The income and labor effects of individual income tax changes in Latin America: Evidence from a new measure of tax shocks *

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Abstract

What is the effect of individual income tax changes on aggregate income? Short-term elasticity of taxable income (ETI) estimates for the United States range between 0.02 in Saez (2004) to 1.2 in Mertens and Montiel-Olea (2018). Little is known about the size of the ETI outside the United States. Why? The main reason is, of course, the absence of readily-available average marginal individual income tax rate ($AMIITR$) series, which mainly reflects the lack of access to administrative data on tax returns, and the individuals' reported income therein.

Our paper contributes to this empirical literature focusing on six large Latin American countries: Argentina, Brazil, Colombia, Ecuador, Paraguay, and Peru. On the measurement front, we propose a novel approach to build $AMIITR$ series relying on the statutory individual income tax code established in different laws, decrees, and regulations and, crucially, individuals' reported income in household survey datasets. When applying this method to our Latin American sample, the geographically disaggregated nature of household survey datasets allows us to build both national and regional $AMIITR$ series (the latter for 139 state-like areas). On the identification front, we contribute by capturing exogenous and unanticipated individual income tax policy changes. To this end, we assign a new narrative-based classification to each tax change (à la Romer and Romer, 2010) and then proceed to establish their anticipated nature.

Armed with proper exogenous and unanticipated $AMIITR$ shocks, we find short-term ETI estimates ranging around 2.5, and 3.5 at the regional and national levels, respectively, pointing to a much larger responsiveness than in the United States. Finally, we also study how individual income taxes distort labor decisions, on both on the extensive and the intensive margins, as well as on labor market informality. To the best of our knowledge, this is the first paper to provide empirical evidence of the effects of $AMIITR$ shocks on aggretate income and labor markets outside the developed world –in our case, for a sample of six large Latin American countries.

Keywords: Fiscal policy, tax, ETI, income tax, Latin America, narrative.
JEL Classification: E62, H24, H30

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1 Introduction

How big or small is the effect of individual income tax changes on aggregate income? Knowing the extent to which individual income taxes distort decisions to work and invest is essential to shape public policies. There is a growing empirical literature addressing these issues for the United States (e.g., Saez, 2004; Romer and Romer, 2014; Mertens and Montiel-Olea, 2018). A related macroeconomic literature, also focusing on the United States, estimates the impact of individual income tax changes on macroeconomic aggregates such as GDP, investment, and consumption (e.g., Romer and Romer, 2010; Barro and Redlick, 2011; Mertens and Ravn, 2012, 2013; Mertens and Montiel-Olea, 2018; Zidar, 2019). While these labor and macroeconomic studies focusing on the aggregate response at the country or state levels are relatively recent, there is also a long-standing microeconomic literature exploiting the individual-level response dating back to the 1980s.[1]

This U.S.-based literature studying the effect of individual income tax changes on aggregate income and labor decisions has made major advances including, crucially, the measurement of the average marginal individual income tax rate ($AMIITR$) and the proper identification of tax shocks.[2] On the measurement front, the availability of individuals' reported income in administrative data on tax returns available from the Internal Revenue Service (IRS) coupled with extensive knowledge of the statutory individual income tax code has allowed researchers to build $AMIITR$ time series estimates.[3,4] On the identification front, the literature has pushed the frontier emphasizing the importance of exogeneity and the control of anticipatory effects when studying the implications of tax policy shocks. The identification of exogenous tax policy shocks has received tremendous influence from the pioneer work of Romer and Romer (2010) –RR hereafter– which used the so-called narrative approach to classify legislated tax changes into exogenous or endogenous based on contemporaneous economic records. This literature has also highlighted the importance of properly identifying tax policy shocks that are not contaminated by anticipatory effects (e.g., Mertens and Ravn, 2012; Mertens and Montiel-Olea, 2018). Armed with proper exogenous and unanticipated $AMIITR$ shocks, several empirical studies estimate the so-called elasticity of taxable income (ETI) which measures the percentage change response of pretax incomes to a one percent net-of-tax rate increase in the $AMIITR$ (i.e., the net-of-tax rate is $1-$ $AMIITR$). While a positive ETI indicates that tax hikes (cuts) cause income to fall (increase), a negative ETI points that tax hikes (cuts)

[1] These micro studies estimate the effect of individual income tax changes on individual income and labor decisions –see Saez et al. (2012) for a literature review–, including the differential response depending on the level of income and sex (Heckman, 1983; Pencavel, 1986; Killingsworth and Heckman, 1986; Munnell, 1986; Blundell et al., 1998; Gooolsbee, 2000; Blau and Kahn, 2007; Keane, 2011).

[2] The $AMIITR$ for a country or region in a specific year is the arithmetic average of the marginal tax rates of each income bracket weighted by pretax income or adjusted gross income.

[3] Barro and Sahasakul (1983, 1986), relying on historical IRS publications, built $AMIITR$ series for the the federal individual income tax dating back to 1913, year in which the 16th Amendment was ratified permanently legalizing an income tax.

[4] Providing gross income data as input, tax code program available in NBER's TAXSIM constructs the $AMIITR$ measure since 1973.

make income to increase (fall). Short-term ETI estimates for the United States range between 0.02 in Saez (2004) and 1.2 in Mertens and Montiel-Olea (2018). While a 0.02 ETI estimate suggests that income is virtually not affected by changes in the tax rate, a 1.2 ETI estimate points to a more than proportional response of income to tax changes. In line with a growing regional multiplier literature, Zidar (2019) further uses the individuals' reported income in administrative data to exploit the large regional heterogeneity present across U.S. states.[5]

Interestingly, and in spite of the critical public policy relevance of this empirical evidence, little is known about the size of the ETI outside the United States. Why? The main reason is, of course, the absence of readily-available $AMIITR$ series which mainly reflects the lack of access to administrative data, and the individuals' reported income therein. Our paper contributes to this empirical literature focusing on six large Latin American countries: Argentina, Brazil, Colombia, Ecuador, Paraguay, and Peru. In particular, we contribute on three relevant fronts:

- As first contribution, we build new $AMIITR$ series for these countries typically starting between the mid-1990s and early 2000s. Given the lack of access to administrative data for this sample of Latin American countries, we propose a novel approach to build $AMIITR$ series relying on the statutory individual income tax code established in different laws, decrees, and regulations and, crucially, individuals' reported income in household survey datasets. As discuss in great detail later in the paper, the proposed strategy is convenient and allows us to maneuver important limitations that characterize administrative data in Latin America:

  - First, an important and recurrent challenge in this literature is related to the measurement of true individual income, especially at the top of the income distribution. There is a largely recognized underreporting problem when relying on administrative data, especially due to tax evasion and avoidance. The incentives individuals have to misreport their incomes to administrative agencies increase when the benefits are sufficiently large (e.g., to avoid tax payments) and/or when the cost of doing so is small.
    Having said that, income underreporting based on individuals' reported income in household surveys is *not* obviously more pronounced or systematic than when relying on administrative data. While it is true that the cost of hiding information is zero in a survey, it is less evident that individuals gain something from underreporting income in household surveys. Moreover, as discussed in Hurst et al. (2014), to the extent that individuals would have to exert effort to provide an accurate response to household surveys or feel compelled to maintain consistency in light of concerns about confidentiality, economic theory would suggest that they would continue to provide similar erroneous information

[5] See, for example, Shoag (2010), Ramey (2011), Suarez Serrato and Wingender (2011), Chodorow-Reich et al. (2012), Clemens and Miran (2012), Wilson (2012), Nakamura and Steinsson (2014), Suarez Serrato and Zidar (2016), and Chodorow-Reich (2019) for state-level spending multipliers.

as reported in other sources, even if there is no direct benefit of doing so. This rationale is consistent with comprehensive evidence that individuals' reported income in household surveys is also subject to underreporting both in developing and in advanced economies, especially at the top of the income distribution.

In fact, when using existing $AMIITR$ series for the United States based on administrative data –as in Saez (2004) and Mertens and Montiel-Olea (2018)– or, alternatively, when relying on our proposed novel approach based on individuals' reported income in United States household survey datasets (using the Annual Social and Economic Supplement) and the individual income tax code, we find very similar results (i) in terms of $AMIITR$ series levels and changes, and (ii) when looking at the $AMIITR$ series focusing on the entire distribution of income or on the top 1% and bottom 99% separately.

- Second, our proposed strategy also grants support –especially in our Latin American sample– considering the large share of nonfilers (i.e., individuals with positive incomes who, according to the contemporaneous legislation, are not required to fill tax returns).[6] The use of tax return data does not allow to identify nonfilers, precisely because only filers are expected to present their tax returns to the respective revenue collecting government agency. While the estimated share of nonfilers (expressed as a percentage of total filers and nonfilers) is currently low in the United States, representing about 10-15% (Saez, 2016), this figure represents about 75% in our Latin American sample. This large share of nonfilers in Latin America is similar to that prevalent in the United States until the early 1940s, as discussed in Barro and Sahasakul (1983).
- Third, our proposed strategy also grants support taking into account the prevalence of labor market informality in Latin America (and more generally in the developing world). In informality, individuals engage in productive activities that are not taxed or registered by the government and, consequently, are by construction not part of any administrative data. This group mainly includes include informal-salaried workers and non-professional self-employed individuals. While the estimated share of labor market informality is moderate for international standards in the United States, representing about 18% in 2013 (ILO, 2018), this group represents about 57% in our Latin American sample.
- Fourth, and very importantly for our proposed approach, household survey datasets provide relevant individual and household information to estimate exemptions and deductions.

As in Saez (2004), our $AMIITR$ series does not include social security contributions. Unlike

[6] This typically occurs because the individual's adjusted gross income (i.e., individual's gross income after considering lawful exemptions and deductions) falls below the filing threshold.

the United States, the individual income tax is solely legislated at the federal level in our Latin American sample and does not emanate from state-like or local jurisdictions. The geographically disaggregated nature of household survey datasets in each Latin American country allows us to count with a total of 139 state-like regions. This rich spatial heterogeneity allows us to create, in addition to series of $AMIITR$ for each country as a whole, series of $AMIITR$ for each regional unit.

- As a second contribution, we identify exogenous and unanticipated individual income tax policy changes for our sample of six Latin American countries. To identify exogenous individual income legislated tax changes, we build a new narrative-based classification using laws or decrees, policymakers' speeches, countries' news articles, and contemporaneous International Monetary Fund documents. While closely following RR's identification strategy, we also incorporate some new elements that arise due to both the developing nature of our sample and the specific tax considered. Based on this approach, more than 75% of individual income tax policy changes are identified as exogenous. Then, to distinguish unanticipated from anticipated legislated tax changes we proceed as in, for example, RR, Mertens and Ravn (2012), and Gunter et al. (2021) using the dates of the promulgation of the law and its implementation as critical information to determine the anticipatory nature of each tax change. Based on this strategy, more than 75% of exogenous legislated tax changes are also unanticipated. Therefore, out of all legislated tax changes around 55% are considered to be proper $AMIITR$ shocks.

- Last, as third contribution, armed with proper exogenous and unanticipated $AMIITR$ shocks, we estimate the ETI for our Latin American sample using diverse specifications and conducting several robustness exercises exploiting both the region- and country-level heterogeneity. Short-term regional and country-based ETI estimates range around 2.5 and 3.5, respectively, pointing to a much larger responsiveness than in the United States. We also find that increases (cuts) in individual income taxes reduce (increase) the willingness to work, on both the extensive and intensive margins, and increase (reduce) the incentives to work informally. To the best of our knowledge, this is the first paper to provide empirical evidence of the effects of $AMIITR$ shocks on aggretate income and labor markets outside the developed world –in our case, for a sample of six large Latin American countries.

The rest of the paper proceeds as follows. Section 2 presents our novel proposed approach to build $AMIITR$ series for our sample of Latin American countries. In line with our first bullet point above, it discusses the main conceptual and practical limitations of using administrative data vis-à-vis household surveys as a source to gather individuals' reported incomes and to build $AMIITR$ series. Section 3 describes some main features of the novel $AMIITR$ dataset. First, we show that our Latin American countries have relatively low $AMIITR$ due to tax concentration at the top of the income distribution. Much like in the United States until the late 1940s, this occurs because this tax mainly reaches the top 10% of the income distribution in our Latin American sample. Second, as in Zidar (2019), we also show that the large within-country variation in $AMIITR$ reflects substantial heterogeneity in income distribution across regions. The within-country variation is between 3 and 10 times larger in our Latin American countries than across U.S. states. Section 4 deals with the identification of exogenous and unanticipated legislated individual income tax changes. It also discusses the importance of measuring $AMIITR$ shocks based on changes in the $AMIITR$ driven solely by legislated tax changes and not by changes in the income distribution (e.g., Barro and Redlick, 2011; Mertens and Montiel-Olea, 2018; Zidar, 2019). Sections 5 and 6 present the econometric specifications and ETI estimates, respectively. Sections 7 and 8 conduct additional robustness exercises and show the practical implications of pitfalls in measurement and identification of $AMIITR$ shocks, respectively. Section 9 analyzes the labor market mechanisms behind the previously depicted response of pretax income to $AMIITR$ shocks. Section 10 offers some quantitative insigths regarding the effects of individual income tax changes on estimated fiscal revenues when properly accounting for behavioral responses. When assuming no behavioral response (i.e., no change in incomes), as it is commonly the case in micro-simulation exercises, tax hikes (cuts) naturally drive revenue increases (reductions). However, and in light of the large behavioral responsiveness found in our Latin American sample, the change in revenues is much weaker when properly accounting for behavioral responses. Section 11 offers some final thoughts on our findings as well as some reflections as to the merits of our novel proposed approach to build worldwide $AMIITR$ series relying, crucially, on individuals' reported income from household survey datasets.

2 Novel proposed approach to build AMIITR series

This Section presents our novel proposed approach to build $AMIITR$ series for six large Latin American countries: Argentina, Brazil, Colombia, Ecuador, Paraguay, and Peru. Section 2.1 revisits the definition and inputs required to construct the $AMIITR$ measure. Section 2.2 discusses the main conceptual and practical limitations of using administrative data vis-à-vis household surveys as a source to gather individuals' reported incomes and build $AMIITR$ series. Section 2.3 compares existing $AMIITR$ series for the U.S. based on administrative data –as in Saez (2004) and Mertens and Montiel-Olea (2018)– with the ones relying on our approach based on individuals' reported income in U.S. household survey datasets (using the Annual Social and Economic Supplement) and the federal individual income tax code.[7]

[7] We focus on the federal invididual income tax for the easiness to calculate such a tax measure relying on household data. See online appendices for details.

2.1 AMIITR measure

We now discuss the definition and inputs required to construct the $AMIITR$ measure. Consider a legislated tax code l of a year t, l_t, and a pretax income $y_{j,t}$ (defined as the gross income minus exemptions) of an individual or tax unit j in a year t. The gross income includes all sources of labor income (wages, salaries, bonuses, and self-employment income), pensions, and capital income (dividends, interest, rents, and realized capital gains), and it excludes government transfers. The function $r_{j_k,t}(l_t, y_{j,t})$ identifies the highest marginal rate k paid by a tax unit j given her pretax income y and the legislated tax code l in year t. For the identification of this highest marginal rate, both income tax brackets as well as deductions are taken into account. For nonfilers, a zero tax rate is imputed. Recall that the pretax income minus deductions is equal to the taxable income. After assigning each tax unit's taxable income to her highest tax income bracket $k = \{1, 2, \ldots, K\}$, the income-weighted $AMIITR$ for each year t is defined as follows:

$$AMIITR(s_t, y_t) = \sum_{k=1}^{K} \sum_{j=1}^{N_k} \frac{y_{j,t}}{y_t} r_{j_k,t}(l_t, y_{j,t}), \tag{1}$$

where $\sum_{j=1}^{N_k} \frac{y_{j,t}}{y_t}$ is the income-weight, based on the pretax income of the N_k tax units in bracket k.

2.2 Source of individual income data

As discussed above, a key input to construct the $AMIITR$ is the gross income of each individual who has positive gross income, whether they file taxes or not. Existing papers for the U.S. rely on individuals' reported income in administrative data from the IRS. This gross income data is then combined with the tax code program TAXSIM from the NBER's Fortran program developed by Feenberg and Coutts (1993) to construct the $AMIITR$ measure since 1973. Work conducted by Barro and Sahasakul (1983, 1986), which also relied on IRS publications, further built these tax series for the the federal individual income tax dating back to 1913 –year in which the 16th Amendment was ratified permanently legalizing an income tax.[8]

The administrative data on tax returns, and individuals' reported income therein, which is fairly accessible for the United States is virtually unavailable in other countries, including our Latin American sample. To get around this limitation, we propose a novel approach to construct the $AMIITR$ by combining detailed information of the individual income tax code structure established in different laws, decrees, and regulations and, crucially, individuals' reported income in household survey datasets. As discussed below, household survey datasets also provide relevant individual and household information to estimate exemptions and deductions.

As is often the case with most measurement issues, alternative sources of individual income offer

[8] Barro and Sahasakul (1986) added the marginal income tax rate from the Social Security (FICA) tax on wages and self-employment income (starting in 1937 for the main Social Security program and 1966 for Medicare). Barro and Redlick (2011) also included the state income tax.

conceptual pros and cons which, in turn, may actually be more or less relevant in practice depending on the nature of the fiscal, institutional, and economic context of each country:

- True individual income: An important and recurrent challenge in this literature is related to the identification of true individual income, especially at the top of the income distribution. There is a largely recognized underreporting problem when relying on administrative data, especially due to tax evasion and avoidance.[9] The incentives individuals have to misreport their incomes to administrative agencies increase when the benefits are sufficiently large (e.g., to avoid tax payments) and/or when the cost of doing so is small. For example, estimates for the United States account for about 15-20% of income underreporting in the federal individual income tax, with larger income underreporting –reaching about 40%– at the top of the income distribution (Slemrod and Yitzhaki, 2002; Saez et al., 2012; Gale and Krupkin, 2019). Naturally, the concern of income underreporting associated with tax evasion and avoidance is even more pressing in the developing world, including our Latin American sample, which lacks strong revenue agencies able to properly fiscalize individual tax returns (Alvaredo and Gasparini, 2015).

  How about underreporting based on individuals' reported income in household surveys? In principle, it is *not* obvious that it is more pronounced or systematic than when relying on administrative data. While it is true that the cost of hiding information is zero in a survey, it is less evident that individuals gain something from underreporting income in household surveys. As discussed in Hurst et al. (2014), to the extent that individuals would have to exert effort to provide an accurate response to household surveys or feel compelled to maintain consistency in light of concerns about confidentiality, economic theory would suggest that they would continue to provide similar erroneous information, even if there is no direct benefit of doing so. This rationale is consistent with comprehensive evidence that individuals' reported income in household surveys is also subject to underreporting both in developing and in advanced economies, especially at the top of the income distribution.[10]

- Nonfilers: The use of tax return data does not allow to identify nonfilers, precisely because only filers are expected to present their tax returns to the respective revenue collecting government agency. For example, papers relying on NBER's TAXSIM program for the United States typically assume nonfilers' adjusted gross income equals 20% of the average reported adjusted gross income per return (e.g., Saez, 2004; Mertens and Montiel-Olea, 2018). In recent times, the estimated share of nonfilers (expressed as a percentage of total filers and nonfilers) has been

[9] See, for example, Clotfelter (1983), Slemrod (1985), Feinstein (1991 and 1996), Andreoni et al. (1998), Gruber and Saez (2002), Slemrod (2007), Feldman and Slemrod (2007), Chetty (2009), Diamond and Saez (2011), Saez et al. (2012), and Mertens and Montiel-Olea (2018).

[10] See, for example, Levy and Murnane (1992), Slemrod (1996), Székely and Hilgert (1999), Piketty and Saez (2006), Burkhauser et al. (2009), Alvaredo (2011), Burkhauser et al. (2012), Hurst et al. (2014), and Alvaredo and Gasparini (2015).

relatively low in the United States, representing about 10-15% (Saez, 2016). Interestingly, as discussed in Barro and Sahasakul (1983), this has not always been the case. Nonfilers used to represent a larger share before World War II due to the higher minimum income required to file for returns. The share of nonfilers in the population aged 18 or more was between 93% and 96% in 1930-1938 and, after World War II, there was a decrease to around 42%-50%. For our Latin American countries, the share of nonfilers is high, representing on average about 75% of individuals with positive incomes.[11] The share of nonfilers also varies importantly across regions in each country, mainly reflecting income heterogeneities across them. For example, while according to our estimates nonfilers represent on average about 83% in Brazil, they reach 93% in the state of Maranhão (one of the poorest Brazilian states populated by about 7 million people) and 63% in the Distrito Federal (where the average income is almost 4 times larger than that of Maranhão). Nonfilers are particularly numerous in our Latin American sample because, as discussed in more detail in Section 3, the individual income tax tends to be concentrated at the top of the income distribution –much more than in the present case of the United States. This implies that not accounting for nonfilers would give, especially in our sample, a largely distorted picture of the $AMIITR$. For example, while the $AMIITR$ is 5.8% in our Latin American sample when properly including nonfilers, it would be 13.4% when excluding them.[12] On the other hand, for the United States in contemporaneous times –using the tax code and household survey–, these $AMIITR$ figures are much closer: 24.1% when including estimates of nonfilers and 24.7% percent when not considering them.

- Informality: our proposed strategy also grants support taking into account the prevalence of labor market informality in Latin American countries (and more generally in the developing world).[13] In informality, individuals engage in productive activities that are not taxed or registered by the government and, consequently, are by construction not part of any administrative data. This group mainly includes include informal-salaried workers and non-professional self-employed individuals such as street vendors, artisans, as well as farm, cleaning, technician, and hospitality and tourism service workers. While the estimated share of labor market informality is quite modest for international standards in the United States, representing about 18% in 2013 (ILO, 2018), they represent about 57% in our Latin American sample.[14] The share of labor market informality also varies importantly across regions in each country, mainly reflecting income, sectorial, and labor market regulation heterogeneities across them. For example, while according to our estimates labor market informality represents on average about 69%

[11] According to our estimates, nonfilers represent about 23% in Argentina, 83% in Brazil, 90% in Colombia, 91% in Ecuador, 98% in Paraguay, and 91% in Peru.

[12] These values were calculated using the pool of $AMIITR$ for all regions and years.

[13] See, for example, Gasparini and Tornarolli (2009), Tornarolli et al. (2014), Perry et al. (2007), Loayza et al. (2009), ILO (2018), Maloney (2004), Bosch and Maloney (2008), Loayza (2018), and Ohnsorge and Yu (2021).

[14] According to our estimates, labor market informality represents about 49% in Argentina, 50% in Brazil, 63% in Colombia, 69% in Ecuador, 73% in Paraguay, and 71% in Peru.

in Ecuador, it reaches 55% in the province of Pichincha (home of capital city of Quito) and about 75% in Ecuador's Central-Sierra provinces such as Cañar, Chimborazo, Cotopaxi, and Tunguragua (where the main economic activities are small-scale family farming and trade coupled with adventure tourism). Considering the large prevalence of labor market informality in our sample, not including this group would give a distorted picture of the $AMIITR$. For example, while the $AMIITR$ is 5.8% in our Latin American sample when properly accounting for labor market informality, it would be 7.7% when not accounting for it. Moreover, if we solely focused on formal and filer individuals (which is what most administrative data may at best be able to provide if available), the $AMIITR$ would increase by a larger margin to 13.8%.

- Exemptions and deductions: Administrative data provides direct information on exemptions and deductions for filers. Fortunately, for the purpose of our novel approach, household surveys offer detailed information on most relevant variables considered for the purposes of exemptions and deductions. This includes, apart from individual and household income data, for example, data on marital status, number of kids in each household (and whether they are attending school or not), governmental transfers, age, and type of worker (employee, employer, or self-employed, as well as public or private).

2.3 Use of administrative data vis-à-vis household surveys to build *AMIITR* series: Evidence from the United States

Interestingly, and in spite of the measurement discussions of Section 2.2, it is encouraging to find that for the United States there seems to be little difference between the series of $AMIITR$ when using administrative data –as in Saez (2004) and Barro and Redlick (2011)– or, alternatively, when using the Annual Social and Economic Supplement household survey.[15,16] Figure 1 shows the $AMIITR$ series considering the entire distribution of income. Panel A shows the $AMIITR$ in levels and Panel B in changes (expressed in percentage points). It is worth noting that estimates of the correlation between our AMIITR series and that of Saez (2004) and Barro and Redlick (2011) are 0.95 and 0.97, respectively, when focusing on levels. When focusing on AMIITR changes, estimates of the correlation between our series and that of Saez (2004) and Barro and Redlick (2011) are 0.82 and 0.76, respectively.

$FIGURE$ 1

Figure 2 further shows that this remarkable similarity also holds when looking at the $AMIITR$ series focusing on the top 1% and bottom 99% of the income distribution. In other words, and

[15] See online appendices for a detailed description of our $AMIITR$ based on U.S. household survey data.

[16] Strictly speaking, Saez (2004) and Barro and Redlick (2011) series reported in Figures 1 and 2 are taken from the updates of the original series until 2012 performed by Mertens and Montiel-Olea (2018). Saez (2004) and Barro and Redlick (2011) original series end in years 2000 and 2006, respectively.

in spite of the previous measurement discussions, when focusing on the United States, our novel approach seems to be able to deliver very similar $AMIITR$ series to those relying on tax returns.[17]

FIGURE 2

3 Main features of novel AMIITR dataset

Several issues are worth noting about our novel $AMIITR$ data. First, given harmonization limitations in household surveys, most of our data starts between the mid-1990s and the early 2000s: Argentina (1997-2017), Brazil (1995-2015), Colombia (2008-2016), Ecuador (2003-2015), Paraguay (2012-2016) and Peru (1998-2015).[18,19] Second, the individual income tax is solely legislated at the federal level in our six Latin American countries. Unlike the United States, there is no individual income tax emanating from state-like or local jurisdictions.[20] Third, as in Saez (2004), the construction of the $AMIITR$ series does not include social security contributions on labor earnings. Fourth, given the geographically disaggregated nature of household survey datasets in each country, we count with a total of 139 state-like regions: 32 in Argentina, 27 in Brazil, 24 in Colombia, 25 in Ecuador, 6 in Paraguay, and 25 in Peru.[21,22,23] This rich regional heterogeneity allows us to create, in addition to series of $AMIITR$ for each country as a whole, series of $AMIITR$ for each regional unit. As discussed in the regional tax multiplier study conducted by Zidar (2019), this source of heterogeneity translates into a substantial source of variation due to the large heterogeneity in the income distribution across regions. We now turn to describe some main features of the novel $AMIITR$ dataset.

[17] We do not include the $AMIITR$ from Barro and Redlick (2011) in Figure 2 because they do not split the AMIITR across the income distribution.

[18] It is worth noting that Paraguay introduced the individual income tax for its first time in 2012 (Law Number 4,673 and Decree Number 9,371).

[19] The harmonized household survey datasets are from the Socioeconomic Database for Latin America and the Caribbean (SEDLAC), a project jointly developed by the Centro de Estudios Distributivos, Laborales y Sociales, at Universidad Nacional de La Plata (Argentina), and the World Bank.

[20] See online appendices for a detailed description of the individual income legislated tax code for each country and year.

[21] The specific nature of the state-like geographical units depends on each country: Argentina (urban areas), Brazil (federative units), Colombia (departments), Ecuador (provinces), Paraguay (departments), and Peru (departments). For the case of Paraguay, and due to gaps in the years available for each department, we grouped the departments into 6 regions: (i) Itapúa, which includes Itapúa, Misiones, and Ñeembucú; (ii) San Pedro, which includes San Pedro, Presidente Hayes, Concepción, Canindeyú, Boquerón, Amambay, and Alto Paraguay; (iii) Caaguazú, which includes Caaguazú, Caazapá, Cordillera, Guairá, and Paraguarí; (iv) Asunción; (v) Central; and (vi) Alto Paraná.

[22] All household surveys have national coverage, except in Argentina, where it is representative of only urban areas (where 63 percent of the population lives as of 2017).

[23] Out of the 32 departments in Colombia, 24 are considered due to harmonization issues. It is worth noting that these 8 excluded departments only represent 3 percent of Colombia's total population.

3.1 Countries' AMIITR is relatively low due to tax concentration at the top

Table 1 describes some key statistics of the $AMIITR$ in Latin America, exploiting both the time and regional heterogeneity. That is to say, each statistic uses a pooled regional-year sample for each country. For comparison purposes, we also include our $AMIITR$ measure for the United States for the period 1980-2017 presented in Section 2.2.

INSERT TABLE 1 HERE

The first salient aspect is depicted in the column "Mean," which reports the mean $AMIITR$. Row 1 shows that the $AMIITR$ is much lower in our Latin American countries than in the United States. While the $AMIITR$ for our Latin American countries is 5.8%, the $AMIITR$ in the United States is 24.1%. The Latin American country with the lowest $AMIITR$ is Paraguay, which "recently" introduced the tax in 2012 (with an $AMIITR$ of 1.1%), and the one with the highest $AMIITR$ is Brazil with an $AMIITR$ of 9.3%.[24]

These relatively low levels of $AMIITR$ in our Latin American sample do not reflect a tax structure with particularly low marginal rates, but rather the large share of tax units facing a zero marginal rate or, put differently, tax concentration at the top of the income distribution. Rows 2 to 10 in Table 1 report the descriptive statistics for different quantiles of the tax units' income distribution showing who actually pays this tax. A cell having a "0" reflects that no tax unit is reached in that income group.[25] Rows 2 to 6, which split the income distribution of each country by quintiles, show that the individual income tax is concentrated at the top 20% of the income distribution in all six Latin American countries, with the exception of Argentina (even though the $AMIITR$ paid by the four lowest quintiles is significantly lower than the $AMIITR$ of the top 20%) and to a lesser extent Peru. This is very different in the United States, where even the first quintile is reached by this tax in a quantitatively relevant manner.

Moreover, when looking at rows 7 and 8 in Table 1, which divide the tax units' income distribution between the top 10% and bottom 90%, it is clear that mainly the top 10% of the income distribution of these countries pay, by and large, the individual income tax. The bottom 90% pays a much significantly lower $AMIITR$ than the top 10%. While the top 10% to bottom 90% $AMIITR$ ratio is about 1.6 in the United States, it is 193 in Peru, 77 in Brazil, 38 in Ecuador, 11 in Argentina, and it is not defined (because the mean $AMIITR$ of the bottom 90% is zero) in Colombia and Paraguay.

Lastly, we further split the top 10% (row 8) between the top 1% (row 9) and the top 10-2% (row 10). Interestingly, when focusing on the top 1%, the $AMIITR$ in most of our Latin American

[24] Individual income tax in Argentina, Brazil, Colombia, Ecuador, and Peru dates back to 1932, 1922, 1927, 1926, and 1934, respectively.

[25] The province of Tierra del Fuego in Argentina does not pay individual income tax since 1974 (Law 19,640), hence the column "Min" is always "0" for this country.

countries (with the exception of Paraguay) now gets much closer to that of the United States. While the $AMIITR$ in the United States for the top 1% reaches 36.9%, it reaches 30.4% in Colombia, 27.3% in Brazil, 23.7% in Argentina, 21.3% in Ecuador, and 17% in Peru. In fact, for example, while the highest marginal individual income tax rate in the United States reached 39.6% in 2017, this value ranged between 27.5% and 35% in Argentina, Brazil and Ecuador.[26] Therefore, Latin American countries' $AMIITR$ is relatively low due to tax concentration at the top of the income distribution.

3.2 Large within-country variation in AMIITR reflects substantial heterogeneity in income distribution across regions

As discussed in Zidar (2019) for the United States, a large part of the variation in the $AMIITR$ across states reflects substantial heterogeneity in the income distribution across them.[27] Columns "SD" and "CV" in Table 1 report the standard deviation and the standardized measure of the coefficient of variation of the $AMIITR$. The variation in the $AMIITR$ –especially when focusing on the coefficient of variation– for each of our Latin American countries is much larger (between 3 and 10 times larger) than that observed in the United States.

Moreover, as in the case of the United States, Figure 3 shows that the larger variation in $AMIITR$ observed for each of our Latin American countries vis-à-vis that of the United States mainly reflects a substantially larger heterogeneity in the income distribution across regions. In particular, as in Zidar (2019), and as per the discussion in Section 3.1, Figure 3 shows the annual average share of the top 10% taxpayers living in each region for each country. Notice that while such a share of the top 10% in each state in the United States ranges between 4.5% in West Virginia and 21.5% in Alaska (and 16.7% in second-highest state Maryland), these discrepancies tend to be much larger in our Latin American countries. For example, in Argentina, such a share of the top 10% living in each province ranges between 4% in Santiago del Estero and 40.6% in Tierra del Fuego. Even the second, third, and fourth regions with the highest shares are above 20% (Santa Cruz with 27.4%, the City of Buenos Aires with 27.1%, and Chubut with 21.5%). In Brazil, such a share of the top 10% living in each state ranges between 3.9% in Maranhão and 26.7% in the Federal District.

INSERT FIGURE 3 HERE

4 Identification and measurement of tax shocks

We now turn to issues of proper measurement and identification of tax policy shocks. Our paper also contributes to the tax multiplier literature by developing a narrative identification approach for

[26] In Paraguay, the highest marginal individual income tax rate is 10%.

[27] Strictly speaking, Zidar (2019) does not measure the AMIITR, but rather tax liabilities.

unanticipated $AMIITR$ shocks in our Latin American sample. Section 4.1 presents the strategy to identify exogenous legislated tax changes following the narrative approach proposed by RR's seminal work and largely used when estimating the ETI for the United States (e.g., Barro and Redlick, 2011; Mertens and Ravn, 2012; Mertens and Montiel-Olea, 2018; Zidar, 2019). Section 4.2 discusses the importance of measuring $AMIITR$ shocks based on changes in the $AMIITR$ driven solely by legislated tax changes and not by changes in income distribution (e.g., Barro and Redlick, 2011; Mertens and Montiel-Olea, 2018; Zidar, 2019). Last, but not least, Section 4.3 discusses the need to use unanticipated (as opposed to anticipated) legislated tax changes to properly identify $AMIITR$ shocks not contaminated by anticipatory effects (e.g., Mertens and Ravn, 2012; Mertens and Montiel-Olea, 2018).

Before turning into all these important measurement and identification discussions, it is worth noting that for our Latin American countries we identify a total of 38 country-level legislated individual income tax changes which, in turn, translate into 989 regional tax changes. The 38 country-level legislated individual income tax changes observed for our sample of Latin American countries imply that, on average, there is a tax change every 2.3 years.[28] Table 2 provides brief details on each individual income tax change including the year of implementation, the law/decree number, and a brief description of the type of tax change to the individual income statutory code (e.g., whether it mainly involved an increase/decrease in deductions, brackets, non-taxable income, or the creation/elimination of new tax brackets).[29]

INSERT TABLE 2 *HERE*

4.1 Exogeneity of legislated tax changes

To properly estimate the effect of tax changes on economic activity (including pretax income), one needs to identify legislated tax changes that are exogenous. To guarantee this, we follow the so-called narrative approach developed by RR in their seminal paper for the United States, by using narrative records, to properly identify policymakers' intention and main motivation behind each legislated tax policy change in our Latin American sample. As sources for the narrative analysis, we use laws or decrees' motivation, policymakers' speeches, countries' news articles, and contemporaneous International Monetary Fund documents such as Staff Reports, Article IV Consultations, Recent Economic Developments, and Selected Issues.

Using these narrative sources we are able to differentiate those legislated tax changes which were directly or indirectly related to the current or expected economic conditions (i.e., endogenous tax changes) from those tax changes driven by reasons unrelated to developments likely to affect income

[28] The equivalent figure for the United States reflects, on average, a tax change every 2.6 years.

[29] Online appendices offer a detailed and systematized description of the invididual income tax code for each country under the period of coverage.

in the near term (i.e., exogenous tax changes). While closely following RR's identification strategy, we also incorporate some new elements that arise due to both the developing nature of our sample of countries and the specific tax considered:

– We allow endogenous tax changes to include countercyclical tax changes (as in RR) as well as procyclical tax changes. While the latter type of policy behavior is not found by RR in the United States, it is of critical importance in the developing world as well as in many other advanced countries (particularly, advanced European countries after the 2007-2008 global financial crisis).[30] The most common procyclical tax change is a tax hike enacted in response to a current (or prospective) recession which has dramatically reduced tax revenues. In effect, particularly when large and/or sudden contractions in economic activity are involved, the increase in the fiscal deficit that results from a sharp fall in tax revenues often leads to an unsustainable public debt. Under these circumstances, it is not uncommon for countries to face a sharp increase in borrowing costs or even lose access to international credit markets altogether, which leaves policymakers with no choice (other than defaulting) but to raise taxes. For example, the Brazilian government quickly increased the top marginal rate from 25 to 27.5 percent in December 1997 in order to deal with the economic slowdown and fall in revenues resulting from the Asian Crisis.

– We allow exogenous tax changes to include inherited deficit-driven changes (as in RR) as well as inherited debt-driven tax changes. The critical point is that in neither fiscal driven case the change in the tax rate responds to the current (or prospective) state of the economy but rather to past actions that may have caused a fiscal deficit to be viewed as too large or a stock of public debt that has come to be seen as unsustainable. While inherited debt-driven tax changes are not found by RR in the United States because, over the last 60 years, the United States has not faced sustainability problems regarding the public debt, this phenomenon is more recurrent in Latin American countries. For example, Peru increased in 2003 the top marginal rate from 27% to 30% to deal with the high foreign-denominated public debt (which was about 85% of total public debt) and high sovereign spread (which was around 750 basis points) resulting mainly from the fiscal stimulus of 2001.

– While Ecuador, Colombia, and Peru have automatic cost-of-living adjustments built into tax provisions to keep pace with past inflation and reduce bracket creep considerations, Argentina, Brazil, and Paraguay (like in the case of the United States before 1985) do not have them. The lack of automatic (and naturally anticipated) cost-of-living adjustments coupled with moderate inflation

[30] Tax hikes (cuts) enacted in response to a current or prospective recession (boom) are defined as procyclical. The natural question is, of course, why would policymakers pursue a tax policy that would tend to amplify the underlying business cycle? In fact, procyclical tax policy falls under the more general phenomenon of procyclical fiscal policy (which would also include increasing government spending in booms and reducing it in recessions) that has been explored in detail in the literature. See, among others, Gavin and Perotti (1997), Kaminsky et al. (2004); Talvi and Vegh (2005), Alesina et al. (2008), Frankel et al. (2013), Vegh and Vuletin (2015) and Avellan and Vuletin (2015). The most common explanations for such procyclical behavior have revolved around (i) political economy pressures that induce policymakers to loosen fiscal policy during booms and (ii) limited access to international credit markets in bad times, which forces policymakers to tighten fiscal policy.

put, as discussed in detail in the narratives, this discretionary tax adjustment in brackets and/or deductions at the core of the policy debate, including some rough negotiations between policymakers and unions.[31] For this reason, we also allow exogenous tax changes to include this discretionary tax adjustment in brackets and/or deductions which are motivated by past inflationary conditions, and not by current or prospective economic activity.[32] We refer to these exogenous tax changes as inflation-driven ones.[33]

– We allow exogenous tax changes to also include tax changes motivated by redistributive considerations. Given the multi-bracket tax rate nature of the individual income tax as well as the presence of numerous tax deductions, individual income tax policy changes could also be motivated by redistributive considerations which are not related to the stance in the business cycle or any intent to affect the current or expected aggregate economic conditions (e.g., reducing tax deductions to favor individuals and families at the lower end of the income distribution). While present, this motivation is quite rare in our sample.[34]

INSERT TABLES 3 *AND* 4 *HERE*

Table 3 shows the result of our classification and Table 4 provides brief details regarding the exogenous or endogenous nature of each legislated tax change.[35] Table 3 shows that more than 75% of all legislated individual income tax changes are identified as exogenous. In particular, 79% (or 30 out of 38) of country-level tax changes and 77% (or 757 out of 989) of regional tax changes are classified as exogenous. Of these exogenous changes, about two thirds are enacted by inflation-driven motivations and 20% are justified by long-run growth considerations. Changes enacted by inherited fiscal factors and redistributive considerations represent less than 10% in each case.

[31] For example, during the period 2005-2017, the average annual inflation rate reached about 25% and 6.4% in Argentina and Brazil, respectively

[32] Naturally, if the underlying narrative-based change in brackets and/or deductions resulting after a bracket creep is motivated by current or prospective economic activity considerations, such a tax change is considered endogenous.

[33] Interestingly, RR classify the large tax cuts of 1964 and 1981 (which followed periods of substantial bracket creep) as exogenous of long-run growth nature. Alternatively, we could have also classified our exogenous inflation-driven tax changes as exogenous long-run growth based on the premise that the change in brackets and/or deductions is motivated by past inflationary conditions which increase bracket creep considerations and is aimed to restore the distortive effect on the labor market. However, we preferred to use an additional exogenous category which more clearly identifies this source of tax change.

[34] If, in an excess of prudence, due to the plausible indirect effect of redistributive changes on aggregate economic conditions, we were to treated the two exogenous redistributive motivated changes as endogenous, our results would remain virtually unchanged. Results are not shown for brevity.

[35] Online appendices provide a comprehensive narrative for each tax change as well as the sources used in each case.

4.2 Measuring AMIITR changes solely driven by legislated tax changes

The change in the $AMIITR$ over time reflects both changes in the statutory code of the legislated tax, l, as well as in the income distribution, y:

$$\Delta AMIITR_t = AMIITR(l_t, y_t) - AMIITR(l_{t-1}, y_{t-1}). \tag{2}$$

To evaluate the impact of tax changes on different economic indicators, it proves essential to solely focus on events where there are legislated tax changes in the statutory code (e.g., Romer and Romer, 2014; Mertens and Montiel-Olea, 2018; Barro and Redlick, 2011; Zidar, 2019; Saez, 2004). In other words, and for the case of the $AMIITR$, it would be conceptually wrong from a measurement point of view to include, as a source of a tax shock, events in which there is a change in the $AMIITR$ in the absence of legislated tax changes (for example, due to changes in the income distribution). This measurement issue has been at the core of a myriad of arguments against the use of tax outcomes such as fiscal revenues (even when cyclicality adjusted), as opposed to tax policy instruments, as a source of tax shocks (e.g., Barro and Redlick, 2011; Riera-Crichton et al., 2016; Mertens and Montiel-Olea, 2018; Zidar, 2019).[36]

While necessary, these efforts to focus on events solely associated with legislated tax changes are still not sufficient to guarantee that $AMIITR$ shocks are not contaminated by factors different from tax policy changes. Why? Because given the individual-income-based nature of the individual income tax, changes in the $AMIITR$ (even those taking place when there is a change in the legislated tax code) could still be contaminated by changes in the income distribution. Equation (2) is very clear about that. In particular, and as discussed in Mertens and Montiel-Olea (2018), the income distribution could change due to several reasons. First, because of changes in other non-tax policy factors such as monetary policy changes. Second, because of changes in non-policy factors such as demographic and sectorial dimensions. Third, the income distribution could also change due to, precisely, tax policy changes. For example, suppose that decreases in tax rates actually make individual incomes to increase. Then, a legislated tax rate cut could, in principle, even reflect an increase in the observed $AMIITR$. This would occur if the downward effect of the legislated tax rate cut on the $AMIITR$ was more than compensated by the upward effect of the increase in income. In principle, this genuine concern would be less pressing in practice if the actual effect of tax changes on economic activity were negligible. Naturally, one would not know the nature of this measurement bias by just appraising the response of the observed $AMIITR$. To get around this limitation, we follow Barro and Redlick (2011) and Mertens and Montiel-Olea (2018), who create a counterfactual measure based on equation (2) in which the pretax income distribution is not allowed

[36] Vegh and Vuletin (2015) also discuss the conceptual problems and practical implications of wrongly using tax outcomes (as opposed to tax instruments) in terms of the cyclicality of tax policy over the business cycle.

to change:

$$\Delta AMIITR_t^{leg} = AMIITR(l_t, y_{t-1}) - AMIITR(l_{t-1}, y_{t-1}). \tag{3}$$

Unlike measure $\Delta AMIITR$ from equation (2), the proposed measure $\Delta AMIITR^{leg}$ captures the change in the $AMIITR$ solely driven by policy changes in the tax legislation (hence the leg superscript).

What does our data indicate about this genuine measurement concern? Figure 4 shows, for each legislated tax change, and at the country level, the $\Delta AMIITR^{leg}$ as per equation (3) in blue, the observed $\Delta AMIITR$ as per equation (2) in green, and the *difference* (defined as $\Delta AMIITR^{leg}$ minus $\Delta AMIITR$) in red. Several issues are worth noting. Using $\Delta AMIITR^{leg}$, 71% (or 27 out of 38) of all tax changes are tax cuts, with a mean $\Delta AMIITR^{leg}$ of -0.66 percentage points. However, when using the observed $\Delta AMIITR$, solely 45% (or 17 out of 38) are tax cuts, and the mean $\Delta AMIITR$ is 0.16 percentage points (i.e., with a positive sign). This significant difference occurs because in all 27 cases where there were legislated tax cuts (i.e., $\Delta AMIITR^{leg} < 0$), the observed $\Delta AMIITR$ is larger than $\Delta AMIITR^{leg}$ (i.e., $\Delta AMIITR > \Delta AMIITR^{leg}$) which, in turn, is reflected in *difference*< 0. Moreover, in half of these cases, the observed $\Delta AMIITR$ were even positive. See for example, the case of Argentina in 2003. Also reflecting a negative association between tax policy changes and the income distribution, in half of the 10 cases where there were legislated tax increases (i.e., $\Delta AMIITR^{leg} > 0$), the observed $\Delta AMIITR$ is smaller than $\Delta AMIITR^{leg}$ (i.e., $\Delta AMIITR < \Delta AMIITR^{leg}$) which, in turn, is reflected in *difference*> 0. Interestingly, this negative association between $\Delta AMIITR$ and $\Delta AMIITR^{leg}$ is so strong that in 37% (or 14 out of 38) of all tax changes, the sign of the $\Delta AMIITR^{leg}$ is the opposite of the one based on $\Delta AMIITR$. All in all, this evidence supports the actual relevance of properly measured $AMIITR$ shocks solely driven by legislated tax changes (i.e., $\Delta AMIITR^{leg}$) as opposed to shocks also driven/contaminated by contemporaneous changes in the income distribution (i.e., $\Delta AMIITR$).

INSERT FIGURE 4 HERE

4.3 Anticipatory effects of legislated tax changes

An important aspect when evaluating the effect of legislated tax changes on economic activity is the issue of anticipatory effects. Announcements of tax changes may lead to responses of economic agents before any tax change is actually implemented (e.g., Romer and Romer, 2010, 2014; Mertens and Ravn, 2012, 2013; Alesina et al., 2015; Mertens and Montiel-Olea, 2018; and Gunter et al., 2021). As discussed in Mertens and Ravn (2012, 2013) and Mertens and Montiel-Olea (2018), including anticipated tax changes as a source of tax shocks or innovation is misleading both conceptually (as there is an intrinsic conflict with its anticipated nature) as well as in practice (because it can bias the true effect of legislated changes depending on its intertemporal substitution effects). For example, in anticipation of an individual income tax hike, workers may choose to work more hours before the effective implementation of the tax hike and, in turn, reduce work hours much more intensively after its implementation (than in the absence of such an anticipation). In this example, the inclusion of an anticipated tax change (even if taken for exogenous arguments) would bias the estimated ETI upwards, pointing to a more positive response of income after the implementation of the tax change, precisely due to the wrongful effect of such an anticipated tax change, as opposed to reflecting the true effect driven by the implementation of the tax shock.

To deal with this issue, we follow Mertens and Ravn (2012, 2013) and Mertens and Montiel-Olea (2018) by solely using those exogenous tax changes that are also unanticipated as sources of tax shocks or innovation, and exclude anticipated legislated tax changes from the analysis. As in Romer and Romer (2010), Mertens and Ravn (2012), and Gunter et al. (2021), we use the dates of the promulgation of the law and its implementation as critical information to determine the anticipatory nature of each tax change.[37] Panel A in Figure 5 shows the histogram of the implementation lag (defined as the difference in days between these two dates) in our Latin American sample. Panel B shows, as reference, the equivalent data for the United States taken from Mertens and Ravn (2012). Two things are worth noting. First, as in the case of the United States, the Latin American data shows a twin-peaked profile with the peaks occurring at 0-30 days and at more than 150 days. In fact, this profile is even more striking in the Latin American sample where these two bins represent 97.4% of all tax changes (vis-a-vis 74.3% in the case of the United States). Second, if we use, as in Mertens and Ravn (2012), the 90-day implementation delay as a threshold to differentiate unanticipated from anticipated tax changes, Latin America shows much less anticipated tax changes. While the ratio of implementation delays occurring within the 0-90 days period to those taking more than 90 days equals 0.9 for the United States (pointing to slightly more tax changes occurring in an anticipated manner), the equivalent ratio is 2.5 for Latin American countries (pointing to much more tax changes occurring in an unanticipated manner).

INSERT FIGURE 5 *HERE*

Based on this approach, Table 3 shows that about 75% of exogenous tax changes are unanticipated and, therefore, properly considered to be used as a source of tax shocks in our empirical analysis. This implies that our sample of Latin American countries has, on average, an exogenous and unanticipated tax change every 4.2 years.[38]

[37] As discussed in Mertens and Ravn (2012), the use of this legislation-based source of anticipation is useful because it removes any uncertainty associated with early announcements of the intention to change taxes which would be present when using other alternative definitions of the annoucement date such as “first mention.”

[38] The equivalent figure for the United States reflects, on average, a proper tax shock also every 4.2 years.

5 Econometric specifications

We first show how we estimate the regional effect of an individual income tax shock on pretax income by relying on a growing regional multiplier literature (e.g., Shoag, 2010; Ramey, 2011; Suarez Serrato et al., 2011, 2016; Chodorow-Reich et al., 2012; Clemens and Miran, 2012; Wilson, 2012; Chodorow-Reich, 2019; Zidar, 2019). To obtain the cumulative impulse response function, we follow the single-equation local-projections approach proposed by Jorda (2005) and Stock and Watson (2007) with instrumental variables for identification.[39] Our baseline regression for each h-step-ahead horizon is as follows:

$$\begin{aligned}\triangle \ln(y_{i,t+h}) \quad &= \quad \beta_h \triangle \ln(1 - AMIITR^{leg}_{i,t}) + \lambda_h \triangle \ln(y_{i,t-1}) + \sum_{q=0}^{h-1} \psi_{qh} \triangle \ln(1 - AMIITR^{leg}_{i,t+h-q}) + \\ &\quad + \alpha_{i,h} + \eta_{t,h} + \mu_{i,t,h}, \end{aligned} \tag{4}$$

where i and t indicate region and year, respectively. $\triangle \ln(y_{i,t+h})$ is the percentage change in regional real pretax income per capita in region i between period $t-1$ and $t+h$.[40] $\triangle \ln(1 - AMIITR^{leg}_{i,t})$ is the annual percent change in the net-of-tax $AMIITR$ in region i solely driven by policy changes in the tax legislation (i.e., using strategy of equation (3) fixing the income distribution).[41] In light of the discussion in, for example, Stock and Watson (2008, 2012, 2018), Mertens and Ravn (2013, 2014), and Ramey (2016), instead of using unanticipated and exogenous changes in the net-of-tax $AMIITR$ directly as shocks (i.e., replacing $\triangle \ln(1 - AMIITR^{leg}_{i,t})$ values by zero when there are legislated endogenous and/or anticipated tax changes) in equation (4), we use them as as instruments for $\triangle \ln(1 - AMIITR_{i,t})$.[42] The terms $\alpha_{i,h}$ and $\eta_{t,h}$ are the horizon-specific region and time fixed effects, respectively. The inclusion of time fixed effects also allows to control for aggregate shocks and aggregate policy at the country level. We also use Teulings and Zubanov (2014) bias correction by including net-of-tax shocks nested between times t and h as controls together with the one-period lag of the dependent variable.[43] Finally, $\mu_{i,t,h}$ are the standard errors drawn from a two-way cluster-robust covariance matrix. Using this methodology, each estimator β_h directly

[39] While there is a growing evidence that LP and VAR estimators may not be conceptually separate procedures and instead simply be different reduction techniques with common estimand but different finite-sample properties, the use of LP provides several distinct advantages as discussed in Plagborg-Møller and Wolf (2021). Specifically, for this paper, given that LP can be estimated by single-regression techniques, it can easily accommodate panel specifications and different error structures that may be impractical in a multivariate SVAR context.

[40] Mertens and Montiel-Olea (2018) normalize income per tax unit as opposed to using population. Similar results are obtained if tax units are used instead. Results are not shown for the sake of brevity.

[41] That is to say, $\triangle \ln(1 - AMIITR_{i,t}) \equiv \ln(1 - AMIITR(l_t, y_{i,t-1})) - \ln(1 - AMIITR(l_{t-1}, y_{i,t-1}))$. We do not include a sub-index i to the terms l_t and l_{t-1} because, as discussed in Section 3, the individual income tax is solely legislated at the federal level in our six Latin American countries.

[42] If our series of unanticipated and exogenous $\triangle \ln(1 - AMIITR^{leg}_{i,t})$ are used directly as shocks, results are very similar. Results are not shown for the sake of brevity.

[43] That is to say, $\triangle \ln(1 - AMIITR_{i,t+h-q}) \equiv \ln(1 - AMIITR(l_{t+h-q}, y_{i,t+h-q-1})) - \ln(1 - AMIITR(l_{t+h-q-1}, y_{i,t+h-q-1}))$. Like in the measure of the tax shock $\triangle \ln(1 - AMIITR_{i,t})$ the distribution of income is kept fixed between the period $t+h-q-1$ and $t+h-q$.

represents the accumulated impulse response at each step h. That is to say, each β_h estimates the ETI at each time horizon h. Meanwhile, λ_h and ψ_{lh} serve as controls, "cleaning" β_h from the dynamic effects of the response variable.

As part of our robustness analysis, we also present the results of using the following extended specification:

$$\begin{aligned}\triangle \ln(y_{i,t+h}) &= \beta_h \triangle \ln(1 - AMIITR^{leg}_{i,t}) + \lambda_h \triangle \ln(y_{i,t-1}) + \sum_{q=0}^{h-1} \psi_{qh} \triangle \ln(1 - AMIITR^{leg}_{i,t+h-q}) \\ &\quad + \theta_h X_{i,t-1} + \alpha_{i,h} + \eta_{t,h} + \mu_{i,t,h}. \qquad (5)\end{aligned}$$

Here, we add a set of important controls in vector X. These controls include the lagged changes of the standard value-added tax rate and the standard corporate income tax rate as well as the lagged changes in the region's individual income tax revenue and governmental transfers. This last variable includes social transfers to families and individuals as well as tax rebates and/or differences in tax liabilities imputed as in Romer and Romer (2014) resulting from retroactive changes in individual income tax legislation.[44] This robust specification aims to control for the assumption that individual income tax shocks are unrelated to spending shocks or other tax policy decisions that may affect the income distribution. If other tax or spending policy decisions occurred simultaneously with our individual income tax shock, our estimate would not reflect only the direct effect of the tax shock but also the effect of the other tax and spending policy changes. Although there are many other variables that, in principle, could affect the pretax income, omitted variables that are orthogonal to the fiscal variables (once lagged business cycle indicators are included) would not bias the estimated effect of the fiscal variables (e.g., Romer and Romer, 2010; Barro and Redlick, 2011).

As discussed before, and in line with a growing regional multiplier literature, we rely on specifications (4) and (5) to exploit the large and rich regional heterogeneity provided in our sample. However, and especially for comparison purposes with several aggregate evidences for the United States, we also estimate similar regressions where the index i in specifications (4) and (5) refers to the country level.

6 Effects of individual income tax shocks

This Section provides the results of the effects of exogenous and unanticipated net-of-tax $AMIITR$ legislated tax changes on pretax income exploiting both regional as well as country-level heterogeneity for our sample of Latin American countries. Previous studies calculating ETI for the United States exploiting country-level heterogeneity include, among others, Feenberg and Poterba (1993), Slemrod (1996), Saez (2004), Mertens and Montiel-Olea (2018). To the best of our knowledge, only

[44] To account for the tax liability paid or withheld by an individual before a retroactive tax change was announced, we control in our regressions for the amount of the tax rebate (i.e., the difference in the tax liability before and after a retroactive tax change).

Zidar (2019) exploits the regional heterogeneity of $AMIITR$ for the United States, yet not focusing on its effect on reported income, but rather on labor and macroeconomic variables including employment, labor force participation, hours worked, real gross domestic product, consumption, and prices.

Before turning to the ETI estimates, Figure 6 shows, for each exogenous and unanticipated $AMIITR$ change, the size of the $\triangle AMIITR^{leg}$ –as per equation (3)– calculated both at the regional level (in blue) and at the country level (in red).[45,46] As depicted in Figure 6, the 580 regional-level changes provide an important source of heterogeneity. In fact, the average standard deviation of regional exogenous and unanticipated $\triangle AMIITR^{leg}$ changes is 0.33 percentage points which indicates that its typical value lies 0.33 percentage points away from the mean (which is -0.58).

INSERT FIGURE 6 *HERE*

6.1 Regional effects of tax changes

Panel A in Figure 7 shows the regional ETI using baseline specification (4). The ETI is significantly positive, especially in the short-term. In particular, the ETI is 2.42 on impact, indicating that a one percent hike (fall) in the net-of-tax $AMIITR$ causes income to increase (decrease) by 2.42 percent. Two facts are also worth noting. First, note that the cumulative positive ETI remains around 2 (2.10 to be precise) after one year of the shock and tends to dissipate after 2 years of the tax shock. This points out that while the short-term ETI is quite positive, the long-term ETI is fairly negligible based on this regional source of variation. This intertemporal profile is similar to that depicted by Mertens and Montiel-Olea (2018) for the United States. Second, the anticipated response of pretax income to an exogenous and unanticipated net-of-tax $AMIITR$ shock is quite negligible. In fact, we cannot reject the null hyphothesis that $\beta_{-3} = 0$ and $\beta_{-2} = 0$ with p-values of 0.66 and 0.11, respectively.[47] This latter evidence helps to validate, in an empirical fashion, the unanticipated nature of these tax shocks based on a narrative approach. As we will discuss later in Section 8, this is not the case when anticipated tax changes are wrongly included.

INSERT FIGURE 7 *HERE*

[45] The effect of the 2014 tax change in Argentina (i.e., ARG.10) is negligible. Given our sample, all individuals in 2013 with a pretax income lower than the fixed deduction were already paying a zero marginal tax rate. The slightly positive effect is due to the 20 percent increase in the deductions to those earning a pretax income between the fix deduction and ARS 25,000. This group only represents 4.3 percent of all tax units. In addition, among this group, only 0.2 percent payed a positive marginal tax rate.

[46] The effect of the 2010 tax change in Ecuador (i.e., ECU.2) is zero. The change implied a reduction in the cap of the total expenses that the individuals were able to deduct. However, this new cap was not binding for any of the individuals in our sample. In other words, even with the change in the cap, the total expenses (and thus, deductions), which were estimated using an expenditure household survey, were lower with both the new and the older cap.

[47] Note that, by construction, $\beta_{-1} = 0$.

Panel B in Figure 7 is analogous to Panel A, yet based on robust specification (5) which also includes, as discussed in Section 5, additional controls such as region's individual income tax revenue, standard value-added tax rate, standard corporate income tax rate, and total governmental transfers. The results are quite similar both in terms of the estimated ETI values as well as on their intertemporal profile. In particular, the ETI is 1.73 on impact and 2.45 after one year of the tax shock.

6.2 Country-level effects of tax changes

Panels A and B in Figure 8 show the country-level ETI based on baseline and robust specifications as per our discussion in Section 5. In spite of the different source of variation vis-à-vis that of Figure 7 which relies on regional variation, all in all, the intertemporal profiles are quite similar. The country-level ETI also points to a significant short-term positive ETI –of about 3.5 on impact– and long-term ETIs becoming statistically insignificant. This country-level-based ETI estimate obtained for the short term is much larger than those estimated with similar empirical strategies for the United States –which range between -0.02 in Saez (2004) to 1.2 in Mertens and Montiel-Olea (2018).

INSERT FIGURE 8 *HERE*

7 Robustness exercises

This section provides additional exercises regarding the robustness of our main finding of Panel B in Figure 7 –that is to say, relying on robust specification (5) and exploiting the regional heterogeneity. As discussed in the growing regional multiplier literature, the regional variation provides a useful and rich source of heterogeneity. Having said that, we now test the extent to which some few regions, or regional tax shocks, may be driving our findings.

Figure 9 shows the result of performing a Monte Carlo exercise using 10,000 repetitions randomly allowing for 20 percent (or 28 out of 139) of the regions to be excluded from the ETI estimation. Figure 9 presents in red the average ETI coefficients of all these 10,000 regressions for each time horizon h. Dark, medium, and light grey areas show 68, 90, and 95 percent confidence intervals, respectively, based on the standard deviation of the 10,000 coefficients obtained for each time horizon h. The tight grey areas hint that our ETI estimates do not seem to be driven by specific regions in our sample.

INSERT FIGURE 9 *HERE*

Figure 10 shows the result of performing a Monte Carlo exercise using 10,000 repetitions randomly allowing for 20 percent (or 116 out of 580) of the regional tax shocks (i.e., region-based exogenous and unanticipated individual income legislated tax changes) to be excluded from the

ETI estimation. The average ETI shown in red as well as the tight grey areas also hint that our ETI estimates do not seem to be driven by particular regional tax shocks in our sample.

INSERT FIGURE 10 *HERE*

8 Implications of pitfalls in measurement and identification of tax shocks

We now discuss the practical implications of pitfalls in measurement and identification of $AMIITR$ shocks, following our discussion of Section 4. We rely on robust specification (5) exploiting the regional heterogeneity and, for illustrative purposes, we discuss one pitfall at a time.

Figure 11 shows the implications of instrumenting by both exogenous unanticipated and exogenous anticipated changes in the net-of-tax $AMIITR$ solely driven by policy changes in the tax legislation (i.e., using the strategy of equation (3) fixing the income distribution). In other words, Figure 11 also includes as instruments exogenous anticipated legislative tax changes which comprise 9 tax changes at the country level and 185 at the regional level (see Table 3). Figure 11 shows that, unlike Figure 7, and in spite of the few aditional tax changes included, there is now a more marked intertemporal substitution effect. Including these exogenous, yet anticipated tax changes, induces pretax income to increase prior to an individual income tax hike. In fact, we cannot reject the null hypothesis that β^B_{-2} is negative with a p-value of 0.003.

INSERT FIGURE 11 *HERE*

Figure 12 shows the implications of instrumenting by both exogenous and endogenous unanticipated $\Delta AMIITR^{leg}$. In other words, Figure 12 also includes as instruments endogenous unanticipated changes in the net-of-tax $AMIITR$ solely driven by policy changes in the tax legislation which comprise 7 tax changes at the country level and 205 at the regional level (see Table 3). Figure 12 shows that this particular identification pitfall still supports a significant positive ETI in the short term. However, the impact in the short and medium term changes fundamentally. In the short term, while panel B in Figure 7 depicts an ETI of 1.73 and 2.45 on impact and after one year of the tax shock, respectively, Figure 12 supports a much smaller (in absolute value) ETI of 0.08 and 0.93 on impact and after one year of the tax shock, respectively. In the medium term, while Figure 7 depicts an ETI that tends to dissipate after 2 years of the tax shock, the ETI in Figure 12 reaches its maximum value of 2.58 after 2 years of the tax shock.

INSERT FIGURE 12 *HERE*

Figure 13 shows the implications of solely changing our tax measurement from one in which the income distribution is kept fixed –in line with metric developed in equation (3)– to one based on the observed change in the $AMIITR$ –in line with equation (2). In other words, Figure 13 shows the findings of still using exogenous and unanticipated legislated changes in individual income taxes, yet using a measure of $AMIITR$ change that is contaminated by changes in the income distribution. Figure 13 supports a *negative* ETI. This finding is strikingly different to that of Figure 7 which is positive on impact (1.73 to be precise). Why does this happen? Recall that, as discussed in section 4.2, $\Delta AMIITR^{leg}$ and $\Delta AMIITR$ are negatively correlated indicating that the change in the pretax income distribution more than compensates, in the opposite direction, the one resulting from a legislative tax change. For example, a tax hike in $\Delta AMIITR^{leg}$ which, in turn, generates a fall in pretax incomes would wrongfully indicate a tax fall when using $\Delta AMIITR$. Naturally, this radical shift in the nature of the tax policy shock wrongfully biases the results indicating the opposite effect of tax changes. This evidence reveals the crucial importance of properly measuring the tax shock to solely capture the change in the tax code (e.g., Barro and Redlick, 2011; Mertens and Montiel-Olea, 2018; Zidar, 2019).

$INSERT\ FIGURE$ 13 $HERE$

9 Labor market mechanisms

This section analyzes the labor market mechanisms behind the previously depicted response of pre-tax income to net-of-tax $AMIITR$ shocks. For this purpose, we continue using robust specification (5) exploiting the regional heterogeneity, yet changing the dependent variable and its one-period lag control variable one-at-a-time. As is common in studies for the United States, we analyze the response of the labor market focusing on the extensive and intensive margins.[48]

Figure 14 shows the results. Panel A shows that, on the extensive margin front, labor force participation increases (decreases) both in the short and long term in response to a net-of-tax $AMIITR$ hike (fall). This positive response is driven by the response of both employed (see Panel C) and unemployed (see Panel D) people. Interestingly, while the positive response of employment occurs on impact, it takes a little longer for unemployment to respond intensively. For these reasons, the unemployment rate responds positively especially after 1 year of the tax shock (Panel E). On the intensive margin front, there is a long-lasting positive response in the weekly hours worked per worker (see Panel B). In sum, as predicted by simple labor market models, the evidence supports that increases (cuts) in individual income tax reduces (increases) the willingness to work both on the extensive and intensive margins. Panel F shows that labor informality also increases (falls)

[48] See, for example, Eissa and Liebman (1996), Auerbach and Siegel (2000), Goolsbee (2000), Meyer and Rosenbaum (2001), Ohanian et al. (2008), Monacelli et al. (2010), Saez et al. (2012), Mertens and Ravn (2012 and 2013), Romer and Romer (2014), Mertens and Montiel-Olea (2018), and Zidar (2019).

following increases (cuts) in the individual income tax.

INSERT FIGURE 14 *HERE*

10 Policy implications for revenue collection

This Section offers some quantitative insigths regarding the effects of individual income tax changes on estimated fiscal revenues when properly accounting for behavioral responses. Considering the large behavioral response observed on impact, and that of the proposed counterfactual excersice described below, we conduct this excersise solely evaluating it effects on impact (i.e., on h=0). We rely on robust specification (6) estimated at the country-level, which is very similar to robust specification (5), yet making 2 changes. First, for easiness to interpret the results, we replace $\triangle \ln(1 - AMIITR^{leg}_{i,t})$ by $\triangle AMIITR^{leg}_{i,t}$ and, naturally, we also change its instrument accordingly. Second, we change the dependent variable using the estimated changes in individual income tax revenues expressed as percent of the previous year pretax income.[49]

$$\begin{aligned} \frac{\triangle \mathrm{Rev}_{i,t}}{y_{i,t-1}} &= \beta \triangle AMIITR^{leg}_{i,t} + \lambda \triangle \ln(y_{i,t-1}) + \sum_{q=0}^{-1} \psi_q \triangle AMIITR^{leg}_{i,t-q} \\ &\quad + \theta X_{i,t-1} + \alpha_i + \eta_t + \mu_{i,t}. \end{aligned} \tag{6}$$

Figure 15 shows the results of using two alternative estimations on how to calculate the estimated changes in individual income tax revenues (i.e., the term $\triangle\mathrm{Rev}_{i,t}$). First, we calculate the change in the estimated individual income tax revenue collection considering both the change in the tax code as well as the change in income between t-1 and t. This is what we call the response in revenues "with behavioral response". Alternatively, we mute the behavioral response channel assuming that there are no changes in income between subsequent periods. In this case, $\triangle\mathrm{Rev}_{i,t}$ is solely driven by changes in the tax code, not in the income distribution. This is what we call the response in revenues "without behavioral response". When assuming no behavioral response (i.e., no change in incomes), as it is commonly the case in micro-simulation exercises, tax hikes (cuts) naturally drive revenue increases (reductions). In particular, on impact, a 1 percentage point increase in the $AMIITR$ increases the estimated individual income tax revenue collection by 0.38 percentage points of previous year pretax income. Moreover, we can reject the null that such a revenue response equals zero with a p-value of 0.008. However, and in light of the large behavioral responsiveness found in our Latin American sample, the change in revenues is much weaker when properly allowing for behavioral responses in incomes affecting the estimated revenue collection. In particular, on impact, a 1 percentage point increase in the $AMIITR$ increases the estimated individual income tax revenue

[49] We use previous year pretax income as opposed to contemporaneous pretax income to fully isolate the behavioral response of the depenent variable in the numerator.

collection by 0.20 percentage points of pretax income; which is almost half the size obtained when not allowing for a behavioral response. Notably, we cannot reject the null that such a revenue response equals zero with a p-values of 0.311.

INSERT FIGURE 15 *HERE*

11 Final thoughts

We contribute to the growing literature evaluating the extent to which individual income tax changes affect aggregate reported income. So far, the empirical evidence is solely based on the United States and finds short-term elasticity of taxable income (ETI) estimates ranging between -0.02 in Saez (2004) to 1.2 in Mertens and Olea (2018). Nothing is known about the size of the ETI outside the United States mainly due to the absence of readily-available $AMIITR$ series which mainly reflects the lack of access to administrative data, and the individuals' reported income therein.

We take upon this challenge and build a novel $AMIITR$ series (both at the regional and country levels) for six large Latin American countries: Argentina, Brazil, Colombia, Ecuador, Paraguay, and Peru. Given the lack of access to administrative data –which is the main practical limitation to conduct empirical studies outside the United States– we propose a novel approach to build $AMIITR$ series relying on the statutory individual income tax code established in different laws, decrees, and regulations and, crucially, individuals' reported income in household survey datasets. We also identify exogenous individual income tax changes relying on a new narrative-based classification of each tax change à la Romer and Romer (2010) and that they are not contaminated by anticipatory effects. Based on this novel measures of tax shocks, we find short-term regional ETI estimates of around 2.5, and country-based ETI estimates of 3.5 , pointing to a much larger responsiveness than in the United States. We also show that this larger responsiveness is reflected in labor responses, on both on the extensive and the intensive margins, as well as on labor market informality. To the best of our knowledge, this is the first paper to provide empirical evidence of the effects of $AMIITR$ shocks on aggretate income and labor markets outside the United States –in our case, for a sample of six large Latin American countries.

While this herculean data and identification effort is very time consuming, we believe this approach offers a credible path to overcome the lack of access to admisnitrative data and the relatively rich and very detailed household survey data available worldwide. For example, household survey data is largely available since the 1980s in European countries and since the 1990s-2000s in a large part of the emerging world.

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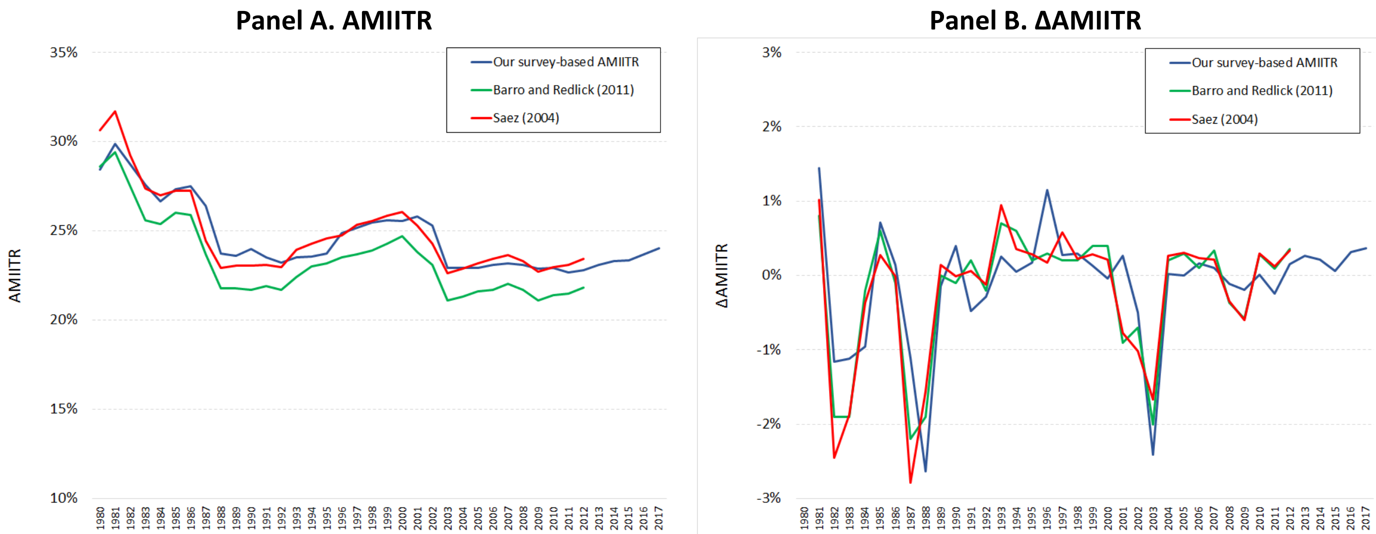
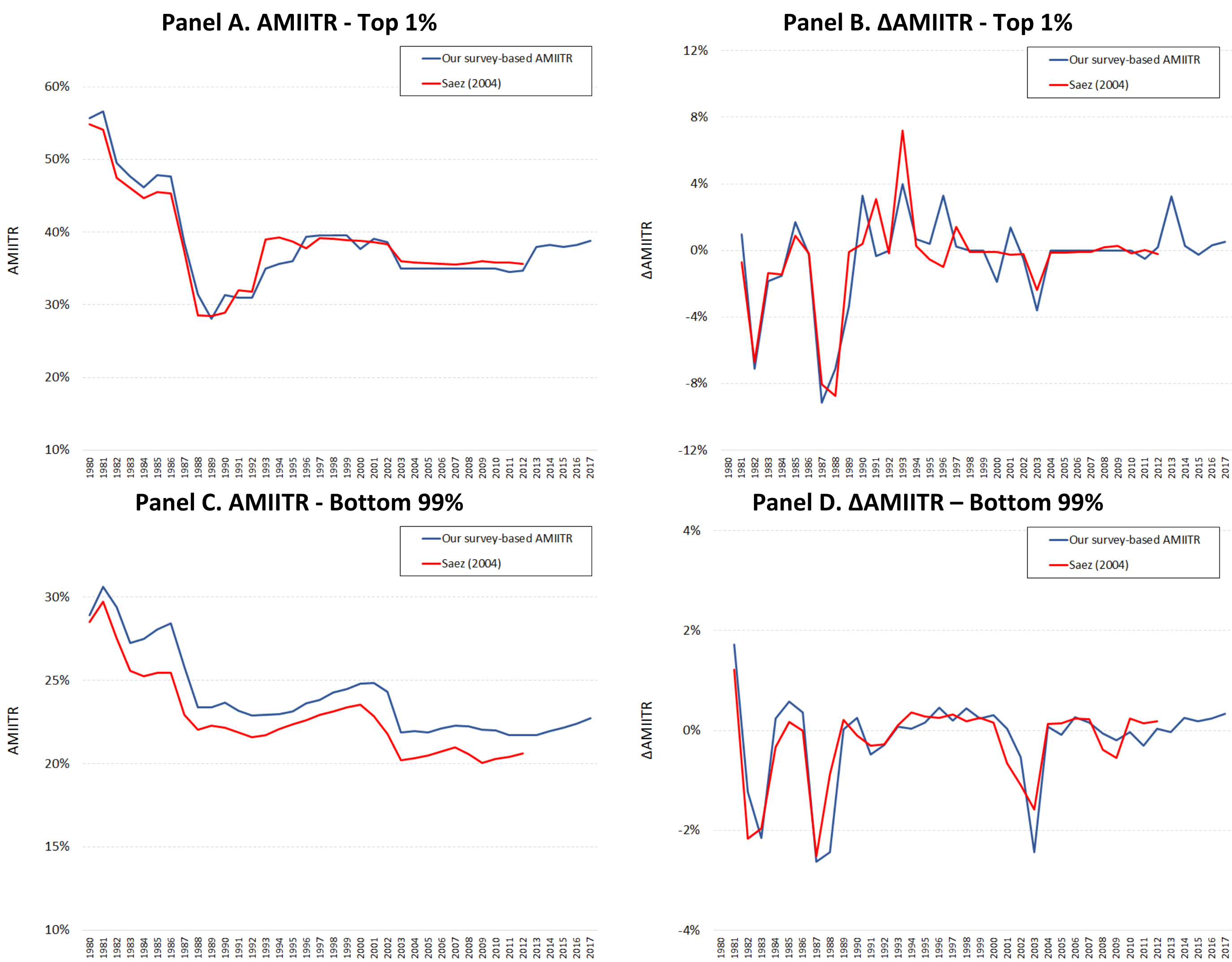
Figure 1. Alternative AMIITR measures for United States considering the entire income distribution.



Notes: All measures solely focus on the federal individual income tax. Strictly speaking, Barro and Redlick (2011) and Saez (2004) series are taken from Mertens and Montiel-Olea (2018), who update the original series from these two studies until 2012. Barro and Redlick (2011) and Saez (2004) original series end in years 2006 and 2000, respectively.

Figure 2. Alternative AMIITR measures for United States for the top 1% and bottom 99% of income distribution.



Notes: All measures solely focus on the federal individual income tax. Strictly speaking, Saez (2004) series are taken from Mertens and Montiel-Olea (2018), who update the original series from these two studies until 2012.

Table 1. Basic AMIITR statistics using a pooled regional-year sample for each country

	Argentina					Brazil				
	Mean	SD	CV	Min	Max	Mean	SD	CV	Min	Max
[1] All Sample	5.4	5.2	1.0	0	27.7	9.3	2.4	0.3	2.0	19.8
[2] Lowest 20%	0.02	0.1	3.2	0	0.7	0	0	0	0	0
[3] Second 20%	0.03	0.1	2.5	0	1.0	0	0	0	0	0
[4] Third 20%	0.11	0.4	3.3	0	2.5	0	0	0	0	0
[5] Fourth 20%	1.2	3.2	2.7	0	15.7	0	0	0	0	0
[6] Top 20%	11.1	8.8	0.8	0	32.6	16.4	2.3	0.1	3.8	23.8
[7] Bottom 90%	1.4	2.7	1.9	0	17.4	0.3	0.3	0.9	0.0	1.3
[8] Top 10%	15.1	10.4	0.7	0	34.9	21.7	2.0	0.1	7.8	25.7
[9] Top 1%	23.7	11.1	0.5	0	35.0	27.3	0.8	0.0	25.0	29.8
[10] Top 10-2%	12.7	10.8	0.8	0	34.8	19.6	2.2	0.1	7.8	24.4

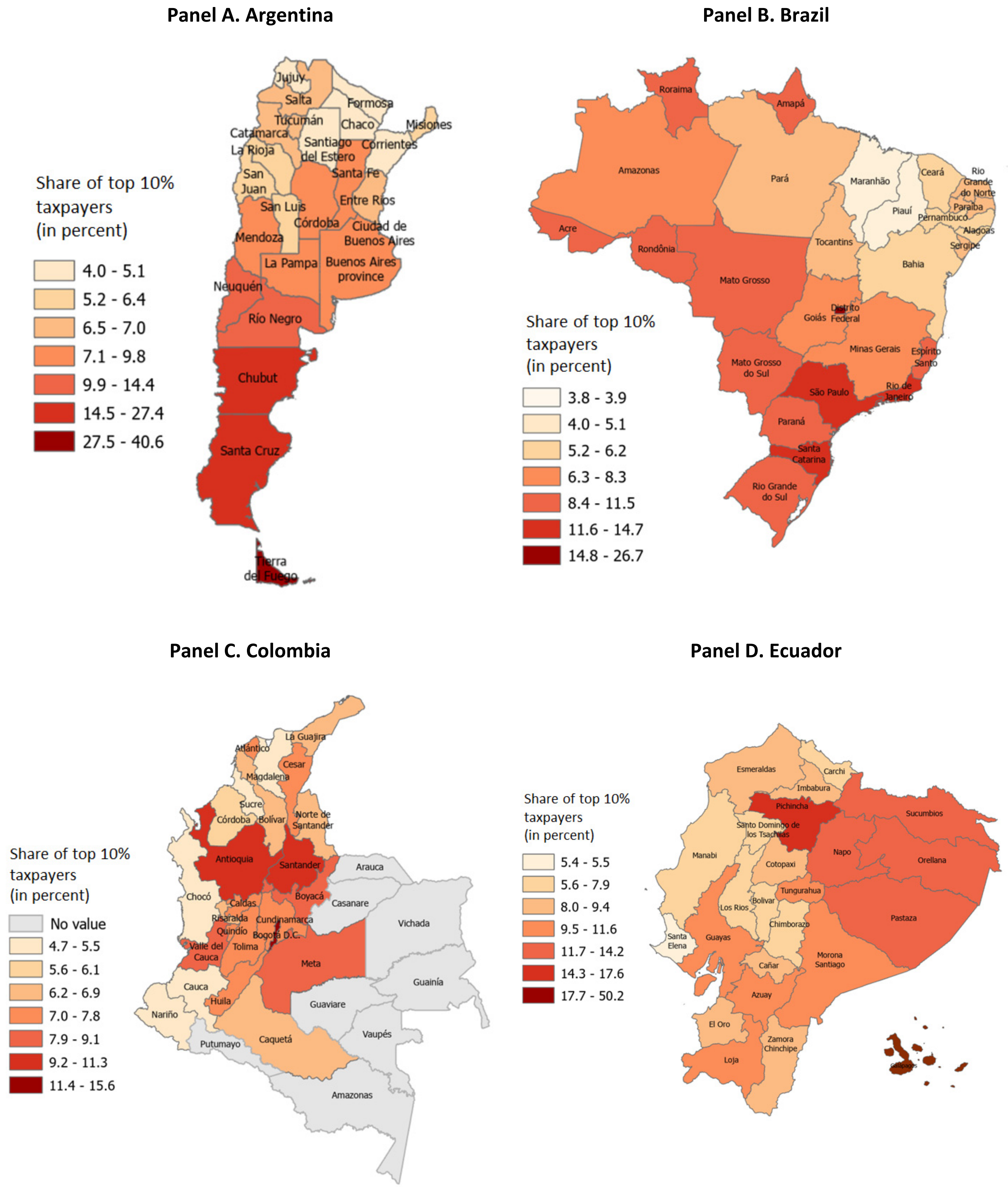
	Colombia					Ecuador				
	Mean	SD	CV	Min	Max	Mean	SD	CV	Min	Max
[1] All Sample	5.0	1.9	0.4	1.7	12.1	4.7	2.5	0.5	0.7	16.2
[2] Lowest 20%	0	0	0	0	0	0	0	0	0	0
[3] Second 20%	0	0	0	0	0	0	0	0	0	0
[4] Third 20%	0	0	0	0	0	0	0	0	0	0
[5] Fourth 20%	0	0	0	0	0	0	0	0	0	0
[6] Top 20%	10.3	2.4	0.2	5.0	17.7	8.9	3.9	0.4	1.6	26.0
[7] Bottom 90%	0	0	0	0	0	0.3	0.5	1.7	0.0	3.6
[8] Top 10%	14.94	2.5	0.2	9.4	22.1	12.1	4.5	0.4	2.6	29.7
[9] Top 1%	30.4	0.8	0.0	28.0	32.5	21.3	5.7	0.3	8.6	34.0
[10] Top 10-2%	10.0	1.6	0.2	5.6	14.9	8.2	3.6	0.4	0	15.0

	Paraguay					Peru				
	Mean	SD	CV	Min	Max	Mean	SD	CV	Min	Max
[1] All Sample	1.1	1.0	0.9	0.1	4.7	3.7	1.8	0.5	0	10.4
[2] Lowest 20%	0	0	0	0	0	0	0	0	0	0.0
[3] Second 20%	0	0	0	0	0	0.002	0.01	4.9	0	0.1
[4] Third 20%	0	0	0	0	0	0.003	0.02	5.1	0	0.2
[5] Fourth 20%	0	0	0	0	0	0.005	0.02	3.9	0	0.2
[6] Top 20%	1.8	1.4	0.8	0.3	6.2	7.5	2.6	0.3	0	13.4
[7] Bottom 90%	0	0	0	0	0	0.06	0.1	2.4	0	1.0
[8] Top 10%	2.3	1.5	0.6	0.4	6.9	11.2	3.3	0.3	0	18.6
[9] Top 1%	6.0	1.8	0.3	2.5	9.1	17.0	2.6	0.2	4.7	26.0
[10] Top 10-2%	0	0	0	0	0	9.5	3.5	0.4	0	15.0

	United States				
	Mean	SD	CV	Min	Max
[1] All Sample	24.1	2.4	0.1	19.5	37.8
[2] Lowest 20%	6.8	1.4	0.2	2.0	10.7
[3] Second 20%	14.7	0.9	0.1	12.9	18.7
[4] Third 20%	19.3	1.7	0.1	16.6	27.0
[5] Fourth 20%	24.0	2.6	0.1	19.5	37.0
[6] Top 20%	31.6	4.1	0.1	26.6	50.3
[7] Bottom 90%	21.2	2.0	0.1	17.5	29.8
[8] Top 10%	33.6	5.0	0.1	27.9	53.2
[9] Top 1%	38.5	6.3	0.2	28.0	63.4
[10] Top 10-2%	32.4	4.9	0.2	27.2	48.3

Source: Authors' calculations.

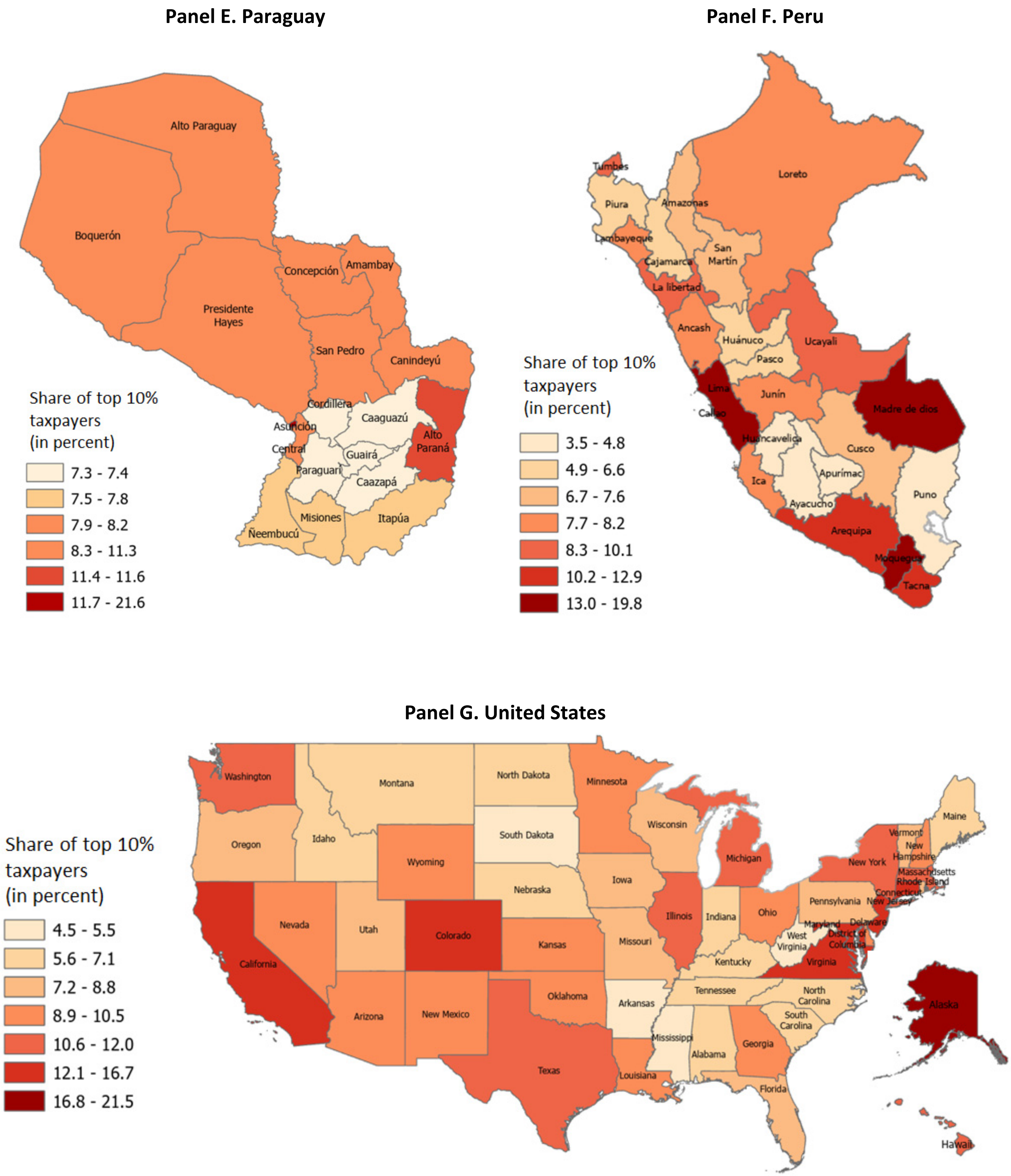
Figure 3. Share of top 10% tax units per region in each country



Source: Authors' calculations.

Notes: Since household surveys in Argentina are representative solely of urban areas and, in some cases, there are more than one urban area belonging to one same province, the value assigned to each province in the map is a population weighted average of the share of top 10% taxpayers of the urban areas belonging to a each province. It is worth noting that the share of urban to total population in Argentinean provinces range between 74% (in Misiones) and 100% (in Ciudad de Buenos Aires). The white regions in Colombia are not included in the country's household survey. It is worth noting that these excluded regions only represent 3 percent of Colombia's total population.

Figure 3 cont. Share of top 10% taxpayers per region in each country



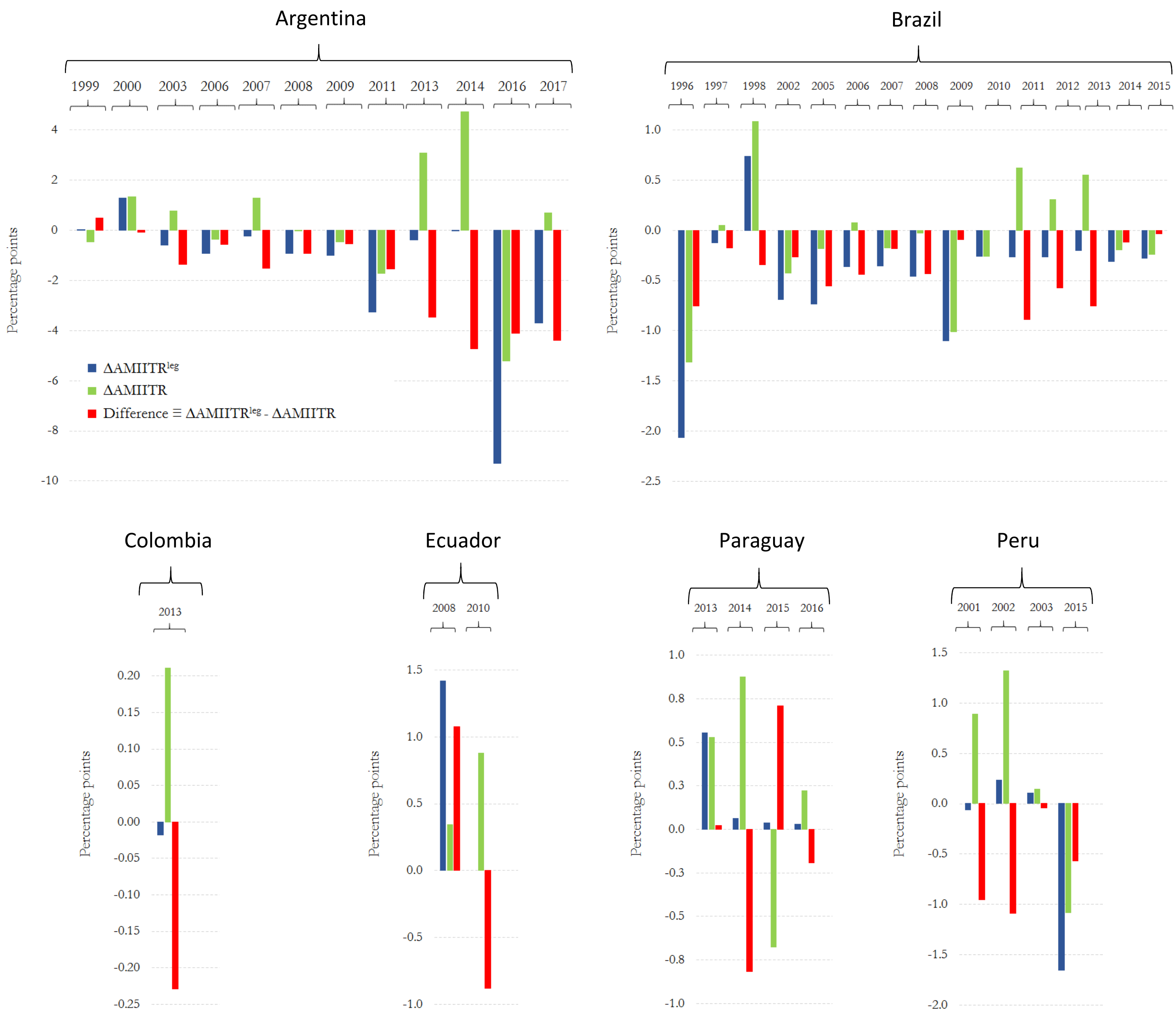
Source: Authors' calculations.

Table 3. Classification of legislated individual income tax changes

	All tax changes		Unanticipated tax changes	
	Country-level	Region-level	Country-level	Region-level
Total Tax Changes	38	989	28	777
Endogenous	8	232	7	205
Countercyclical	5	141	4	114
Procyclical	3	91	3	91
Exogenous	30	757	21	572
Inflation-driven	19	548	13	381
Long-run growth	6	83	3	65
Inherited fiscal factors				
Debt-driven	1	25	1	25
Deficit-driven	2	51	2	51
Redistributive	2	50	2	50

Source: Authors' calculations.

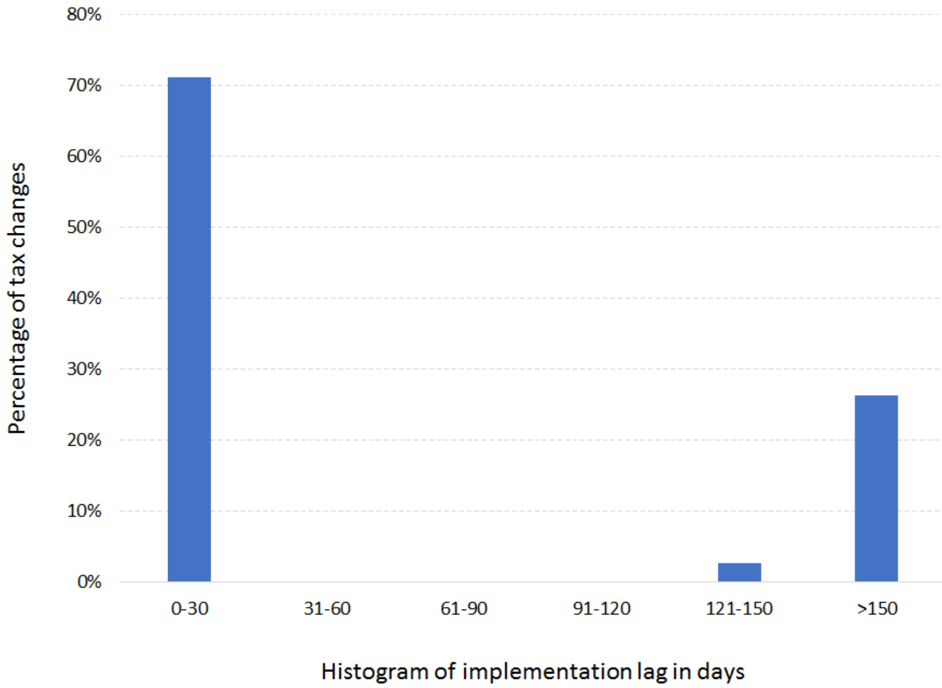
Figure 4. Country-level legislated individual income tax changes: ΔAMIITR vs ΔAMIITR^{leg}



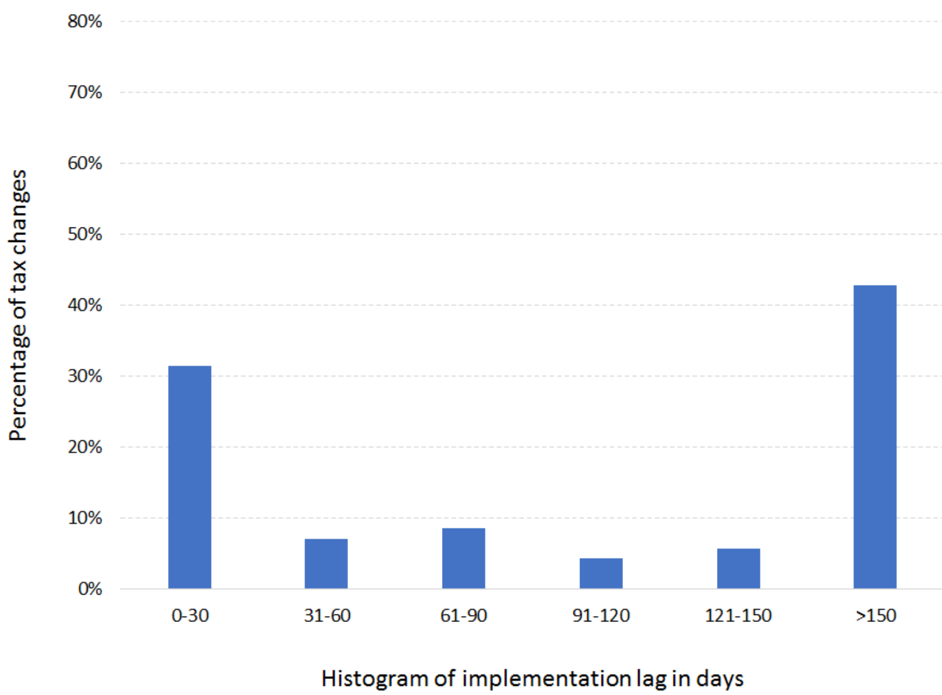
Source: Author's calculations.

Figure 5. The distribution of implementation lags across tax changes

Panel A. In our sample of Latin American countries



Panel B. In the United States (from Mertens and Ravn, 2012)

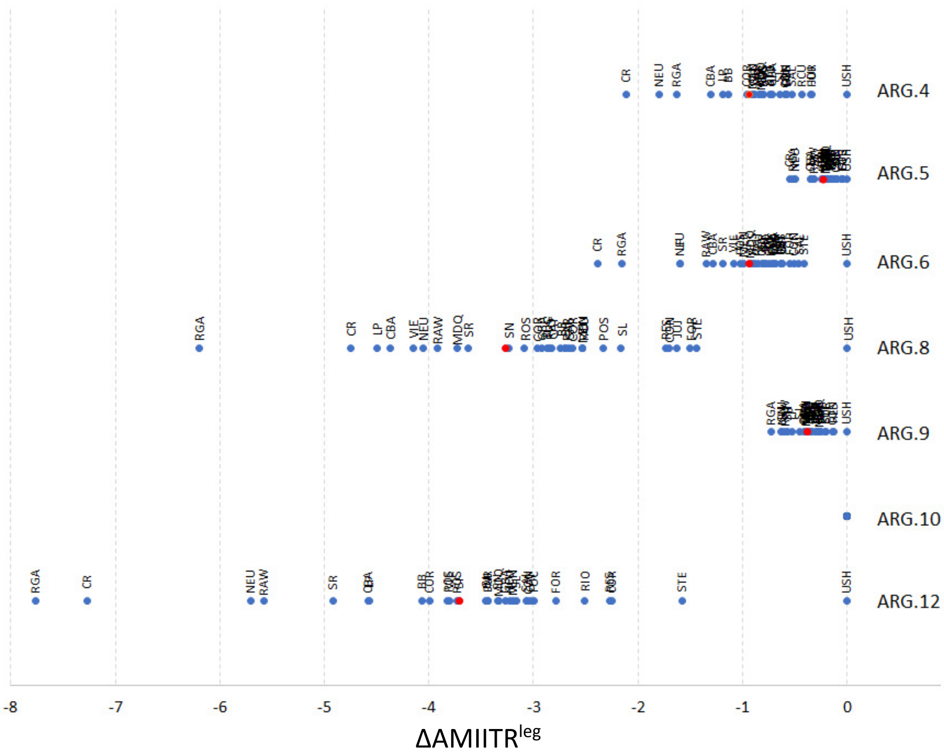


Source: Authors' calculations.

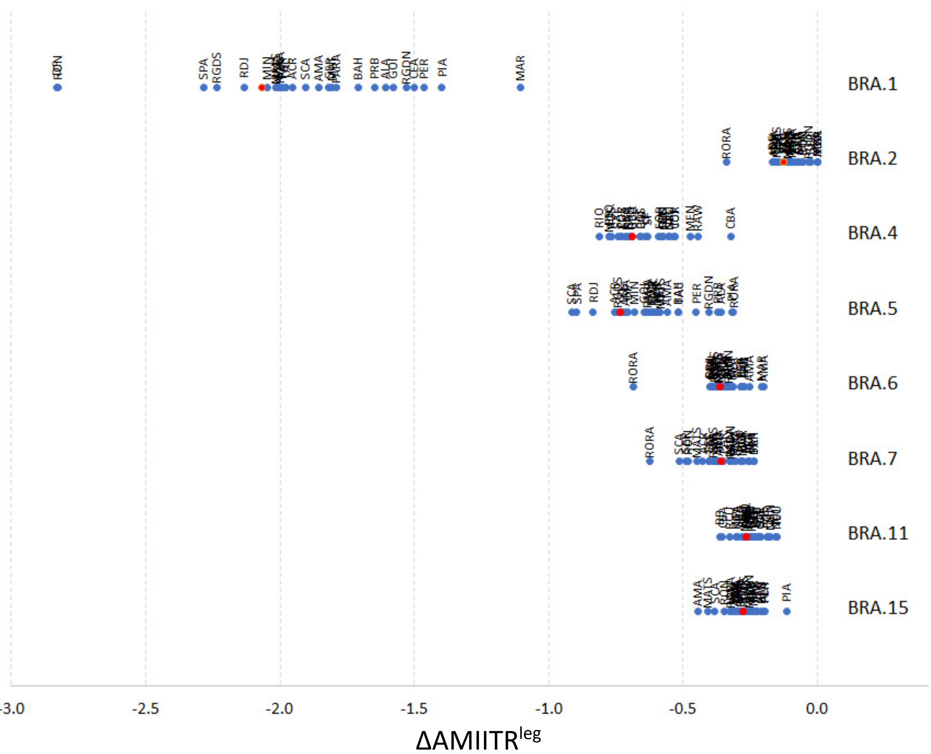
Notes: In Panel A, for 2 out of the 38 country-year observations in which the change in the individual income tax change have two referenced decree or law changes (ARG.7 and ARG.8), the average anticipatory horizon for each country-year observation was used. When anticipatory horizon was negative (e.g., BRA.4 with an anticipatory horizon of -4 days), we imputed, for the purpose of this histogram, zero days in implementation lag reflecting no implementation delay.

Figure 6. Regional and country-level $\Delta AMIITR^{leg}$ for each exogenous and unanticipated legislated individual income tax change

Panel A. Argentina, 1997-2017



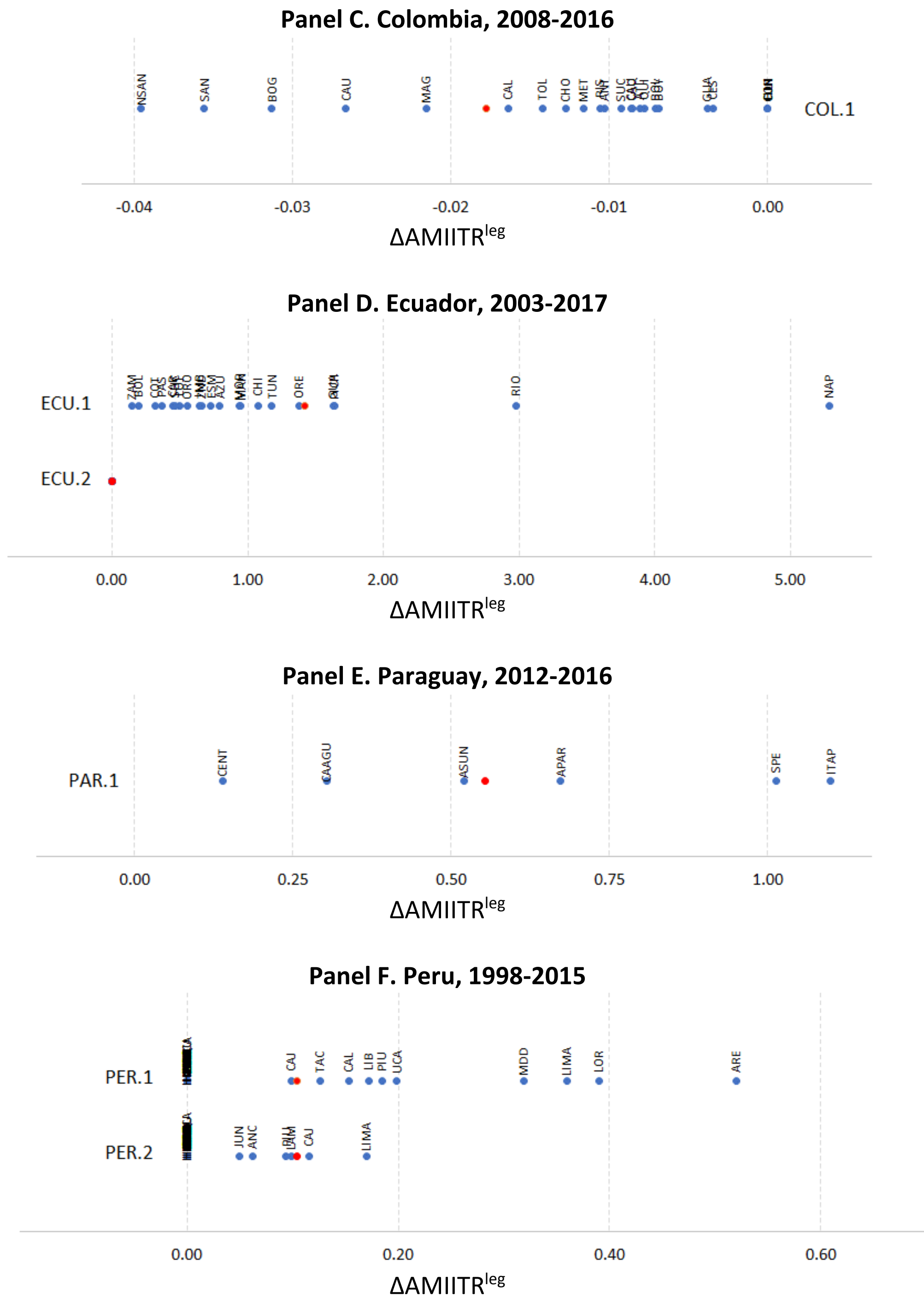
Panel B. Brazil, 1995-2015



Source: Authors' calculations.

Notes: Blue dots identify $\Delta AMIITR^{shock}$ at regional-level and red dot measures $\Delta AMIITR^{shock}$ at country-level. For Argentina, the 32 regional units are Bahia Blanca - Cerri (BB), Gran Catamarca (CAT), Ciudad de Bs As (CBA), Concordia (CON), Corrientes (COR), Gran Cordoba (COR), Cdro. Rivadavia - R.Tilly (CR), Formosa (FOR), Partidos del GBA (GBA), Jujuy - Palpala (JUJ), Gran La Plata (LP), Mar del Plata - Batan (MDQ), Gran Mendoza (MEN), Neuquen - Plottier (NEU), Gran Parana (PAR), Posadas (POS), Rawson - Trelew (RAW), Rio Cuarto (RCU), Gran Resistencia (RES), Rio Gallegos (RGA), La Rioja (RIO), Gran Rosario (ROS), Salta (SAL), Gran Santa Fe (SF), Gran San Juan (SJ), San Luis - El Chorrillo (SL), San Nicolas - Villa Constitucion (SN), Santa Rosa - Toay (SR), S.del Estero - La Banda (STE), Gran Tucuman - T. Viejo (TUC), Ushuaia - Rio Grande (USH), Viedma - Carmen de Patagones (VIE). For Brazil, the 27 regional units are ACR (Acre), ALA (Alagoas), AMA (Amapa), AMA (Amazonas), BAH (Bahia), CEA (Ceara), DF (Distrito Federal), ESS (Espirito Santo), GOI (Goias), MAR (Maranhao), MAT (Mato Grosso), MATS (Mato Grosso do Sul), MIN (Minhas Gerais), PAR (Parana), PARA (Para), PER (Pernambuco), PIA (Piaui), PRB (Paraiba), RDJ (Rio de Janeiro), RGDN (Rio Grande do Norte), RGDS (Rio Grande do Sul), RON (Rondonia), RORA (Roraima), SCA (Santa Catarina), SER (Sergipe), SPA (Sao Paulo), TAC (Tocantins).

Figure 6 cont. Regional and country-level $\Delta AMIITR^{shock}$

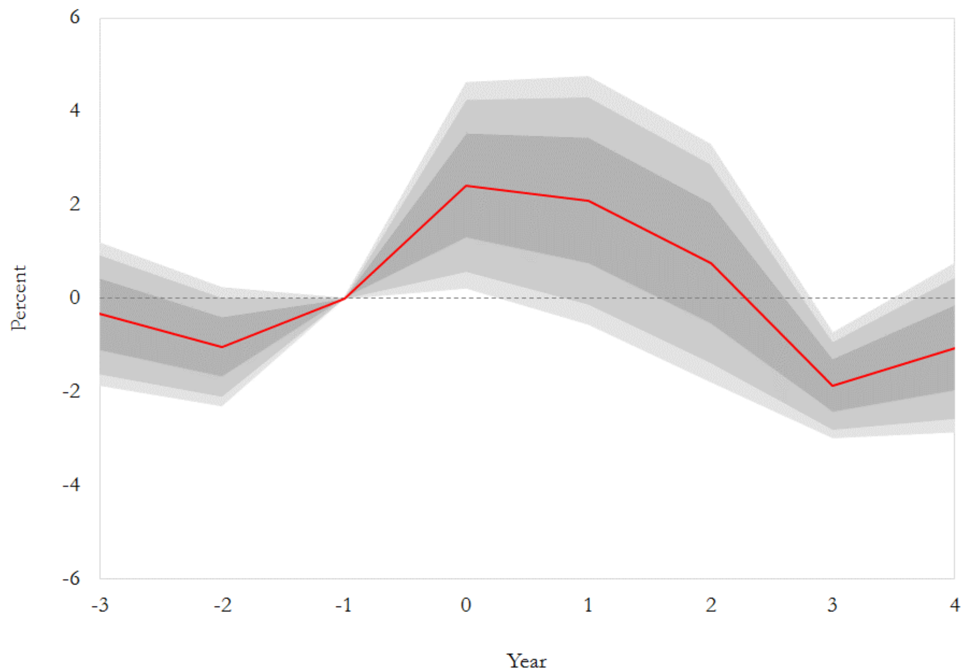


Source: Authors' calculations.

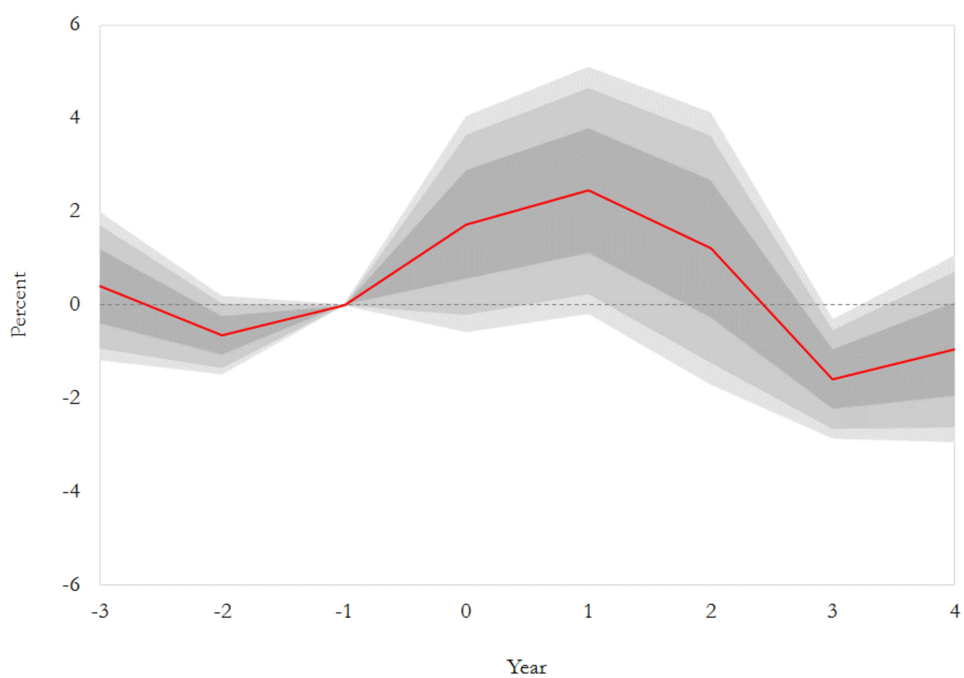
Notes: Blue dots identify $\Delta AMIITR^{leg}$ at regional-level and red dot measures $\Delta AMIITR^{leg}$ at country-level. For Colombia, the 23 regional units are ANT (Antioquia), ATL (Atlantico), BOG (Bogota), BOL (Bolivar), BOY (Boyaca), CAL (Caldas), CAQ (Caqueta), CAU (Cauca), CAU (Valle del Cauca), CES (Cesar), CHO (Choco), COR (Cordoba), CUN (Cundinamarca), GUA (La Guajira), HUI (Huila), MAG (Magdalena), MET (Meta), NSAN (Norte de Santander), QUI (Quindio), RIS (Risaralda), SAN (Santander), SUC (Sucre), TOL (Tolima). For Ecuador, the 21 regional units are AZU (Azuay), BOL (Bolivar), CAR (Carchi), CHI (Chimborazo), COT (Cotopaxi), ESM (Esmeraldas), GUA (Guayas), IMB (Imbabura), LOJ (Loja), MAN (Manabi), MOR (Morona Santiago), NAP (Napo), ORE (Orellana), ORO (El Oro), PAS (Pastaza), PICH (Pichincha), RIO (Los Rios), SUC (Sucumbios), TUN (Tungurahua), ZAM (Zamora Chinchipe), ZND (Zonas No Delimitadas). For Paraguay, the 6 regional units are APAR (Alto Parana), ASUN (Asuncion), CAAGU (Caaguazu), CENT (Central), ITAP (Itapua), SPE (San Pedro). For Peru, the 25 regional units are AMA (Amazonas), ANC (Ancash), APU (Apurimac), ARE (Arequipa), AYA (Ayacucho), CAJ (Cajamarca), CAL (Callao), CUZ (Cuzco), HUANCA (Huancavelica), HUANU (Huanuco), ICA (Ica), JUN (Junin), LAM (Lambayeque), LIB (La Libertad), LIMA (Lima), LOR (Loreto), MDD (Madre de Dios), MOQ (Moquegua), PAS (Pasco), PIU (Piura), PUN (Puno), SMA (San Martin), TAC (Tacna), TUM (Tumbes), UCA (Ucayali).

Figure 7. Region-level response of pretax income to an exogenous and unanticipated net-of-tax AMIITR shocks solely driven by legislated tax changes.

Panel A. Basic specification



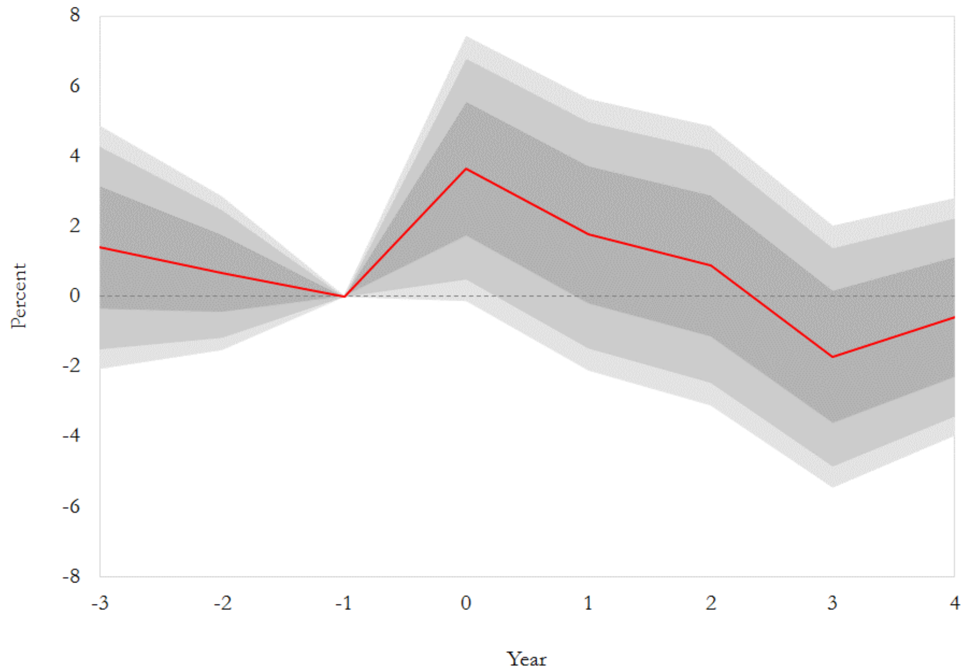
Panel B. Robust specification



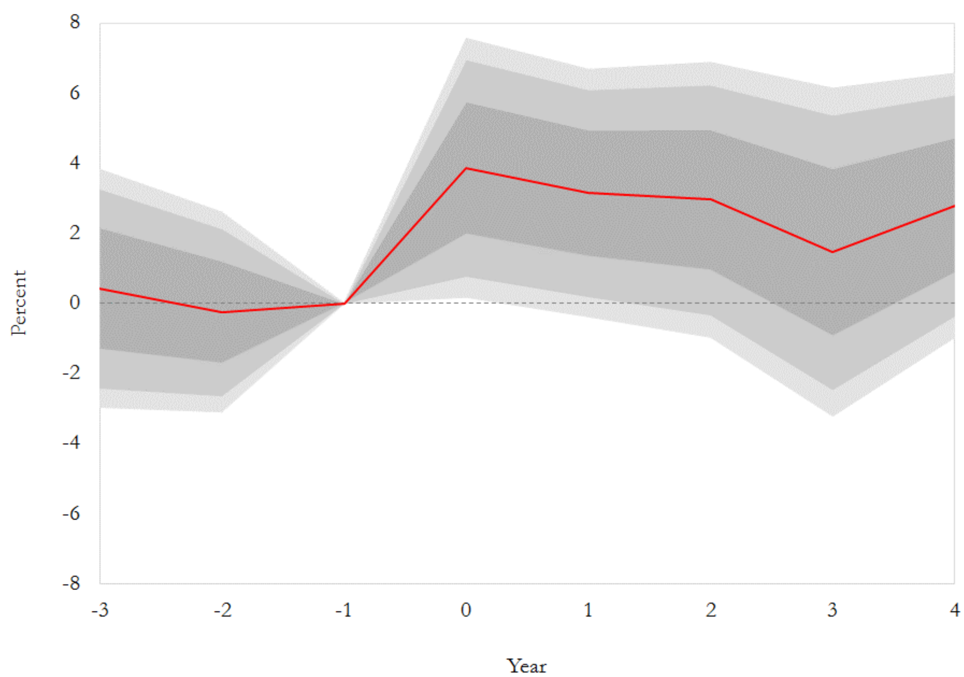
Notes: Dark, medium, and light grey areas show 68, 90, and 95 percent confidence intervals, respectively. The average number of observations for each step-ahead horizon h is 1743 in Panel A and 1253 in Panel B.

Figure 8. Country-level response of pretax income to an exogenous and unanticipated net-of-tax AMIITR shocks solely driven by legislated tax changes.

Panel A. Basic specification

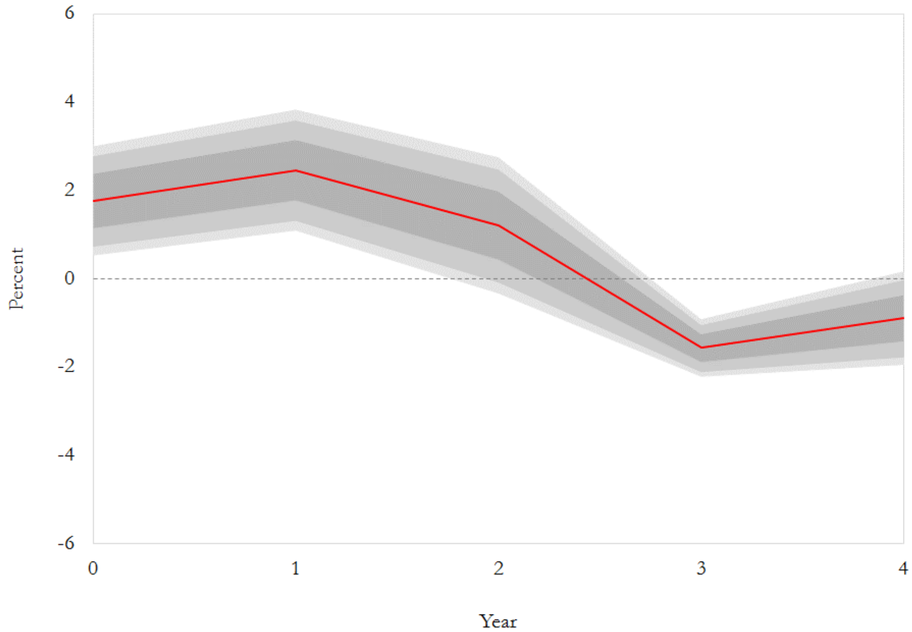


Panel B. Robust specification



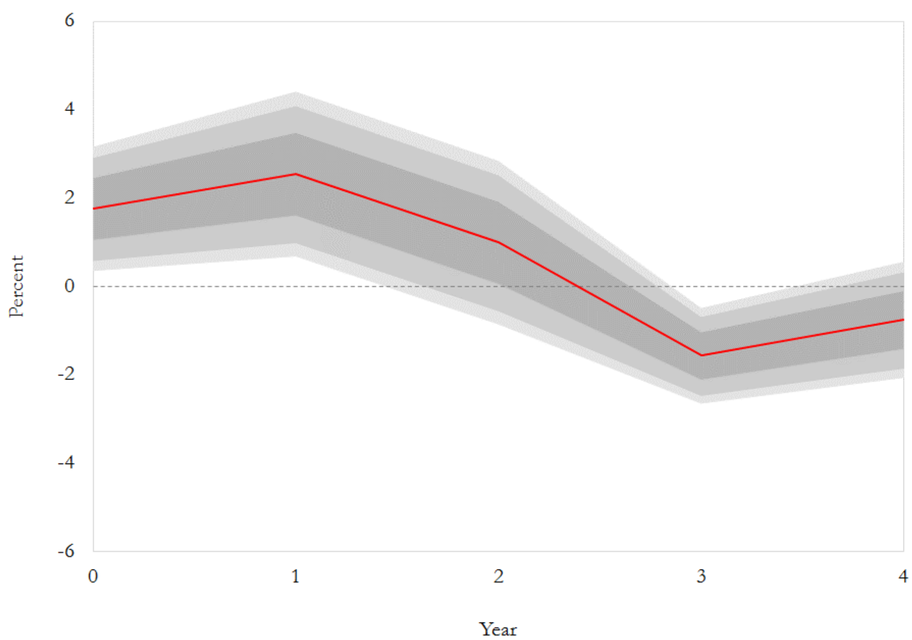
Notes: Average number of observations for each step-ahead horizon h is 68 in Panel A and 51 in Panel B.

Figure 9. Region-level response of pretax income to an exogenous and unanticipated net-of-tax AMIITR shocks solely driven by legislated tax changes. Results from Monte Carlo exercise excluding 20 percent (or 28 out of 139) of regions at-a-time.



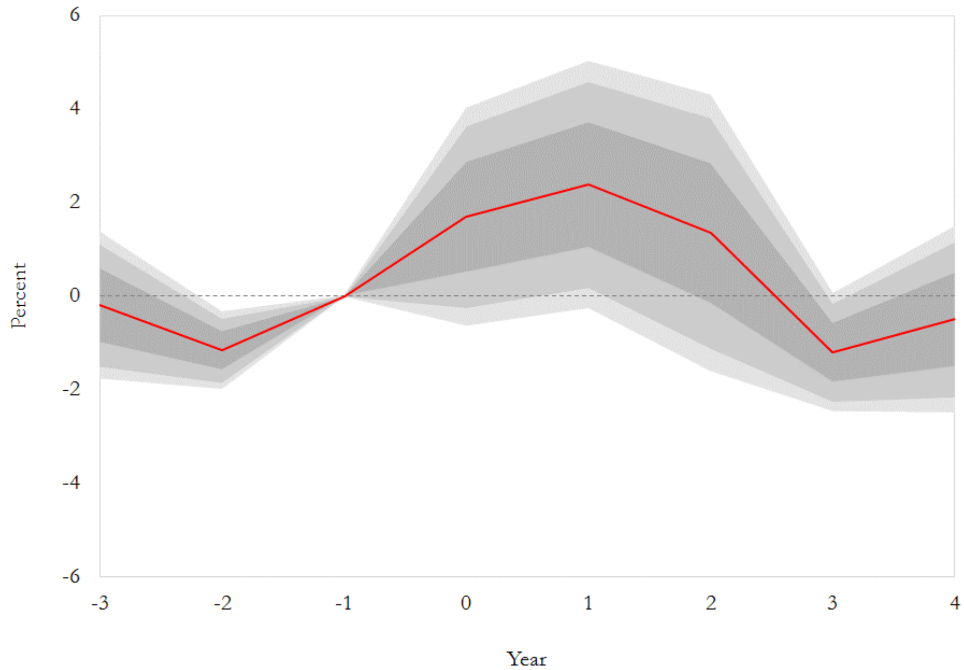
Notes: This Figure is the result of a Monte Carlo exercise conducted with 10,000 repetitions for each time horizon h. In each repetition of each time horizon h, 20 percent (or 28 out of 139) of regions are randomly dropped from the estimation procedure of specification (4). That is to say, the estimation is equivalent to the one reported in Panel B of Figure 9, yet with 20 percent less of regions. After conducting each repetition of each time horizon h, the estimated coefficient is stored. The procedure is repeated 10,000 times for each time horizon h. The red line represents the average coefficient (associated with these 10,000 repetitions) for each time horizon h. Dark, medium, and light grey areas show 68, 90, and 95 percent confidence intervals based on standard deviation of the 10000 coefficients obtained for each time horizon h.

Figure 10. Region-level response of pretax income to an exogenous and unanticipated net-of-tax AMIITR shocks solely driven by legislated tax changes. Results from Monte Carlo exercise excluding 20 percent (or 116 out of 580) of region-based tax shocks at-a-time.



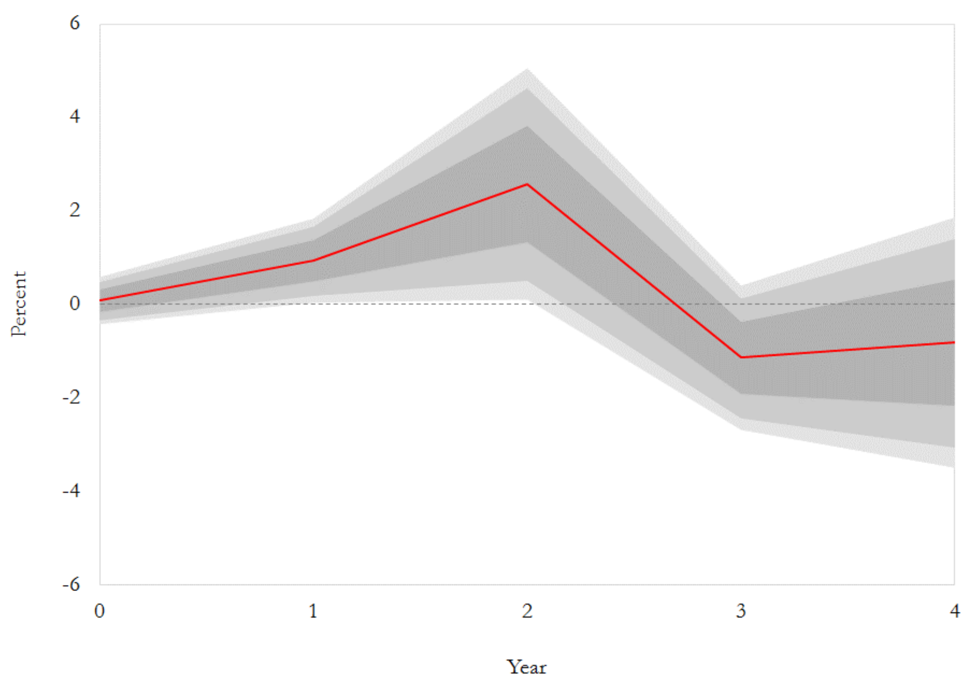
Notes: This Figure is the result of a Monte Carlo exercise conducted with 10,000 repetitions for each time horizon h. In each repetition of each time horizon h, 20 percent (or 116 out of 580) of region-based exogenous and unanticipated individual income legislated tax changes are randomly dropped from the estimation procedure of specification (5). That is to say, the estimation is equivalent to the one reported in Panel B of Figure 9, yet with 20 percent less of region-based exogenous and unanticipated individual income legislated tax changes. After conducting each repetition of each time horizon h, the estimated coefficient is stored. The procedure is repeated 10,000 times for each time horizon h. The red line represents the average coefficient (associated with these 10,000 repetitions) for each time horizon h. Dark, medium, and light grey areas show 68, 90, and 95 percent confidence intervals based on standard deviation of the 10000 coefficients obtained for each time horizon h.

Figure 11. Region-level response of pretax income to exogenous unanticipated and exogenous anticipated net-of-tax AMIITR changes solely driven by legislated tax changes. Robust specification



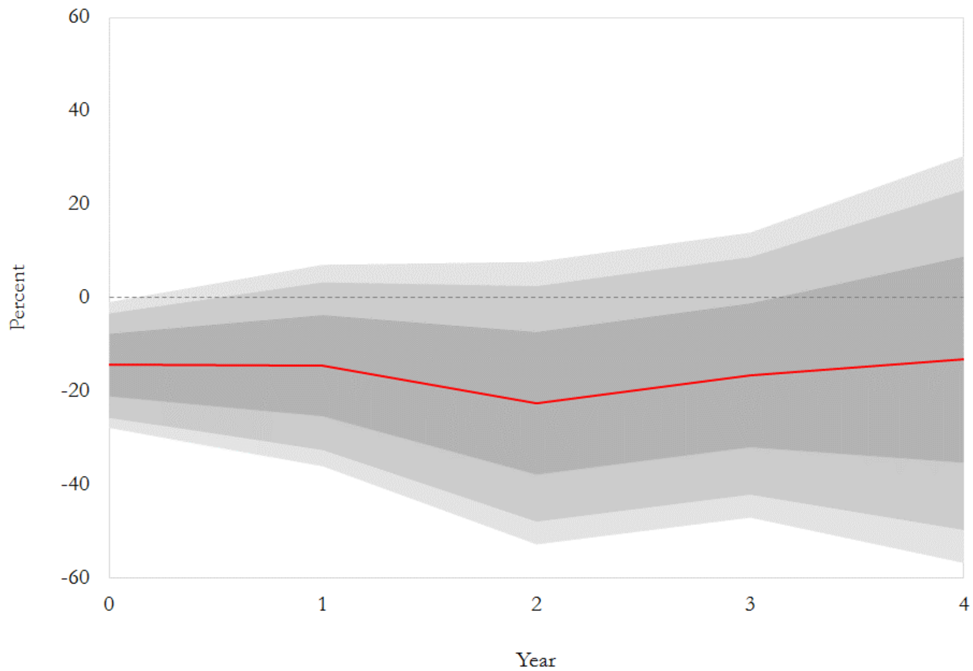
Notes: Average number of observations for each step-ahead horizon h is 1253.

Figure 12. Region-level response of pretax income to exogenous unanticipated and endogenous unanticipated net-of-tax AMIITR changes solely driven by legislated tax changes. Robust specification



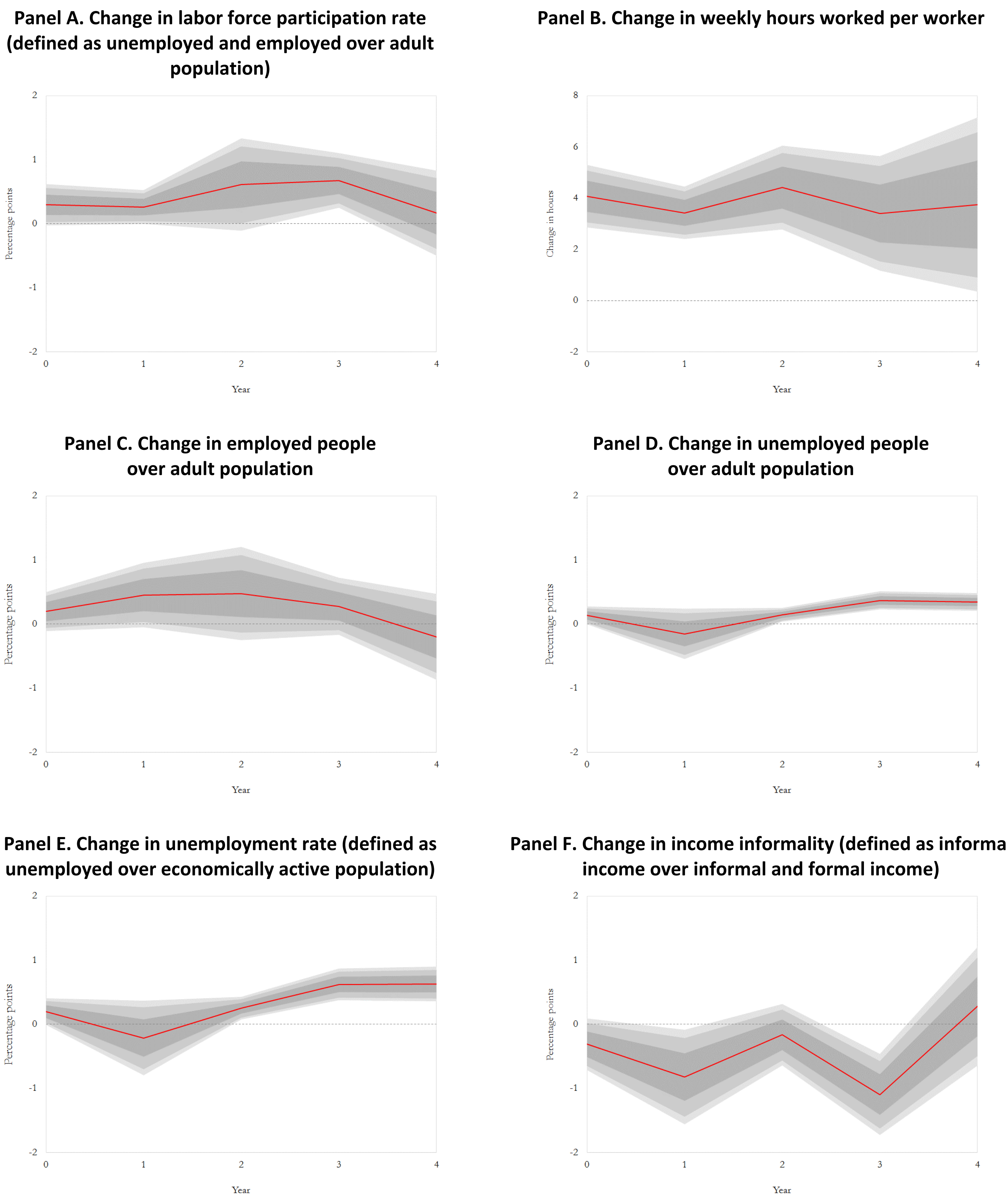
Notes: Average number of observations for each step-ahead horizon is 1253.

Figure 13. Region-level response of pretax income to exogenous unanticipated net-of-tax AMIITR changes driven by legislated tax changes as well as changes in income distribution. Robust specification



Notes: Average number of observations for each step-ahead horizon h is 1253.

Figure 14. Response of labor market extensive and intensive margins to net-of-tax AMPITR changes.



Notes: Adult population is defined as those 15 and older. Economically active population is defined as unemployed plus employed people. Average number of observations for each step-ahead horizon h is 1212 in Panels A, B, and C, 1208 in Panels D and E, and 1189 in Panel F.

Figure 15. Country-level impact response of individual income tax revenues to exogenous unanticipated AMIITR changes. Robust specification