Tax disincentives to formal employment in Latin America

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Tax Disincentives to Formal Employment in Latin America

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To finance increased public spending and social programs, Latin America's tax systems need to develop further. Yet taxation can reduce the tax base by discouraging formal employment. Evidence on the intensity of the problem is limited and tends to focus on specifically large reforms of the tax system. Conversely, and to improve external validity, we study whether routine changes in tax policies also alter labor market formalization. Our approach is based on grouped-data estimations of formal employment responses to policy changes. We exploit tax variation across three countries (Bolivia, Ecuador and Colombia) and three periods (2008, 2014/15, 2019). We use precise calculations of counterfactual tax burdens when moving from informal to formal jobs, i.e. formalization tax rates (FTRs). For most countries and pairs of years, FTRs have a negative and significant effect on formal employment, particularly when wages are held constant across periods – in order to extract the pure policy effect – and in a series of sensitivity checks.

Keywords: Labor Supply, Informality, Taxation, Benefits.

JEL: H24, H31, J24, J46

1. Introduction

Tax-benefit systems have been in place for some decades in Latin America and the Caribbean (LAC), steadily combining the development of progressive income tax schemes and the implementation of cash transfer programs targeting vulnerable populations (Robles et al., 2015). While the decline in household income inequality since the early 2000s is mainly attributable to a reduction in wage inequality, changes in tax-benefit systems have played their part in the equalizing process (De la Torre et al., 2017).[1] Yet, income inequality remains high and tax systems could be further developed to increase public spending, expand redistributive programs or increase the progressivity of tax schedules. To this end, a broad tax base is needed, both to collect the necessary resources and to make redistribution more effective. The well-known difficulty is the presence of a large and persistent informal sector, which considerably narrows the current tax base. Equally problematic, possible transitions from formal to informal jobs could further reduce the tax base if the tax burden on formal work were to increase in the future, precisely because of a greater tax progressivity or the increase in contributions to finance social systems.[2]

We propose to study this issue using multiple tax reforms as quasi-experiments to capture tax disincentives to formal employment in three LAC countries. A first motivation pertains to the fact that empirical evidence on labor supply elasticities in the presence of informal employment is still limited and fragmented.[3] A second aspect is that existing studies tend to focus on specifically large tax reforms, chosen precisely because they provide the necessary power to detect potential responses at the formal-informal employment margin (for instance, Cruces et al., 2010, studies major policy shifts in labor taxation in Argentina during the period 1995-2001). Empirically, these reforms may not represent all the types of disincentive effects that could take place. Governments also need to draw policy implications of more routine tax revisions, especially if the succession and accumulation of customary reforms eventually leads to large disincentives. The central endeavor of this paper is therefore to assemble several years

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[1] Around one-third of the reduction in income inequality in the 2000s can be attributed to the development of tax-benefit systems (Cord et al. 2014, Abad and Lindert, 2017). Yet, direct taxation in LAC represents only 23% of GDP, compared to 34.3% in OECD countries, while indirect taxation play a bigger role. Transfers to the working-age population, mainly conditional cash transfers, represent only 0.4% of GDP compared to 4.2% in the OECD (Bargain et al. 2021).

[2] Financial disincentives to work are a global concern for policy design but materialize in different ways. They result in voluntary unemployment in countries characterized by generous welfare programs and unemployment benefits (Blundell et al. 1998). They rather lead to informal work in low and middle income countries, and in particular in LAC economies (Perry et al, 2007). In the latter setting, the financing of social systems and the reduction of income inequality can alternatively be achieved by combining VAT (for tax collection) and redistribution through public goods and universal transfers. Yet this form of redistribution may reduce poverty only to some extent and may not be able to reduce inequality in the upper part of the distribution. Moreover, social insurance or benefits that are universal and not attached to formal employment may reduce the necessity to be formally employed, a mechanism that we also address in our analysis.

[3] See recent evidence for Argentina in Cruces et al. (2010), for Chile in Edwards and Edwards (2000) and for Colombia in Kugler and Kugler (2009), Morales and Medina (2017), Antón (2014), Fernandez and Villar (2017) and Bernal et al. (2017), further discussed below.

of microdata and tax information to check this point. Namely, we aim to use 'normal' variation in tax systems over time and across regions to see whether such policy changes allow detecting responses in terms of formal-to-informal employment transitions. We also want to check whether the elasticities obtained are in line with those of 'big' reforms taken from the literature. By doing so, our work aims to increase the external validity of previous studies on sector choice responses to the tax burden.

Our empirical work starts with a significant effort on data provision. Specifically, we have collected microdata and tax-benefit rules for three countries, Bolivia, Colombia, and Ecuador, and three recent and comparable points in time, namely 2008, 2014/15 and 2019.[4] For each year and country, we have coded the corresponding tax-benefit systems, which allows us to microsimulate the levels of tax paid (and benefits received) by each household in the data. This is done for employed adults in their actual occupation (formal or informal sector) but also in the counterfactual situation (i.e. when the person moves to the other sector). This intensive data work therefore provides a way to measure very precisely the tax burden of formalization by calculating, for each person in our samples, a *formalization tax rate* (FTR). This is simply the change in the disposable income of her household when this person hypothetically switches from informal to formal employment, i.e. whatever her actual employment sector.

To benefit from policy changes in absence of panel data, we use grouped-data estimations of formal employment on FTRs, which allows us to assess the potential disincentive effects of variation in the fiscal pressure associated with formal work. Precisely, we create pseudo panels for each country, with cells defined as gender x age x education x urban groups. Estimations control for group fixed effects and, hence, infer the impact of tax policy changes on formal employment from the group differential moves between sectors attributed to group differential variations in FTRs. This grouped estimation approach has been used to exploit exogenous tax reforms in the more classic literature on labor supply responses to financial incentives (Blundell et al., 1998, Jäntti et al., 2015). To our knowledge, it has hardly ever been used to study transitions between formal and informal employment in response to tax incentives, and never in the context of LAC countries.[5] Our favorite specification links grouped estimations with the approach suggested in the Elasticity of Taxable Income (ETI), as summarized in Saez et al. (2012). In the ETI literature, changes in marginal tax rates are calculating when holding taxable income constant over time, as actual incomes are otherwise endogenous to tax reforms. In the same spirit, we focus on grouped estimations whereby FTRs

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[4] Datasets for years 2020 and 2021 were available but we have refrained from using them given the upheaval caused by the pandemic crisis and the difficulty we would have to interpret results for those years.

[5] Two other studies are discussed extensively hereafter – they also propose grouped estimations but with significant methodological differences: McKay et al. (2023), which focuses on Africa, and Bargain and Silva (2023), which focuses mainly on the overall employment margin for LAC countries using long-term wage variation for identification.

are computed based on constant earnings, namely group earnings fixed at the group average over the three years, hence capturing the pure policy effect.

The main results are as follows. We find evidence of a quasi-systematic response of formal employment to variation in tax pressure. FTR-elasticities of formal employment are significant for a majority of country x year pairs. This result seems quite remarkable if we consider the fact that we do not focus on major reforms. Despite the small scale of 'everyday' reforms, our setting allows detecting the disincentive effect of taxation on formal employment. As a summary measure, our international elasticity (i.e. pooling all countries and years) is around -.3. This order of magnitude is close to past estimates based on large reforms, and reasonable in view of the more general literature on labor supply responses to tax policy changes (Blundell et al., 1998, Bargain et al., 2014, Saez et al., 2012). Results are robust to alternative specifications, for instance the introduction of wage dynamics (i.e. of a term reflecting changes in wage differentials across sectors). We also examine the sensitivity to sample size bias or potential responses along other margins (e.g. migration). Finally, we check how FTR-elasticities of formal employment change when including unconditional cash transfers. These benefits, which are not means-tested on the basis of (formal) earnings, generate income effects that could reduce the incentives to work formally.

This paper provides four main contributions. *First*, it adds to the emerging literature on the employment responses to financial incentives in LAC countries. Our study suggests a rare 'multiple' quasi-experiment, as it exploits exogenous changes in tax-benefit systems for three LAC countries and three pairs of years to measure potential tax disincentives to formal employment. Another attempt to gather data for many LAC countries and years is suggested in Bargain and Silva (2024). This study also makes use of grouped estimation but exploits long-run variation in wages for identification (as suggested in Devereux, 2003, 2004, for standard labor supply in the US) rather than middle-term changes in tax-benefit policies as we do here. Beyond methodological differences, the outcome is also different since Bargain and Silva (2024) focus on traditional labor supply elasticities (participation and worked hours) for LAC countries in their main analysis. We rather explore whether a sequence of usual tax reforms allow detecting formal employment responses, which directly inform us about an efficiency constraint for governments willing to expand redistributive systems.

Second, we suggest one of the rare applications of grouped estimations to identify transitions between formal and informal employment in response to variation in tax-benefit policies. Another interesting attempt is suggested in McKay et al. (2023) to elicit formal employment responses to taxation for several African countries. Several problems arise in their context, which they acknowledge and extensively discuss. The central difficulty is the initially very high level of informal employment in most African countries and the weak presence and enforcement of direct tax systems. Informality is not only high but also reflects possibly strong degrees of labor market segmentation, meaning that whatever the tax incentives, informal

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workers may not have the possibility to access formal employment.[6] In this set-up, formal-employment tax elasticities are expected to be small or insignificant, as found by the authors. The LAC context presents the same kind of difficulty but to a lesser degree: the transition between sectors is probably more fluid and labor markets less segmented (see for instance Maloney, 1999, or Bargain and Kwenda, 2014). At the very least, the elasticities obtained represent a lower bound of the behavioral responses to taxation, and if there are significantly nonzero, we can conclude about the presence of tax disincentives – which is the case in our findings. We stress other differences linked to methodological choices. McKay et al. (2023) calculate the disincentives associated with the taxation of formal income by comparing net incomes of formal and informal workers within each cell used in the grouped estimations. We prefer to compute FTRs based on counterfactual simulations of disposable incomes in the two situations, formal versus informal employment, for each worker composing a cell. Maybe most importantly, as aforementioned, we calculate FTRs holding wages constant, which allows us to extract the pure policy effect in the same logic as in the ETI literature (Saez et al, 2012).

Third, we consider multiple reforms and modest policy changes, which makes our work complementary to studies focusing on single reforms. The few studies that have looked at the response of formal employment to changes in tax-benefit systems have indeed done so by concentrating on a specific, often large, tax change in a single country. These includes quasi-experiments for Argentina in Cruces et al. (2010) and Chile in Gruber (1995) and Edwards and Cox-Edwards (2000). A large increase in payroll tax in Colombia in 1993 also triggered reductions in formal employment, as examined by Kugler and Kugler (2009), while a significant decrease in payroll tax in 2012 in this country was associated with positive effects on formal employment, as shown in Morales and Medina (2017), Antón (2014), Fernandez and Villar (2017), Bernal et al. (2017). The specificities of a single country and a large-scale tax reform can make generalization of results more difficult, so we hope our estimates will bring greater external validity to this relatively small literature (along more recent estimates for relatively poor and understudied countries). All the more so as our results cover several periods and several countries, capturing not just the variation of a single instrument, but composite policy changes where several instruments vary over time and affect different population groups at different intensities.

Fourth, we also isolate the role of universal, unconditional benefits on formal employment. Hence, we contribute to the small literature that documents the link between social protection and formal employment. It describes situations where the access to social insurance for workers and their families is tied to formal jobs (e.g. in Bergolo and Cruces, 2014) or, inversely, when holding a formal position is less of a necessity because healthcare (e.g. in Azuara and

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[6] The implementation and enforcement of tax systems are very limited in poor countries because of the difficulty of identifying an income for many households (due to the high level of auto-consumption) and also of limited administrative capacities. It is therefore difficult to make direct household taxation effective.

Marinescu, 2013) or cash transfers (e.g. Garganta and Gasparini 2015) are universal. Our results point in this direction: corresponding income elasticities are systematically negative, as expected. Yet, they are rarely significant, which is again reminiscent of the findings in the traditional labor supply literature (e.g. Blundell et al, 1998, Bargain et al., 2014, or, for LAC countries, Bargain and Silva, 2024).

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

2. Data and Empirical Strategy

We first present the data and tax-benefit simulations, discuss the key variables used in the analysis, and finally explain in detail the grouped estimation strategy to analyze the effect of fiscal policy on formal employment.

2.1. Data and Tax-benefit Simulations

Surveys and Selection. Our analysis is based on nationally representative household surveys from Bolivia, Colombia and Ecuador, namely *Encuesta de Hogares*, *Gran Encuesta Integrada de Hogares*, and *Encuesta Nacional de Empleo, Desempleo y Subempleo* respectively. They are described in Table A1. These are well-known and robust datasets used in several labor market or welfare analyses in the literature (for instance, Pradhan and van Soest, 1997, Behrman et al., 2007, Gasparini et al., 2011, Levy and Schady, 2013, Gómez-Salcedo et al., 2017, Cuadros-Meñaca, 2020, Busso et al., 2020). All surveys are cross-sectional and contain information on employment, earnings, non-labor income, private transfers, pensions, as well as personal and household characteristics.

Three rounds of survey are used in each country and for similar years, namely 2008, 2014/2015 and 2019. Importantly, these periods were chosen as regularly spaced points over time, according to data availability and to obtain recent estimates,[7] but *not* to delimit a particular tax reform. As motivated in the introduction, we simply consider the series of tax parameter changes that take place during this time frame and treat them as routine tax adjustments. Some countries and pairs of years may be characterized by more important revisions of tax-benefit parameters than others (see the description of tax changes below), but the idea is precisely to check whether a tax elasticity of formal labor is detectable only in this case or more generally.

Our analysis aims to be as representative as possible about potential tax disincentives affecting the labor force of Bolivia, Colombia and Ecuador, hence we keep the whole sample of adults aged 18-65 with positive labor income (earnings from employment or from self-employment)

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[7] We try to consider recent years but also avoid the 2020+ due to the pandemic, during which labor market behaviors have changed in an usual way.

in the data.[8] All three surveys contain information about affiliation to social security, which we use to define formal employment. Note also that modern empirical work tends to rely on administrative data for precise characterization of labor market behavior based on large and robust information, all the more so as this type of data becomes increasingly available (see our own use of it for Ecuador, in Bargain et al., 2023, for instance). However, administrative data does not allow capturing informal workers, by definition. Thus, survey data of good quality must be mobilized, as we do here, since it allows us to identify and measure (in)formality. Nevertheless, we will point out data limitations as they arise.

Tax-benefit simulations. We enrich our data with the calculation of FTRs for each household in each survey. To do so, we use the infrastructure of recently developed tax-benefit calculators for Latin American countries: BOLMOD (Bolivia), COLMOD (Colombia) and ECUAMOD (Ecuador). These calculators apply country-specific coded policy rules to simulate, for each household in the data, the taxes and social insurance contributions paid as well as the cash transfers received. Taxes and social contributions are assumed to be paid only by those affiliated to social security, i.e., those in formal employment. As described below, FTRs essentially represent the fiscal payments arising when the person is formally employed (relative to her household disposable income when she is not declared). Our data contribution goes as follows. In all datasets, income concepts have been harmonized to achieve comparability in the tax-benefit simulations. We have coded the tax-benefit rules for the first and second periods in the data used (2008 and 2014/15), as they were not available in the original calculators. For the most recent year (2019), we use the existing policy coding but adapt it to the dataset at hand (which was not the one originally used by the developers). More information about this process and the tax-benefit calculators is provided in appendix Table A1 and the text below. The tax-benefit systems for the three countries and the different years are detailed in Table A2, which reflects all the policy changes we exploit in our analysis. The reforms are examined in more detail hereafter.

2.2 Key Variables

Formal employment. The outcome of our estimation strategy is formal employment. As mentioned, our analysis defines formal employment as affiliation to social security, which is reported in the data for all countries. Contrary to the 'productive' view (whereby formality is defined according to job types or firm size), the 'social security' definition seems more appropriate since it refers to compliance with formal registration, taxation, and labor regulation as well as the access to social protection for workers. This legalistic view also corresponds to a broader definition of (in)formality and tends to be favored in the literature

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[8] We use nationally representative survey weights in our estimations but results are very similar without. These weights are harmonized to sum up to one in each country when we pool the different countries for the estimation of an international elasticity.

(see Perry et al. 2006, Bargain and Kwenda, 2014). In our context, it is directly consistent with our attempt to assess whether socio-fiscal policies affect formal employment over time.

Formalization tax rates (FTRs). Reforms may modify the fiscal burden an individual faces when formalizing. Higher floors of social security contributions or more progressive taxation, for example, imply higher costs of moving from informal to formal employment. To capture this financial cost of formalization, we opt for FTRs as a simple and transparent summary measure. Their construction starts from the possibility offered by tax-benefit calculators plugged to microdata to simulate the *disposable income* of each household, defined as market income minus direct taxes and social insurance contributions plus cash transfers and pensions. In all three countries, the main components of disposable income are simulated, including employee and self-employed social insurance contributions, personal income tax, and the main cash transfer programs (see Tables A1 and A2).

To quantify the financial cost of moving from informal to formal employment, we simulate the disposable income of the household in both statuses. Formally, let $D_{its}(X_{it}, Z_{it})$ represent the household disposable income of worker i at time t in sector $s = 1$ (formal) or 0 (informal), which depends on the household gross market income X_{it} (including the worker's own labor income) and Z_{it} the set of household characteristics relevant for tax-benefit rules (household composition and demographics). Household disposable income can be expressed as:

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

where $Ben_{it}(Z_{it})$ represents cash transfers received by the household and $Tax_{it}(X_{it}, Z_{it})$ refers to direct taxes and social contributions paid. In the countries studied, benefits are not means-tested: they are universal or proxy-means tested, which is to say that entitlement to benefits is not conditional on earned income or formality status.[9] The FTR is then calculated as:

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

It represents the proportion of income that would be lost due to taxes and contributions on entering formal employment. Thus, it captures the financial (dis)incentives to work formally as embedded in the tax-benefit system. This measure is not susceptible to non-compliance with tax obligations or benefit non-take-up. It indeed represents the theoretical fiscal pressure an individual faces when moving from informal to formal work, thus avoiding problems of endogeneity between the actual sector choice and other behaviors (compliance, take-up, etc.).

2.3 Grouped Estimations

Principles. We move to the estimation framework used to assess whether changes in tax policies, captured by changes in FTRs, impact the level of formal employment in the studied

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[9] If this were the case, it would just mean a change in our notations (to reflect the fact that some benefits vary with labor income or status). The calculation of FTRs would automatically take this into account.

countries. A possibility is to use a difference-in-difference approach when population subgroups are clearly identified as affected by a reform (while others are not). This is for instance the case for payroll tax reductions concerning workers with earnings above a certain threshold (see e.g. Fernandez and Villar, 2017). Given the fact that we are not focusing on a specific policy changes but on an array of tax parameters changing over time and in different countries, we opt for an approach that embraces all these sources of variation in incentives to work formally, and do so in a harmonized way for all countries. Precisely, we construct pseudo-panels based on individuals grouped according to simple socio-demographic dimensions (gender x age/cohorts x education x urban). Then, we effectively follow these cells over time to exploit tax variation. That is, with group estimations, the differential variation in FTRs across groups is assumed to yield differential changes in formalization.

Identification. Several remarks regarding our inference of the tax effect on formalization are in order. *First*, we define groups according to socio-demographic dimensions that cannot be manipulated by individuals in the short-run, so that their differentiated exposures to policy reforms is relatively exogenous. This would not be the case if the groups were defined according to earnings levels for instance (i.e. as in the difference-in-difference example given above).[10] *Second*, our treatment is continuous, i.e. groups are more or less treated, and our strategy implies that, while limiting endogeneity issues, we also reduce the contrast in reform exposure between groups (compared to set-ups where group are defined according to clear-cut treatment variables based on earnings thresholds, for instance). The requirement is however the same: reforms should not be uniform and should affect different groups differently. We will see below that this is clearly the case for the countries and periods under study. *Third*, another requirement pertains to the fact that identification is based on movers: people should have the possibility to move across sectors, hence to respond to tax incentives. As discussed in the introduction, this might not be the case in more constrained contexts, such as African labor markets (see McKay et al., 2019). Fortunately, there is more evidence of a competitive and fluid labor market in LAC (see for instance Maloney, 2004, Bosch and Maloney, 2010, Pratap and Quintin, 2006, Marcouiller et al., 1997, Diaz et al., 2018, Bargain and Kwenda, 2011, 2014). Besides, the bulk of the reforms at stake consists of an *increase* in the tax burden, which should push some workers out of formal work. Obviously, segmentation matters less in this direction.[11] *Fourth,* grouped estimations have been used in other contexts, also relying on policy changes, most notably by Blundell et al. (1998) who examines how labor

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[10] Assume that a change in social contribution affects individuals above a certain earnings threshold: difference in difference (or difference in discontinuity) approaches could be used but the composition of the groups below and above the cutoff would necessarily depend on other margins of response to tax incentives, such as wage negotiation, worked hours, etc. Another remark about the pseudo-panel approach is that it makes groups relatively comparable over time, while earnings groups might vary according to complex forces. That said, our groups might be sensitive to two composition effects pertaining to age and migration, which receive special attention in what follows.

[11] If some workers in our data were rationed out of formal employment, and if reforms implied moves in the other direction – decreases in taxation making formal work more profitable – then our estimates would represent a lower bound of the tax incentives to formalize.

supply responded to tax reforms in the UK. As such, our study provides an original adaptation of this strategy to the context of extensive margin choices, namely moves between formal and informal sectors, which is utmost relevant to the context of Latin America. Note that another study adopts grouped estimation for LAC countries, Bargain and Silva (2024), but relies on long-term wage variation and focuses mainly on the employment margin (as in Angrist, 1991, and Devereux, 2003, 2004, for the US).[12]

Implementation. For each country and year in our set of data, we group individuals into 48 cells as follows: gender x 4 age groups (18-30, 30-40, 40-50 and 50-65 years old) x education (low, middle and high education levels) x location (urban, rural).[13] As mentioned, we will control for possible sensitivity in group composition along two margins, age and migration. In particular, defining groups according to age makes that groups are not comparable over time in terms of cohort, so we will adopt the alternative design using fixed cohort groups, rather than fixed age groups, in sensitivity analyses. For migration, we will also provide additional estimates by restricting the sample to households that declare they have been geographically stable.

Next, once groups are defined, we compute the group average of all the relevant variables. In particular, for each country and year t, we denote Y_{gt} the formal employment rate (%) for group g and we also calculate the group-average FTR (in %) for that group as:

$$T_{gt} = \sum_{i \in g} T_{it}(X_{it}, Z_{it}) / \sum_{i \in g} n_i. \tag{3}$$

Grouped estimations are based on the following model:

$$Y_{gt} = \alpha_g + \theta_t + \beta T_{gt} + \gamma Z'_{gt} + \delta B_{gt} + \varepsilon_{gt} \tag{4}$$

estimated for each country by pooling all years and weighting group-year observations by group relative size $\sum_{i \in g} n_{it} / \sum_{g=1}^{G} \sum_{t=1}^{T} \sum_{i \in g} n_{it}$. The model aims to explain formal employment by T_{gt}, the group mean FTR, representing the relative tax pressure upon group g in year t, along additional controls. Precisely, α_g represents group fixed effects, which account for the long-term role of socio-demographic characteristics on sector allocation. This is a flexible way to account for demographics, since the persistent effect of being, say, a low-educated rural young woman is more precise than controlling separately for a person's gender, education, age, etc. We also include θ_t, which represents time fixed effects and captures the common changes in tax pressure at every period. In this model, behavioral parameters related to tax incentives, including preferences and compensating differentials across sectors, are assumed to be either constant (and captured by group fixed effects) or common to all groups (and

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[12] The advantage of using short/middle-term tax reforms, as we do, is that they are more exogenous than group-level wage variation. Nonetheless, long-term wage variation is a reasonable alternative and both approaches can be seen as complementary.

[13] For education, low, middle and high education groups correspond to (i) primary education (completed or not), (ii) lower/upper secondary, and (iii) post-secondary to tertiary education, respectively.

captured by time trends). We add further heterogeneity, with a set Z_{gt} of time-varying group-mean characteristics, including marital status and household size. Finally, B_{gt} denotes group-mean universal cash transfers (as explained, they do not depend on the formality status). We will focus mainly on our treatment effect β, but will also take a look at the income effect δ that might influence somehow the choice of working (in)formally.[14]

Neutralizing earnings dynamics. In the basic model summarized by equations (3)-(4), FTRs depend on earnings X_{it} reported by individuals in the different surveys. There are two potential issues. First, these earnings are likely to depend on the sector (we could extend notation, writing X_{its}, to reflect it) so that relative earnings may influence sector choice. Second, within a sector, earnings may change over time in a way that is related to policy changes (impact of minimum wage policies, incentives to work more/less hours, etc.). Thus, a cleaner and favorite specification will rely on the pure policy effect obtained when FTRs are calculated on the basis of *fixed* earnings. For this specification, we nominally adjust earnings by CPI to 2019 levels and compute group intertemporal mean earnings, i.e. we take the average of individual earnings within a group over the three time periods, namely $\bar{X}_g = \frac{1}{T}\sum_{t=1}^{T}(1 + r_t)X_{gt}$ (with r_t denoting the uprating factor to express all earnings in 2019 levels). We fix group earnings at this level when calculating group-mean FTRs at each period, written:

$$\tilde{T}_{gt} = \sum_{i\in g} T_{it}(\bar{X}_g, Z_{it})/\sum_{i\in g} n_i, \tag{3'}$$

so the empirical model becomes:

$$Y_{gt} = \alpha_g + \theta_t + \beta\tilde{T}_{gt} + \gamma Z'_{gt} + \delta B_{gt} + \varepsilon_{gt}. \tag{4'}$$

This approach is directly inspired from the literature on the elasticity of taxable income (ETI) summarize in Saez et al. (2012). In this literature, the underlying idea is to estimate the response of income groups to a change in tax rates modeled by imputing the sole policy change, i.e. holding income levels constant when calculating the individual tax burden. Here, the specification of equations (3')-(4') reflects the fact that earnings dynamics are neutralized so that relative changes in FTRs across groups reflect the pure policy effect, i.e. workers' relative exposure to changes in policy parameters.

Pooled model. Finally, we can also estimate an average FTR-elasticity based on a sample pooling all countries and years. It is similar but corresponds to the formal employment rate of group g in year t and country c, specified as:

$$Y_{gct} = \alpha_{gc} + \theta_{ct} + \beta\tilde{T}_{gct} + \gamma Z'_{gct} + \delta B_{gt} + \varepsilon_{gct}. \tag{5}$$

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[14] Note that these coefficients are *semi-elasticities* of formal employment, the former with respect to FTR and the latter with respect to universal benefits. To obtain formal employment elasticities, coefficient estimates are multiplied by mean FTRs in the former case (mean benefits in the latter) and divided by the average formal employment rates.

In this model, fixed effects α_{gc} characterize a country-group, capturing for instance the persistent labor market characteristics of being a low-educated rural young Bolivian woman. Time effects are replaced by country-time effects θ_{ct}, which represent the country overall tax pressure at each period. This model exploits spatial and time variation in policy parameters (3 years x 3 countries, hence 9 policy settings). It should also be more precise given the larger number of groups, providing more power to detect the impact of policy changes.

3. Labor Markets and Institutional Backgrounds

Before turning to the results, this section provides a first look at the data, with a discussion on labor markets and (in)formality in Bolivia, Colombia and Ecuador. We also present background information on tax-benefit policies, with a description of the reforms and how they have impacted FTRs.

3.1 Formal and Informal Labor Markets

Informal employment has decreased on average over the past decades in Latin America but informality rates remain high and represent a significant challenge for the region, hindering fiscal capacity and limiting the expansion of redistributive systems. (Maurizio and Monsalvo 2021, Maurizio 2021, ILO 2023, IDB 2023). This is in particular the case with the countries under study, where informality rates have slightly declined but remain large: 81.5% in Bolivia, 62% in Colombia and 63.5% in Ecuador in 2019 (against 85.9%, 66.8% and 72.6%% respectively in 2008).[15] Moreover, (in)formality rates and their evolution vary enormously within countries. Figure 1 depicts formal employment rates plotted against the average earnings decile of each group used in our pseudo-panels. This representation seems to be a convenient way to show both group dispersion and, at the same time, the relationship between (in)formality and income levels. We see an increased prevalence of informal employment at lower earnings quintiles in all countries. This is naturally related to the fact that low-skill workers tend to work more informally due to the nature of their occupations (Perry et al. 2007) but tax disincentives may also play a role, as discussed below. If we look at the nature of the pseudo-panel groups located in the lower deciles (unreported), it appears to be consistent with stylized facts for LAC countries and notably the fact that informal employment is primarily concentrated among youths, older workers, women and individuals with lower levels of education (Gasparini and Tornarolli 2009, Ulyssea 2020). Nonetheless, informality concerns most groups at different degrees.

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[15] These figures are based on the ILO's database: https://ilostat.ilo.org/topics/informality/p and similar to those in the data we use.

Figure 1: Formal employment rates by group, 2008-2019
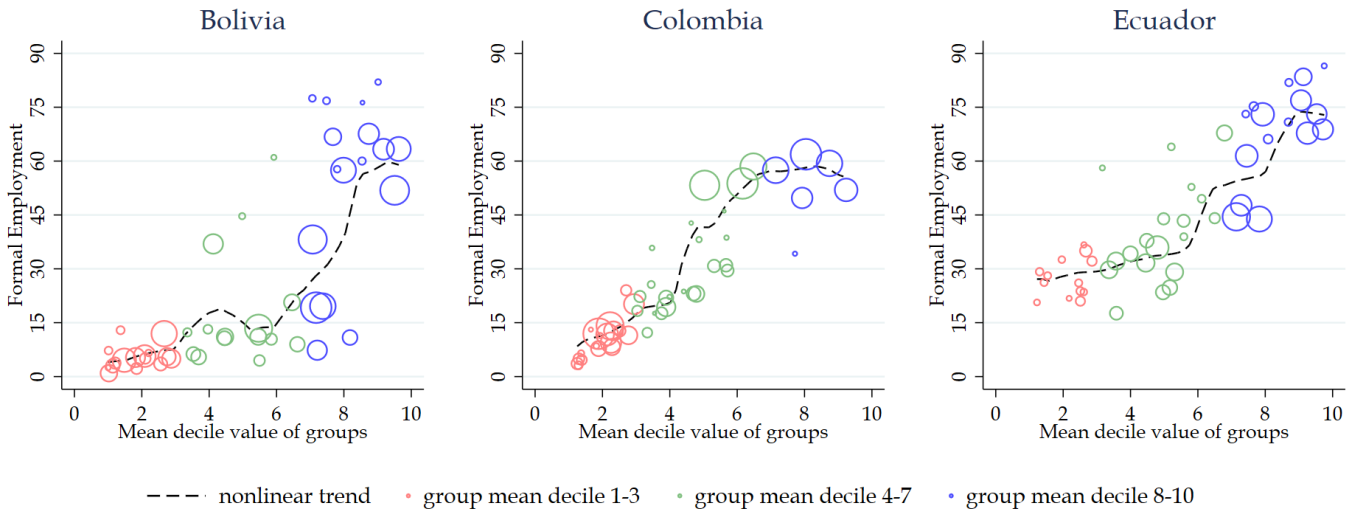
Note: graphs plot the group-level formal employment rates by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to). Groups are defined according to gender x age x education x region.
The relative size of each group is indicated by the size of the hollow circle

3.2 Tax-benefit Policies and Reforms

Tax-benefit systems. The size of tax-benefit systems in LAC remains modest compared to high-income countries. In 2019, the tax-to-GDP ratio in LAC stood at 22.9%, well below the OECD average of 33.1% (OECD et al. 2021). The countries under study were close to the LAC average (24.7% in Bolivia, 20% in Colombia and Ecuador). We extensively document the structure and key parameters of the tax-benefit instruments of all three countries in the Appendix Table A2. The three of them have social security contributions, which comprise one or several rates. Payroll taxes tend to be flat, while more redistribution would require some progressivity. Earnings are not taxed up to a certain income level in Colombia and Ecuador, but this threshold is very high, and the tax system also includes generous deductions, so that few people are concerned. Conversely, flat-rate taxation without exemption tends to be more effective in Bolivia. Overall, personal income tax accounts for a relatively small share of tax revenue (9% of total tax revenue in LAC countries, and slightly lower levels in our three countries, compared to 24% average in the OECD), while social insurance contributions often represent a larger share (i.e. between 25% and 27% in Bolivia and Ecuador, but only 9.5% in Colombia). On the benefit size, redistribution is also modest. Direct cash transfers to vulnerable populations, mainly made of proxy means-tested transfers, represent only 0.14% of GDP (i.e. 0.1% in Colombia, 0.2% in Bolivia and 0.6% in Ecuador, see Bargain et al., 2021). Ecuador has the most redistributive system, with performances close to the LAC average (see Bargain et al., 2017, Arancibia et al., 2019). Specific programs, described in Table A2, are analyzed in many contributions, especially the *Bono de Desarrollo Humano* in Ecuador (see for instance Carrillo and Ponce, 2009, Fernald and Hidrobo, 2011, or Bosch and Schady, 2019) but

also *Bono Juancito Pinto* in Bolivia (e.g. Bauchet et al., 2018) and *Familias en acción* in Colombia (e.g. Attanasio et al., 2009, 2015).

FTR distribution. FTRs vary widely across countries in the Andean region, reflecting different prevalence levels of fiscal instruments. Nonetheless, a common feature to most countries is the fact that financial disincentives to formal employment tend to be higher at lower earnings levels (Jara et al., 2023a). This is due in particular to higher rates of social security contributions at low deciles caused by minimum contribution levels. This pattern is highly visible in our context. In Figure 2, we report mean FTRs by groups, as a function of the average decile value of each group. There is a broad negative relationship between tax pressure and income levels overall, specifically in Colombia and Ecuador, but also in the first half of the distribution in Bolivia. In this country, the FTR distribution is more U-shaped due to the role of income tax. As mentioned, a flat tax is effective at every level and in particular at the top; together with a progressive schedule for social insurance contributions, this explains rising FTRs in the upper part of the distribution.

Figure 2: Formalization tax rates by group
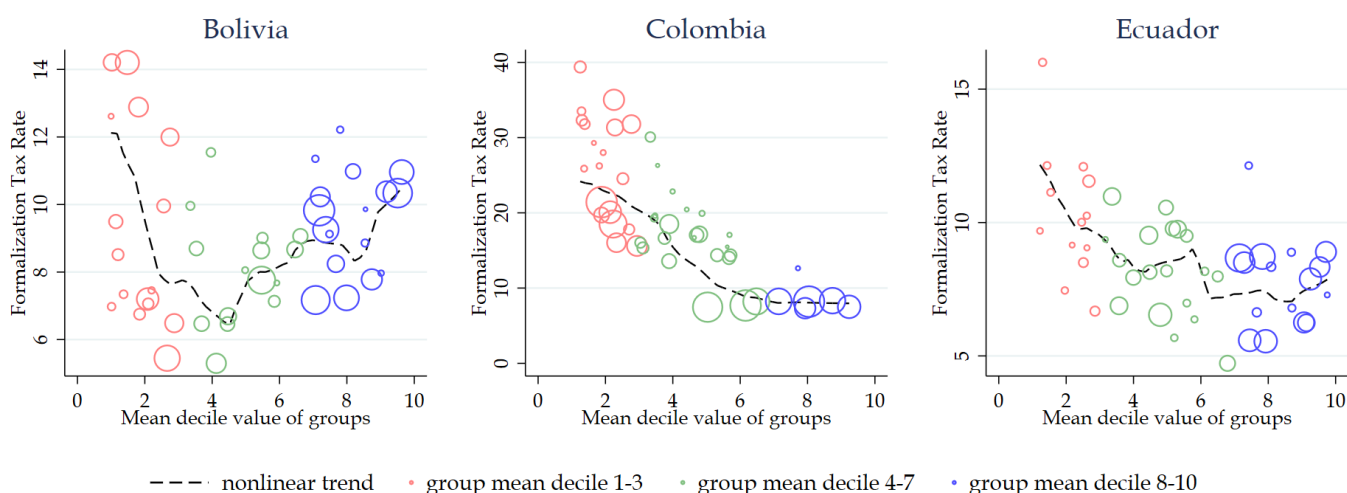
Note: graphs plot the group-level formalization tax rates (FTRs) by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to). Groups are defined according to gender x age x education x region.
FTRs capture the mean change in disposable income from moving from informal to formal work.
The relative size of each group is indicated by the size of the hollow circle.

Recall that these patterns do not represent *actual* tax levels but the relative change in disposable income incurred when a worker moves into the formal sector. In particular, high FTRs at the bottom of the distribution reflect the potentially high cost that low-skill workers would face, in terms of social insurance contribution payments, if they worked formally. Indeed, while contribution floors are set at the contribution level corresponding to the minimum wage (as documented in Table A2), many low-decile workers receive earnings below the minimum wage (see Figure A1, panel a). This is the case for self-employed workers (Figure A1, panel b),

who represent the bulk of workers at these low income levels (as depicted in Figure A2), but also for low-skill employees (Figure A1, panel c).

From this picture, it is tempting to conclude that very high informality at low earnings levels (as seen in Figure 1) is due to the excessive potential taxation, relative to living standards, in case of formalization (as measured in Figure 2). Even if compelling, this association is not causal. Many other reasons (than tax pressure) may indeed explain that low-skill workers stay in informal employment or self-employment. This is the reason why we are going to rely on *changes* in FTRs over time and relatively across groups for more causal inference.

Figure 3. Decomposition of the change in FTR (2008-19) by income decile groups
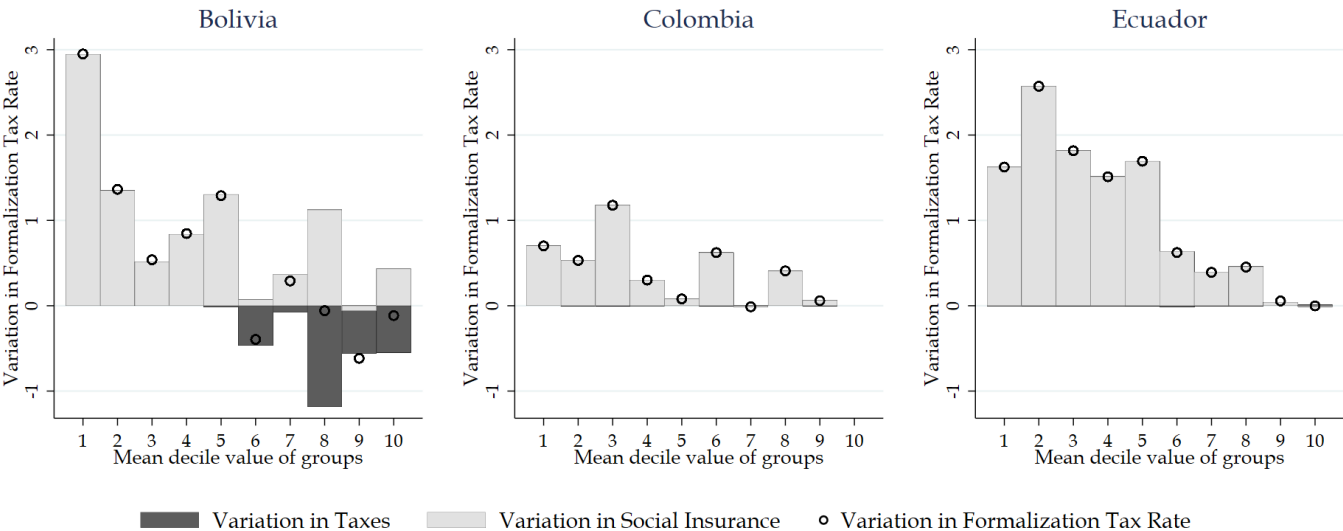
Note: graphs plot the decomposition of changes in formalization tax rates (FTRs) from 2008 to 19 by income decile groups
FTRs capture the mean change in disposable income from moving from informal to formal work.

Policy changes affecting FTRs. Even if we do not seek to characterize major reforms specifically, as we explained in the introduction, movements in fiscal parameters are nevertheless necessary to have a chance of detecting labor market reactions. A number of policy changes have effectively taken place over the 2008-2019 period, as presented in Table A2. They represent a set of routine changes in the parameters of income tax scales, social contribution rates and benefits, which drive changes in FTRs. This is illustrated in Figure 3 by the total change in FTRs between 2008 and 2019 by deciles and their decomposition between the contributions of the different instruments. The main policy reforms can be summarized as follows. *First*, successive increases in minimum wages have raised contribution floors and further increased FTRs at low deciles. This can be seen for all three countries, but the progression was stronger in Bolivia and Ecuador, which transpires in larger FTRs increases at lower deciles.[16] *Second*, social insurance contributions also increased in these two countries due

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[16] All countries have experienced a large increase in the minimum wage in real terms, especially over the first period of analysis, 2008-2014/15: 86% in Bolivia, 13% in Colombia, and 35% in Ecuador. Earnings densities for all countries

an increase in contribution rates, and the introduction of an additional contribution (a progressive solidarity contribution) in Bolivia, during the first period (2008-2014/2015). These reforms contribute to the increase in FTRs at all points of the distribution in Figure 3. *Third*, personal income tax changed as well. In Bolivia, the zero-tax threshold (above which the flat tax kicks in) was uprated at a higher rate than the minimum wage (in particular between 2008-2015). This means that the fraction of people liable to personal income tax dropped, which explain the decline in FTRs at the top in Figure 3.[17] *Fourth*, on the benefit side, social transfers are proxy means-tested in Ecuador and Colombia,[18] while a universal transfer for specific population categories is in force in Bolivia. As such, being in formal or informal employment does not affect eligibility to cash transfers (i.e., entering formal employment does not imply benefit withdrawals). However, changes in benefit amounts or the introduction of new cash transfer programs might influence the decision to move from formal to informal employment through an income effect (see e.g. Bosch and Campos-Vazquez, 2014), which should be captured by term δ in the model. A number of changes in benefits are in fact observed over the period under consideration.[19]

3.3 A first look at the result: formalization shift versus change in FTRs

To give a visual foretaste of our results, we plot changes in formal employment over the whole period 2008-2019 against the corresponding changes in FTRs (calculated using fixed earnings). These changes are represented at group level in Figure 4 along regression lines and their confidence bounds (accounting for group weights). Subtitles show the slopes (with standard errors) and correlation rates. It turns out that ordinary policy changes produce sufficient variation in FTRs over time and across groups to contribute significantly to a decline in formalization. The negative correlation is slightly below -0.3 in all countries and statistically different from zero. This non-parametric illustration anticipates our results in the next section, where we shall additionally control for individual characteristics and provide other refinements. But it is similar in essence since using time differences here is equivalent to the time-demeaning version of our panel estimations with group fixed effects. Note also that the effects generated in each country are consistent with the policy changes described above. For instance, Colombia and Ecuador were mainly characterized by increased social contributions

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and periods, along minimum wages, are represented in Figure A4 and show that the number of informal workers paid below minimum wage levels, both employees and self-employed, has increased, especially over the first period.

[17] In Ecuador and Colombia, progressive income tax schedules have experienced some changes but the high concentration of tax payers at the very top of the distribution means that these changes are not visible in a decile classification.

[18] Eligibility is based on a multidimensional index encompassing a variety of housing and household characteristics.

[19] As detailed in Table A2, some cash transfer programs have been introduced, e.g., the *Juana Azurduy* transfer in Bolivia, the *Jovenes en Acción* and *Colombia Mayor* transfers in Colombia, all between 2008 and 2015. For existing programs, some benefit amounts were fixed over the period of analysis, resulting in a drop in real terms due to inflation while others have increased (such as Bono de Desarrollo Humano in Ecuador and Renta Dignidad in Bolivia).

at low earnings levels and indeed, Figure 4 shows a higher density of low-decile groups (red hollows) with both increasing FTRs and an associated decrease in formal work. As seen, rising contributions also concern intermediary earnings levels in Ecuador, which transpires on the graph (green hollows). For Bolivia, it illustrates two mechanisms detailed before, namely rising social contributions at low deciles, which yields a drop in formalization (red), and cuts in income tax, associated with increasing formalization at the top (blue).

Figure 4: Change in formal employment against change in formalization tax rates, 2008-2019
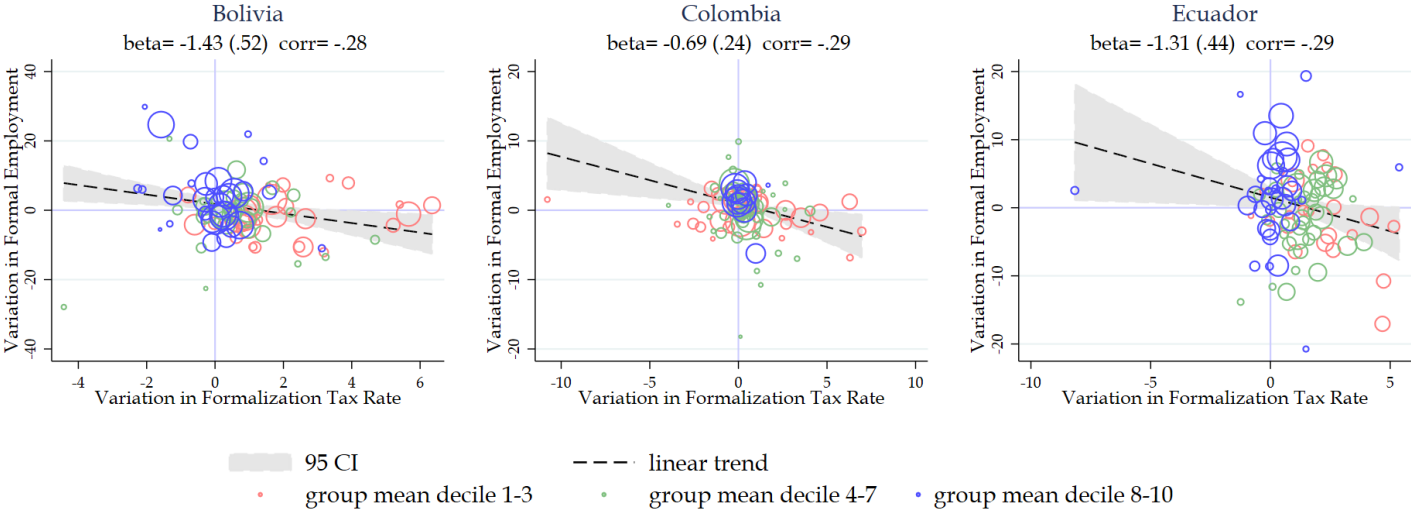
Note: graphs plot the group-level variation in formal employment rates against the variation in formalization tax rates (FTRs) over all pairs of years for each country. FTRs capture the mean change in disposable income from moving from informal to formal work and are based on 'fixed earnings'. Groups 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

4. Results

This section presents the results of our grouped estimations for Bolivia, Colombia and Ecuador, as well as a pooled estimation for all countries. We start with baseline estimation results and turn to sensitivity analyses and informative heterogeneous results.

4.1 Baseline Effects of Policy Changes

FTR-elasticities of formal employment. Results for our baseline grouped estimations are presented in Table 1. For each country, we first indicate average formal employment rates and average FTRs. Then we report FTR-elasticities derived from the estimation of the model where FTRs are calculated using actual earnings (equations 3-4) or earnings fixed to group intertemporal means (equations 3'-4'). Column (1) presents results without group fixed effects, which capture both 'between' and 'within' variation. We observe very large FTR-elasticities in all three countries in this case. This is obviously due to the influence of the cross-sectional negative correlation between FTRs and formal employment. Indeed, as illustrated above,

poorer workers face more fiscal pressure if they formalize, due to large social contribution payments in particular, and happen to be informal workers (often self-employed).

Table 1: Formal employment response to formalization tax rates (grouped estimations)

	Formal empl. rate (%)	Mean FTR (%)	FTR-Elasticities		
			(1)	(2)	(3)
Bolivia (years: 2008, 2015, 2019)					
Actual earnings	23.8	11.0	-1.38 ***	-0.26 **	-0.26
			(0.22)	(0.11)	(0.16)
Fixed earnings	23.8	8.7	-0.64 *	-0.44 ***	-0.44 **
			(0.34)	(0.13)	(0.21)
Colombia (years: 2008, 2014, 2019)					
Actual earnings	41.3	12.3	-1.06 ***	-1.06 ***	-0.25 ***
			(0.05)	(0.05)	(0.05)
Fixed earnings	41.3	12.5	-0.98 ***	-0.25 ***	-0.25 ***
			(0.04)	(0.07)	(0.08)
Ecuador (years: 2008, 2014, 2019)					
Actual earnings	41.6	8.8	-1.14 ***	-1.14 ***	-0.30 ***
			(0.10)	(0.10)	(0.11)
Fixed earnings	41.5	8.6	-1.05 ***	-0.38 ***	-0.38 ***
			(0.10)	(0.09)	(0.10)
Pooled					
Actual earnings	35.6	10.7	-0.83 ***	-0.83 ***	-0.22 ***
			(0.05)	(0.05)	(0.08)
Fixed earnings	35.5	10.0	-0.70	-0.38 ***	-0.38 ***
			(0.65)	(0.06)	(0.09)
Group FE (gender x age x education x location)			NO	YES	YES
Year FE (Country x Year in the Pooled model)			YES	YES	YES
Robust S.E.			NO	NO	YES

Note: The table reports formal employment elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs), controlling for time dummies, group fixed effects as indicated, and controls (percentage of married people in the group, average household size, unconditional benefit levels, i.e. benefits unrelated to formal/informal sector status). FTRs capture the mean change in disposable income from moving from informal to formal sector. Standard errors indicated in parentheses and significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For all countries, n=144 (3 years x 48 groupes), and for pooled countries estimations, n=432.

To provide more causal inference, we then focus on 'between' variation, i.e. relying solely on exogenous time variation in policy parameters. Results correspond to the estimates of column (2) where we control for group fixed effect. Column (3) additionally shows corresponding estimates with robust standard errors. The first conclusion is that routine reforms manage to generate significant responses in terms of sector allocation. We observe a systematic decline in formal employment when tax pressure increases. Robust estimates are statistically significant in all countries and models, except for Bolivia when actual earnings are used to calculate FTRs. Elasticities are significant everywhere (at least at 5% and often 1%) when using our favorite

specification with fixed earnings, i.e. when focusing on the pure policy effect as in the ETI literature. Thus, these findings confirm a negative relation between net tax pressure and formal employment. They show that even usual changes in social contributions or direct tax parameters trigger disincentive effects to labor formalization.

Magnitude. Another interesting result is the magnitude of elasticities. It is reasonable and at a relatively average level compared to the literature (as commented below), providing extra external validity to a set of past studies based on single reforms. Also, elasticities are not extremely contrasted between countries. They range from -0.44 in Bolivia to -0.25 in Colombia, while the pool model points to an average elasticity of -0.38. This country average entails that a 10% increase in FTRs, which correspond to an additional net taxation of 1 percentage point, would decrease formal employment by 3.8%, which corresponds to about 1.4% of the total labor force of the three countries, i.e. around 530,000 persons. Thus, the behavioral responses associated with tax pressure in these three LAC countries contributes to a substantial decrease in the tax base and should be accounted for in policy design.

Comparison with the literature. Our estimates are consistent with the general labor supply literature that points to modest *micro* elasticities (compared to much larger macro elasticities, which reflect changes in many other dimensions including human capital, see Chetty et al., 2011). For instance, using structural model estimations, Bargain et al. (2014) report *net wage* elasticities between .1 and .6 across 15 European countries and the US. With the more reduced-form ETI approach, Saez et al. (2012) obtain net wage elasticities of a magnitude between 0.2 and 0.5. Closer to us geographically, Bargain and Silva (2024) use long-term wage variation in grouped estimation to estimate participation elasticities and find an average of around 0.25 for LAC countries. These different estimates refer to standard labor supply (participation, worked hours or earnings), but are still interesting to recall the order of magnitude expected for micro elasticities.[20]
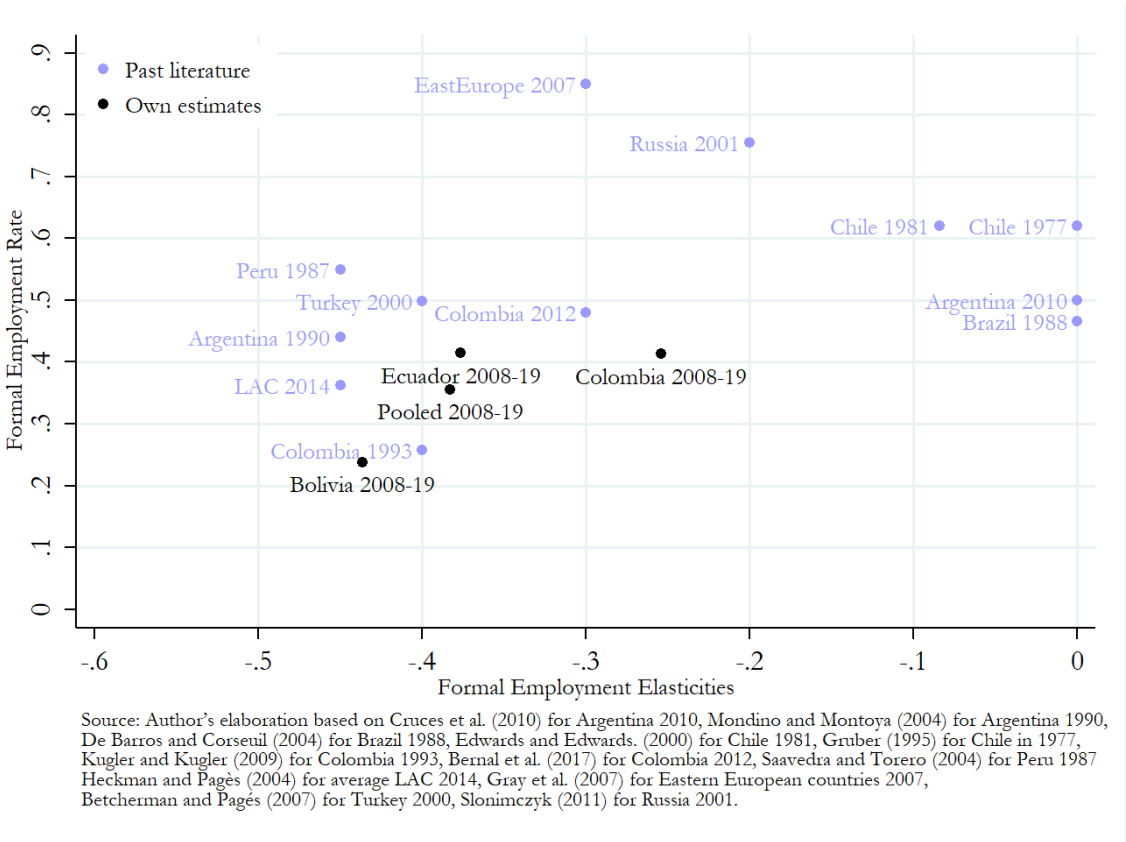
Additionally, even though evidence on formal employment elasticities remains scarce and fragmented in Latin America, we have assembled a few estimates from past studies to benchmark our own results. These studies do not all provide estimates that are readily comparable, so some adjustments and normalizations have been necessary.[21] Adjusted

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[20] The margin we study for LAC, formal versus informal work, is analogous to the extensive labor supply margin in rich countries, where non-formal workers are rather unemployed/inactive than informal. Yet it can be interesting to consider also the unemployment/inactivity status for LAC. This is undertaken in Bargain and Silva (2024), who examine transitions between formal employment, informal employment and unemployment driven by long-term wage changes. Our focus on transitions across sector for those in work is a reasonable simplification in the context of *poorer* LAC countries, where unemployment is less frequent. Before the pandemic, unemployment rates were as low as 5.6% in Bolivia and 3.7% in Ecuador (SEDLAC 2023). Contributory unemployment insurance systems are also rare in LAC countries and exist essentially in richer countries (Argentina, Brazil, Uruguay) and Venezuela, as discussed in Jara (2018).

[21] Despite this attempt, the comparability may be limited, also because very different methodologies are used (simple estimations, natural experiments, etc.), which would necessarily affect the magnitude of the elasticities inferred from these different studies. For instance, some studies report only changes to formal employment

formalization elasticities are plotted against formal employment rates in Figure 5. Given the broader scope of the literature on informality and fiscal disincentives, we also extend the comparison beyond LAC, including some studies on Eastern European countries characterized by fairly high rates of informal work. Overall, and despite the underlying differences in methods, it is reassuring that our estimates fall within the same order of magnitudes as those from these studies. In particular, Heckman and Pagès (2004) find an elasticity of -0.45 for LAC countries on average, which is close to our pooled estimate. Our elasticities are also close to past estimates for Colombia, which is probably the closest benchmark we can find (in particular Kugler and Kugler, 2009, and Bernal et al., 2017).

Figure 5: Formal Employment Elasticities in the Literature

Source: Author's elaboration based on Cruces et al. (2010) for Argentina 2010, Mondino and Montoya (2004) for Argentina 1990, De Barros and Corseuil (2004) for Brazil 1988, Edwards and Edwards. (2000) for Chile 1981, Gruber (1995) for Chile in 1977, Kugler and Kugler (2009) for Colombia 1993, Bernal et al. (2017) for Colombia 2012, Saavedra and Torero (2004) for Peru 1987 Heckman and Pagés (2004) for average LAC 2014, Gray et al. (2007) for Eastern European countries 2007, Betcherman and Pagés (2007) for Turkey 2000, Slonimczyk (2011) for Russia 2001.

Sub-period estimations. Admittedly, our estimations benefits from three periods for each country, hence using in principle more variation than usually provided in quasi-experiments focusing on a single reform (or set of reforms). The idea was indeed to see how *cumulated* routine reforms might generate equally disincentive effects to formal work. It is nonetheless interesting to check if policy changes during sub-periods were also enough to generate

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following specific reforms (e.g. a change in payroll tax rates), in which case we calculate the corresponding elasticity based on the change in taxes or social insurance contributions and the level of formal employment in the period of analysis. The authors can provide details of the adjustments made for each study. Note that when studies examine specific reforms and report an insignificant effect, this mechanically leads to zero elasticities in our comparison (for instance Cruces et al. 2010, for Argentina, or Gruber 1995, for Chile).

significant responses. To do so, we replicate our estimations on pairs of years using FTRs based on fixed earnings (i.e. calculated as the group mean earnings over the two years). Table 2 presents the results of the separate estimations for 2008-2014/15 and 2014/15-2019. Despite a focus on sub-periods characterized by more limited sets of reforms, results are remarkable: there is still a quasi-systematic negative relationship between changes in formal employment and changes in FTRs, i.e. estimates happen to be statistically significant in a majority of cases.

Table 2: Formal employment elasticities from sub-period estimations

	Subperiod 2008-2014/15			Subperiod 2014/15-19		
	Formal empl. rate (%)	Mean FTR (%)	FTR-Elasticities	Formal empl. rate (%)	Mean FTR (%)	FTR-Elasticities
Bolivia	21.3	8.5	-0.34 (0.23)	26.0	9.0	-0.53 ** (0.26)
Colombia	39.7	12.7	-0.23 ** (0.09)	43.2	12.4	-0.33 *** (0.12)
Ecuador	40.6	8.1	-0.55 *** (0.12)	46.2	9.2	-0.01 (0.15)

Note: The table reports formal employment elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs), controlling for time dummies, group fixed effects and controls (percentage of married people in the group, average household size, unconditional benefit levels, i.e. benefits unrelated to formal/informal sector status). FTRs capture the mean change in disposable income from moving from informal to formal sector (here calculated using fixed earnings, calculated as group means over pairs of year). Standard errors indicated in parentheses and significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For each country x pair of years, n=96 (2 years x 48 groupes).

This detailed set of estimates can be interpreted according to the more specific nature of policy changes at each sub-period. We also notice the reduced power of these estimations due to a drop of a third in sample size. Regarding *Bolivia*, the FTR-elasticity is not significant in the first sub-period but the magnitude of the elasticity is actually similar to the previous estimate obtained over two periods. It is noticeable that estimates were already less precise for this country, due to small sample size (see Table A1), so that a slight reduction in power leads to this result. For *Colombia*, the estimated FTR-elasticities of formal employment are consistently negative and significant in the two sub-periods of analysis and the magnitude remains broadly the same. In *Ecuador*, the FTR-elasticity is large and significant in the first period (-0.55), but close to nil in the second sub-period, which was in fact characterized by limited changes in tax-benefit policies. As seen before, they consisted mainly of a small increase in the minimum wage and no change in social contributions, which highly contrasts with the substantial increase in social contributions during the first sub-period. Additionally, since 2014, the drop in commodity prices (on which Ecuador is dependent) has worsen labor market conditions and possibly increased segmentation (see Bargain et al. 2023), so that the responsiveness of formal employment to fiscal pressure would be more difficult to identify. Our results,

however, show that under more normal circumstances, as in the previous sub-period, modest variation in tax-benefit policies in Ecuador could generate significant formal employment responses. Overall, this analysis based on a thinner time decomposition does not fundamentally change our conclusions.

Benefit-elasticities of formal employment. Our analysis also provides results on the effect of cash transfer programs on formal employment. As already discussed, these benefits are unconditional on the formality status of the person.[22] They would therefore act through an income effect that may reduce the incentives to work formally. Table 3 shows that with FTRs calculated using fixed earnings, the benefit-elasticity of formal employment is indeed negative. Yet it is rarely significant, which is a relatively usual finding in the broader labor supply literature (Blundell et al., 1998, Bargain et al., 2014) and for LAC countries in particular (Bargain and Silva, 2023).

Table 3: Formal employment response to benefits (grouped estimations)

	Formal empl. rate (%)	Mean benefit (in % of household income)	Benefit-Elasticities		
			(1)	(2)	(3)
Bolivia	23.8	2.0	-0.239 ***	-0.012	-0.012
			(0.05)	(0.02)	(0.02)
Colombia	41.3	0.9	0.070	-0.028 *	-0.028
			(0.05)	(0.02)	(0.02)
Ecuador	41.5	2.1	-0.10 **	0.003	0.003
			(0.04)	(0.02)	(0.02)
Pooled	35.5	1.7	-0.05 ***	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Group FE (gender x age x education x location)			NO	YES	YES
Year FE (Country x Year in the Pooled model)			YES	YES	YES
Robust S.E.			NO	NO	YES

Note: The table first report formal employment rates and mean benefit (in % of mean household income), then it shows formal employment elasticities of benefits, derived from grouped estimations of formal employment on unconditional benefit levels (i.e. benefits unrelated to formal/informal sector status, in PPP USD 2019) and controlling for FTRs (calculated using fixed earnings), time dummies, group fixed effects as indicated, and controls (percentage of married people in the group, average household size). Standard errors indicated in parentheses and significance levels: *** p<0.01, ** p<0.05, * p<0.1. For all countries, n=144 (3 years x 48 groupes), and for pooled countries estimations, n=432.

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[22] Some papers find traces of a disincentive to stay formally employed when cash transfers do not depend on (formal) earnings (see de Holanda Barbosa and Corseuil, 2014, or Brauw et al., 2015, for conditional cash transfers in Brazil, and Garganta and Gasparini 2015 for universal child benefits in Argentina). Non-contributory programs seem to show a negative impact on formal employment through the implicit tax stemming from entering formal work (Bergolo and Cruces 2021). A related literature examines universal healthcare, showing there is less necessity to hold a formal employment in this case (evidence for Seguro Popular in Mexico is provided by Azuara and Marinescu 2013, Juarez 2009, Aterido et al. 2011, Campos-Vazquez & Knox 2013, Bosch and Campos-Vazquez 2014; for social protection in Colombia, see Camacho et al. 2013). A related literature describes the reverse situations where access to social insurance for workers and their families is tied to formal jobs (Levy, 2008; Levy and Schady, 2013) and when social security conditional on registration encourages formal work (see Bergolo and Cruces, 2014, for Uruguay).

4.2 Sensitivity Checks

We now present a series of robustness checks that correspond to the usual questions surrounding grouped estimations or to more specific points regarding our empirical strategy.

Accounting for earnings dynamics. Calculating FTRs on the basis of fixed earnings allowed us to address the possibly confounding effects of earnings change over time and the way they could compound with tax policy changes. Yet, our empirical model may be seen as incompletely specified, since group earnings variation also matters for transitions across sectors. Thus, we additionally control for earnings dynamics in the model, using three alternative methods described below Table A3, which document the results. Essentially, our conclusions are preserved with this augmented model.

Small sample bias. So far, we have not imposed any restrictions on the groups created for grouped estimations. A usual check, however, concerns the risk of small sample bias in our estimators (Deaton 1985) when grouping observations. The number of observations per groups, i.e. its mean and distribution, is reported in the last column of Table A1. There are large differences across countries. In particular, the small sample size (and cell size) in Bolivia certainly explains the less precise estimates obtained for that country. Nonetheless, we seem to have a reasonable number of observations per group compared to the benchmark of past studies.[23] Also, we use the recommendations of Devereux (2004) and rerun estimations while implementing alternative restrictions on the groups. Table A4 presents the results for three sets of restrictions. Comments below the table convey that our conclusions are robust to trimming overall and cell exclusions when formal workers are under-represented.

Migration. Our results could be biased if the composition of groups changed over the whole period. The latter is too short for significant upheavals to take place in the age or education structure. However, one of the criteria for group construction was the urban versus rural location, which may be a potential source of compositional change if migration is important. To mitigate this concern, or at least check if it alters our estimates significantly, we restrict the sample to cases in which households declare to have been stable for at least the past five years. Estimates are reported in Table A5 together with changes in sample size. There are few changes to the results, only slightly smaller estimates when movers are omitted, which is consistent with similar checks and findings in Devereux (2003).

Age versus cohort. In our baseline, one of the dimensions to build groups is age. Following the same age categories over time makes sense, all the more so as this is an important factor explaining informality. However, there might be cohort effects that are not taken into account (for instance related to shocks affecting individual attachment to the labor market, education

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

quality, etc.). As Devereux (2003), we suggest an alternative, symmetrical grouping whereby cohort groups are used in place of age groups. It actually makes the pseudo-panel more similar to a true panel, as we follow the same generations over time (the 'panel' is ageing). Results, reported in Table A4, are again not fundamentally different from the baseline.

4.3 Heterogeneity

The analysis has focused on formal employment elasticities for the population as a whole. However, there might be variation in responses to net tax pressure across certain population subgroups. In particular, much of the policy changes is related to increases in social security contribution floors at low earnings levels, which might affect self-employed and salary workers differently. Also, the literature often documents gender differential in labor supply elasticities while, arguably, we might not be expected this type of difference when it comes to individuals in work.

Employees versus self-employed. Salary workers and self-employed are in relatively balanced proportion in the three countries (employees represent around half of the pooled sample, 48% in Bolivia, 54% in Colombia, 64% in Ecuador, see Table A5, column 1). However, their nature is very different. Self-employed workers are very heterogeneous, from poor street vendors to entrepreneurs.[24] As expected, they are rarely declared compared to salary workers (13% versus 55% on average, cf. columns 2-3 for country variation). For this reason, we want to check if there are potential behavioral responses especially on behalf of salary workers, who represent the bulk of formal work and hence the greater margin of potential disincentive effects of policy changes. We carry out estimations while identifying the role of FTR changes among both subpopulation. Results are reported in Table A5 (columns 5-6). It turns out that compared to the baseline (column 4), FTR-elasticities for salary workers are smaller in some cases (Bolivia, Ecuador) but are still very significant. Elasticities for self-employed are much less precisely estimated, possibly because of the great heterogeneity composing that population.[25]

Men versus women. Another interesting angle concerns gender heterogeneity in formal employment responses. Informal employment is often deemed to be more prevalent among women (IDB 2023, ILO 2023), as they face greater barriers to formal employment opportunities. Women might also react to tax pressure by moving out of paid work, rather than from formal to informal paid work, as characterized in Bargain and Silva (2023). Finally, the more standard literature on labor supply elasticities points to larger responses by women,

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[24] See for instance Yamada (1996) and Saavedra and Chong (1999) for Peru, Maloney (1999, 2004) for Mexico, Pratap and Quintin (2006) for Argentina and Marcouiller et al. (1997) for several LAC countries.

[25] Self-employed may be expected to be less responsive given great risks of labor market segmentation in their case. Yet, as documented, they are disproportionately represented in the lower and middle parts of the distribution (see Figure A1), with below-minimum wage earnings, so that increases in social contribution floors may further discourage their formalization.

in particular married women (Blundell et al., 1998, Bargain et al. 2013), precisely because of the participation margin, which generates more responses than for men. However, when focusing on adults in paid work, it is not sure that we might expect so strong gender differences. In our context, women and men share similar rates of formal work (36.5% for women versus 35% for men in the pooled sample), which means that women are not necessarily rationed out of formal work more than men. Women also tend to face similar tax pressure to men (mean FTRs are 9.2% for women versus 10.5% for men in the pooled sample). Beyond this cross-sectional similarity, we also see in Table A5 (columns 7-8) that there is no substantial differences in responses to policy changes. Elasticities are of similar magnitude for men and women overall and in each country (except Bolivia, but as signaled before, smaller sample sizes in this country makes estimates much less precise). This result is in relative contrast to the gender asymmetries that exist in other domains of the literature on labor supply, as recalled above, and is important because it shows that tax disincentives are generalized, i.e. not concentrated on a particular segment of the population.

5. Conclusion

To elicit the role of tax-benefit reforms on labor market behavior, most quasi-experiments focus on major reforms. Instead, we ask whether routine changes in the tax burden allow detecting associated behavioral responses. We focus on movements between formal and informal sectors in Latin America. To gain external validity and generate sources of variation, we mobilize data for three periods and three countries, Bolivia, Colombia and Ecuador. For each of the nine datasets, we suggest a precise measure of the fiscal cost of being in the formal sector by coding detailed simulations of the nine actual tax-benefit systems and by producing a counterfactual *formalization tax rate* (FTR), i.e. the change in household disposable income if a person moves from informal to formal employment, whatever her actual status. FTRs are grouped at the level of cells defined according to gender, age, education and location, so that grouped estimation techniques can be used to exploit time and spatial variation in tax incentives. Results point to quasi-systematic disincentives to formal-sector taxation.

Our analysis is robust to a variety of specification checks: freezing earnings to extract the pure policy effect (as in the Elasticity of Taxable Income literature), but also controlling separately for earnings dynamics; conducting sub-period analyses; checking for potential small sample bias or the role of migration and cohort effects. We find that ordinary reforms affect the formalization rates of different subgroups, including salary workers (who represent the bulk of formal work) and men and women almost equally. This means that due to their scope (reaching different population subgroups) and frequency, routine tax and contribution changes can have a cumulated detrimental effect on the tax base – as much as large occasional reforms – and should be incorporated more systematically in the 'incentive-compatibility constraint' of tax design. These policy implications are all the more critical as LAC countries must attempt to expand fiscal systems to improve capacities and redistribution.

Further work should precisely try to incorporate the FTR-elasticities of formalization in relevant tax reform simulations, for the calibration of optimal tax models in presence of informality (Boadway and Sato 2009, Besley and Persson 2013) or for the calibration of CGE models in contexts with informality (Auriol and and Warlters, 2009, Elgin and Toruly 2018). Also, our work was a first attempt to characterize 'usual' reforms with an international and intertemporal sample, but future projects should aim at a much larger scope than what could be proposed. Covering more countries and periods would help not only to corroborate results but also to perform heterogeneity analyses regarding the nature and impact of various policy instruments. For instance, does the same change in FTR driven by social contribution rather than tax generates more responses due to differences in perception (regarding the instrument) or in affected populations, etc.? This is indeed crucial to better explore contexts in which tax policy changes particularly discourage formal employment.[26]

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[26] Nonetheless, while a broader set of countries would be ideal, the effort to code tax-benefit systems for many years and countries is substantial. In particular, we leave the addition of richer regions such as Chile, Brazil, Argentina and Uruguay for future research. This will be a different analysis altogether, since these richer countries have more progressive tax systems so tax reforms may concern more intensively the upper part of the distribution, while the reforms we study mostly characterized the lower parts, with changes in social contributions in particular.

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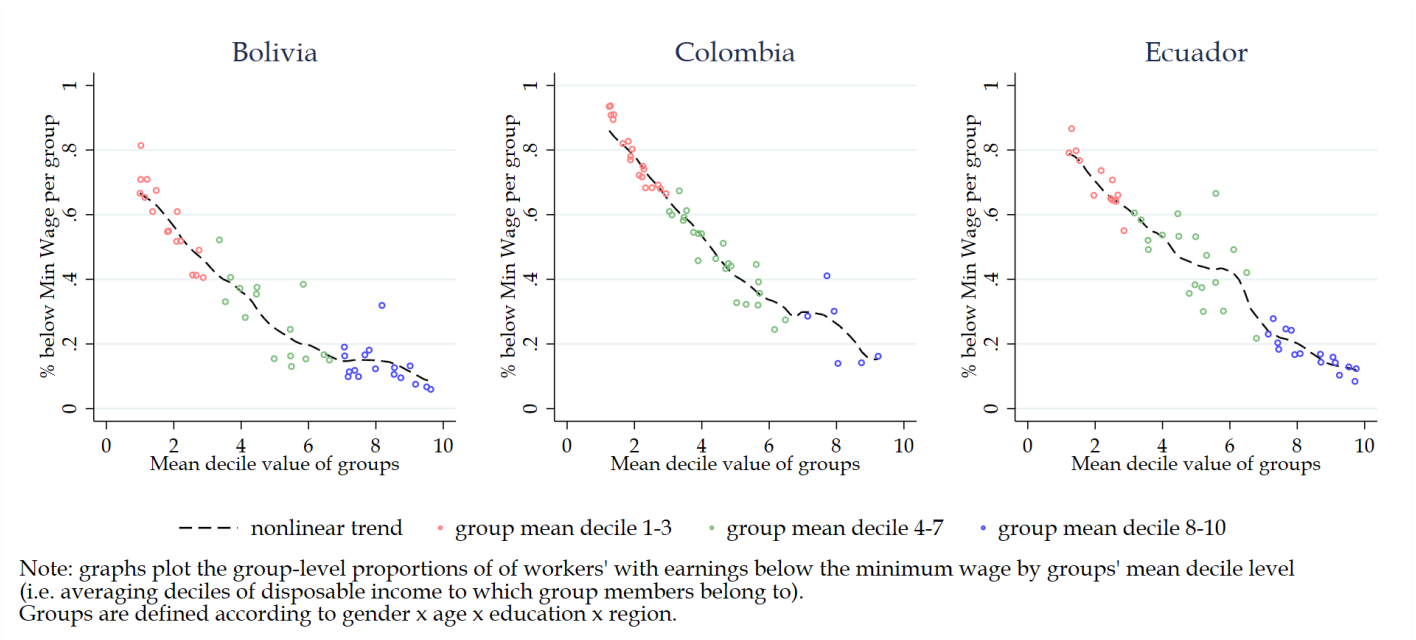
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Appendix 1: Graphs

Figure A.1: Share of workers below the minimum wage by mean decile of each group

(a) All workers
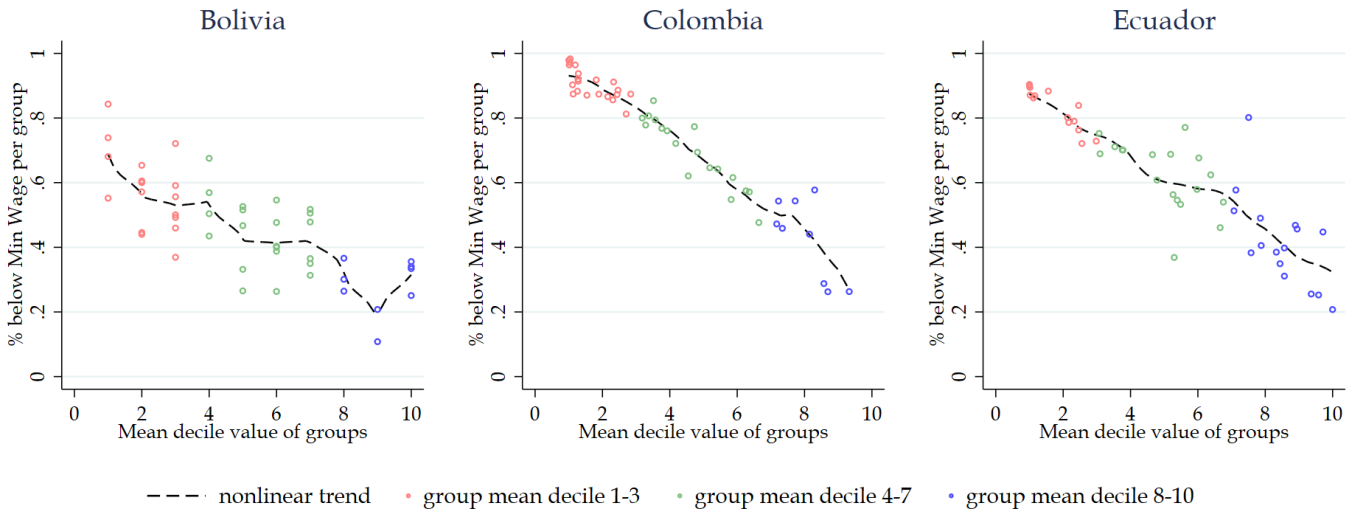
Note: graphs plot the group-level proportions of of workers' with earnings below the minimum wage by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to).
Groups are defined according to gender x age x education x region.

(b) Employees

Note: graphs plot the group-level proportions of of workers' with earnings below the minimum wage by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to).
Groups are defined according to gender x age x education x region.

(c) Self-employed –

Note: graphs plot the group-level proportions of of workers' with earnings below the minimum wage by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to).
Groups are defined according to gender x age x education x region.

Figure A.2: Share of self-employed by mean decile of each group
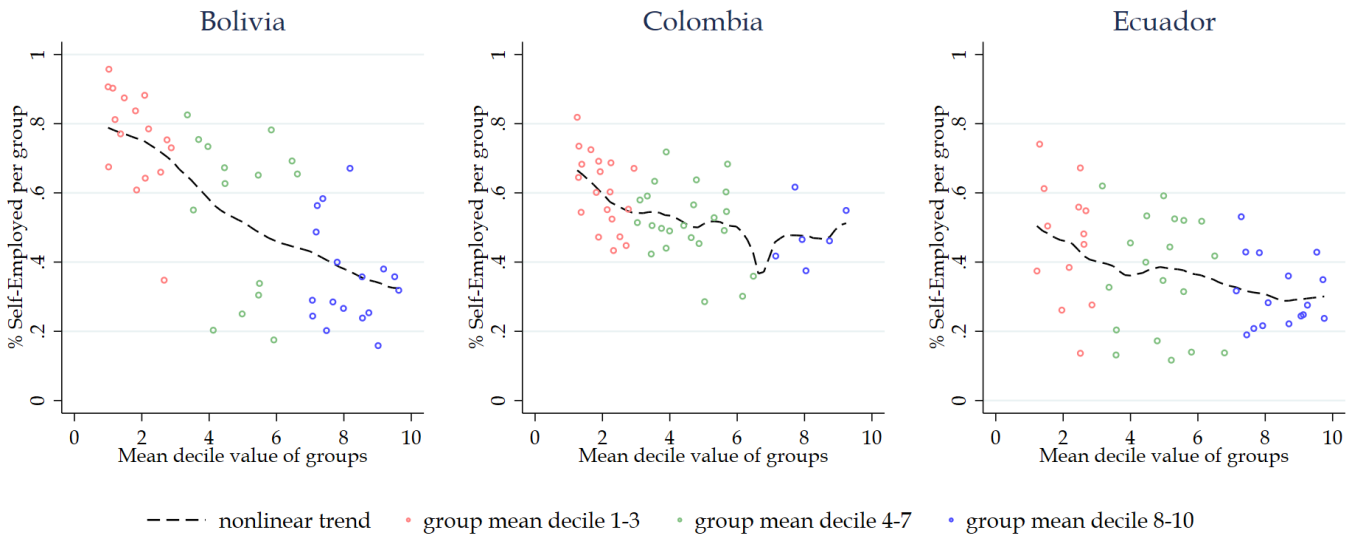
Note: graphs plot the group-level proportions of self-employed by groups' mean decile level (i.e. averaging deciles of disposable income to which group members belong to). Groups are defined according to gender x age x education x region.

Figure A.3: Kernel Earnings Density of Workers' Earnings
by formality status and type employment
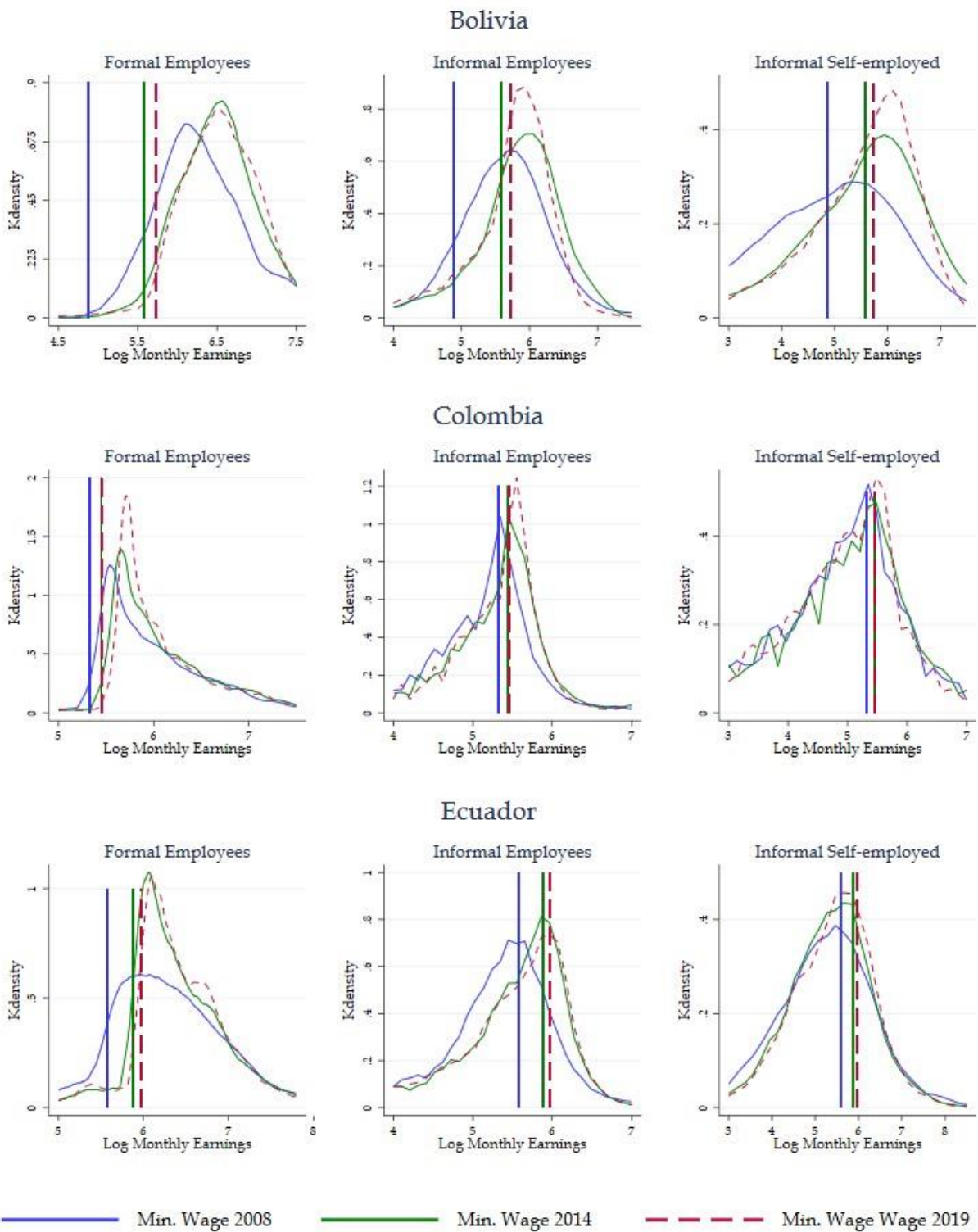
Note: graphs represent the distribution of log earnings.
All earnings have been uprated to 2019 levels for comparability.

Appendix 2: Descriptive Tables

Table A.1: 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	10 671	222
Colombia	Gran Encuesta Integrada de Hogares (GEIH)	COLMOD	2008, 2014, 2019	299 298	6 235
Ecuador	Encuesta Nacional de Empleo, Desempleo y Subempleo (ENEMDU)	ECUAMOD	2008, 2014, 2019	28 565	595

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. See the country reports for Bolivia (Arancibia and Macas 2023), Colombia (Rodriguez et al. 2023) and Ecuador (Jara et al. 2023b) for more information.

(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.

Note on tax-benefit microsimulation. Market incomes and sociodemographic characteristics from the microdata are used as input to calculate taxes and benefits for each household. Market income is defined as the sum of employment and self-employment earnings, capital and property income, and other relevant resources for tax calculations. Imputed rent is not included as part of market income. Tax-benefit calculations follow as much as possible the rules of each instrument as stipulated in the national legislation (see the Country Reports in Arancibia and Macas 2023, Rodríguez et al. 2023, Jara et al. 2023b). The tax-benefit calculators have been originally developed as part of the SOUTHMOD project (Decoster et al. 2019) and implemented using the EUROMOD software to enable comparative cross-country analysis.[27] As explained in the main text, we calculate FTRs by comparing a person's household disposable income if she is formally employed to her household disposable income if she is informal. In the former (latter) situation, we switch the simulation of the taxes and social contributions paid on (off). All tax-benefit instruments in force in each country x period are simulated, except contributory pensions for which we use information in the data directly (pensions cannot be simulated because of the lack of information about contribution history in the surveys). All the reforms exploited in our empirical work and described in the evolution of tax-benefit systems in Table A2 below are considered in our simulations.

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[27] The models for Bolivia, Colombia and Ecuador are developed, maintained and managed by UNU-WIDER in collaboration with SASPRI (Southern African Social Policy Research Insights), the International Inequalities Institute at the London School of Economics and Political Science, and local partners in selected developing countries in the scope of the SOUTHMOD project. The models are freely available for non-commercial research use and their results have been validated against external statistics. 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

Table A.2: Characteristics of tax-benefit systems in the countries under study, 2008-2019

Instrument	Parameter	Bolivia 2008	Bolivia 2015	Bolivia 2019	Colombia 2008	Colombia 2014	Colombia 2019	Ecuador 2008	Ecuador 2014	Ecuador 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*:	131.3	244.2	307.1	209.3	236.4	238.2	264.8	357.7	394
	in % of average labor income* **	34%	56%	68%	62%	62%	64%	63%	73%	87%
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
	high tax band (htb)	-	-	-	41,000	43,255	313,370	105,912	111,754	115,290
	low tax rate	13% (flat)	13% (flat)	13% (flat)	0%	0%	0%	0%	0%	0%
	high 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 old-age pension	income test	*Renta Dignidad*: universal elderly above 60			-	*Colombia Mayor*: proxy means-test		*Bono de Desarrollo Humano*: proxy means-test		
	max amount (per month)*	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.

* All monetary parameters are expressed in USD 2019 PPP.

** Average labor income comes from SEDLAC (CEDLAS and The World Bank) downloaded march 2024

Appendix 3: Sensitivity Analyses, Accounting for Earning Dynamics

Table A3: Formal employment elasticities when accounting for earnings dynamics

	Mean	FTR-Elasticities		
		(1)	(2)	(3)
Bolivia				
FTR "fixed earnings"	8.7	-0.40 *	-0.43 *	-0.45 **
		(0.21)	(0.21)	(0.21)
(x1-x0)/x0	0.2	0.03		
		(0.04)		
x1-x0 (PPP)	119.7		0.004	
			(0.01)	
FTR "fixed policy"	7.2			-0.02
				(0.04)
Colombia				
FTR "fixed earnings"	12.5	-0.25 *	-0.25 *	-0.23 **
		(0.07)	(0.09)	(0.08)
(x1-x0)/x0	0.7	0.14		
		(0.10)		
x1-x0 (PPP)	164.5		-0.06	
			(0.06)	
FTR "fixed policy"	4.5			0.02
				(0.01)
Ecuador				
FTR "fixed earnings"	8.6	-0.37 ***	-0.43 ***	-0.29 ***
		(0.11)	(0.10)	(0.11)
(x1-x0)/x0	0.4	0.06		
		(0.08)		
x1-x0 (PPP)	125.0		0.08	
			(0.08)	
FTR "fixed policy"	5.3			0.02
				(0.02)
Pooled				
FTR "fixed earnings"	10.0	-0.34 ***	-0.36 ***	-0.32 ***
		(0.09)	(0.09)	(0.08)
(x1-x0)/x0	0.4	0.06		
		(0.05)		
x1-x0 (PPP)	136.4		0.02	
			(0.02)	
FTR "fixed policy"	5.7			0.02
				(0.01)

Note: The table reports formal employment elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs), controlling for time dummies, group fixed effects, controls (percentage of married people, average household size), unconditional benefit levels (i.e. unrelated to sector) and a measure of earnings differential between formal and informal sector. FTRs capture the mean change in disposable income from moving from informal to formal sector. Robust standard errors indicated in parentheses and significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For all countries, n=144 (3 years x 48 groupes), and for pooled countries estimations, n=432. Earnings differential between sectors are of three types:

- (x1-x0)/x0 : relative difference in (group-averaged) predicted earnings between sectors
- x1-x0 : difference in (group-averaged) predicted earnings between sectors, in PPP terms (2019 USD).
- FTR "fixed policy": formalization tax rate when holding tax-benefit policies constant (fixed to the last year)

Note on earnings dynamics. To account for earnings dynamics, we suggest a preliminary step, namely the estimation of sector-specific earnings equations at the individual level and the prediction, for each person in our sample, of earnings in formal and informal status.[28] Then we propose three ways to account for the evolution of earnings differentials across sectors in the model. *First*, we average predicted earnings at the group level, distinguishing group-mean earnings in formal and informal employment, denoted $\hat{X}_{1_{gt}}$ and $\hat{X}_{0_{gt}}$ respectively. We add to the model the relative earnings differential $(\hat{X}_{1_{gt}} - \hat{X}_{0_{gt}})/\hat{X}_{0_{gt}}$. *Second*, we proceed in the same way but include the differential earnings in levels, i.e. $\hat{X}_{1_{gt}} - \hat{X}_{0_{gt}}$. *Third*, we opt for a measure of earnings differentials across sectors that is expressed in terms of disposable incomes, i.e. incomes net of taxes and social contribution. The logic is similar to that of FTR with fixed earnings: this measure aims to capture the change in household disposable income from entering formal employment, but holding the tax-benefit parameters constant over time. Formally, this term is written:

$$\tilde{\tilde{T}}_{gt} = \sum_{i \in g} \bar{\bar{T}}_{it}(\hat{X}_{itY_i}, Z_{it})/\sum_{i \in g} n_i \text{ , with } \bar{\bar{T}}_{it}(\hat{X}_{itY_i}, Z_{it}) = -\frac{\bar{D}_{i1}(\hat{X}_{it1}, Z_{it}) - \bar{D}_{i0}(\hat{X}_{it0}, Z_{it})}{\bar{D}_{i0}(\hat{X}_{it0}, Z_{it})},$$

with $\bar{D}_{is}$ referring to the disposable income obtained when applying to any earnings the tax-benefit rules of the last year for a person in the formal or informal sector (*s=1 or 0*).[29] This term reflects the potential increase in earnings when moving to the formal sector but net of the taxes and contributions one has to pay in this case. Its sign is therefore undetermined. Over time, this FTR "constant tax policy" captures how the changes in relative earnings affect labor allocation across sectors while our previous FTR "fixed earnings" captures the pure policy effect. Results are reported in Table A3. The table first shows the mean values of the key variables, i.e. FTRs and the three alternative variables capturing earnings dynamics, followed by the corresponding estimates. Coefficients on earnings differentials are never significant.[30] Most importantly, when accounting for them, FTR-elasticities of formal employment remain significant and similar to baseline.

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[28] We use regressions that are specific to the sector (formal or informal), by gender and by period, in order to obtain the most accurate predictions. Estimations include region, education dummies and a cubic form of age as key determinants. We refrain from using group dummies instead, as it would boil down to simply use cell-mean wage for formal or informal workers within each cell, which is not a good predictor of, say, potential informal wages for formal workers, if there are relatively few informal workers in that particular cell. Detailed wage regressions are available upon request.

[29] We use the 2019 system and apply it to the data of the previous years. Doing so, we must nominally adjust the *monetary* tax-benefit parameters of the year 2014/15 and 2018 to reflect differences in cost of living. Otherwise, we would for instance have 2019 earnings applied to the tax schedule of 2008 whereby tax bands are lower, hence distorting the taxes paid in this early year (see detailed explanation of the issue of `bracket creep' in Bargain and Callan 2010).

[30] We expect these coefficients to be positive, as they should reflect the impact of the relative progression of formal sector wages on formalization

Appendix 4: Additional Sensitivity Checks

Table A.4: FTR-elasticities of formal employment, sensitivity checks

	Baseline	Small sample checks			Dropping geographical movers	Groups defined using cohort rather than age
		trim 5% of smallest cells	trim 5% smallest formal and n<15	5% smallest formal and 5% smallest cells		
		(1)	(2)	(3)	(4)	(5)
Bolivia	-0.44 **	-0.42 *	-0.50 *	-0.50 *	-0.42 **	-0.46 **
	(0.21)	(0.21)	(0.27)	(0.27)	(0.19)	(0.19)
		Reduction in number of cells			Reduction in sample size	
		-5%	-10%	-10%	-4%	-18%
Colombia	-0.25 ***	-0.26 ***	-0.26 ***	-0.27 ***	-0.31 ***	-0.19 ***
	(0.08)	(0.08)	(0.09)	(0.09)	(0.10)	(0.06)
		Reduction in number of cells			Reduction in sample size	
		-5%	-10%	-10%	-25%	-17%
Ecuador	-0.38 ***	-0.39 ***	-0.39 ***	-0.42 ***	-0.40 ***	-0.25 ***
	(0.10)	(0.11)	(0.11)	(0.11)	(0.10)	(0.09)
		Reduction in number of cells			Reduction in sample size	
		-5%	-10%	-10%	-3%	-17%
Pooled	-0.38 ***	-0.36 ***	-0.39 ***	-0.39 ***	-0.36 ***	-0.28 ***
	(0.09)	(0.09)	(0.10)	(0.10)	(0.08)	(0.07)
		Reduction in number of cells			Reduction in sample size	
		-5%	-7%	-10%	-11%	-17%

Note: The table reports formal employment elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs), controlling for time dummies, group fixed effects and controls (percentage of married people in the group, average household size, unconditional benefit levels, i.e. benefits unrelated to formal/informal sector status). FTRs capture the mean change in disposable income from moving from informal to formal sector (here calculated using fixed earnings, calculated as group means over pairs of year). Robust standard errors indicated in parentheses and significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Sensitivity checks go as follows: (1) Small sample issues: we drop the 5% smallest cells in each country; (2) Small sample issues: we drop the 5% cells with the smallest number of formal workers plus, in the remaining, cells with less than 15 observations in total; (3) Small sample issues: we drop the 5% cells with the smallest number of formal workers plus, in the remaining, the 5% smallest cells; (4) Group definition: we keep households that declare being geographically stable over the past 5 years; (5) Group definitio: we replace age by cohort in group design, using 4 cohort groups (born 1978-80, born 1968-78, born 1958-68, born 1949-58).

Note on small sample bias. Following Devereux (2005), we proceed with alternative trimming of our pseudo-panel. Results are presented in Table A4. Column (1) provides results where drop the 5% smallest groups in each country, which 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 ensure that we have a sufficient number of observations for formal and informal workers in each cell, in particular, some groups might have few formal observations. Therefore, column (2) show estimates on sample where we drop the 5% groups with the smallest number of formal workers and, in addition, those with less than 15 observations. Finally, column (3) presents results where we drop the

5% smallest number of formal workers and, in the remaining groups, the 5% smallest groups in each country. The results show that the estimated FTR-elasticities of formal employment are broadly robust to the exclusion of small groups and groups with a small relative proportion of formal workers.

Appendix 5: Heterogeneous Estimates

Table A.5: FTR-elasticities of formal employment, heterogeneity

	% self-employed	% formal among self-employed	% formal among employees	FTR-Elasticities				
				Baseline	Self-employed	Employees	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bolivia	0.52	0.05	0.45	-0.44 **	-0.94 *	-0.16 **	-0.26 *	-0.57 **
				(0.21)	(0.53)	(0.08)	(0.15)	(0.29)
Colombia	0.46	0.13	0.66	-0.25 ***	-0.46	-0.30 ***	-0.30 ***	-0.22 **
				(0.08)	(0.49)	(0.09)	(0.11)	(0.10)
Ecuador	0.36	0.20	0.54	-0.38 ***	-0.15	-0.19 ***	-0.39 ***	-0.31 ***
				(0.10)	(0.18)	(0.06)	(0.11)	(0.10)
Pooled	0.45	0.13	0.55	-0.38 ***	-0.40 **	-0.12 ***	-0.35 ***	-0.35 ***
				(0.09)	(0.20)	(0.03)	(0.10)	(0.11)

Note: The table reports formal employment elasticities derived from grouped estimations of formal employment on formalization tax rates (FTRs), controlling for time dummies, group fixed effects and controls (percentage of married people in the group, average household size, unconditional benefit levels, i.e. benefits unrelated to formal/informal sector status). FTRs capture the mean change in disposable income from moving from informal to formal sector (here calculated using fixed earnings, as group means over the 3 year). Robust standard errors indicated in parentheses and significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. For all countries, n=144 (3 years x 48 groupes), and for pooled countries estimations, n=432.