. It reports levels of (and changes in) personal income tax and, for self-employed workers, social security contributions. Taxes and contributions are normalized to sum up to 100 before reform. The simulated reform combines two components: (i) for self-employed workers, elimination of the social contribution floors indexed to minimum-wage income equivalent, (ii) for all, increases in marginal income tax rates, calibrated to achieve revenue neutrality. Behavioral responses are based on heterogeneous FTR-elasticities of formal employment. FTR (formalization tax rates) are computed as the change in disposable income when moving from informal to formal sector. FTRs decrease for the bulk of self-employed workers after abolishing the contribution floor (the majority of whom is informal before reform); FTRs increase among tax payers at the top of the earnings distribution due to enhanced progressivity. Change in vertical redistribution is captured by the Reynolds-Smolensky index.

[13] Formal salary workers are not supposed to earn below the minimum wage, although a non-negligible fraction do (cf. **Appendix E**).

Table 4 reports summary statistics for this budget-neutral scenario, with normalized levels in tax and social contribution revenues, as well as the decomposition of the changes into static and behavioral components. Eliminating contribution floors generates a fiscal cost, reflecting reduced payments by formal workers previously constrained by the floor (column 2′), only partly offset by new contributions from workers entering formal employment (column 3′). Higher marginal tax rates at the top increase tax revenue (column 2) while inducing modest withdrawals from formality (column 3). Overall, the reform leads to a 7–17% increase in formal work, alongside an overall rise in tax progressivity and vertical redistribution (we report an increase of 9–10% in the Reynolds-Smolensky index).

These counterfactual results should be interpreted only as illustrative partial-equilibrium estimates with behavioral responses, as they abstract from general equilibrium effects (such as wage adjustments and firm-level labor demand responses). Nonetheless, they indicate that expanding redistribution and preserving incentives need not be mutually exclusive in Latin American labor markets.

Income Effects derived from Benefit Reforms. In addition to fiscal disincentives, we examine the effect of changes in unconditional cash transfer programs on formal employment. Because these transfers are not conditional on formal employment status, policymakers have long expressed concern that, in highly informal contexts, they may weaken incentives to work formally. As shown in **Appendix D**, the estimated benefit elasticities of formal employment are generally negative but rarely statistically significant (**Table D1**). Thus, income effects associated with changes in cash transfers appear weaker and less easily detectable than responses driven by changes in FTRs, at least with the data and reforms at hand.

These findings are consistent with the broader labor-supply literature, which typically reports modest or statistically insignificant income effects (Blundell et al., 1998; Bargain et al., 2013). Yet, they contrast with some country-specific studies of major benefit reforms in LAC, which have documented more pronounced disincentive effects (see **Appendix D**). That said, recent research also documents positive labor-supply responses within formal employment when cash transfers stimulate local economic activity, potentially offsetting or even dominating the standard negative income effect (Gerard et al., 2026).

5. Conclusion

This paper exploits multiple quasi-experiments generated by variations in taxes and social contributions across three Latin American countries over 2008–2019. We construct formalization tax rates (FTRs)—the implicit tax on moving from informal to formal work—and estimate how changes in FTRs affect formal employment. Ordinary reforms are sufficient to detect behavioral responses in most cases, with magnitudes comparable to those found in single-reform studies and sometimes slightly larger when using heterogeneity-robust estimators that address attenuation bias. Our findings are robust to a range of specification

checks, including freezing earnings to isolate pure policy effects, additionally controlling for earnings dynamics, conducting subperiod analyses, and testing for potential small-sample bias as well as migration and cohort effects.

Effective taxation of formal work reduces formal employment, but responses are heterogeneous and concentrated among low-skilled, often self-employed workers facing contribution floors upon formalization. In contrast, behavioral responses are smaller at higher income levels. These patterns have direct implications for the equity–efficiency trade-off that constrains redistribution in high-informality settings. Removing contribution floors can yield sizable formalization gains while limiting efficiency losses. Simulations suggest that combining such a measure with higher taxation at the top can increase both formal employment and progressivity in a revenue-neutral framework. While these counterfactual results should be interpreted cautiously, especially given the partial-equilibrium interpretation, they indicate that expanding redistribution and preserving incentives need not be mutually exclusive in Latin American labor markets.

Future work could extend this approach to additional countries, incorporate dynamic labor-market adjustments, and examine interactions between taxation and social protection programs in order to better characterize formalization incentives across institutional and labor-market settings. Moreover, because FTRs are defined in terms of changes in household disposable income upon formalization, they can be readily integrated into applied theoretical frameworks and used for more advanced welfare analysis. In particular, FTR-based elasticities of formal employment could inform optimal-tax models with an informal sector (Boadway & Sato, 2009; Besley & Persson, 2013) as well as computable general-equilibrium models featuring informality (Auriol & Warlters, 2009; Elgin & Torul, 2018). These developments would draw on evidence that the redistributive capacity of economies with pervasive informality is determined primarily by the distribution of formalization incentives rather than by average tax burdens alone.

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Appendix A: Tax-Benefit Policies and Formalization Tax Rates

Tax-benefit models. Our analysis relies on a new set of tax–benefit microsimulation models for Bolivia (BOLMOD), Colombia (COLMOD), and Ecuador (ECUAMOD). These models were originally developed within the SOUTHMOD project launched by UNU-WIDER, in collaboration with SASPRI, the International Inequalities Institute at the London School of Economics, and local partners (Decoster et al. 2019).[14] Microsimulations are carried out on household survey data containing market incomes—including wages, self-employment earnings, capital and property income—together with sociodemographic characteristics to compute country-specific tax–benefit rules. Simulations replicate national legislation as closely as possible, and baseline outputs (tax liabilities, benefit receipts, and related aggregates) have been validated against external statistics (Arancibia & Macas, 2023; Rodríguez et al., 2023; Jara et al., 2023). All tax–benefit instruments in force for each country–period are simulated, except contributory pensions, for which observed data are used, because contribution histories are unavailable in the surveys. Country-specific survey weights are adjusted to address the underrepresentation of top incomes in household surveys.[15] **Table A1** summarizes the underlying data and model features.

Table A1: Data Sources and Tax-benefit Microsimulation

Country	Data	Tax-benefit microsimulation	Years (a)	Micro data: sample size (b)	Grouped estimation: mean cell size (b)
Bolivia	Encuesta de Hogares (EH)	BOLMOD	2008, 2015, 2019	10,787	225
Colombia	Gran Encuesta Integrada de Hogares (GEIH)	COLMOD	2008, 2014, 2019	299,523	6240
Ecuador	Encuesta Nacional de Empleo, Desempleo y Subempleo (ENEMDU)	ECUAMOD	2008, 2014, 2019	29,085	606

Note: The tax-benefit microsimulations used are hosted by UNU-WIDER and are freely available for non-commercial research use (see: https://www.wider.unu.edu/project/southmod-simulating-tax-and-benefit-policies-development-phase-2). Tax-benefit policies for the last year 2019 come from the original models while those of older periods (2008 and 2014/15) have have been coded by the authors.

(a) These years correspond to both the micro survey used and the tax-benefit rules coded to fit these datasets.

(a) Average mean over the three years.

[14] We are indebted to the many people who have contributed to the development of SOUTHMOD. The results and their interpretation presented in this publication are solely the authors' responsibility. For more information: https://www.wider.unu.edu/project/southmod-simulating-tax-and-benefit-policies-development-phase-2 and https://www.lse.ac.uk/international-inequalities/research/global-inequalities-observatory/southmod-simulating-tax-and-benefit-policies-for-development

[15] We follow the reweighting method of Blanchet et al. (2022). The procedure assumes that above the 95th percentile, the true income distribution follows a Pareto tail. The Pareto shape parameter is calibrated for top income shares to match externally specified targets drawn from tax record estimates (Piketty et al., 2018).

FTR Calculation and Policy Changes. The tax–benefit models are used to compute FTRs by comparing a person's household disposable income under formal versus informal employment, as described in Section 2.2 (Equations 1 and 2). In the formal (informal) scenario, the simulation switches taxes and social contributions on (off). FTRs therefore incorporate all tax and contribution rules in force in each period, summarized in **Table A2**. The policy changes documented in this table are exploited to capture sectoral transitions across the three survey waves. Changes in FTRs are illustrated in **Figure 3** in the main text: it shows lots of variation across pseudo-panel groups, which contributes to identify behavioral responses.

The underlying reforms can be described by comparing the different policy years in **Table A2** and be summarized as follows. *First*, social security contributions are characterized by contribution floors corresponding to contributions paid at minimum-wage-level earnings; these minimum levels have increased over the period (particularly in Bolivia and Ecuador); in addition, higher contribution rates were set (mainly between the first two waves in Bolivia and Ecuador) and an additional contribution has been added (a progressive solidarity contribution, in Bolivia). These reforms have affected all workers but low-skilled in particular.

Second, personal income taxation has also changed over the period. In all countries, the zero-tax threshold was increased relative to the minimum wage (most markedly in Bolivia), thereby reducing the share of individuals liable for personal income tax. This shift contributed to higher FTRs for some groups of medium- and high-skilled workers, particularly in Bolivia. For other groups, however, tax reforms led to declining FTRs, most notably in Colombia following the introduction of new upper marginal tax rates. Because taxpayers are highly concentrated at the very top of the distribution, these latter changes remain only weakly visible in the aggregate patterns of **Figure 3**.

Third, on the benefit side, social transfers rely on proxy means tests (PMTs) based on housing and household characteristics in Ecuador and Colombia, while Bolivia operates universal transfers for specific population groups. As discussed in the main text, formal employment status does not affect eligibility for cash transfers; however, changes in benefit levels or the introduction of new programs may influence transitions between formal and informal employment through income effects (see, e.g., Bosch & Campos-Vázquez, 2014). Several such changes occurred over the period considered. As detailed in **Table A2**, new cash transfer programs were introduced between 2008 and 2015—notably *Juana Azurduy* in Bolivia and *Jóvenes en Acción* and *Colombia Mayor* in Colombia. Among existing programs, some benefit amounts remained nominally fixed—implying declines in real terms due to inflation—while others increased, such as *Bono de Desarrollo Humano* in Ecuador and *Renta Dignidad* in Bolivia.

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Table A2: Summary Characteristics of Country-Specific Tax-benefit Systems (2008, 2014/15, 2019)

		Bolivia			Colombia			Ecuador		
Instrument	**Parameter**	**2008**	**2015**	**2019**	**2008**	**2014**	**2019**	**2008**	**2014**	**2019**
Social security contributions (SIC)	Contribution rate for:									
	salary workers:	12.21%	12.71% + up to 10% (solidarity contrib.)		8% or 10%	8% or 10%	8% or 10%	9.35% or 11.35%	9.45% or 11.45%	9.45% or 11.45%
	self-employed:	13.92%	14.42% + up to 10% (solidarity contrib.)		28.5% or 30.5%	28.5% or 30.5%	28.5% or 30.5%	17.50%	20.50%	20.50%
	Minimum contribution: this rate x the minimum wage; the min. wage is equal to:									
	as monthy earnings (a):	131.3	244.2	307.1	209.3	236.4	238.2	264.8	357.7	394
	in % of mean labor income (b)	31%	49%	51%	60%	65%	70%	55%	64%	78%
Personal income tax (PIT)	# tax bands	1	1	1	4	4	7	9	9	9
	low tax band (ltb)*	2,362	7,590	9,724	10,900	11,500	11,019	10,393	10,952	11,310
	in % of mean labor income (b)	6	15	20	31	32	32	21	20	22
	high tax band (htb)	-	-	-	41,000	43,255	43,255 for 33%; 313,370 for new top rate	105,912	111,754	115,290
	in % of mean labor income (b)				118	120	126 / 915	219	199	229
	lowest tax rate	13% (flat)	13% (flat)	13% (flat)	0%	0%	0%	0%	0%	0%
	tax rate above 1st threshold				19%	19%	19%	5%	5%	5%
	top tax rate	-	-	-	33%	33%	39%	35%	35%	35%
	max deduction	-	-	-	1480 monthly in expenditures on health, dependants and mortgage	1561 monthly in expenditures on health, dependants and mortgage	1497 monthly in expenditures on health, dependants and mortgage	1.3*ltb + 2*ltb (old-age) + 3*ltb (disability)	1.3*ltb + 2*ltb (old-age or disability)	
Social assistance/ family cash tranfers	income test	(i) *Bono Juancito Pinto (BJP)* : universal for children in public educ. - (ii) *Bono Juana Azurduy (BJA)* : universal for pregnancy or children under 2			(i) *Familias en acción* : proxy means-test	(i) *Familias en acción (FA)* : proxy means-test (ii) *Jóvenes en acción (JA)* : proxy means-test		*Bono de Desarrollo Humano* : proxy means-test		
	max amount (per month)*	(i) BJP: 45.5	(i) BJP: 29.5 (ii) BJA: 16.31	(i) BJP: 28.9 (ii) BJA: 16	(i) *FA* : 40.81	(i) *FA* : 45.01 (ii) *JA* : 65.23	(i) *FA* : 45 (ii) *JA* : 51.83	39.72	52.61	50
Non-contributory pension	income test	*Renta Dignidad* : universal elderly above 60			-	*Colombia Mayor* : proxy means-test		*Bono de Desarrollo Humano* : proxy means-test		
	max monthyl amount (a)	29.5	39.94	55.92	-	42.2	33.5	39.72	52.61	100

Source: Author's elaboration using tax-benefit policy description in Arancibia and Macas (2023) for Bolivia, Rodriguez et al. 2023 (2023) for Colombia and Jara et al. (2023b) for Ecuador.

(a) All monetary parameters are expressed in USD 2019 PPP.

(b) Mean labor income computed using our pseudo-panel cells.

Appendix B: WAS Generalization and Tests of Parallel Trends

WAS with Continuous Treatment: Extension to Three Periods. When considering more than two periods, we adapt the multi-period generalization proposed by de Chaisemartin et al. (2025) in the presence of stayers to our setting with *quasi-stayers*, i.e., groups experiencing only marginal treatment variation. The multi-period WAS parameter aggregates period-specific WAS estimands using weights proportional to the average absolute treatment change between consecutive periods (de Chaisemartin et al., 2025, Section 6.1):

$$\tilde{\beta}^{T\geq 3,WAS} = \sum_{t=2}^{T} \frac{E(|\Delta T_t|)}{\sum_{k=2}^{T} E(|\Delta T_k|)} \tilde{\beta}_t^{WAS}.$$

Substituting the two-period WAS expression, i.e. our equation (6), into this aggregation and simplifying yields a convenient closed form for consecutive periods:

$$\tilde{\beta}^{T\geq 3,WAS} = \frac{\sum_{t=2}^{T} E\left(sgn(\Delta T_t)\left(Y_t(T_t) - Y_t(T_{t-1})\right)\right)}{\sum_{k=2}^{T} E(|\Delta T_t|)},$$

where expectations are taken over the super-population at each transition $t-1$ to t. Identifying assumptions remain similar to the two-period case and require parallel trends over each pair of consecutive periods, rather than over the full duration of the panel.

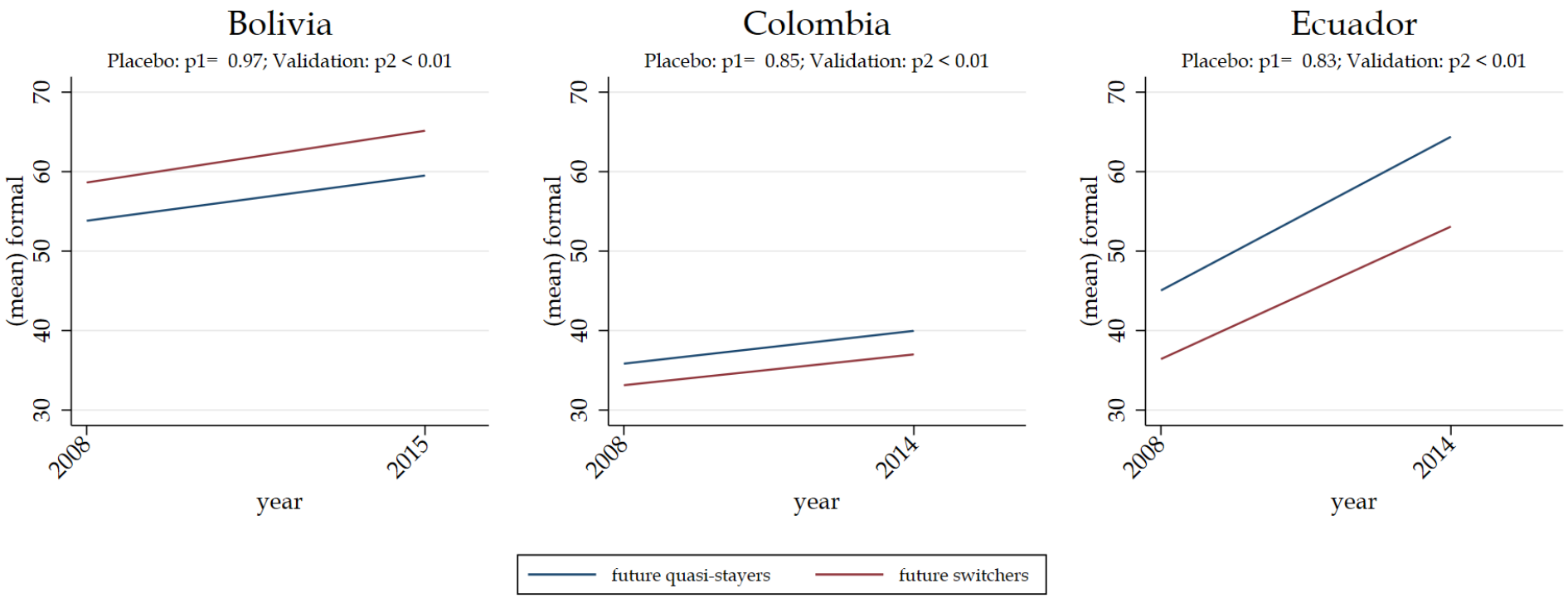
Placebo Estimation. To assess the credibility of the canonical DiD assumption, we implement a placebo test tailored to the continuous-treatment setting without stayers. It follows the suggestion in de Chaisemartin et al. (2024, p. 2), extending the placebo approach developed for the model with stayers (Chaisemartin et al., 2025, section 6.2). Specifically, we examine changes in formal employment between 2008 and 2014/15, restricting the sample to quasi-stayers over this window. Within this subsample, we compare trends depending on whether groups remain quasi-stayers in 2014/15–2019 ("future quasi-stayers") or instead experience substantial treatment changes ("future switchers").

We define quasi-stayers as individuals whose ΔFTR falls below a country-specific threshold, indicating that treatment changes are sufficiently small relative to the larger variations shown in Figure 3. Importantly, because the WAS estimator in equation (6) weights units by their variation in treatment, the influence of quasi-stayers on the estimation tends toward zero. Specifically, quasi-stayers are those below the first quartile of the within-country ΔFTR distribution; while this cutoff differs across countries, it remains small in practice.

For this definition, **Figure B1** shows no visual evidence of differential pre-trends across countries. The reported placebo p-values (p1), testing equality of changes over 2008–2014/15,

are large, so we do not reject the parallel-trends assumption. We also report validation p-values (p2) for differences in formal employment changes over 2014/15–2019. Their near-zero values indicate clear divergence between switchers and quasi-stayers in the second subperiod. Detectable effects even in this restricted sample further support the credibility of our identification strategy. Finally, note that alternative reasonable thresholds for the definition of quasi-stayers do not materially affect the results.

Figure B1: Placebo estimation



Note: The figure plots mean formal employment rates for groups with very small treatment changes ("quasi-stayers") between periods 1 (2008) and 2 (2014/15). Within this population, we distinguish between those that continue to experience marginal changes between periods 2 and 3 ("future quasi-stayers") and those experiencing substantial changes ("future switcher"). Subtitles report p-values for the placebo test (p1), which tests equality of changes between periods 1 and 2 across the two groups, and for a vali--dation test (p2), which tests equality of changes between periods 2 and 3.

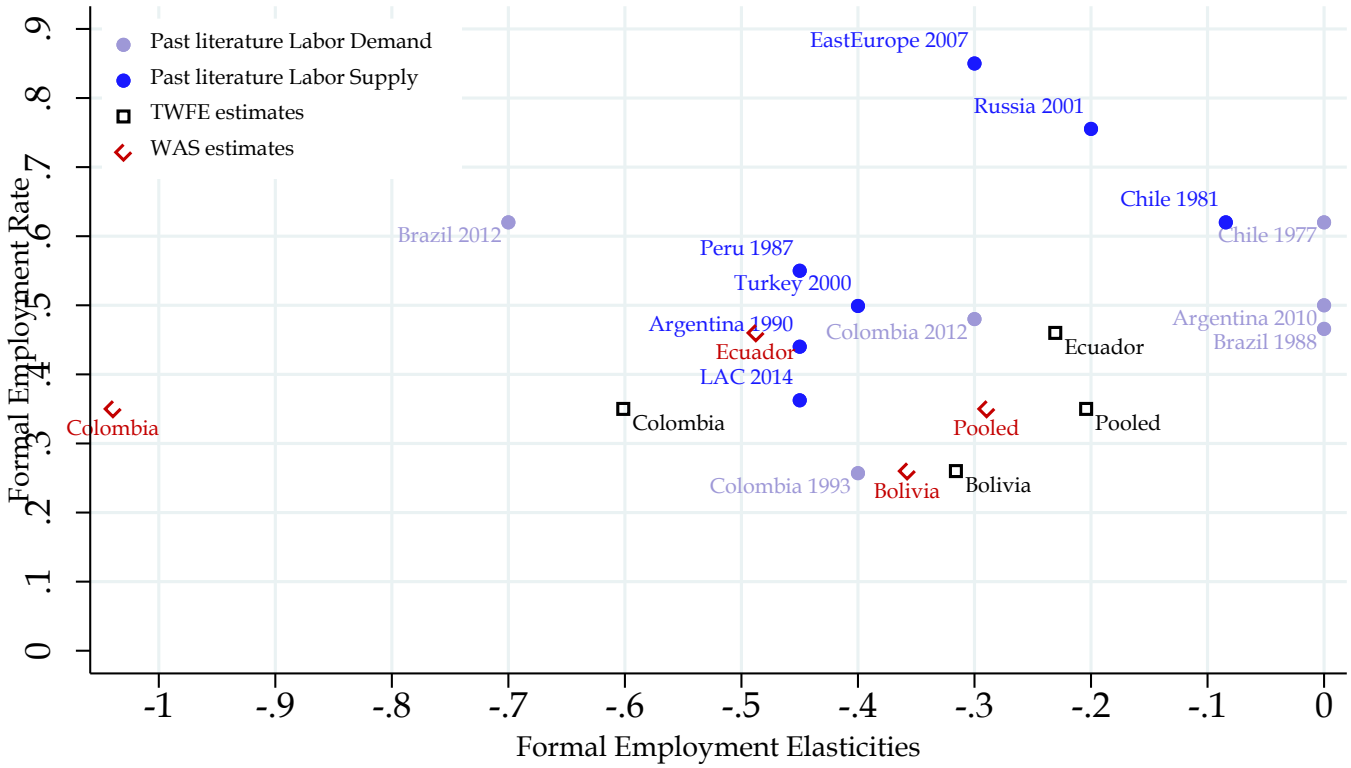
Appendix C: Comparison with the Literature

Our baseline estimates are consistent with the broader labor supply literature documenting modest micro elasticities (in contrast to much larger macro elasticities that reflect adjustments along additional margins such as human capital accumulation, cf. Chetty et al., 2011; Jäntti et al., 2015). For instance, based on structural model estimations, Bargain et al. (2014) report net wage elasticities between 0.1 and 0.6 across 15 European countries and the US. Using a reduced-form ETI approach, Saez et al. (2012) obtain net wage elasticities in the range of 0.2 to 0.5. Closer to our context, Bargain and Silva (2024) exploit long-term wage variation in grouped estimations to estimate participation elasticities in LAC countries, finding an average of around 0.25. Although these estimates refer to standard labor supply margins (participation, hours worked, or earnings), they provide a useful benchmark for the order of magnitude typically observed for micro elasticities.

Although evidence on formal employment elasticities in LAC remains scarce and fragmented, we have attempted to assemble a set of estimates from previous studies to benchmark our results. We extend this comparison beyond LAC to include selected studies on transition economies, where informal employment is also prevalent. Because these studies do not always report elasticities directly, some imputations and harmonization were required. Most importantly, many studies focus on employer-side contributions (Cruces et al., 2010; De Barros and Corseuil, 2004; Gruber, 1995; Kugler and Kugler, 2009; Bernal et al., 2017) while our estimates capture changes in personal income taxation and employee or self-employed contributions (studies also reflecting supply-side elasticities, broader reforms or labor costs falling on workers include: Mondino and Montoya, 2004; Edwards and Edwards, 2000; Saavedra and Torero, 2004; Heckman and Pagès, 2004; Gray et al., 2007; Betcherman and Pagès, 2007; Slonimczyk, 2011). As such, the elasticities are not all directly comparable since they reflect different incidence patterns and adjustment margins. However, they remain informative as reduced-form measures of how changes in the overall tax wedge on formal work affect formalization across contexts.

Results are presented in **Figure C1**, where we plot formalization elasticities against formal employment rates. The bulk of supply-side estimates from the literature (excluding ours) lies between −0.5 and −0.1. Our estimates fall within a similar [-.6;-.2] range across countries and specification, with the exception of the WAS estimate for Colombia, which is somewhat larger in magnitude (yet, sensitivity analyses yield smaller elasticities, closer to −0.6, in alternative specifications, cf. **Table 3**). The mean elasticity across the supply-side studies reviewed in **Figure C1** is −0.3, close to our pooled TWFE (≈ −0.2) and WAS (≈ −0.3) estimates.

Figure C1: Formal Employment Elasticities in the Literature



Source: Author's elaboration based on studies focusing on how changes in the tax wedge affect formalization across contexts. Most studies examine demand-side (employer payroll contributions or labor cost) elasticities: Cruces et al. (2010) for **Argentina 2010**, De Barros and Corseuil (2004) for **Brazil 1988**, Gruber (1995) for **Chile 1977**, Kugler and Kugler (2009) for **Colombia 1993**, Bernal et al. (2017) for **Colombia 2012**. Studies reflecting supply-side elasticities and broader institutional contexts include: Mondino and Montoya (2004) for **Argentina 1990**, Edwards and Edwards (2000) for **Chile 1981**, Saavedra and Torero (2004) for **Peru 1987**, Heckman and Pagès (2004) for **LAC 2014**, Gray et al. (2007) for **Eastern Europe 2007**, Betcherman and Pagès (2007) for **Turkey 2000**, Slonimczyk (2011) for **Russia 2001**. Our own estimates are reported as square dots (TWFE) and diamond dots (WAS).

Additional References (not in main text):

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Betcherman, G. and C. Pagés (2007). Estimating the Impacts of Labor Taxes on Employment and the Balances of the Social Insurance Funds in Turkey. World Bank, Washington, DC.

De Barros, R. P. and C. H. Corseuil (2004). The Impact of Regulations on Brazilian Labor Market Performance, In J. Heckman & C. Pagés (Eds.), *Law and employment: Lessons from Latin America and the Caribbean* (pp. 273–350). University of Chicago Press.

Gray, C., T. Lane and A. Varoudakis (2007). *Fiscal policy and economic growth: Lessons for Eastern Europe and Central Asia*. World Bank.

Gruber, J. (1997). The incidence of payroll taxation: Evidence from Chile. Journal of Labor Economics, 15(3, Part 2), S72–S101.

Heckman, J. J., & Pagés, C. (Eds.). (2004). *Law and employment: Lessons from Latin America and the Caribbean*. U. of Chicago Press.

Mondino, G., & Montoya, S. (2004). The effects of labor market regulations on employment decisions by firms: Empirical evidence for Argentina. In Heckman & Pagés (Eds.), *Law and employment: Lessons from Latin America and the Caribbean*, U. of Chicago Press.

Saavedra, J., & Torero, M. (2004). Labor market reforms and their impact on formal labor demand and job market turnover: The case of Peru. In Heckman & Pagés (Eds), *Law and Employment: Lessons from Latin America and the Caribbean* U. of Chicago Press.

Slonimczyk, F. (2011). The effect of taxation on informal employment: evidence from the Russian flat tax reform, In H. Lehmann & K. Tatsiramos (Eds.), *Informal employment in emerging and transition economies* (Research in Labor Economics, Vol. 34, pp. 55–88). Emerald.

Appendix D: Benefit-elasticities of Formal Employment

We estimate income effects by examining how changes in unconditional benefit levels affect formal employment. **Table D1** reports formal employment elasticities with respect to benefit levels, using the grouped WAS framework. At the country level, benefit elasticities are small and statistically insignificant in most cases. Point estimates fluctuate across subperiods and countries, with no systematic pattern. In Bolivia, the elasticity turns negative in the second subperiod but remains imprecisely estimated. In Colombia and Ecuador, estimates are close to zero in both subperiods. In pooled estimates, we find a statistically significant negative elasticity over 2008–2014/15, but the effect vanishes in the second subperiod. Overall, these results suggest limited and unstable income effects of benefit changes on formal employment, especially when compared to the more robust responses to changes in FTRs.

Table D1: Formal employment response to benefits (grouped estimations)

Benefit-Elasticities	2008-14/15	2014/15 -19
	(1)	(2)
Bolivia	0.08	-0.17
	(0.11)	(0.27)
Colombia	0.06	-0.01
	(0.07)	(0.07)
Ecuador	0.04	-0.03
	(0.15)	(0.05)
Pooled	-0.24 ***	-0.002
	(0.05)	(0.03)

Note: The table reports formal employment elasticities from changes in benefits using grouped estimations on unconditional benefit levels, group and time fixed effects, formalization tax rates, and additional controls (% of married individuals and average household size). Estimations are conducted over pairs of years using the Weighted Average on Switchers' Slopes (WAS) estimator for continuous treatments without stayers, following de Chaisemartin et al. (2024). Bootstrap standard errors (in parenthesis) are computed using 100 replications and clustered at the group level. Significance levels are *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$. For each country n=144 (3 years × 48 groups); for pooled-country estimations, n=432.

The most plausible explanation lies in the institutional context: benefit levels do not vary substantially in real terms over time (see **Table A2**), limiting the scope for detectable income effects. More generally, the broader labor-supply literature also tends to report insignificant income effects (Blundell et al., 1998; Bargain et al., 2013). In the LAC context, however, several studies document disincentives to remain formally employed when cash transfers are not conditioned on (formal) earnings—for instance in the case of conditional cash transfers in Brazil (de Holanda Barbosa and Corseuil, 2014; de Brauw et al., 2015) or universal child

benefits in Argentina (Garganta and Gasparini, 2015). Such effects may be amplified when transfers interact with the implicit tax burden associated with entering formal employment (Bergolo and Cruces, 2021). A related literature examines universal healthcare expansions, which reduce the necessity of holding formal employment to access social insurance.[16] Conversely, when access to social insurance for workers and their families remains tightly linked to formal jobs, formal employment may increase (Levy and Schady, 2013, for LAC; Bergolo and Cruces, 2014, for Uruguay). Finally, recent work shows that large-scale social assistance can generate local multiplier effects, whereby increased economic activity offsets or even dominates traditional income effects, potentially leading to higher formal employment (Gerard et al., 2026; Leite, 2025).

Additional References (not in main text):

Aterido, R., M. Hallward-Driemeier and C. Pagés (2011). Does Expanding Health Insurance Beyond Formal-Sector Workers Encourage Informality? Measuring the Impact of Mexico.s Seguro Popular, IZA DP 5996 and Policy Research working paper no. WPS 5785 World Bank.

Azuara, O. and I. Marinescu (2013). Informality and the expansion of social protection programs: Evidence from Mexico, Journal of Health Economics, 32, issue 5, p. 938-950.

Brauw, A., D. Gilligan, J. Hoddinott, S. Roy (2015). Bolsa Família and Household Labor Supply. Economic Development and Cultural Change, 63

Camacho, A., E. Conover, and A. Hoyos (2013). Effects of Colombia's social protection system on workers' choice between formal and informal employment. World Bank Economic Review, 28(3):446–466.

Campos-Vazquez, R. M. and M. A. Knox (2013). Social protection programs and employment: The case of mexico's seguro popular program. Economıa mexicana. Nueva epoca, 22:403–448.

De Holanda Barbosa A., and C. Corseuil (2014). Conditional cash transfer and informality in Brazil. IZA Journal of Labor & Development, 3, 37.

Garganta, S. and L. Gasparini (2015). The impact of a social program on labor informality: The case of AUH in Argentina, Journal of Development Economics, 115 (C), 99-110.

Juarez, L. (2009). Are Informal Workers Compensated for the Lack of Fringe Benefits? Free Health Care as an Instrument for Formality. Working Paper presented at IZA/SOLE Meeting

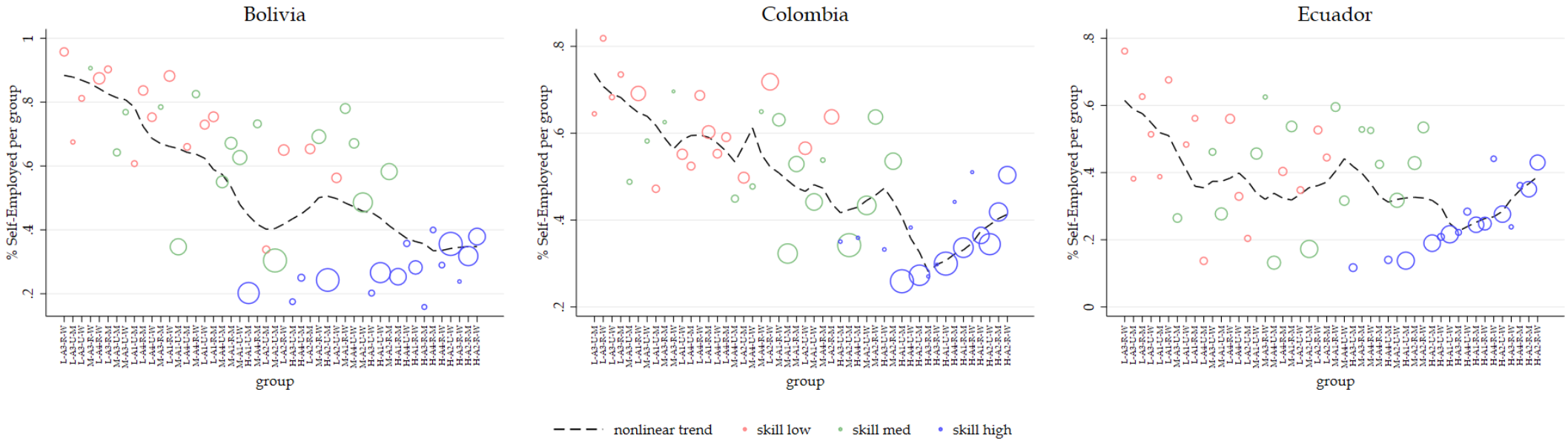
Leite, G. (2025) Cash Transfers and Women's Labor Supply: Evidence from the World's Largest Program, LSE working paper.

[16] Evidence for Mexico's Seguro Popular is provided by Azuara and Marinescu (2013), Juárez (2009), Aterido et al. (2011), Campos-Vázquez and Knox (2013), and Bosch and Campos-Vázquez (2014), while Camacho et al. (2013) document similar patterns for social protection reforms in Colombia.

Appendix E: Earnings Levels and Self-Employed vs. Salary Workers

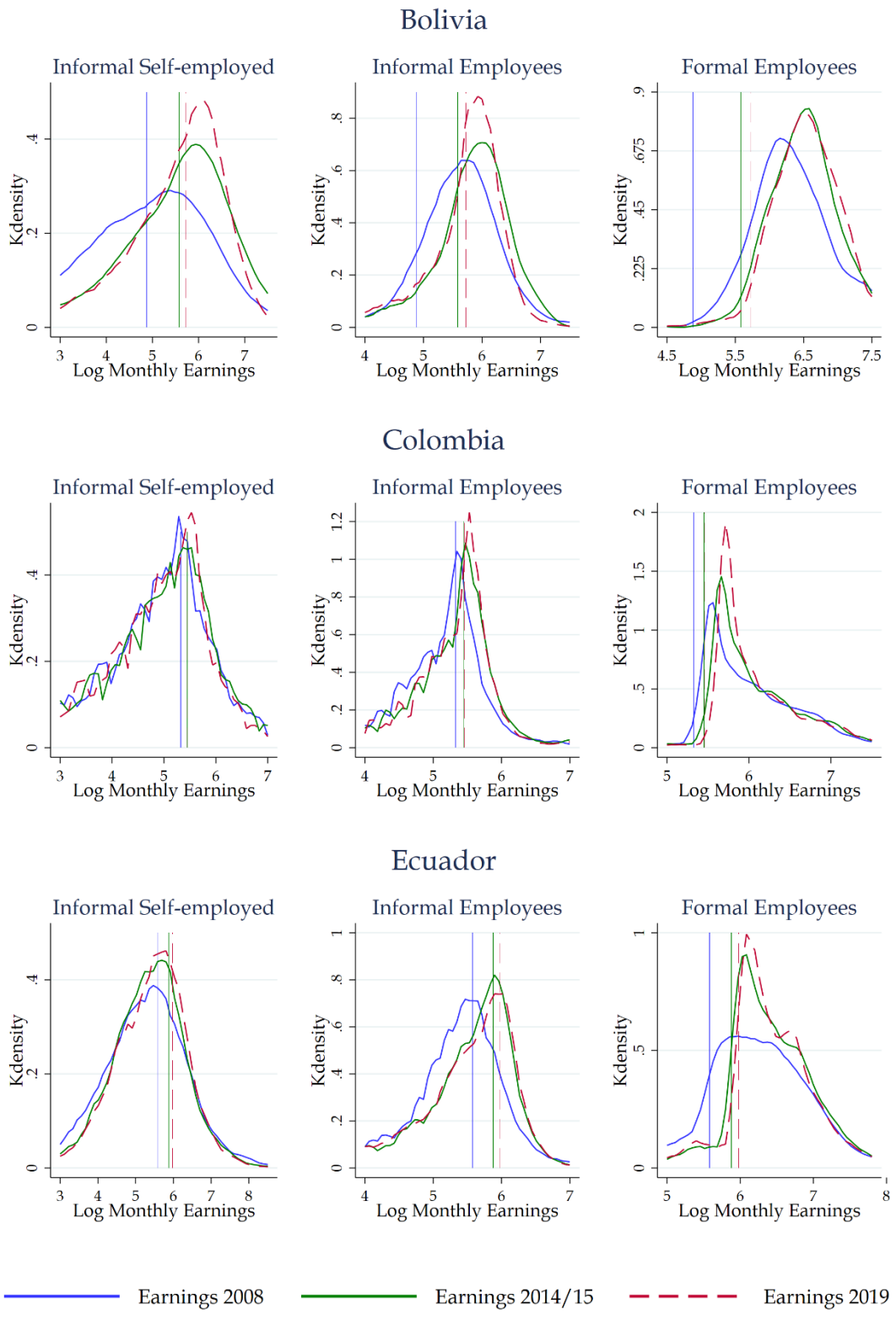
The fraction of self-employed workers by group is shown in **Figure E1**. Pseudo-panel groups are ranked by earnings levels, with skill categories (one of the criteria used to construct the groups) highlighted. The figure indicates that self-employment is concentrated primarily among lower-earning and lower-skilled groups. Earnings densities for all countries and periods are presented in **Figure E2**. They show that a substantial share of informal workers, particularly among the self-employed, earn below statutory minimum wage levels.

Figure E1 : Share of Self-employed Workers by Group



Note: graphs plot the group-level self-employment rates. Groups are defined according to gender x age x education x region. Labels represent groups' education (e.g. Low-L), age groups (e.g. Young-A1), area (e.g. Urban-U) and gender (e.g. Men-M). Groups are ranked by group mean earnings.
The relative size of each group is indicated by the size of the hollow circle.

Figure E2: Kernel Density of Workers' Earnings by Formality Status and Employment Type



Note: graphs represent the distribution of log earnings; vertical lines represent minimum wage levels. All values have been updated to 2019 levels, so that increases in earnings an minimum wage levels can be interpreted in real terms.