Globalization and Factor Income Taxation*

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Abstract

This paper builds and analyzes a new global macro-historical database of effective tax rates on labor and capital in 155 countries. Effective capital tax rates fell in developed countries between 1965 and 2018, but rose in developing countries since the mid-1990s. Event-studies and instrumental variable regressions show that a significant share of the rise in developing countries can be explained by trade openness, which increases the share of output produced in large corporations, where effective capital taxation is higher. In contrast to a commonly held view, globalization appears in many countries to have supported governments' ability to tax capital.

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1 Introduction

How has globalization affected the relative taxation of labor and capital, and why? Has globalization uniformly eroded the amount of taxes paid by capital owners, shifting the tax burden to workers? Or have some countries managed to increase effective capital tax rates, and if so how? Answering these questions is critical to better understand the macroeconomic effects and social sustainability of globalization.

Based on a new global database of effective tax rates, we document that in developing countries, effective capital tax rates have increased in the post-1995 era of hyper-globalization. Consistently across multiple research designs, we find that a significant part of this rise can be explained by trade liberalization. By expanding economic activity in larger, formal corporate structures relative to smaller informal businesses, trade improves the effective collection of taxes, particularly of corporate taxes. Of course, globalization has also had widely noted negative effects on capital taxation, due to international tax competition. On balance, we find the pro-tax-capacity effect of trade we uncover has prevailed in developing economies. Globalization is an important process in many developing countries but, due to limited data, its revenue consequences have not been systematically investigated. In contrast to a commonly held view, our findings suggest that globalization has not uniformly eroded the ability of governments to tax capital and in fact appears to have supported it in many countries.

To establish these results, this paper makes two contributions. The first is to build and analyze a macro-historical database of effective tax rates on labor (ETR_L) and capital (ETR_K) covering 155 countries, with over half starting in 1965. Each ETR divides all actual taxes collected on the factor by the national income that accrues to it; by relying on actual taxes collected, $ETRs$ capture the net past effect of all statutory tax rules and, importantly in a development context, tax evasion and tax avoidance. Complementary to existing ETR series that focus on high-income countries, our data expands the coverage to developing countries by digitizing and harmonizing thousands of historical and recent public finance records. The global database allows us to systematically characterize the evolution of effective tax rates in developing countries and thus to compare the trends of tax structures across development levels.

A simple and novel fact emerges from this database. We uncover an asymmetric evolution of capital taxation in the era of hyper-globalization. In high-income countries, effective capital tax rates declined, from 36-38% in 1965 to about 30% in 2018.

For instance, in the US, ETR_K dropped from more than 40% in 1965 to 25% in 2018. By contrast, in developing countries, effective capital tax rates have been on a rising trend since the 1990s, albeit starting from a low level. Effective capital tax rates rose from about 10% in the early 1990s to 19% in 2018, with more pronounced increases in larger economies. Between 1990 and 2018, for example, ETR_K rose from 5% to 25% in China, 10% to 27% in Brazil, 6% to 12% in India, and 5% to 10% in Mexico.

This rise of capital taxation in low- and middle-income countries had not been noted in the literature before, due to the limited data on the evolution of tax structures in developing countries. The finding appears to be robust. It holds when we exclude China and oil-rich countries; when we restrict the analysis to a balanced sample of countries; and under different weighting schemes. It holds with alternative approaches to computing capital and labor income in non-corporate businesses, where factor shares are not directly observable. It is also robust to alternative ways of assigning the personal income tax to capital versus labor.

Our second contribution is to formulate and test a hypothesis to shed light on the rise of capital taxation in developing countries. Our hypothesis is motivated by the observation that the increase in ETR_K coincides with their trade liberalization. Between the late 1980s and early 2000s, many countries opened their markets and reduced tariffs, leading to a boom in international trade that reshaped the economy of countries such as Mexico, India, and China. We hypothesize that openness exerts a positive effect on developing countries' capacity to raise taxes, consistent with trade leading to the expansion of larger firms relative to smaller ones (Mrázová & Neary, 2018) and firm-level effective taxation rising with size (revenue) (Almunia & Lopez-Rodriguez, 2018; Basri, Felix, Hanna, & Olken, 2021). By disproportionately benefiting larger firms, trade increases the share of economic activity in firms more likely to be incorporated and formalized, where effective taxation of capital (and labor) is higher.[1]

To test this hypothesis, we implement three research designs. First, we run non-parametric estimations of within-country associations between changes in ETR and changes in trade openness. Second, we analyze major trade liberalization events which occurred in seven large developing countries, including the often-discussed WTO accession of China in 2001 (Brandt, Biesebroeck, Wang, & Zhang, 2017; Goldberg &

[1]Improved effective taxation in the corporate sector stems from both stronger enforcement and higher statutory tax burdens than in the non-corporate sector. The ability to impose a higher statutory tax burden is endogenous to stronger enforcement. Our notion of tax capacity is that these co-determined forces jointly lead to higher ETR_K as a function of firm size.

Pavcnik, 2016). These events caused large and sharp reductions in trade barriers. We use synthetic control methods and present event-study results. Last, we extend the instruments for trade openness presented in Egger, Nigai, and Strecker (2019).

In each case we find that trade leads to a large increase in ETR_K in developing countries (and a smaller increase in ETR_L). The effect is sizable, and suggests trade openness may account for 17-37% of the documented long-run rise in ETR_K. Studying macro-economic outcomes is useful to establish findings in a broad sample of countries, but naturally also presents identification challenges. With these in mind, it is helpful that the results are consistent across the research designs, which differ in their identification strategies, and robust to a large number of sensitivity checks.

The ETR-results shed light on governments' ability to effectively tax capital and labor, which is relevant in developing countries that face revenue constraints. Changes in ETR, however, reflect both the statutory tax code and economic behavior. Investigating mechanisms is therefore important, which we do using IV and event-studies. Consistent with the tax-capacity mechanism, we find that trade increases the share of domestic output from the corporate sector. This leads to a larger share of output being produced in a sector with more effective tax collection (Slemrod & Velayudhan, 2018) and a higher effective capital tax rate. The corporate output-share increased from 55% to 65% in developing countries between 1995 and 2018. Our mechanism is motivated by the conjecture in Abbas and Klemm (2013) that the robust performance of corporate income tax (CIT) collection in these countries is related to the corporate sector's expansion, which Mansour and Keen (2009) in turn relate to globalization.

We find that trade also increases the average effective capital tax rate of the corporate sector. In the tax-capacity mechanism, increases in the corporate sector's output-share and average effective tax rate both raise the country-level ETR_K. The increase in the average effective tax rate may be driven by the trade-induced income accruing to firms where ETR_K increases in size, and it occurs despite trade causing a decrease in the statutory CIT rate, which reflects the race-to-bottom mechanism.

To unpack these results at the corporate sector level, we therefore conduct a firm-level analysis inside the corporate sector in Rwanda. We merge administrative datasets to observe each firm's tax payments and integration to international trade, where the integration measure accounts for the firm's indirect exposure to trade through its production network. Using the shift-share design of Hummels, Jørgensen, Munch, and Xiang (2014) for identifying variation, we find that increased trade integration at the firm level raises the firm's corporate ETR_K and its size. These results provide firm-level evidence for trade's positive impact on ETR_K and support the tax-capacity mechanism whereby the impact is mediated by a positive size-ETR_K gradient.

The IV provides suggestive evidence that trade has a negative effect on ETR_K in developed countries, where additional results show trade has no impact on the tax-capacity mechanism[2] yet causes a decrease in the CIT rate. On net, the trade-induced increase in tax capacity dominates the CIT rate reduction in developing countries and increases ETR_K, but the race-to-the-bottom effect that exerts downward pressure on capital taxation appears to prevail in developed countries.

We find that trade's positive impacts on ETR_K and tax-capacity mechanism hold without, but are enhanced by, domestic tax enforcement policies that developing countries have implemented over time.[3] Trade's effect on ETR_K is also larger in countries with capital flow restrictions and large populations, suggesting that larger markets and capital account management reduce exposure to race-to-bottom effects.

Combining new data and several empirical strategies, our results at the country, corporate sector, and firm-level consistently suggest that increased openness has supported the effective taxation of capital in developing countries and contributed to the newly documented rise in ETR_K since the mid-1990s. Ultimately, despite potential revenue losses at the border, we find that trade's positive impacts on the domestic tax bases of capital and labor are sufficiently large that openness increases overall revenues (as a % of GDP). This is a policy relevant result, as potential revenue losses from trade liberalization remain an important concern amongst practitioners in developing countries (World Bank, 2020). Previous studies on trade's total tax impact have produced mixed findings, owing to differences in methods and samples (including Baunsgaard & Keen, 2009; Buettner & Madzharova, 2018; Cagé & Gadenne, 2018). Our findings derive from implementing multiple identification strategies based on recent empirical methods in the largest sample to date. We conclude by discussing implications for the distributional impacts of globalization (Goldberg, 2023).

Section 2 discusses related literature. Section 3 describes the methodology and data. Section 4 presents findings on the long-run evolution of ETR. Section 5 analyzes trade's impact on ETR and Section 6 investigates mechanisms. Section 7 concludes.

[2]Where the corporate output-share has remained stable at a high level since the 1970s, which may suggest that constraints on effective taxation have been steady and are not as binding in these countries.
[3]Including large taxpayer units (Almunia & Lopez-Rodriguez, 2018) and integrated customs-tax agencies. The literature has yet to establish a causal link between these policies and globalization.

2 Related literature

Globalization and tax structure Our paper contributes to the macro-oriented literature on globalization and tax structure (Alesina & Wacziarg, 1998), reviewed in Adam, Kammas, and Rodriguez (2013). The 'race to the bottom' hypothesis postulates that governments reduce taxes on factors that become more mobile (e.g., capital) following trade liberalization (Slemrod, 2004). To achieve revenue-neutrality, governments raise taxes on the less mobile factor (e.g., labor).[4] The 'social insurance' hypothesis postulates that governments raise revenue to provide insurance for workers displaced by international competition, often through social security and payroll taxes (Rodrik, 1998). These studies have mainly focused on high-income countries. By expanding the scope to developing countries, we formulate and test an additional mechanism, where trade increases ETR by expanding firms with higher effective tax collection. We find that globalization has not uniformly reduced tax capacity and appears in many countries to have supported the ability of governments to tax capital.

Our results are based on a new global dataset of effective tax rates, which complements existing datasets (including Carey & Rabesona, 2004; Kostarakos & Varthalitis, 2020; McDaniel, 2007) by expanding coverage to developing countries (details in Section 3).[5] Our ETR measure is complementary to the literature on forward-looking capital tax rates (including Devereux & Griffith, 1999), which measures in detail the statutory tax burden a firm would face under different conditions. This literature finds that the statutory tax burden on capital has fallen in developed and developing countries (including Devereux, Griffith, & Klemm, 2002; Kumar & James, 2022; Steinmüller, Thunecke, & Wamser, 2019), consistent with the 'race to bottom' mechanism.

Effective taxation and trade in developing countries Our paper contributes to the micro-oriented literature on trade and public finance in developing countries. Most studies focus on evasion of *border taxes* (e.g., Fisman & Wei, 2004; Javorcik & Narciso, 2017) or cross-border income-shifting by firms and individuals (e.g., Bilicka, 2019; Londoño-Vélez & Tortarolo, 2022; Wier, 2020). Our paper focuses instead on the impacts of trade on domestic economic structure and *domestic tax bases* of capital and

[4]Within labor, Egger et al. (2019) find that globalization in the post-1994 era led to a reduction in income taxes for the top 1% of workers and an increase in income taxes for middle class workers.

[5]We complement other work in economic history on taxation (including Cogneau, Dupraz, Knebelmann, & Mesplé-Somps, 2021), by providing long-run measures of factor effective tax rates.

labor.[6] Our results are intuitive when considering that the trade literature finds positive effects on domestic outcomes including market shares (McCaig & Pavcnik, 2018), firm size (Alfaro-Ureña, Manelici, & Vasquez, 2022), and local development (Méndez & Van Patten, 2022), which the public finance literature has separately identified as determinants of effective taxation (Besley & Persson, 2014; Best, Shah, & Waseem, 2021).[7] We contribute by linking these two bodies of work and directly studying trade's impacts on domestic tax bases at the country, corporate sector and firm level.

These impacts are mediated by the tax capacity mechanism, which is rooted in two separately established insights from the trade and public finance literatures in developing countries. First, a large class of models predicts that trade leads to the expansion of large firms relative to small firms (Mrázová & Neary, 2018); for empirical evidence, see Bernard, Jensen, Redding, and Schott (2007). Second, small firms are largely informal, and effective taxation increases with firm size. Bachas, Brockmeyer, Dom, and Semelet (2023) find a positive size-ETR_K gradient on average for corporate firms in 13 developing countries.[8] Effective tax collection is stronger in larger firms such as corporations due to visibility, complex production structures, and employment of many workers (Almunia & Lopez-Rodriguez, 2018; Waseem, 2020). This results in information trails that improve enforcement (Naritomi, 2019; Pomeranz, 2015), though with limits (Carillo, Pomeranz, & Singhal, 2017).[9] Closely related to the mechanism, Abbas and Klemm (2013) conjecture that the corporate sector expansion could explain why the reduction in corporate statutory tax burdens in developing countries has not lead to a reduction in CIT revenue (as a % of GDP).[10] The mechanism is also motivated by studies in high-income countries that link CIT collection to the corporate sector's statutory tax burden, output-share and profitability (Clausing, 2007; Griffith & Miller, 2014; Sørensen, 2007). We focus on a specific mechanism based on size, but many links between international trade, firm structure, and taxation remain to be explored in developing countries (Atkin & Khandelwal, 2020; Parenti, 2018).

[6]The theoretical literature has focused on trade's impact on the optimal indirect tax mix between border and consumption taxes in developing countries (e.g. Emran and Stiglitz, 2005) and mainly abstracted from direct taxes. Benzarti and Tazhitdinova (2021) study the impact of indirect taxes on trade flows.

[7]Our results, which focus on the corporate output-share, are compatible with findings from trade-formalization studies, which instead focus on the share of formal workers or firms (Section 6).

[8]See also Kopczuk and Slemrod (2006), Kleven, Knudsen, Kreiner, Pedersen, and Saez (2011), La Porta and Shleifer (2014), Bachas, Fattal, and Jensen (2019) and Best et al. (2021).

[9]In developed countries including the US, the large corporate sector is considered an important determinant of effective tax collection (Kleven, Kreiner, & Saez, 2016; Slemrod & Velayudhan, 2018).

[10]See also Quinn (1997), Kumar and Quinn (2012) and Abramovsky, Klemm and Phillips (2014).

3 Construction of Effective Tax Rates

This section presents our new database of effective tax rates (ETR) on labor and capital, which covers 155 countries, starting in 1965 when possible, until 2018. We first outline the conceptual framework to build ETR, then present the data sources, and finally discuss the sample coverage. Further details are in Appendix B.

3.1 Methodology

Effective tax rates We compute macroeconomic effective tax rates following the methodology of Mendoza, Razin, and Tesar (1994). The effective tax rate on labor, denoted ETR_L, is the total amount of taxes effectively collected on labor divided by total labor income in the economy; similarly for capital, denoted ETR_K:

$$ETR_L = \frac{T_L}{Y_L} \quad \text{and} \quad ETR_K = \frac{T_K}{Y_K} \tag{1}$$

To construct the numerators, each type of tax revenue is assigned to labor or capital:

$$T_L = \sum_j \lambda_j \cdot \tau_j \quad \text{and} \quad T_K = \sum_j (1 - \lambda_j) \cdot \tau_j \tag{2}$$

where λ_j is the allocation to labor of each type j of tax τ_j. Types of taxes j follow the OECD Revenue classification. We allocate taxes as follows: (1) corporate income taxes, wealth taxes, and property taxes are allocated to capital; (2) payroll taxes and social security payments are allocated to labor; (3) personal income taxes (PIT) are allocated partly to labor and partly to capital, in a country-time specific manner (details below). Indirect taxes are neither assigned to labor nor to capital (but analyzed directly in Section 5.3). Table B3 provides a detailed allocation summary.
To construct the denominators, we decompose net domestic product as follows:

$$Y = Y_L + Y_K = \underbrace{CE + \phi \cdot OS_{PUE}}_{Y_L} + \underbrace{(1 - \phi) \cdot OS_{PUE} + OS_{CORP} + OS_{HH}}_{Y_K} \tag{3}$$

Labor income Y_L equals compensation of employees (CE) plus a share ϕ of mixed income (operating surplus of private unincorporated enterprises, OS_{PUE}). Capital income Y_K equals the remaining share $(1 - \phi)$ of mixed income, plus corporate firms' profits net of depreciation (operating surplus of corporations, OS_{CORP}), plus actual and imputed rental income (operating surplus of households, OS_{HH}).[11]

We also measure the effective tax rate on corporate profits, $\overline{ETR}_C^K$, as the ratio of corporate income taxes to corporate profits. This is an average tax rate at the corporate sector level; in Section 6, we analyze the firm-level corporate effective tax rate.

The ETRs are macroeconomic effective tax rates that provide a backward-looking measure of which factor of production has effectively paid what amount in taxes. Since national account statistics are compiled following harmonized guidelines, these ETRs are conceptually comparable over time and across countries, although a number of data limitations need to be kept in mind (discussed below). By relying on taxes actually collected, the ETRs incorporate the net past effects of all tax rules—including base reductions, exemptions, and tax credits. The ETRs also incorporate all avoidance and evasion behavior; this 'de facto' incidence of actual payment is particularly important in a development context, where evasion is widespread and information on current statutory tax rules provides only a partial insight into the actual tax burden.

The ETRs provide comprehensive measures of the effective tax burdens on capital and labor, that are helpful for three reasons. First, knowing how much revenues are effectively collected from each factor is important when governments face revenue constraints. This is a feature of many developing countries (Besley & Persson, 2014), where potential tax revenue losses or gains is a key factor in policy-making – including in relation to globalization.[12] Second, the level of the effective tax rate, and its deviation from a relevant statutory rate, is often used as an input into policy-evaluation (e.g. the recent focus in international tax reform on minimum effective tax rates). This point is most relevant at the firm and corporate sector levels, where documenting trends in ETRs and investigating their determinants can provide policy-relevant insights. Finally, the tax burden levied on each factor is a starting point, though by no means sufficient, to determine the ultimate economic incidence of a tax system (Section 7).

An important limitation of the macro-economic ETR is that it is impacted by both the tax code and economic changes. As such, studying ETRs is most helpfully done in combination with analyzing its mechanisms - which we turn to in Section 6.

[11] We decompose net domestic product (NDP), which subtracts consumption of fixed capital from gross domestic product (GDP). NDP is lower than GDP, by 10% on average. We exclude capital depreciation since it does not accrue to any factor of production and is usually tax-exempt. Factor incomes also excludes indirect taxes (which are also excluded in the numerator of ETR).

[12] For example, fiscal concerns were a dominant theme during the WTO's Doha Round (Hallaert, 2010).

We emphasize that neither in developed nor in developing countries should the ETR be interpreted as a proxy for the statutory tax burden. An important complementary body of work carefully measures these tax burdens (Devereux & Griffith, 1999). This literature constructs forward-looking average tax burdens on capital, based on the simulated present value of returns and costs of a new investment, which can be used to study the impact of the tax code on incentives to invest and produce.[13] Driven by differing objectives, the backward-looking and forward-looking measures are naturally distinct, but also related. In particular, $\overline{ETR}_C^K$ is closely related to forward-looking measures in the corporate sector, but will differ for two reasons (details in Appendix B.2). First, the denominator in $\overline{ETR}_C^K$ is based on national accounts, where corporate profits differ both conceptually and empirically from how companies' profits might be measured in tax data (including due to tax evasion). Second, due to the underlying tax code, the corporate statutory tax burden can vary across firms that differ in economic characteristics including profitability and size (Devereux, Griffith, & Klemm, 2004; Kumar & James, 2022). Changes in these economic variables will be reflected in $\overline{ETR}_C^K$, but may not necessarily be fully captured in the statutory measures.

Our macroeconomic ETRs rely on several conventions and assumptions (see Carey & Rabesona, 2004). First, as is done in the literature, they do not factor in economic incidence in the sense that the economic cost of taxes is not “shifted" from one factor of production to another; all labor taxes are allocated to labor and all capital taxes are allocated to capital. Second, the tax revenue streams need to be comparable to their macroeconomic tax bases measured in the national accounts. This generates two key challenges for our ETRs: (i) for the numerator, what share of personal income tax revenues to allocate to capital versus labor; and (ii) for the denominator, what share of mixed income to allocate to capital versus labor. We outline below our benchmark assumptions, while an in-depth discussion is provided in Appendix B.2.

Allocation of personal income taxes (PIT) The main empirical difficulty in assigning taxes to labor or capital concerns the allocation of the PIT. A naive procedure allocates 70% of the PIT to labor and 30% to capital, roughly matching the labor and capital shares of domestic product. In practice, however, recent work highlights that not all labor and capital income is subject to PIT, since not all individuals are required to file PIT and exemptions apply to some income types (Jensen, 2022). Exemptions for

[13]See studies cited in Section 2. This literature refers to forward-looking measures as 'effective tax rates' and backward-looking measures as 'implicit rates' (which we refer to as effective tax rates).

capital (e.g., imputed housing rents, undistributed profits) are typically larger than for labor (e.g., pension contributions). Further, labor and capital income might not face the same tax rate: dual income tax systems tax labor income with progressive rates but capital income with flat rates. In the United States, Piketty, Saez, and Zucman (2018) use detailed tax and national accounts data to measure that 75% of labor income was subject to PIT in 2015, versus a third of capital income. This suggests allocating 15% of the personal income tax to capital and 85% to labor.[14]

Starting from this baseline where 15% of PIT revenues derive from capital, we perform two country-year adjustments: (i) we raise capital revenues for country-years with a high PIT exemption threshold in the income distribution (Jensen, 2022); (ii) we lower it in country-years where dividends face lower taxes than wages. The resulting capital share of PIT revenue varies between 7% and 32% across country-years. Over time, this share falls from a global average of 19% in 1965 to 14% in 2018, due to a reduction in PIT exemption thresholds and increased prevalence of dual tax systems.

In the absence of detailed tax records in every country and year, these adjustments provide an imperfect approximation of the true capital share of PIT. We therefore implement two simple robustness checks where the share allocated to capital is fixed over time at either 0% or 30%, representing low and high-end scenarios.

The labor share of mixed income The labor share of mixed income (unincorporated enterprises) is hard to measure.[15] For our benchmark series we assume $\phi = 75\%$, i.e., 25% of mixed income is considered capital income.[16] In the absence of a consensus over alternatives this assumption has the advantage of being transparent, though factor shares are unlikely in practice to everywhere be time and country-invariant. We therefore implement two robustness checks, which create time-year varying measures of ϕ. The first method, based on ILO (2019), uses micro-data to estimate the labor income of self-employed based on the observable characteristics of these workers and

[14] If 75% of labor income is taxable and labor income is 70% of national income (resp. 33% and 30% for capital income), then $75\% \times 70\%/(75\% \times 70\% + 33\% \times 30\%) = 84\%$ of the PIT is labor income.

[15] The UN's national accounts framework outlines the combination of multiple, exhaustive methods to overcome challenges of measuring the *level* of mixed income in economies with widespread informality. While information on the methods used is not readily available on a country-year basis, a careful inspection of the published frameworks over time suggests no change in outlined methodologies, for mixed income or more generally, that coincides with the change in LMIC-trends in Section 4.

[16] This is slightly lower than the 30% used in Distributional National Accounts (DINA) guidelines (Blanchet, Chancel, Flores, & Morgan, 2021), but given that the global average of the capital share in the corporate sector is 27%, assuming that the capital share of unincorporated enterprises is slightly lower may be reasonable (see Guerriero, 2019).

their comparison with employees.[17] Second, we assign to ϕ the observed labor share of the corporate sector (as in Gollin, 2002).

The exact ETR formulas which integrate the above adjustments are in Appendix B.2, including details on time-variant and invariant components.

3.2 Data sources

3.2.1 National income

To measure factor incomes for 155 countries since 1965 when possible, we create a panel of national accounts using data from the System of National Accounts (SNA) produced by the United Nations. We begin by using the 2008 SNA online repository that has global coverage in more recent decades. In turn, the UN Statistics Division provided us with access to the 1968 SNA offline data which covers historical observations from the 1960s and 1970s for most countries. To the best of our knowledge, our paper is the first to harmonize and integrate the 2008-SNA and 1968-SNA datasets. To estimate factor incomes requires information on all the components of national income (equation 3). Whenever we have national income for a country-year in an SNA dataset but information on a component is missing, we attempt to recover it using both information from the second SNA dataset as well as national accounting identities with non-missing values for the other income components. In the remaining cases, we impute component values using methods developed in the DINA guidelines (Blanchet et al., 2021). Details are in Appendix B.1.[18]

3.2.2 Tax revenue

We construct a new tax revenue dataset that dis-aggregates taxes by type following the OECD Revenue Statistics classification of taxes. Our database includes all taxes—on personal and corporate income, social security and payroll, property, wealth and inheritance, consumption and international trade—at all levels of government. We ensure a systematic separation of income taxes into personal and corporate income. We collect new archival data and integrate it with existing data sources.

[17] Details are in Appendix B.2. A challenge with this estimation method is that it can create implausible estimates of the level of mixed income that are much larger than their actual values in national accounts. We implement an adjustment to help with this limitation but also choose, for this reason, to use ILO (2019) as a robustness check.

[18] Relative to recent work (including Guerriero, 2019; Karabarbounis & Neiman, 2014), our data expands coverage in space and time, mainly to developing countries, and systematically attempts to measure factor incomes for total domestic output (vs. only for the corporate sector).

When available, OECD Revenue Statistics data (link) is our preferred source, as it covers all types of tax revenues and goes back to 1965 for OECD countries. It accounts for 2,866 country-year observations (42.3% of the sample). Its drawback is its limited coverage of non-OECD countries, as it covers 93 countries in total and only developing countries more recently. We add data from ICTD (link). ICTD includes most developing countries, with coverage that starts in the 1980s. ICTD sometimes combines personal and corporate income taxes, and sometimes lacks social security. ICTD adds 1,249 country-year observations (18.3% of the sample).

To complement these pre-existing sources, we conducted an archival data-collection to digitize and harmonize data from historical public budgets and national statistical yearbooks. This data exercise adds 2,011 new country-year observations.[19] We supplemented the archival data-collection with countries' online publications and offline data from the IMF Government Finance Statistics (1972-1989). In total, the new data-sources add 2,681 observations (39.4% of the sample).

Building a dataset based on historical sources necessarily requires making a number of decisions. To help increase the credibility of the new data, we follow four principles. First, we seek to build long historical time-series that overlap in years with pre-existing sources. Ultimately, we aim to only use two data-sources per country, but we use the overlapping years between multiple sources to corroborate that they are comparable in levels of revenue collected and types of taxes in place.[20] For this reason, a switch in data-source rarely leads to a significant change in trend. Second, for the historical time-periods where no overlap exists with pre-existing data, we corroborate the levels of tax/GDP with other academic or policy studies.[21] Third, we draw on historical studies to verify that large changes in revenues collected likely reflect policy, economic or political changes rather than data artifacts. Fourth, we aim to be conservative and ultimately exclude countries in time periods where significant concerns exist about data quality, due in part to the economic and political context.

To help assess our approach, we summarize the main considerations and choices relating to these four principles for all 155 countries in Table B2. The table emphasizes the uncertainty surrounding specific countries in certain time periods, and we flag

[19] These archives were accessed in the Government Section of the Lamont Library (website link).

[20] OECD is the preferred starting point and archival data is initially second in priority since it often dis-aggregates tax types and goes back far in time, but we revise this based on the source that best matches the OECD data. Table B2 summarizes the data-sources used for each country.

[21] We only interpolate up to 4 years of gaps in coverage.

instances where the data appears worthy of inclusion but should be interpreted with caution (results are unchanged if we exclude these instances). The supplementary appendix contains a report with 67 country case-studies that provide more details (the report will ultimately cover all countries in our sample). The case-studies also provide direct access to the original historical records used in each country, and we invite comments from researchers to help improve the accuracy of the series as more case studies are built and the data is updated to more recent years.

3.3 Data coverage of effective tax rates

Our final ETR sample contains 6,816 country-year observations in 155 countries (Figure A1). The number of countries starts at 78 in 1965 and grows to 110 by 1975 (due to independence or country creation). The key jump in coverage —from 117 to 148 — corresponds to the entry of ex-communist countries in 1994, including China when it arguably built a modern tax system (Appendix B.1). The data is effectively composed of two quasi-balanced panels. The first covers 1965-1993 and excludes communist regimes, accounting for 85-90% of world GDP. The second covers 1994-2018 and includes former communist countries, accounting for 98% of world GDP. Figure A1 shows coverage by development level. We use the World Bank income classification in 2018, assigning low and middle-income countries (LMICs) as developing countries and high-income countries (HICs) as developed countries. We refer interchangeably to LMICs as developing countries and HICs as developed countries. Our sample contains 5,198 observations in LMICs and 1,618 observations in HICs.

Comparison with pre-existing studies Our database complements previous ETR series by expanding coverage to LMICs. Table B4 summarizes the coverage of pre-existing ETR series, which focus mainly on HICs (Carey & Rabesona, 2004; Kostarakos & Varthalitis, 2020; McDaniel, 2007; Mendoza et al., 1994). Our benchmark ETR formulas are based on a specific set of methodological choices, and different choices could be made. In Appendix B.2, we discuss the methodological differences with the pre-existing ETR series, which relate mainly to allocating capital to mixed income and PIT. We discuss how the alternative methodological choices are in practice covered by the robustness checks outlined in Section 3.1 (and implemented in Section 4.2).

Our database on $\overline{ETR}_C^K$ relates to the measure of CIT-efficiency in LMICs produced by IMF (2014). In the supplementary appendix we find that CIT-efficiency measured using our data but in the IMF's sample matches well the IMF (2014) series.

4 Stylized Facts on Global Taxation Trends

4.1 Evolution of effective tax rates on capital and labor

Figure 1 documents the global evolution of effective tax rates on capital and labor from 1965 to 2018. These time series follow our benchmark assumptions. Aggregates are dollar-weighted, i.e., the global effective tax rate on capital equals worldwide capital tax revenues divided by worldwide capital income. This series can be interpreted as the average tax rate on a dollar of capital income derived from owning an asset representative of the world's capital stock. The top panel shows global trends and the bottom panels separate trends between HICs and LMICs.

Globally, effective tax rates on labor and capital converged between 1965 and 2018, due to a rise in labor taxation and a drop in capital taxation. The global ETR_L rose from 16% in the mid-1960s to 25% in the late 2010s, while ETR_K fell from 32% to 26%. The decline in capital taxation is driven by the corporate sector: the global effective tax rate on corporate profits fell from 27% in 1965 to 18% in 2018.

The global trends mask heterogeneity by development levels. While labor taxation rose everywhere, the decline in capital taxation is concentrated in HICs, where the effective tax rate on capital fell from 36-38% to about 30% between 1965 and 2018. In contrast, ETR_K increased in LMICs, albeit from a low baseline: it rose from 10% to 19%, with the increase happening entirely since the 1990s. The secular decline in ETR_K in HICs has been documented before (Dyreng, Hanlon, Maydew, & Thornock, 2017; Garcia-Bernardo, Janský, & Tørsløv, 2022), but the rise in ETR_K in LMICs starting in the 1990s is novel. We therefore need to establish that this result is robust to the assumptions used to construct the ETR series.

4.2 The rise of capital taxation in developing countries

When creating our ETR series, we make four key methodological decisions: (1) how to allocate PIT revenue to capital vs labor; (2) how to allocate mixed income to capital vs labor; (3) balanced vs. unbalanced panel of countries; (4) use of weights to aggregate countries. Our benchmark series: (1) allocates PIT to capital vs. labor for each country-year using data on tax exemption thresholds and differential tax treatment of dividends relative to wages; (2) allocates 25% of mixed income to capital; (3) consists of two quasi-balanced panels before and after 1994 (when China, Russia and other former command economies enter the sample); and (4) weighs countries using their share of worldwide factor income in each year. We assess how results change when varying one, several, or all of these choices at the same time.

Figure 2 tests the robustness of the ETR_K trend in LMICs.[22] Panel (a) varies the allocation of personal income tax (PIT) revenue. While our benchmark assignment is based on a country-year varying allocation, we consider two simple robustness scenarios where the share allocated to capital is fixed over time, at either 0% or 30% (low and high-end scenarios, respectively). Due to high PIT exemption thresholds in developing countries, the benchmark country-specific assignment is closer in levels to the 30% than to the 0% allocation. Though the capital share allocated to PIT does change over time (Section 3.1), the time-invariant robustness series track the trends in the benchmark series closely. This occurs because the PIT remains limited in scope in LMICs, meaning its split into labor vs. capital makes little difference for our results.

Panel (b) shows the effect of varying our assumptions on the labor share of mixed income (unincorporated enterprises). We implement two robustness checks, creating country-year varying mixed income labor share based on either the ILO (2019) method or the labor share in the corporate sector. Both robustness series are slightly below the benchmark ETR_K in terms of levels but track its evolution closely over time.

Panel (c) quantifies the effect of country entry into the panel.[23] In our benchmark series, China, Russia, and other former command economies enter in 1994. In this robustness check, we balance the panel by imputing missing country observations between 1965 and 1993; we use the observed value of ETR_K for that country in 1994 and the trends in ETR_K observed for LMICs with data 1965-1993. This imputation raises ETR_K between 1965 and 1993, since the new entrants (especially Russia) have relatively high ETR_K and a high global weight when they enter the sample in 1994.

Panel (d) aggregates countries using net domestic product (NDP) weights, instead of capital income weights. The NDP weights are either time varying or fixed in 2010. The figure shows that the weighting procedure has limited impact on the results.

Finally, panel (e) considers all 54 combinations of the 4 methodological choices: the rise in ETR_K in LMICs between 1994 and 2018 is clearly apparent in all 54 series. How wide is the range of increases and how does our benchmark series compare? Computing the 1994-2018 change in the 54 series, we obtain a meaningfully tight range

[22]Figure A2 shows the robustness checks for ETR_L in LMICs and ETR_L and ETR_K in HICs.

[23]For many developing countries with a switch in tax data-source, this occurs in the late 1980s-early 1990s – prior to the observed break in ETR_K trend in the mid-1990s. Moreover, due to our data collection approach, the switch in source is rarely associated with a change in trend (Section B.1).

of ETR_K increases between 5.6% and 8.9%, with our benchmark series in the middle at 7.6% (with larger increases in 21 series and smaller increases in 32 series).[24]

Comparison with pre-existing studies Recall that the pre-existing ETR series mainly covered OECD and European countries. Implemented in the exact samples of those studies, the trends are comparable between our benchmark ETR and the alternative ETR series (Figure B1), though they differ, on average, by 16.5% in levels. This wedge is due to differences in methodology and data-sources, which we review in Appendix B.2. We discuss how the methodological differences relate primarily to varying assumptions for the allocation of capital to PIT and to mixed income, and how the alternative assumptions are in practice covered by our robustness checks. Consequently, the series that would result by applying the methodologies from the pre-existing studies to our sample are in effect contained within the range of ETR trends produced across the 54 robustness combinations of methodological choices. In HICs, this range for ETR_K-trends across the 54 combinations is quite wide (Figure A2). But, as noted above, the range of ETR_K-trends for our novel finding in LMICs is meaningfully tight. As we shall see in Section 6, this is because the rise in ETR_K in LMICs is driven by mechanisms (the corporate sector's output-share and average effective tax rate) that are not strongly affected by the methodological differences between our study and pre-existing studies.

4.3 Where has capital taxation risen the most?

Figure 3 shows the evolution of ETR_K for major developing countries and sub-samples of countries. Panel (a) plots the ETR_K series for the four largest LMICs: Brazil, China, India, Indonesia. All display a marked increase in ETR_K since the early 1990s: from 10% to 27% in Brazil, 5% to 25% in China, 6% to 12% in India, and 10% to 15% in Indonesia. China's global income weight implies that it plays an important role in the aggregate rise in ETR_K in developing countries.

Panel (b) plots ETR_K in sub-samples of developing countries. When excluding China, the rise in ETR_K is more muted, going from 10% in 1989 to 14% in 2018. Oil-rich countries (defined as deriving at least 7% of GDP from oil in 2018) have volatile corporate tax revenues. Excluding oil-rich countries yields a more pronounced ETR_K

[24]Setting 1994 as the base year is partly arbitrary, but at that time all countries have entered the sample and, as argued later, the 1990s correspond to a period of rapid trade liberalization. If we instead compute the change in ETR_K between 2018 and the lowest point in a given series, the range of changes is 6.9%-11.6% across the 54 series, with our benchmark series at 9.3%.

rise (from 10% in 1989 to 23% in 2018), and a flatter ETR_K series pre-1989 as the revenue impacts of the 1970s oil shocks are removed. If we exclude both China and oil-rich countries, we again observe a substantial rise in ETR_K.

Panel (c) shows that, among non oil-rich countries, the ETR_K rise is stronger in the 19 largest LMICs (with a population above 40 million in 2018). Even when excluding China, the ETR_K of the other 18 most populated countries rose from 9% to 17% between 1989 and 2018; in smaller countries, ETR_K rose from 9 to 14%.[25] The ETR_K has risen by more than 5 percentage points in 13 of the 19 largest LMICs in the past 30 years, and has only fallen in one country (Russia).[26] In short, the rise in effective capital taxation in LMICs is more pronounced in larger economies, including China, but appears to be a general pattern in developing countries.

4.4 Suggestive evidence for the role of globalization

We found that ETR_K has fallen in HICs but risen in LMICs. The rise in ETR_K in LMICs is robust to our assumptions and, while more pronounced in larger countries, is a widespread pattern. Importantly, this rise occurred in the 1990s to early 2000s, during the period of "hyper-globalization" which should a priori have made capital more mobile and harder to tax. Instead, could globalization have caused a rise in ETR_K in LMICs? Here we take a first pass at investigating this question. We create 5-year growth rates within countries in trade and ETRs. We plot binned scatters of ETR against trade openness (measured as the share of imports and exports over NDP), after residualizing all variables against year fixed effects. Figure 4 depicts these within-country associations, which condition on global time trends. Mirroring the heterogeneity in long-run trends, we observe differences by development level in the association between trade and ETR_K: openness is associated with increases in ETR_K in LMICs, but with decreases in ETR_K in HICs.[27] In sum, from a global and historical perspective, the correlational evidence suggests that trade may have contributed to the newly documented rise in ETR_K in developing countries.

Naturally, LMICs have undergone significant development since the 1960s and this growth is likely to also have contributed to the long-run rise in ETR_K. In supp.

[25] As discussed below, we find larger trade-impacts on ETR_K in more populous countries (Section 6.5).
[26] The supplementary appendix (link) shows the individual countries' ETR_K and ETR_L time series.
[27] Figure A3 further shows that early globalized LMICs (pre-1995) saw trade and ETR_K rise in tandem prior to the 1990s and stagnate thereafter. By contrast, LMICs which participated in the second wave of globalization post-1995 saw a rise in trade and ETR_K in the 1995-2018 period.

appendix, we do find that the associations in Figure 4 hold in LMICs when controlling for GDP per capita growth. The correlational evidence in this section, combined with the observation that globalization is an important process in LMICs whose revenue impacts have not been precisely established (Section 1), motivate us in the next sections to investigate trade as a determinant of ETR and study its mechanisms.

5 Globalization and Capital Taxation

In this section, we implement two distinct research designs to investigate the impact of trade openness on capital and labor taxation in developing countries.

5.1 Event-studies for trade liberalization

5.1.1 Empirical design

In the first design, we implement event studies of trade liberalization policy events in key developing countries. To discern sharp breaks from trends in our outcomes, our selection criteria was to select events which caused large trade barrier reductions and which have been studied in the literature. This led us to select the six events from the review papers by Goldberg and Pavcnik (2007, 2016) (Colombia in 1985, Mexico in 1985, Brazil in 1988, Argentina in 1989, India in 1991, Vietnam in 2001), and add the well-known event of China's accession to WTO in 2001 (Brandt et al., 2017). These liberalization events led to large reductions in tariffs: from 59% to 15% in Brazil; 80% to 39% in India; and, 48% to 20% in China. We can rely on pre-existing narrative analyses to discuss threats to identification and interpretation of results.[28] Appendix C.1 provides more details on our selection criteria and the liberalization events.

For each of the seven treated countries and outcomes, we construct a synthetic control country, as a weighted average over the donor pool of never-treated countries (Abadie, Diamond and Hainmueller, 2010).[29] We match on the level of each outcome in the 10 years prior to the event, while minimizing the mean squared prediction error between the event-country and the synthetic control (Table A1).[30] We plot the average levels of the outcome variable for treated countries vs. synthetic control countries by

[28]The reductions in trade barriers are sometimes implemented over several years. To be conservative, we focus on the earliest start year for each event as defined in published studies.

[29]For each country-event, we can include eventually-treated countries in the donor-pool (excluding those with treatment within 5 years of the event); the results, available upon request, are similar.

[30]Table A1 details the synthetic control matching for each event and each outcome.

relative time to the event. We also estimate the event-study model in the 10 years before and 10 years after the events:

$$y_{ct} = \sum_{e=-10, e\neq -1}^{10} \beta_e \cdot \mathbb{1}(e=t)_t \cdot D_c + \theta_t + \kappa_c + \pi_{Year(t)} + \epsilon_{ct} \tag{4}$$

where we include fixed effects for event-time, θ_t, country κ_c, and calendar year, $\pi_{Year(t)}$ (the latter control for shocks that correlate with events clustered in calendar time). D_c is a dummy equal to one if country c is treated. The coefficient β_e captures the difference between treated and synthetic control countries in event time e, relative to the pre-reform year $e = -1$ (omitted period). Since inference based on small samples is challenging, we plot 95% confidence bounds using the wild bootstrap, clustered at the country event level. In Table A2 we estimate the simple difference-in-differences, which captures the average treatment effect in the 10 years post-liberalization, and the imputed treatment effect based on Borusyak, Jaravel, and Spiess (2021), which addresses challenges from two-way fixed effects and heterogeneous event-times.

5.1.2 Event-study results

Figure 5 displays the event studies in levels (left-hand panels) and the dynamic regression coefficients (right-hand panels). The top panels show that, as expected, trade rises in the year of the event and its trend changes in post-reform years compared to pre-liberalization years.[31] ETR_K sharply rises following the liberalization event. Both ETR_K and ETR_L break from stable pre-trends at the time of liberalization, but the effect on capital taxation is double that on labor. Despite the small sample size, the dynamic post-treatment coefficients are often significant at the 5% level. The p-values for the joint significance of all post-reform dummies are well below 0.05. Panel A of Table A2 reports the DiD results, which are marginally more significant when estimated from imputed treatment effects. Panel B shows the effects remain comparable when we jointly match on all outcomes for each country-event.

The identifying assumption is that there are no changes in confounding determinants of ETR which coincide with the liberalization events. The breaks from stable pre-trends imply that confounding changes would have to sharply coincide with the event onset. Narrative analyses of the timing for each event (Appendix C.1) do not

[31]The absence of a pre-reform dip limits concerns about inter-temporal substitution, although some of the liberalization events may have been predictable, including China's WTO accession.

suggest obvious concurrent changes. Moreover, using data from Wacziarg and Wallack (2004), we verify that other cross-border reforms (e.g. capital liberalization) or domestic reforms (e.g. privatization) do not occur in the same year as the trade events.[32] Such reforms do, however, sometimes occur in the post-event years; for example, Mexico joined NAFTA and removed capital inflow restrictions, Argentina and Brazil joined MERCOSUR, and India liberalized its FDI rules (Appendix C.1). These cross-border reforms occurred several years after the trade events,[33] yet ETRs sharply rise in the immediate post-event years. This discussion highlights that the interpretation of trade centered macroeconomic reforms requires caution. A plausible interpretation is that the short-run increases in ETRs with sharp breaks from stable pre-trends reflect the impact of trade liberalization, but that the medium-run coefficients also reflect the impacts of additional, mainly cross-border, reforms.

We conduct further analyses to probe the identification and robustness of our results. First, given the limited number of liberalization events, we verify that the average effects are not unduly influenced by one particular event. Figure A5 removes one treated country at a time: the dynamic treatment effects for all subsets of events are similar to the full sample. Second, the limited set of events arose from our specific selection criteria (Section 5.1.1). In Appendix C.3, we study the robustness to using (very) different selection criteria for trade liberalization; we re-estimate the event-study using the 68 liberalization events in LMICs from Wacziarg and Welch (2008) and find comparable impacts on ETR. Third, in Table A2 we address concerns related to the control group. We find similar results when we remove from the donor pool each liberalizing country's 5 major export and import trading partners (measured in the immediate pre-event years), alleviating concerns of spill-over to countries in the synthetic control.[34] Results are comparable when the donor pool excludes countries that have already liberalized (based on Wacziarg & Welch, 2008), to guard against the concern that the trends in the synthetic control group reflect longer-run effects of the treatment (liberalization). Finally, to lessen the concern that treated and control

[32]Only Mexico had a concurrent domestic reform, and results hold without it (Figure A5).

[33]Only Brazil and Colombia implemented domestic reforms in the post-liberalization years.

[34]We also verify that none of the main countries in the synthetic control (Table A1) had external or domestic reforms in the event-year or in the post-event periods (using the data in Wacziarg & Wallack, 2004). Consistent with this, the levels of the outcomes in the synthetic control are relatively stable throughout the event periods. Finally, note that if the spillovers correspond to coordination of policies, this would likely bias our estimation towards finding null effects.

countries experience different unobservable shocks, we find similar results when the donor pool for each treated country is restricted to the same region (or to LMICs).

5.2 Regressions with instrumental variables for trade

5.2.1 Empirical design

Our second design employs instrumental variables for trade. One attractive feature is that the IV provides causal estimates under different identifying assumptions than the event-study. Moreover, while it is harder to directly inspect the identifying assumptions than in the event-study, the IV permits a precise investigation of mechanisms (Section 6). We estimate the following model in developing countries:

$$y_{ct} = \mu \cdot trade_{ct} + \Theta \cdot X_{ct} + \pi_c + \pi_t + \epsilon_{ct} \tag{5}$$

where y_{ct} is the ETR in country c in year t, $trade_{ct}$ is the share of import and exports in NDP and π_c and π_t are country and year fixed effects.[35] We cluster ϵ_{ct} at the country level. X_{ct} contains confounding determinants of ETR: the exchange rate, gross capital formation, log of population, and capital openness (Chinn & Ito, 2006; Rodrik, 1998). ETR time-series are sometimes volatile (Figure 3), so we winsorize ETR at the 5%-95% level by year separately for LMICs and HICs.

OLS estimation of equation (5) may be biased due to reverse causality and unobservable confounding factors which correlate with trade. To try to address these issues, we use the two instruments for trade from Egger et al. (2019). The first instrument, denoted $Z^{gravity}$, relies on the structure of general equilibrium models of trade. Under the standard gravity model assumptions, it uses the average bilateral trade frictions between exporting and importing countries as variation (aggregated to the country-year level). This instrument is valid if the distribution (not the level) of trade costs among individual country-trading pairs is not influenced by ETRs in the import or export country. The second instrument, denoted $Z^{oil-distance}$, interacts time-series variation in global oil prices with a country-specific measure of access to international markets. Access is captured by the variance of distance to the closest maritime port for the three most populated cities. This time-invariant measure captures the internal geography of a country and impacts transportation costs: following

[35]We include fixed effects for imputed and interpolated values, as well as for each tax and national account data-source (Section 3.2), to ensure results are not driven by changes in data-quality. Results also hold without imputed/interpolated values and within each data-source (Table A4).

a global shock to oil prices, transportation costs will be higher in countries with less concentrated access to ports, leading to a larger drop in imports and exports. This instrument is valid if the interaction between global oil prices and country-specific measures of spatial concentration is uncorrelated with changes in ETRs. Conceptually, both instruments capture variation in trade costs driven by economic forces that are plausibly exogenous to ETRs and their determinants (details in Appendix D).

In LMICs, Figure A4 shows the 1^{st}-stage is stronger in the 2000s and at higher income levels for Z^{oil}, and in earlier periods and at lower income levels for $Z^{gravity}$. Restricting the analysis to sub-samples where one of the instruments has a strong first-stage introduces bias (Mogstad, Torgovitsky, & Walters, 2021). Instead, we combine the two instruments to estimate a local average treatment effect that is representative of LMICs across income levels and time periods. Table A3 shows the 1^{st}-stage.[36]

5.2.2 Instrumental variable results

Table 1 presents the results in LMICs for ETR_K in Panel A and ETR_L in Panel B.[37] In column (1), OLS uncovers positive, significant associations between trade and both ETRs. In column (2), we employ the two instruments. The 1^{st}-stage Kleibergen-Paap F-statistic is 24.57. The IV shows that trade causes an increase in both effective tax rates, but the magnitude is twice as large for ETR_K (0.109) than for ETR_L (0.056).

In the remaining columns, we conduct three sets of robustness checks. In the first set, we modify the specification. Column (3) shows that the results remain unchanged when we use non-winsorized ETRs. Column (4) re-estimates the IV with NDP weights (used in Section 4 for representativity), which increases magnitudes but decreases statistical significance. Results remain similar in column (5) when we include controls in X_{ct}. Results also hold when controlling for GDP per capita (not shown). In column (6), results are robust to allowing oil-rich countries to be on a separate non-parametric time path, addressing the concern that the identifying variation for $Z^{oil-dist}$ is correlated with trends in ETRs specific to oil-rich countries (Figure 3). In column (7), results remain similar when we winsorize the trade variable.

[36]Table E1 shows the instruments impact imports and exports, and trade in intermediate goods-services (G-S) and final G-S. Thus, our IV-estimates comprehensively reflect the impacts of trade through rises and falls in final and intermediate goods and services that flow both in and out of the country.

[37]There is a 4% drop in sample size relative to ETR coverage (Section 3.3) due to availability of instruments. Relative to previous versions of this paper, recent access to trade data from Harvard Growth Lab increased the sample size for the instruments and led to updated results.

In the second robustness set, we implement the alternative capital vs labor assignments from Section 4.2. In our benchmark, the capital share of mixed income is time-invariant, yet trade may cause factor shares to change. In columns (8)-(9), we allow factor shares to respond to trade by implementing the two methods which create country-year varying capital-shares of mixed income; results remain comparable. Results also remain similar when we assign the K-share of PIT to be 0% (column 10) or 30% (column 11). In the third robustness set (columns 12-13), we estimate IVs using each instrument separately. The 1^{st}-stage F-statistic is 45.17 for $Z^{gravity}$ and 10.80 for Z^{oil}. The IV estimates are comparable, though larger when based on Z^{oil}.

Leveraging the opposite sign effects of the two instruments on trade, the reduced form results (Table A3) suggest that openness effects are symmetric: increased trade increases both ETR_L and ETR_K, while reduced trade decreases both ETRs.

Finally, our results are based on an unbalanced panel and multiple data-sources (Section 3.2- 3.3). In Table A4, we find that the results are qualitatively similar within each data-source for taxes (newly digitized government records; OECD; ICTD) and national accounts (SNA1968; SNA2008), as well as in both quasi-balanced panels (pre and post-1994) and in a strongly balanced panel (1965-2018).[38]

Taking stock How much of the ETR_K rise in LMICs since the 1990s can be accounted for by increased trade? Between 1994 and 2018, ETR_K rose by 7.6ppt (Section 4.2) and trade by 12.8ppt. The NDP-weighted IV for trade's impact (col.4 of Table 1) is arguably most comparable, since the ETR_K trends in Section 4 are also weighted by national income. Using this estimate would imply that trade openness can account for 37% of the rise in ETR_K ($0.222 * 0.128/0.076 = 0.374$). Considering all estimates in Table 1 generates a range of 17%-37% (main specification in column 2 at 19%).[39]

5.3 Impacts of trade openness on overall taxation

We find positive effects on capital and labor taxes, but what are the implications for trade's impact on *overall* revenues? This is a relevant question, as trade-induced revenue losses remain an important concern amongst practitioners (World Bank, 2020). We investigate trade's impacts on total taxes (% of NDP) in LMICs in Table 2. Total taxes include direct taxes on capital and labor and indirect taxes (sum of taxes on trade and domestic consumption).[40] Both in OLS and IV, the trade-coefficient for total tax

[38] Variation between coefficients may reflect data-quality or 1^{st}-stage and treatment heterogeneity.
[39] For reasons discussed in 5.1.2, we do not rely on the event-study estimates for this exercise.
[40] Long-run trends in taxation by type and development level are in the supplementary appendix.

collection is positive and significant. This increase in total revenues is mainly driven by corporate income taxes and social security.[41] Trade has a statistically insignificant impact on indirect taxes. Trade's impact on total taxes is robust to: using NDP-weights; including controls; winsorizing trade; using each instrument separately (Table A5).

We can also study the impact of the trade liberalization events from Section 5.1 on total tax revenue. Using the event-study methodology, Figure A6 shows that the trade events led to an increase in overall tax collection, with a break from stable pre-trend.

One limitation is we do not separately study openness' impacts on trade and consumption taxes, as our data does not contain a systematic breakdown. This reflects our initial focus on direct capital and labor taxes, but additional data-work would permit a systematic measure of these indirect taxes. Interestingly, the sign of openness' impact on trade taxes may differ if the reduction in trade costs is initially due to economic forces (as in the IV) versus policy changes (as in the event-study), while we find positive impacts in both cases on domestic capital and labor taxes, and total taxes.

Both the event-study and the IV indicate that openness leads to an increase in overall tax take in LMICs. These results relate to the pre-existing literature on trade's net impact on total tax collection, which has produced mixed findings due to differences in samples, measures and empirical strategies (Section 1).[42] We contribute by providing multiple identification strategies that are based on recent empirical methods and that are implemented in the largest sample of developing countries to date.

In summary, the IV and event-study results in this section consistently show that trade increases ETR_K and ETR_L, as well as the total tax take. In the next section, we investigate mechanisms for trade's impact on ETR_K.

6 Mechanisms

6.1 Outlining the tax capacity mechanism

The *tax capacity* mechanism combines two separate insights from the trade and public finance literatures: trade leads to the expansion of large firms relative to small firms; effective taxation increases with firm size (measured by revenue).[43] To study this

[41] CIT revenue, as a share of GDP, grew by 59.6% in LMICs between 1965 and 2018 (supp. appendix).

[42] An important study in this literature, Baunsgaard and Keen (2009) write in their conclusion: "it is possible that indirect effects operating through higher levels of openness and income consequent upon trade reform have more than offset the direct loss of revenue identified here."

[43] See studies cited in Section 2. For example, Best et al. (2021) uncover a negative size-evasion gradient using randomized audit data on firms in Pakistan, finding also that firm-size is the most significant

mechanism we focus on corporations, given the evidence on effective taxation in these larger firms (Section 2) and because outcomes related to corporations are consistently defined and measured in national accounts and tax data.[44] The role of corporations can be seen in the following decomposition of ETR^K (in a given country-year):

$$ETR^K = \int_{i \in C} ETR_i^K f(i)\, di + \int_{i \in NC} ETR_i^K f(i)\, di \tag{6}$$

$$= \mu_C^K \cdot \overline{ETR}_C^K + (1 - \mu_C^K) \cdot \overline{ETR}_{NC}^K \tag{7}$$

where μ_C^K is the corporate share of (capital) national income of agents i with density $f(i)$, and $\overline{ETR}_C^K$ and $\overline{ETR}_{NC}^K$ are the average effective tax rates on capital in the corporate (C) and non-corporate (NC) sectors, respectively.

In national accounts, $\overline{ETR}_C^K$ is the average effective tax rate on corporate profits, which is measurable in our data.[45] In LMICs, $\overline{ETR}_C^K$ is 50% larger than the overall ETR^K (19.9% versus 13.3%). This improved effective taxation in the corporate sector stems from both stronger enforcement and higher statutory tax burdens than in the non-corporate sector. The ability to levy higher statutory taxes is endogenous to stronger enforcement (Bergeron et al., 2023). The tax capacity mechanism considers that these co-determined elements jointly lead to higher ETR^K as a function of size.

In equation (6), trade can impact the corporate share of national income (μ_C), through two distinct channels (see Dix-Carneiro, Goldberg, Meghir, & Ulyssea, 2021). First, trade openness can lead to increased market opportunities that disproportionately benefit large exporters (Melitz, 2003), causing an increase in the market income-share of firms that are initially larger and likely to be corporations (McCaig & Pavcnik, 2018). Second, trade can expand the availability of intermediate goods and lower their prices, which may also disproportionately benefit initially larger firms (for example due to fixed costs as in Kugler & Verhoogen, 2009), and similarly cause an increase

predictor of evasion. Models of tax compliance provide micro-foundations for the negative size-evasion gradient (including Kleven et al., 2016; Kopczuk & Slemrod, 2006).

[44] Several theories predict heterogeneous trade impacts on ETR across industries (including Romalis, 2004). However, testing these predictions is challenging as data on taxes collected by industry is infrequently reported in official publications (and not harmonized across countries).

[45] To accommodate additional notation, ETR on capital is interchangeably referred to with a K subscript or superscript in this section. $\overline{ETR}_{NC}^K$ contains a heterogeneous set of non-corporate agents (taxes on self-employed, property and wealth), for which our data does not contain a systematic breakdown.

in the income-share of formal and incorporated firms. All else equal, trade increases ETR^K by raising the income-share of the sector with higher effective taxation.

In (6), trade can impact $\overline{ETR}_C^K$. All else equal, $\overline{ETR}_C^K$ will rise if the trade-induced corporate income accrues to firms where the ETR^K-size gradient is positive. Bachas et al. (2023) calculate firm-level ETR_i^K using administrative data from 13 LMICs and find that the ETR^K-size gradient for corporate firms is positive everywhere, until the top 1 percent of size where it becomes negative.[46] This positive gradient arises in part because the corporate tax code in LMICs sometimes provides deductions and reduced rates as a function of size or characteristics associated with size such as profitability (Kumar & James, 2022). The positive size-gradient can also be driven by compliance, if larger firms are less able to manipulate the tax code to lower their liability.[47] At the same time, if some of the trade-induced corporate income accrues to the top 1% largest firms, this will, all else equal, tend to reduce $\overline{ETR}_C^K$. Related, trade's potential negative impact on the CIT rate ('race to bottom' mechanism) will, all else equal, reduce $\overline{ETR}_C^K$. The net effect of trade on $\overline{ETR}_C^K$ is therefore ambiguous. Even if trade reduced $\overline{ETR}_C^K$, it could still have a net positive effect on ETR^K via μ_C.[48]

Trends in corporate sector share To gauge this mechanism's plausibility, Figure 6 plots the evolution since 1965 of μ_C, the share of domestic income that originates from the corporate sector (sum of corporate profits and employee compensation). We observe a sizeable uptick in the corporate-share in LMICs in the mid-1990s, from 55% to 65%, which coincides with trade liberalization and the ETR^K rise. The share of mixed income (i.e., income of self-employed individuals and unincorporated businesses) falls around that time, consistent with an expansion of formal income relative to informal activities. Thus, since the 1990s, a growing fraction of output is produced in corporations in LMICs and the timing of the rise suggests it could be linked to trade liberalization. In HICs, μ_C has been stable around 70% since the early 1970s.

[46] In South Africa and Ethiopia, Mascagni and Mengistu (2016) and Carreras et al. (2017) find that corporate ETR^K rises with size towards the top, but is largest for the smallest corporations. Wier and Erasmus (2022) find that profit shifting in South Africa is concentrated among the very largest firms; this would impact the ETR^K-size gradient if the difference for these profit-shifting firms in corporate taxes paid is disproportionate to the difference in underlying domestic reported profits.

[47] $\overline{ETR}_C^K$ can also increase if the trade-induced corporate income leads to more of the corporate profits captured in national accounts also being recorded in tax returns (Section 3.1).

[48] Positive impacts on μ_C and $\overline{ETR}_C^K$ could also occur if trade led to uniform growth for firms of different initial sizes - so long as the growth occurs where the size-gradients for ETR^K and the likelihood of incorporating are positive. Uniform trade-benefits can arise if the foreign inputs are widely accessible and encourage all firms to become more productive (Nataraj, 2011).

6.2 Main results on mechanism outcomes

We investigate the tax capacity mechanism, and 'race-to-the-bottom' and 'social insurance' mechanisms (Section 2), in developing countries. Table 3 shows OLS in Panel A and IV in Panel B. Consistent with race-to-bottom, in column (1) trade causes a decrease in the statutory corporate income tax (CIT) rate (significant at 10% in the IV).[49] The CIT rate is an imperfect proxy for a firm's tax incentives, including due to its abstraction from the base (Abbas & Klemm, 2013), but is observable in our full sample. Columns (2)-(3) show trade increases the corporate share of national income (μ_C), and decreases mixed income by an equivalent magnitude.[50] This is consistent with the tax capacity mechanism, whereby trade disproportionately benefits larger firms and causes an expansion of market income in more formal firms relative to smaller, more informal firms. In column (6), trade raises $\overline{ETR}_C^K$, consistent with the trade-induced corporate income accruing to firms where the ETR_K-size gradient is positive.

How is the additional corporate income allocated between capital and labor? Columns (4)-(5) show that the corporate sector rise is driven by an increase in capital corporate income (corporate profits), while the growth in labor corporate income (employee compensation) is smaller in magnitude and statistically insignificant.[51] This, in turn, causes trade to have a positive impact on the capital-share, both of national income and inside the corporate sector (columns 7-8).[52]

Table A5 shows that the mechanism IV-results are robust to several checks: using NDP representative weights; including different controls; winsorizing the trade variable; and, estimating IVs separately based on each instrument. The CIT rate result remains less robust than the other mechanism results in these checks.

[49]The outcome is the first-differenced tax rate (Romer & Romer, 2010). Table A5 shows results with the level of the CIT rate. We combine data from Végh and Vuletin (2015), Egger et al. (2019), Tax Foundation (link) and country-specific sources. A next step could be to study trade's impact on the more detailed statutory measures (Section 2). The downward trend in CIT rates in LMICs (supp. appendix) is related to, but does not fully capture, changes over time in the detailed measures.

[50]The quality of data-sources used by national statistics offices can affect the measurement of mixed income in LMICs, but we find no impact of trade on countries' statistical capacity ((World Bank link).

[51]There is a null effect of trade on households' operating surplus OS_{HH} (result not shown).

[52]This may occur due to an increase in markups. De Loecker and Eeckhout (2021) find markups have risen in most regions over the past 40 years. De Loecker, Goldberg, Khandelwal, and Pavcnik (2016) and Goldberg (2023) study the impact of trade on markups. Gupta (2023) and Atkin et al. (2015) find that markups increase with firm size, respectively in India and Pakistan. The increase in corporate profits and limited change in employee compensation may also arise if trade raises firms' labor market power (Felix, 2022). Finally, it may arise if trade benefits more capital-intensive production in developing countries, including through the reduction in the CIT rate (Kaymak and Schott, 2023).

Figure A6 shows the same mechanism-outcomes but using the event-study design (Section 5.1). The trade-liberalization events led to a decrease in the CIT rate and increases in corporate income (μ_C) and the effective corporate tax rate ($\overline{ETR}_C^K$). Some individual event-time coefficients are less precisely estimated, but the post-event dummies are jointly statistically significant for all outcomes. Although they are based on different identifying variation in openness, the event-study and IV therefore both provide evidence consistent with the tax-capacity and race-to-bottom mechanisms.

6.3 Firm-level analysis

Section 6.1 highlights that trade's net impact on the average effective corporate tax rate is conceptually ambiguous, as it combines multiple potential channels. To unpack trade's effects inside the corporate sector, we conduct a firm-level analysis in Rwanda between 2015-2017, to study the impact of trade on the individual corporate firm's effective tax rate. To our knowledge, there is limited firm-level evidence in LMICs on how trade impacts a firm's domestic effective tax rate. Rwanda provides an interesting setting as the corporate sector, starting from a comparatively low output share, has grown significantly since the mid-1990s, in tandem with a rise in both trade openness and tax revenues. Moreover, we can combine multiple administrative datasets to observe each formal Rwandan firm's exposure to trade and domestic tax payments.

We use corporate income tax returns to measure each firm's effective tax rate ETR_i^K as the ratio of corporate taxes paid divided by net profit.[53] Net profit is revenue minus material, labor, operational, depreciation and financial costs. In Rwanda, this firm-level ETR_i^K varies across firms due to characteristics (including size), reduced rates and exemptions (Mascagni, Monkam and Nell, 2016). Indeed, the corporate ETR_i^K in Rwanda increases everywhere with firm size, apart from in the top percentile (Figure 2 in Bachas et al., 2023). Outside of the very top, an increase in firm size may raise ETR_i^K, due to statutory incentives and reduced ability to manipulate tax liability.

We merge with customs data, which record firms' direct trade exposure. Following recent work (reviewed in Atkin and Khandelwal, 2020, Bernard & Moxnes, 2018), we measure a firm's total exposure to trade by accounting for the firm's indirect exposure through its linkages to domestic suppliers that use traded goods in their production process.[54] We merge administrative data which record transaction linkages between

[53] Recall this measure differs from $\overline{ETR}_C^K$ for conceptual and empirical reasons discussed in Section 3.1.
[54] Recent papers study domestic linkages in LMICs and their role in propagating trade-shocks (including Almunia, Hjort, Knebelmann, & Tian, 2023; Fieler, Eslava, & Xu, 2018; Javorcik, 2004).

formal firms (details on data-sources and sample in Appendix E.1). To measure a firm's total trade exposure in a network setting, we follow the methodology in Dhyne, Kikkawa, Mogstad, and Tintelnot (2021) who use similar datasets to measure Belgian firms' exposure to trade. Specifically, we define firm i's total foreign input share as the share of inputs that it directly imports (s_{Fi}), plus the share of inputs that it buys from its domestic suppliers l (s_{li}), multiplied by the total import shares of those firms:

$$s_i^{Total} = s_{Fi} + \sum_{l \in V_i} s_{li} \cdot [s_{Fl} + \sum_{r \in V_l} s_{rl} \cdot (s_{Fr} + ...)] \tag{8}$$

where V_i is the set of domestic suppliers of firm i, and V_l is the set of domestic suppliers of firm l. The denominator of the input shares is the sum of purchases from other firms and imports. In practice, we limit the recursive calculation in (8) to inputs from a firm's immediate suppliers l and the suppliers to their suppliers r (adding more network-levels only marginally increases s_i^{Total}).[55] Inspecting s_i^{Total} and s_{Fi} reveals that while just under 30% of Rwandan formal firms import directly, 93% rely on trade either directly or indirectly through suppliers which use foreign inputs in their production process. Indeed, most firms are strongly dependent on foreign trade, but only a limited number show that dependence through the direct foreign inputs observed in customs data. The median total foreign input share is 48%.

In Table 4, we estimate regressions in the sample of corporate firms of the form:

$$ETR_{it}^K = \mu \cdot s_{it}^{Total} + \Theta \cdot X_{it} + \pi_t + \pi_i + \epsilon_{it} \tag{9}$$

where ETR_{it}^K and s_{it}^{Total} are the corporate effective tax rate and total trade exposure of firm i in year t, and π_t and π_i are year and firm fixed effects. X_{it} includes number of employees and number of clients and suppliers, and ϵ_{it} is clustered at the firm level. OLS estimation of (9) shows that a within-firm increase in trade exposure is associated with a higher corporate effective tax rate (columns 1 to 4 of Panel A).

We implement an IV which generates firm-level variation in trade exposure using the shift-share design from Hummels et al. (2014). The identifying variation is trade shocks from changes in world export supply of specific country-product combinations in which a Rwandan firm had a previous import relationship.[56] We build these trade

[55]We focus on firms' exposure to imports through their supplier network; we find qualitatively similar results when we study firms' exposure to exports through their client network (results available).
[56]Specifically, the direct import trade shock for firm i in year t is:

shocks for all firms. In turn, the 1^{st}-stage instruments are the firm's own trade shocks, as well as the trade shocks to its suppliers and to the suppliers of its suppliers.[57] We find that both direct trade shocks to a firm's own imports and indirect shocks to a firm's network of suppliers cause significant changes to the firm's total exposure s_{it}^{Total}, generating a strong 1^{st}-stage (Kleibergen-Paap F-statistic of 18.17). Using the IV, we find that trade causes an increase in the individual firm's effective tax rate on capital (column 5). In Panel B, the IV shows that trade causes an increase in firm size (proxied by revenue), while OLS regressions in Panel C show a positive association between firm size and ETR_i^K (we cannot use the IV in Panel C due to the exclusion restriction).

The identification strategy relies on the argument that changes in world export supply are plausibly exogenous and on the empirical observation that Rwandan firms do not have all inputs in common. To support the exogeneity assumption, we find that results are robust to controlling for trade shocks to firm i's potential suppliers (firms that operate in the same industry and geographical area as i's current suppliers but are not currently supplying to i) and horizontal suppliers (firms that are suppliers to firm i's current clients). More details are provided in Appendix E.1.[58]

These results serve two purposes. First, they provide firm-level identified evidence on trade's positive impact on ETR_K in a developing country, which complements our country-level results in LMICs. Second, they support the mechanism interpretation that trade's impact on ETR_K is mediated by a positive size-ETR_K gradient.

6.4 Discussion

In this subsection we discuss how the tax capacity mechanism relates to governments' enforcement policies and the trade-formalization literature in developing countries.

$$logM_{it}^D = log \sum_{a,c} s_{ic,t-1}^{a,M} \cdot WES_{a,c,t}$$

where $s_{ic,t-1}^{a,M}$ is the share of imports of firm i in year $t-1$ that falls on product a from country c, and $WES_{a,c,t}$ is the world export supply (excluding sales to Rwanda) of country c for product a.

[57] The 1^{st}-stage regression is:

$$s_{it}^{Total} = \beta_1 \cdot logM_{it}^D + \beta_2 \cdot logM_{it}^S + \beta_3 \cdot logM_{it}^{SS} + \kappa_t + \kappa_i + \epsilon_{it}$$

where $logM_{it}^D$, $logM_{it}^S$, and $logM_{it}^{SS}$ are, respectively, the trade shocks to firm i, to firm i's suppliers, and to the suppliers of firm i's suppliers. We construct weighted averages of trade shocks in the supplier network using the recursive formulation in (8). See Appendix E.1 for details.

[58] In an extension, we find that increased *output* exposure to imports through the client network has positive effects on ETR^K, though this average effect could mask heterogeneity across firms.

Enforcement reforms Over our sample period, LMICs have implemented tax enforcement policies. A challenge for the mechanism interpretation is that openness, potentially due to concerns over border revenue losses, prompted governments to implement enforcement reforms which increased ETR_K. To investigate this, we measure the year of adoption (if any) in LMICs of four policies which raise domestic tax enforcement: (i) large taxpayer unit; (ii) organizational integration of customs and domestic tax authorities; (iii) VAT; (iv) international accounting standards (IAS).[59] Table A6 shows positive IV effects of trade on ETR_K without the enforcement measures, but larger effects with them.[60] Trade has the same impact on the corporate income-share (μ_C) in both settings, but trade's positive impact on $\overline{ETR}_C^K$ is amplified when the enforcement policies are adopted.[61] In other words, the trade induced expansion of the corporate sector occurs regardless, but the extent to which the additional corporate income translates into higher effective corporate capital taxation is reinforced with enforcement policies.[62] The positive trade effect on ETR_K therefore does not hinge upon, but is magnified by, investments in tax enforcement.[63] Whether these investments are themselves driven by globalization is a topic for future research.

Links to trade-formality literature We find positive effects of trade on outcomes related to formalization. Recent studies focused on the number of formal versus informal firms or formal versus informal workers and found mixed evidence that trade increases formality by these measures (reviews in Engel & Kokas, 2021; Ulyssea, 2020).[64] One way to reconcile our results with these studies is to note that our focus is on the share of output produced in formal versus informal firms: output expansion in larger, formal firms may occur without changes to the number of formal or informal firms, and does not imply an increase in the number of formal workers, since informal workers may work in formal firms and contribute to their output (Ulyssea, 2018).

[59] The enforcement focus on large firms increases collection (Almunia & Lopez-Rodriguez, 2018; Basri et al., 2021). The customs-tax unification improves domestic audit capacity (IMF, 2022). The VAT creates information trails (Almunia, Henning, Knebelmann, Nakyambadde, & Tian, 2023; Waseem, 2020). IAS make accounting requirements more comprehensive (Barth, Landsman and Lang, 2008).

[60] Timing of adoption for each reform is endogenous; however, our focus is on the trade coefficients with and without these reforms in place, which are identified (Bun and Harrison, 2019).

[61] Of the four administrative reforms, only the VAT was in place in all countries (except for India) by the time of the liberalization events studied in Section 5.1.

[62] Intuitively, the enforcement policies all (weakly) disproportionately raise enforcement on larger firms, thereby increasing the slope of the ETR_K-size gradient inside the corporate sector.

[63] Moroever, in the supp. appendix we find trade's positive impacts on ETR and mechanisms hold outside of all the episodes of trade-induced tariff revenue loss captured in Cagé and Gadenne (2018).

[64] Goldberg and Pavcnik (2003), Bosch et al. (2012), Cruces et al. (2018), Dix-Carneiro and Kovak (2019).

Moreover, trade models highlight that impacts on formality depend on the nature of the trade shock. In Appendix E.2, we investigate if the ETR and mechanism impacts differ along two dimensions (Dix-Carneiro et al., 2021). Using both instruments in the LMIC sample, we find that *exports* increase ETR_K and the corporate income-share (μ_C), while *imports* decrease both outcomes. These results are consistent with 'Melitz-type' demand effects, whereby increased exports represent a pure positive demand shock for export-oriented firms, while increased imports may constitute a negative demand shock for domestic firms, disproportionately affecting larger ones. In additional IV regressions, trade in *intermediate G-S* increases ETR_K and μ_C, while trade in *final G-S* decreases both outcomes.[65] Results are similar for $\overline{ETR}_C^K$. These results are consistent with the increased availability of intermediate goods benefiting larger firms; by contrast, the increased availability of final goods may constitute a negative domestic demand shock, particularly for larger firms. These findings suggest that the impacts on mechanisms and ETR depend on the nature of the trade shock.

6.5 Heterogeneity: Developing vs developed countries

We provided supporting evidence for the tax capacity mechanism in LMICs. We now expand our sample to HICs to investigate if trade's impact on this and other mechanisms, and ultimately on ETR, differs across development levels. If so, then openness may have contributed to the divergent trends in ETR_K between HICs and LMICs (Figure 1). We conjecture the tax capacity mechanism is unlikely to operate in HICs, where constraints on effective taxation are stable and not as binding (e.g. Figure 6 shows the corporate income share has been stable over the past 40 years), while the race-to-bottom and social insurance mechanisms are likely active, given previous research. We estimate heterogeneous IV effects by development level:

$$y_{ct} = \mu \cdot trade_{ct} + \kappa \cdot trade_{ct} \cdot \mathbb{1}(HighIncome)_c + \Theta \cdot X_{ct} + \pi_c + \pi_t + \epsilon_{ct} \qquad (10)$$

We note these results should be interpreted with caution, given the econometric challenges of estimating IV effects with multiple endogenous regressors (Andrews, Stock, & Sun, 2019).[66] With this in mind, the patterns in Table 5 suggest heterogeneous impacts. Trade increases ETR_K in LMICs but decreases it in HICs (column 1), though

[65] Which suggests that imports of intermediate (final) G-S increases (decreases) ETR_K and μ_C.

[66] The Kleibergen-Paap F-statistic depends on whether the two instruments $Z^{gravity}$ and Z^{oil} generate sufficiently distinct variation in the endogenous regressors, which is not guaranteed in our setting.

the HIC coefficient is not statistically significant. The negative race-to-bottom effect on the CIT rate appears most pronounced in HICs (column 3). The positive impacts of trade on tax capacity outcomes (μ_C, $\overline{ETR}_C^K$) are limited to LMICs, with largely null effects in HICs (columns 4-10). Table 5 reveals qualitative differences in coefficients by development levels, but we cannot statistically reject their equality in most cases.[67]

These results suggest countervailing mechanisms that differ by development level, through which openness may have contributed to the divergent long-run ETR_K trends between HICs and LMICs. While we have focused on ETR_K, the results for ETR_L are worthy of further investigation. Table 5 suggests the social insurance mechanism may be present in all countries, and the labor tax-capacity mechanism in LMICs.[68]

Table A7 shows trade's negative CIT-rate impact is larger in smaller countries and with fewer capital restrictions (Alesina & Wacziarg, 1998), where capital flight concerns are stronger (Hines, 2006). Mirroring this, trade's positive impact on ETR_K is limited to populous countries with more capital restrictions. The tax capacity and race-to-bottom mechanisms appear to occur simultaneously: countries with larger markets and limited capital mobility reap more of the tax-capacity effects of trade.

6.6 Capital openness

To finish the analysis, we note that our focus throughout the paper has been on one key dimension of globalization: trade openness. Given our interest in capital taxation, another relevant dimension is capital openness (Ilzetzki, Reinhart, & Rogoff, 2019; Van Patten, 2022). However, due to differences in countries' reporting requirements, data on capital openness is not as available and comparable as trade data. Finding credible exogenous variation for capital openness is also challenging. Notwithstanding these challenges, in Appendix F we try to investigate the impact of capital openness on ETR. We rely on the capital inflow liberalization events for 25 developing countries from Chari, Henry, and Sasson (2012), which capture the first time when foreign investment in the domestic stock market is allowed. Employing the same event-study design as in Section 5.1, we find that the events lead to both increased capital openness and higher ETR_K, which is qualitatively consistent with the trade-liberalization results. This suggests that the positive impact of globalization on ETR_K in LMICs may be

[67]The IV-coefficients for developing countries differ qualitatively between Table 5 and Tables 1-3. This is because the two instruments' strength change in the 1^{st}-stage regression (Table A3).

[68]Corporations serve as third-party reporters and withholding agents for employees' income, which increases the effective taxation of labor income on employees relative to self-employed workers.

robust to using capital instead of trade openness. However, given the limitations with the measurement of capital flows, we consider that our trade results provide more robust insights into globalization's impacts on effective taxation.

7 Conclusion

This paper provides evidence on trends and causal effects of globalization on tax structures. We make two main contributions. First, we build and analyze a global macro-historical database of effective tax rates on labor and capital covering 155 countries, with over half starting in 1965. The key novel fact is the asymmetric evolution of capital taxation by development level: while ETR_K fell in rich countries, it rose in developing ones since the 1990s. Our second contribution is to formulate and test a hypothesis that sheds light on the rise in ETR_K in LMICs. Across multiple research designs, we find evidence of a pro-tax capacity effect of international trade: trade increases ETR_K (and ETR_L), by expanding larger firms where effective taxation is higher. We provide evidence for trade's positive impact on ETR_K at the country, corporate sector and firm level. In developing countries, the pro-tax capacity effect prevails over the well-known negative effect of international tax competition.

This paper's findings have implications for public finance and globalization in developing countries. By improving effective taxation and positively impacting domestic taxes, trade increases overall revenue. This result runs counter to a persistent policy-concern over tax losses from trade liberalization, and previous academic work has mainly abstracted from investigating trade's impacts on domestic capital and labor taxes. By incorporating domestic tax bases, we take a step towards a comprehensive analysis of the revenue consequences of globalization. We focus on a specific mechanism, but many links remain to be explored between trade, firms, and taxation.

Across our research designs, the positive effect of trade is systematically larger for ETR_K than ETR_L in LMICs. As capital income is more concentrated than labor income, this result is a relevant input for the broader study of the distributional effects of globalization on post-tax income in LMICs. While we adopted a macro perspective on tax systems, a next step could be to combine our ETRs with individual-level estimates of the progressivity of labor and capital taxes. This would make it possible to compare the distributional effects of globalization on pre-tax versus post-tax income (Goldberg, 2023; Pavcnik, 2017), and raises empirical questions for future research.

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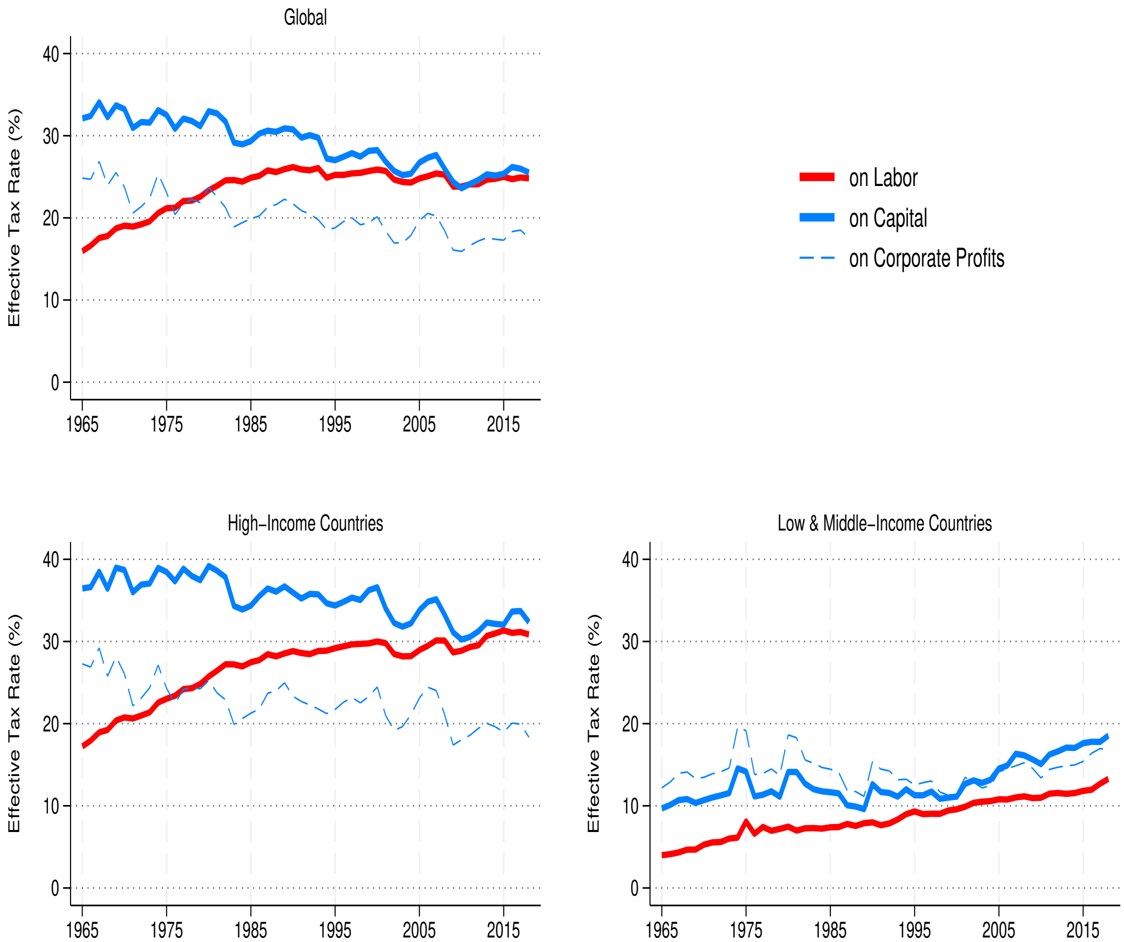
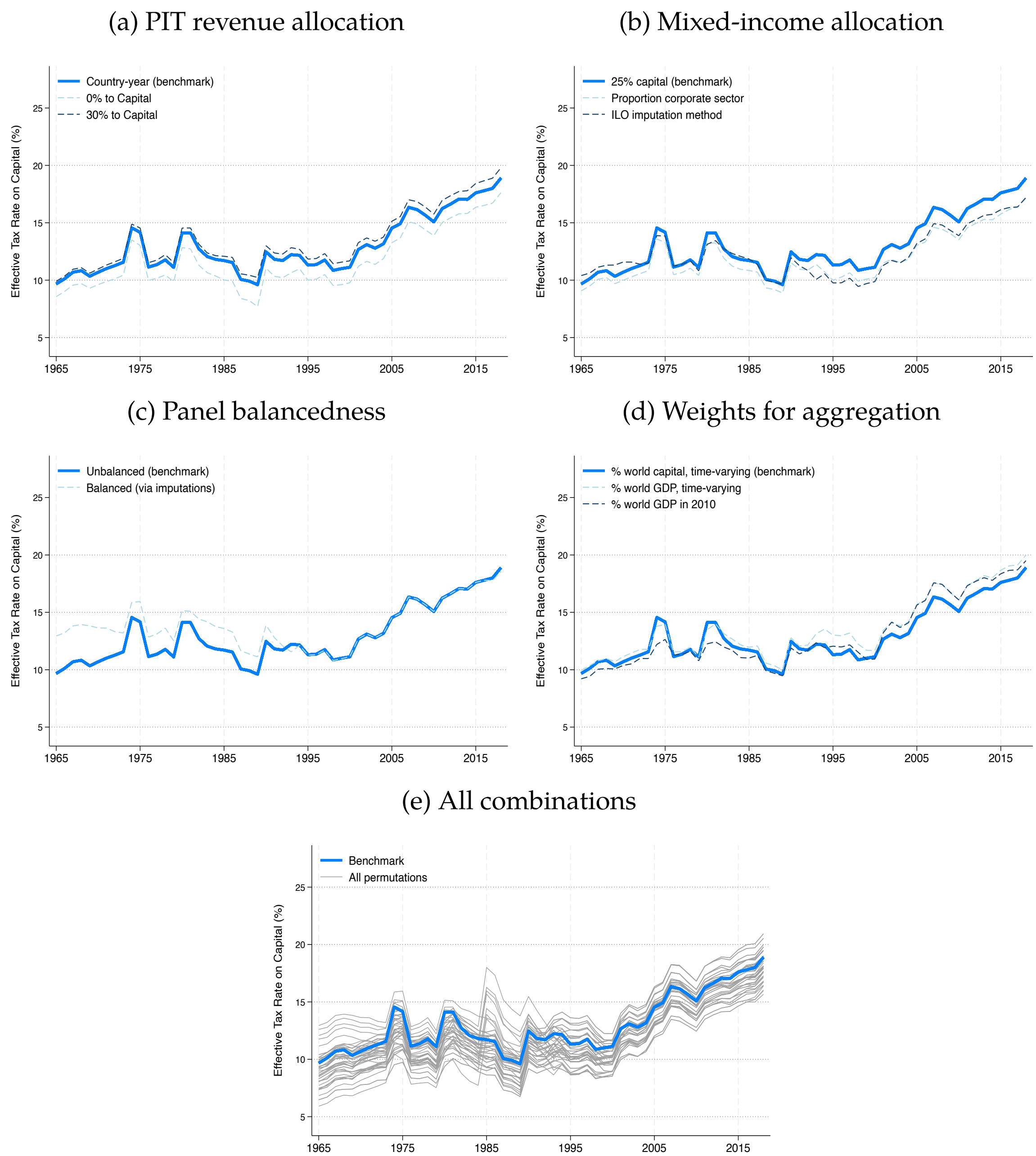
Figure 1: Effective Taxation of Capital and Labor



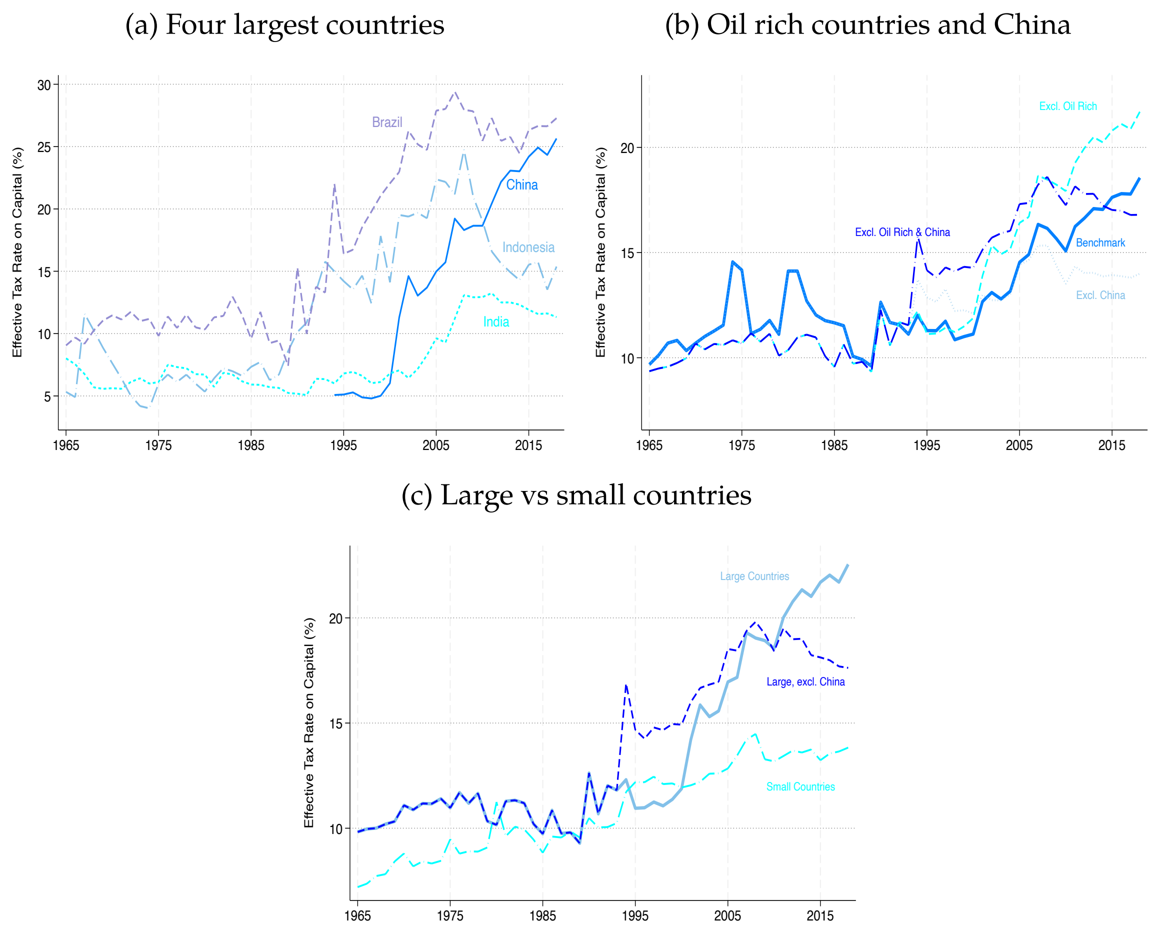
Notes: This figure plots the time series of average effective tax rates on labor (red) and capital (blue), as well as the average effective tax rate on corporate profits (blue dashed line). The top-left panel corresponds to the global average, weighting country-year observations by their share in that year's total factor income, in constant 2019 USD (N=155). The bottom-left panel shows the results for high-income countries (N=37), and the bottom-right panel for low- and middle-income countries (N=118). Income classification is based on the World Bank income groups in 2018. The dataset is composed of two quasi-balanced panels. The first covers the years 1965-1993 and excludes communist regimes. It accounts for 85-90% of world GDP during those years. The second covers 1994-2018 and integrates former communist countries, in particular China and Russia, and accounts for 97-98% of world GDP. This figure is discussed in Section 4.1.

Figure 2: Robustness of Effective Capital Taxation in Developing Countries



Notes: These panels show trends in the effective tax rate on capital in the 118 developing countries in our sample. The panels vary our four key methodological choices: the allocation of personal income tax revenue to capital vs labor (panel a); the allocation of mixed income to capital vs labor (panel b); presenting results for an unbalanced panel of countries vs a balanced panel via imputations (panel c); and, the use of weights to aggregate individual countries' time-series (panel d). Panel (e) shows all 54 possible combinations that can be constructed by combining these choices. In all panels, the blue line corresponds to our benchmark series. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. This figure is discussed in Section 4.2.

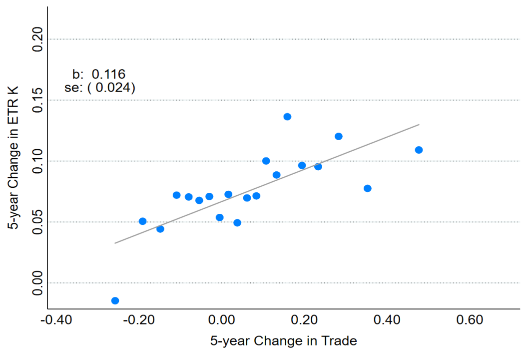
Figure 3: Heterogeneity of Effective Capital Taxation in Developing Countries



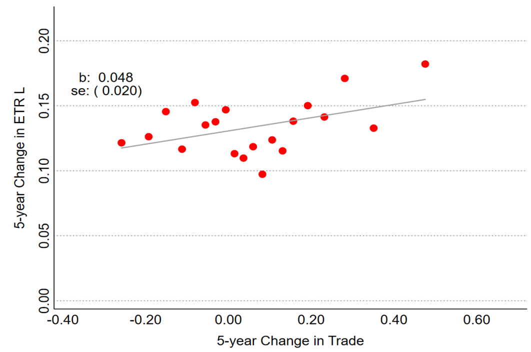
Notes: These panels show the evolution of the effective tax rate on capital, ETR_K, for major developing countries and sub-samples of developing countries. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. Panel (a) plots the ETR_K series for the four largest developing countries: Brazil, China, India, Indonesia. Panel (b) compares our benchmark series to: a series without China; a series without oil-rich countries (countries with more than 7% of GDP from oil in 2018); and, a series without China and oil-rich countries. Within the sample of non-oil rich developing countries, panel (c) compares large countries to small countries. Large countries are defined as having a population above 40 million in 2018. This figure is discussed in Section 4.3.

Figure 4: Within-Country Associations between Effective Tax Rates and Trade

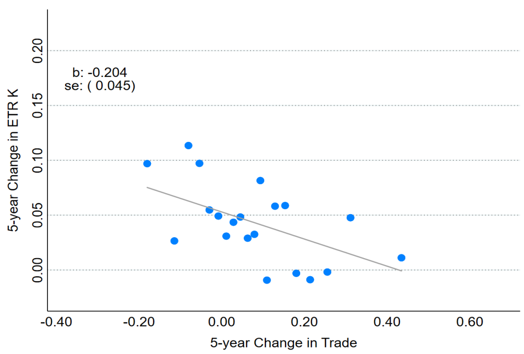
(a) ETR_K: All countries



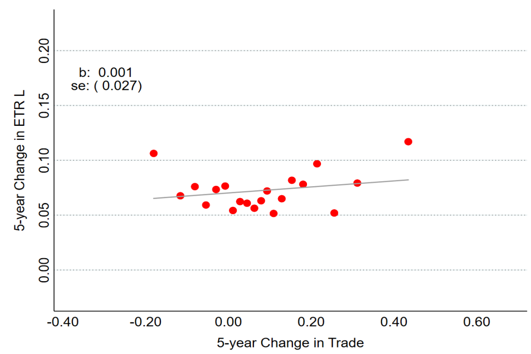
(b) ETR_L: All countries



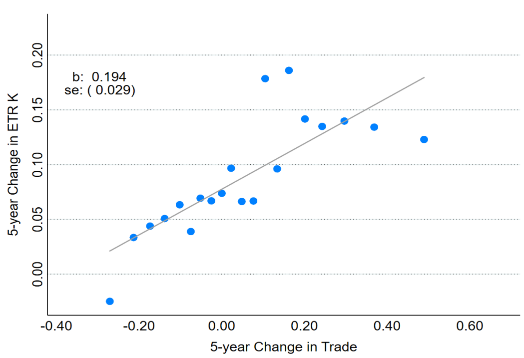
(c) ETR_K: High-income



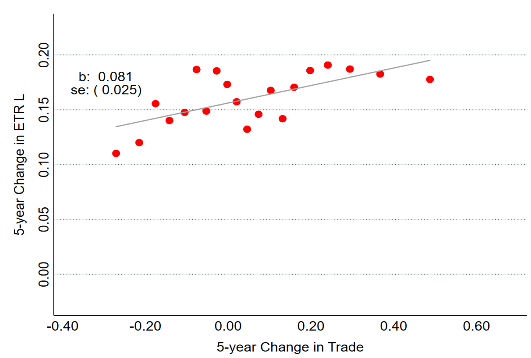
(d) ETR_L: High-income



(e) ETR_K: Low & middle-income

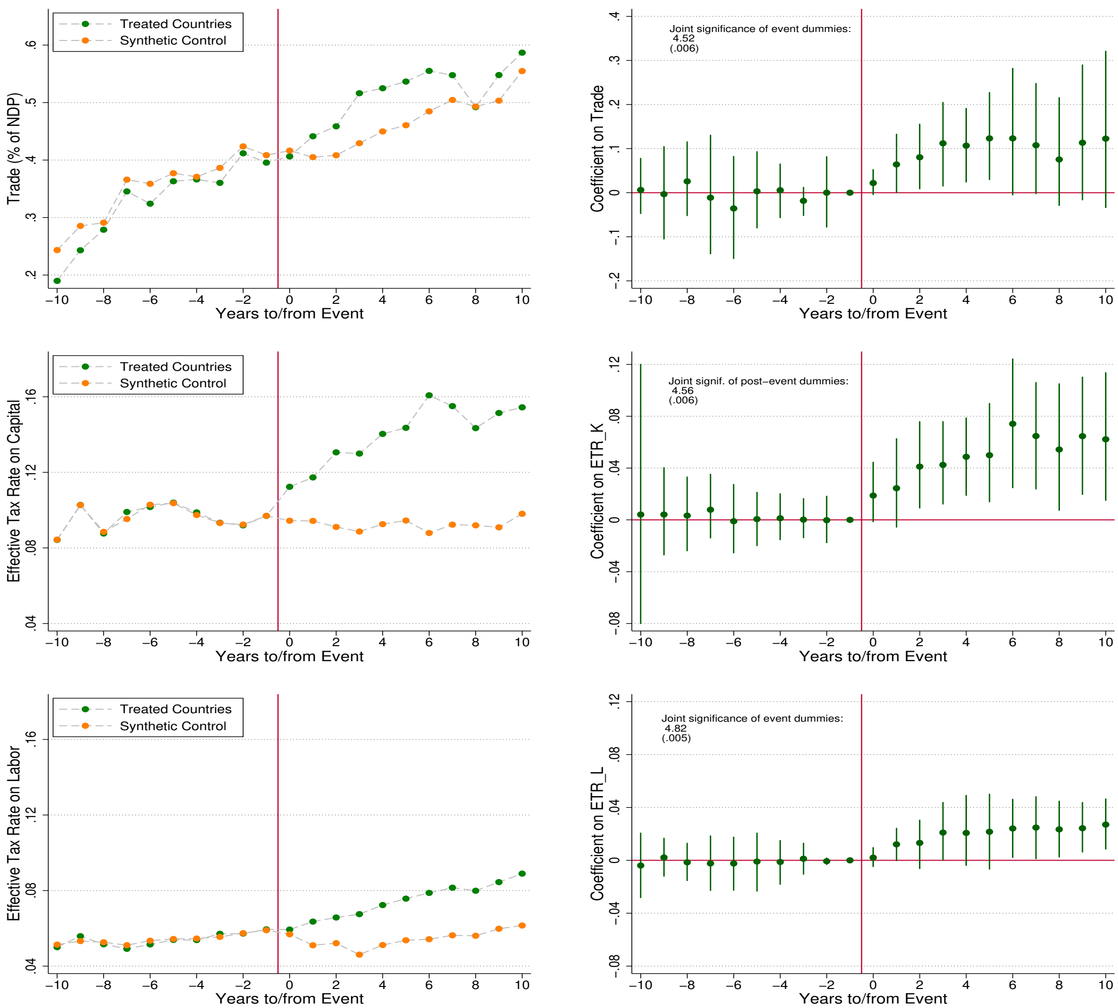


(f) ETR_L: Low & middle-income



Notes: These panels shows the association between trade and effective tax rates. The outcome is the effective tax rate on capital, ETR_K, and on labor, ETR_L, in the left-side and right-side panels, respectively. The top panels show the associations in all countries; the middle panels show the associations in high-income countries (based on World Bank income classification in 2018); the bottom panels show the associations in low and middle-income countries. Trade is measured as the sum of import and exports as a share of net domestic product. Both the x-axis and y-axis are measured as within-country percent changes over 5 years. Each graph shows binned scatter plots of each outcome against trade, after residualizing all variables against year fixed effects. Each dot corresponds to a ventile (20 equal-sized bins) of the residualized trade variable, with average values of trade and ETR calculated by ventile. In each graph, the line represents the best linear fit based on the underlying country-year data, with the corresponding slope-coefficient and standard error reported in the top-left corner. For more details, see Section 4.4.

Figure 5: Event Study of Trade Liberalization Reforms



Notes: These figures show event-studies for trade liberalization in seven large developing countries: Argentina, Brazil, China, Colombia, India, Mexico and Vietnam. The panels correspond to different outcomes: trade (top panels); effective tax rate on capital (middle panels); effective tax rate on labor (bottom panels). The left-side graphs show the average level of the outcome in every year to/since the event for the treated group and for the group of synthetic control countries. The right-hand graphs show the β_e coefficients on the to/since dummies, based on estimating the dynamic event-study regression in equation (4). The bars represent the 95% confidence intervals. Standard errors are clustered at the country-event level and estimated with the wild bootstrap method. The top-left corners report the F-statistic on the joint significance of the post-event dummies, with the p-value in parentheses. Details on methodology in Section 5.1.1.

Figure 6: Corporate Sector Income and Mixed Income, by Development Level

Notes: These panels plot the time series of corporate sector income and of mixed income between 1965 and 2018 and by level of development. Both outcomes are expressed as a percent of net domestic product and weighted by country-year net domestic product in constant 2019 USD. Corporate income is the sum of corporate profits and corporate employee compensation. The left panels show the results for high-income countries (N=37), and the right panels show the results for low- and middle-income countries (N=118), based on the World Bank income classification in 2018. The dataset is composed of two quasi-balanced panels. The first covers the years 1965-1993 and excludes communist regimes. The second covers 1994-2018 and integrates former communist countries, in particular China and Russia. For more details, see Section 6.

Table 1: Trade Impacts on Effective Taxation of Capital and Labor in Developing Countries

	Benchmark		Robustness: Specification and covariates					Robustness: $K-L$ assignment to taxes and factor shares				Robustness: Individual instruments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A: ETR_K													
Trade	0.032***	0.109***	0.118***	0.222*	0.106**	0.102***	0.115***	0.150***	0.100**	0.116***	0.124***	0.108***	0.164*
	(0.010)	(0.033)	(0.041)	(0.120)	(0.046)	(0.033)	(0.032)	(0.048)	(0.039)	(0.039)	(0.042)	(0.034)	(0.087)
Panel B: ETR_L													
Trade	0.011**	0.056***	0.049***	0.062	0.046**	0.058***	0.059***	0.041***	0.053***	0.052***	0.045***	0.054***	0.140**
	(0.004)	(0.016)	(0.015)	(0.042)	(0.020)	(0.017)	(0.016)	(0.015)	(0.016)	(0.016)	(0.015)	(0.016)	(0.061)
Specification	OLS	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV	IV
1^{st} stage Kleibergen-Paap F-statistic		24.57	24.57	31.24	14.24	23.09	34.83	24.57	24.57	24.57	24.57	45.17	10.80
Modifications to IV in col. (2)			No ETR winsorize	NDP weights	Include country-year controls	Include **1**(oil-rich)*year fixed effects	Winsorize trade	Assign based on ILO (2019)	Assign based on corp. K-share	Assign 0% of PIT to capital	Assign 30% of PIT to capital	Only use $Z^{gravity}$ instrument	Only use $Z^{Oil-Dist}$ instrument
N	4970	4970	4970	4970	3984	4970	4970	4970	4970	4970	4970	4970	4970

Notes: This table presents results from estimating the effect of trade on effective tax rates in developing countries. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. The outcome is the effective tax rate on capital, ETR_K, in Panel A and the effective tax rate on labor, ETR_L, in Panel B. Trade is measured as the sum of exports and imports divided by net domestic product (NDP). Column (1) presents the OLS results from estimating equation (5). All other columns use IV; at the bottom of each column, we report the 1^{st}-stage Kleibergen-Paap F-statistic. The benchmark IV specification is in column (2), with the corresponding 1^{st}-stage regression reported in Table A3. The remaining columns modify the benchmark specification of column (2). In column (3), the outcome is non-winsorized, while in column (4) we include country-year NDP weights. In column (5), we include the country-year controls described in Section 5.2.1. In column (6), we include interactive fixed effects between a dummy for oil-rich countries and year dummies. Oil-rich countries derive more than 7% of GDP from oil in 2018. In column (7), we use the trade variable which is winsorized at the 5%-95% percentile on a yearly basis. In columns (8)-(9), we modify the assignment rule for mixed income's capital factor share, respectively by using the ILO (2019) method and by assigning the capital share in the corporate sector. In columns (10)-(11), we assign respectively 0% and 30% of personal income taxes (PIT) to capital taxes. In columns (12)-(13), we estimate the IV using the individual instruments $Z^{gravity}$ and $Z^{oil-distance}$, respectively. For more details, see Section 5.2. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table 2: Trade Impacts on Types of Taxes (% of NDP) in Developing Countries

	Total taxes (1)	CIT (2)	Property and Wealth (3)	PIT (4)	Social Security (5)	Indirect (6)
Panel A: OLS						
Trade	0.033***	0.018***	-0.001	0.003	0.002	0.009
	(0.011)	(0.003)	(0.001)	(0.003)	(0.002)	(0.006)
Panel B: IV						
Trade	0.098***	0.047***	0.004	0.010*	0.015**	0.019
	(0.033)	(0.013)	(0.003)	(0.005)	(0.006)	(0.022)
1^{st}-stage Kleibergen-Papp F-statistic	24.57	24.57	24.57	24.57	24.57	24.57
N	4970	4970	4970	4970	4970	4970

Notes: This table shows the impacts of trade on collection of types of taxes, expressed as a percent of net domestic product (NDP), in developing countries. OLS results are in Panel A and IV results are in Panel B. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. Trade is measured as the sum of exports and imports divided by NDP. All regressions in Panel B are based on the IV model described in Section 5.2. At the bottom of each column, we report the 1^{st}-stage Kleibergen-Paap F-statistic. The corresponding 1^{st}-stage regression is reported in Table A3. The outcome differs across columns: Column (1) is total taxes, which is the sum of direct taxes on capital and labor and indirect taxes on trade and domestic consumption; column (2) is corporate income taxes (CIT); column (3) is taxes on property, wealth and inheritance; column (4) is personal income taxes (PIT); column (5) is social security and payroll; column (6) is indirect taxes, which combines trade taxes and domestic consumption taxes. For more details on these types of taxes, see Table B3 and Appendix B.1. For more details on the IV, see Section 5.2. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table 3: Trade Impacts on Mechanism Outcomes in Developing Countries

		National income components					Factor shares	
	First-diff. CIT rate (1)	Corporate totl. income (2)	Household mixed income (3)	Corporate profits (4)	Employee compensation (5)	Corporate ETR_K (6)	Capital share natl. income (7)	Capital share corp. sector (8)
Panel A: OLS								
Trade	-0.003***	0.040***	-0.017	0.027***	0.006	0.063***	0.021**	0.031**
	(0.001)	(0.013)	(0.011)	(0.009)	(0.010)	(0.019)	(0.008)	(0.012)
Panel B: IV								
Trade	-0.012*	0.183***	-0.193***	0.184***	0.014	0.142*	0.161***	0.206***
	(0.006)	(0.043)	(0.041)	(0.036)	(0.032)	(0.074)	(0.034)	(0.048)
1^{st} stage Kleibergen-Paap F-Statistic	24.57	24.57	24.57	24.57	24.57	24.57	24.57	24.57
N	4970	4970	4970	4970	4970	4970	4970	4970

Notes: This table presents results from estimating the effects of trade on mechanism outcomes in developing countries. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. Trade is measured as the sum of exports and imports divided by net domestic product (NDP). Panel A presents OLS results and Panel B presents the IV results, based on the instruments described in Section 5.2. At the bottom of each column in Panel B, we report the 1^{st}-stage Kleibergen-Paap F-statistic. Across the columns, the outcome differs: column (1) is the first-differenced statutory corporate income tax (CIT) rate; column (2) is the corporate income share of net domestic product, where corporate income is the sum of corporate profits and corporate employee compensation; column (3) is the mixed income share of net domestic product; column (4) is the corporate profit share of net domestic product; column (5) is the employee compensation share of net domestic product; column (6) is the average effective tax rate on corporate profits; column (7) is the capital share of net domestic product; column (8) is the capital share of corporate income. For sake of space, we omit showing the insignificant impact of trade on OS_{HH}, the remaining component of national income. For more details on the outcomes, see Section 3.1 and Section 6.1. For more details on the instrumental variables, see Section 5.2. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table 4: Firm-Level Regressions in Rwanda: ETR^K, Trade and Size

	(1)	(2)	(3)	(4)	(5)
Panel A outcome: ETR^K					
S^{Total}	0.100***	0.087***	0.075***	0.025*	0.133**
	(0.021)	(0.017)	(0.017)	(0.014)	(0.060)
Panel B outcome: Log revenue					
S^{Total}	1.362***	1.351**	1.078**	0.202*	1.444***
	(0.466)	(0.542)	(0.475)	(0.107)	(0.233)
Panel C outcome: ETR^K					
Log revenue	0.040*	0.092***	0.077**	0.029***	-
	(0.023)	(0.029)	(0.027)	(0.003)	-
Estimation	OLS	OLS	OLS	OLS	IV
1^{st}-stage Kleibergen-Paap F-statistic					18.17
Year FEs	Y	Y	Y	Y	Y
Industry-Geography FEs		Y	Y		
Firm controls			Y	Y	Y
Firm FEs				Y	Y
N	18478	18478	18478	18478	18478

Notes: This table presents firm-level regression results from corporate firms in Rwanda between 2015 and 2017. The outcome differs across panels: Panels A) and C) is the effective tax rate on corporate profits, ETR^K; Panel B) is log of annual revenue. In Panels A) and B), the reported regression coefficient is for total foreign input share, S^{Total}; in Panel C), it is for log annual revenue. Columns (1)-(4) present OLS results from estimating variations of equation (9): Column (1) includes year fixed effects; column (2) adds industry-geography fixed effects; column (3) adds firm-year controls (firm age, number of employees, and total number of clients and suppliers); column (4) adds firm fixed effects. Column (5) is the IV estimation where the total foreign input share (S^{Total}) is instrumented with trade-shocks to firms and their supplier network based on the shift-share design of Hummels, Jørgensen, Munch, and Xiang (2014). The instruments are described in detail in Section 6.3 and Appendix E.1. In column (5), we also report the 1^{st}-stage Kleibergen-Paap F-statistic from estimating the 1^{st}-stage in equation (16). Details on the sample are provided in Appendix E.1. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the industry-geography level in columns (1)-(3), and at the firm-level in columns (4)-(5) (results are robust to clustering at firm-level in all columns).

Table 5: Heterogeneous Impacts of Trade by Development Level

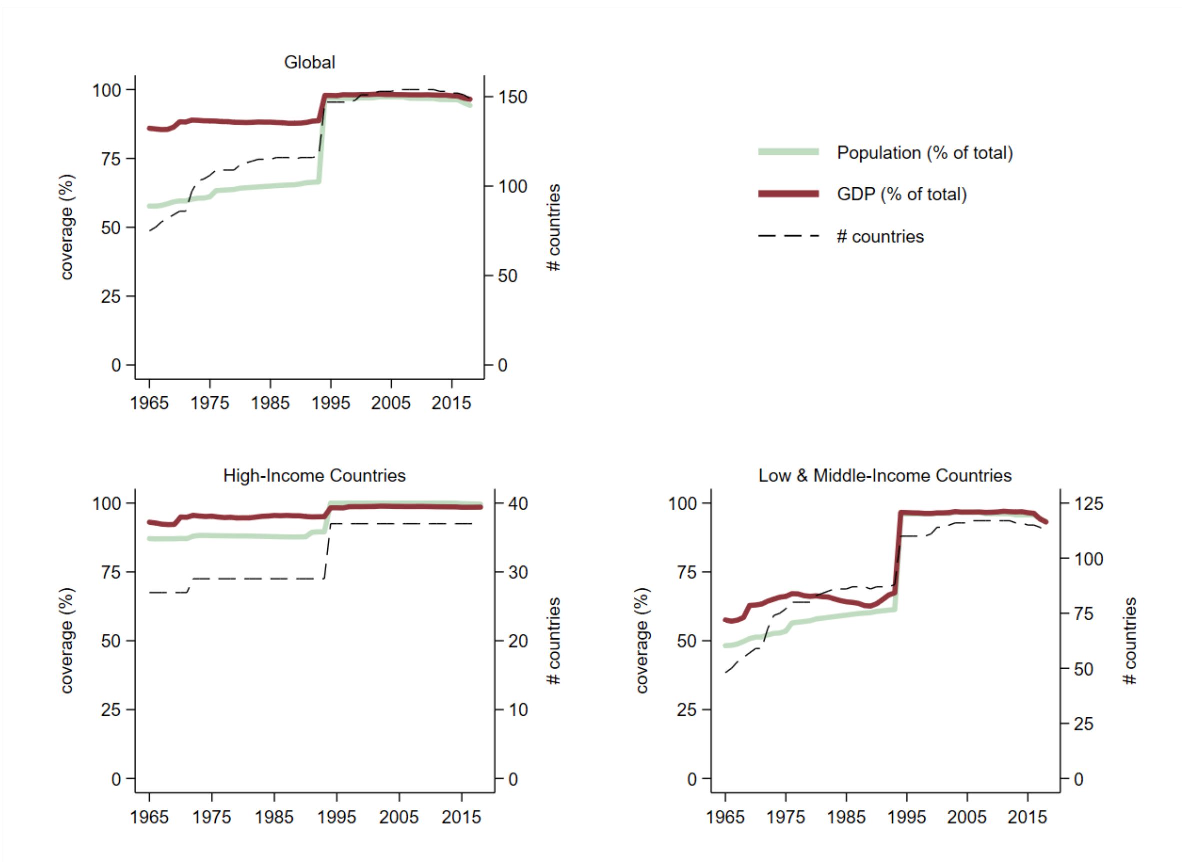
	ETR_K (1)	ETR_L (2)	First-diff. CIT Rate (3)	Corp. Totl. Income (4)	Mixed Income (5)	Corp. Profits (6)	Employee Comp. (7)	Corp. ETR_K (8)	Natl. K-Share (9)	Corp. K-Share (10)
Trade	0.267**	0.123**	-0.015	0.340**	-0.200*	0.211***	0.088	0.341**	0.132***	0.167***
	(0.134)	(0.050)	(0.020)	(0.133)	(0.116)	(0.057)	(0.098)	(0.134)	(0.048)	(0.051)
Trade$*\mathbb{1}$(High-inc.)	-0.315	0.012	-0.070**	-0.545***	0.340**	-0.333***	–0.239**	-0.142	-0.194**	-0.238**
	(0.231)	(0.110)	(0.032)	(0.174)	(0.141)	(0.103)	(0.116)	(0.261)	(0.076)	(0.095)
Implied coef. for Trade in High-inc.	-0.047	0.135	-0.085***	-0.204	0.140	-0.121*	-0.150	0.198	-0.061	-0.071
	(0.134)	(0.090)	(0.020)	(0.141)	(0.135)	(0.071)	(0.125)	(0.156)	(0.055)	(0.077)
1^{st}-stage Kleibergen-Papp F-statistic	14.39	14.39	14.39	14.39	14.39	14.39	14.39	14.39	14.39	14.39
N	6544	6544	6544	6544	6544	6544	6544	6544	6544	6544

Notes: This table presents IV results from estimating the effects of trade on ETR and mechanism outcomes in the full sample of developing and developed countries. Trade is measured as the sum of exports and imports divided by net domestic product (NDP). We estimate the IV described in equation 10. The first-stage regression is reported in Table A3. At the bottom of each column, we report the implied coefficient and estimated standard error based on the linear combination of the $Trade$ and the $Trade * \mathbb{1}$(High-inc.) coefficients. High-income is based on the World Bank income classification in 2018. We also report the 1^{st}-stage Kleibergen-Paap F-statistic. Across the columns, the outcome differs: column (1) is the effective tax rate on capital; column (2) is the effective tax rate on labor; column (3) is the first-differenced statutory corporate income tax (CIT) rate; column (4) is the corporate income share of net domestic product, where corporate income is the sum of corporate profits and corporate employee compensation; column (5) is the mixed income share of net domestic product; column (6) is the corporate profit share of net domestic product; column (7) is the employee compensation share of net domestic product; column (8) is the average effective tax rate on corporate profits; column (9) is the capital share of net domestic product; column (10) is the capital share of corporate income. For more details on the outcomes, see Section 3.1 and Section 6.1. For more details on the instrumental variables, see Section 5.2. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Appendix

Appendix A Additional Figures and Tables

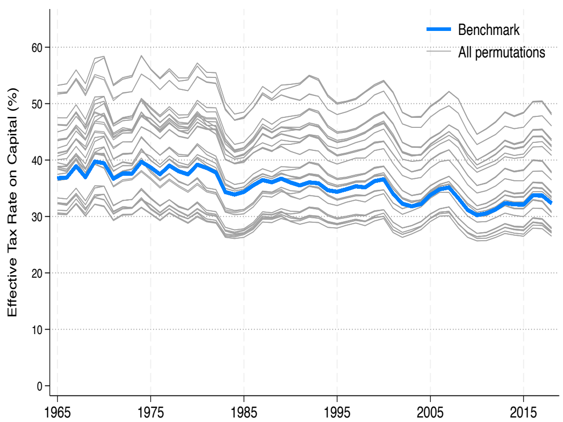
Figure A1: Data Coverage of Effective Tax Rates



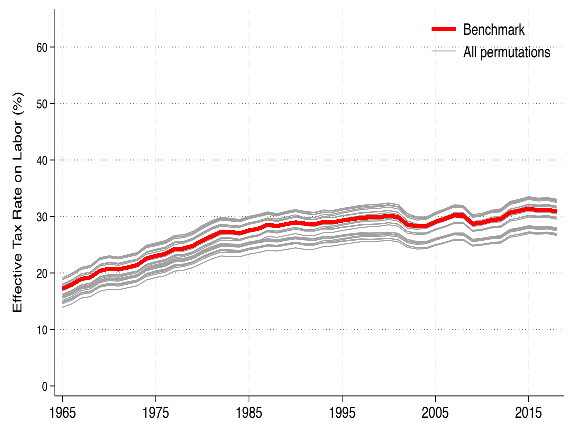
Notes: These panels show the coverage of our effective tax rate data between 1965 and 2018 at the global level (top left panel), in high income countries (bottom left panel), and in low- and middle-income countries (bottom right panel). Low, middle and high-income countries are based on the World Bank income classification in 2018. The solid lines plot the percent of total population and GDP that are covered in our data (left axis). The dashed lines show the number of countries in the data (right axis). The dataset is composed of two quasi-balanced panels. The first covers the years 1965-1993 and excludes communist regimes. The second covers 1994-2018 and integrates former communist countries, in particular China and Russia. See Section 3.3 for more details.

Figure A2: Robustness of ETR_K and ETR_L Trends by Development Levels

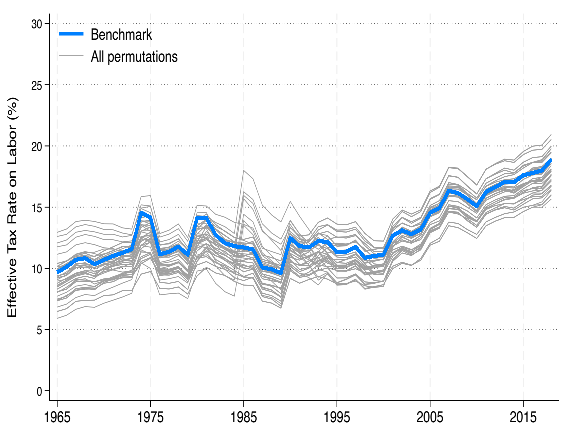
(a) ETR_K in HICs



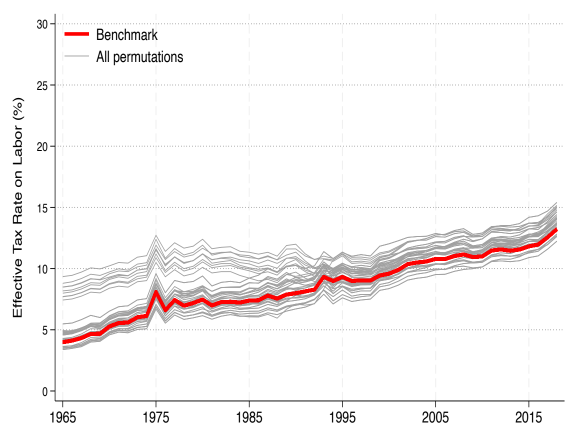
(b) ETR_L in HICs



(c) ETR_K in LMICs

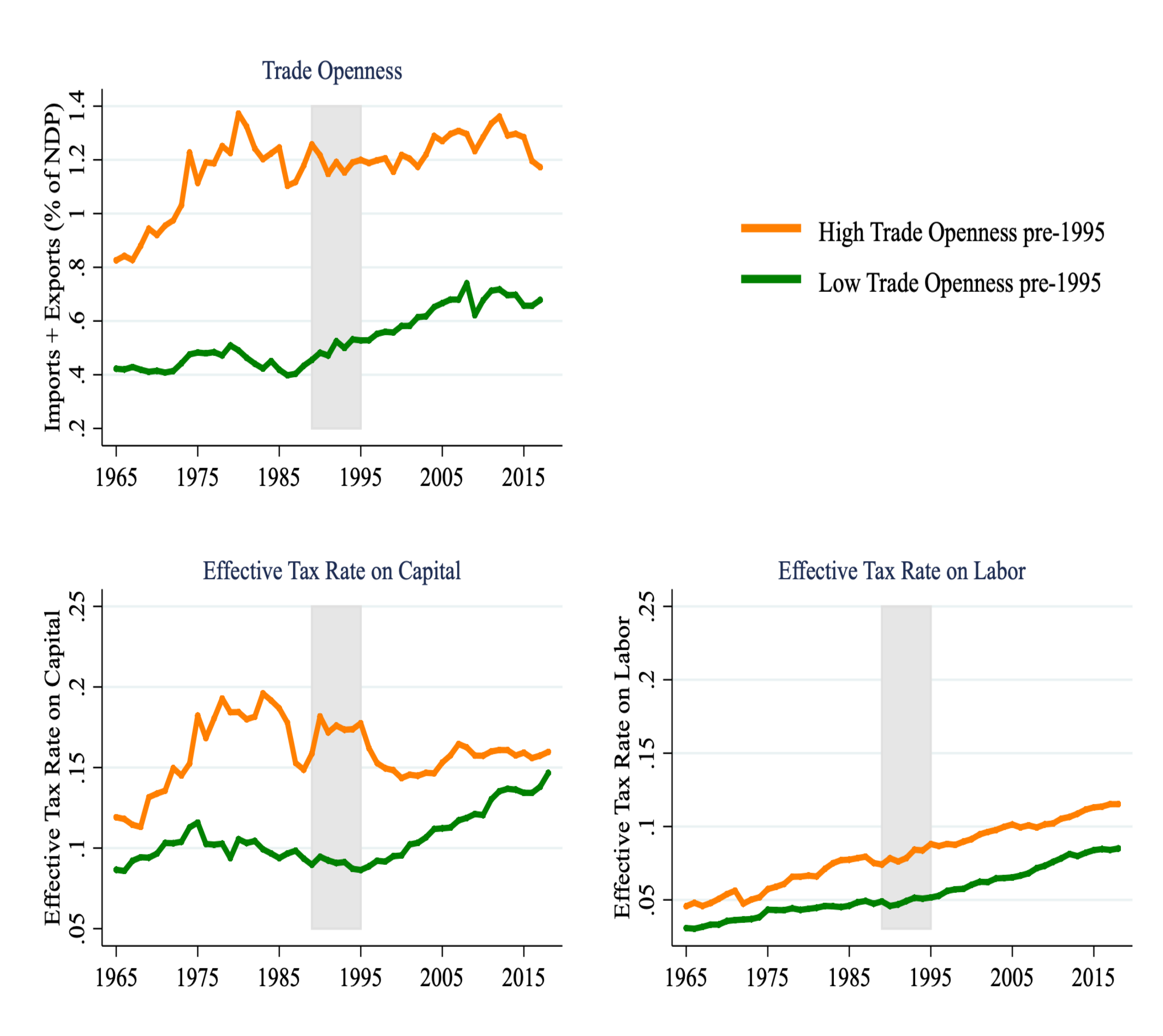


(d) ETR_L in LMICs



Notes: These panels show trends in the effective taxation of capital and labor for high-income countries (HICs, top panels) and low and middle-income countries (LMICs, bottom panels). Low, middle and high-income countries are based on the World Bank income classification in 2018. The benchmark series are denoted by the thick colored lines and the grey lines denote all 54 possible permutations of the series when varying the four key methodological choices (detailed in Section 4.2): the allocation of personal income tax revenue to capital vs labor; the allocation of mixed income to capital vs labor; presenting results for an unbalanced panel of countries vs a balanced panel via imputations; and, how to weight individual countries' series when aggregating them. Panel (c) corresponding to the ETR_K for low and middle-income countries is further decomposed in Figure 2.

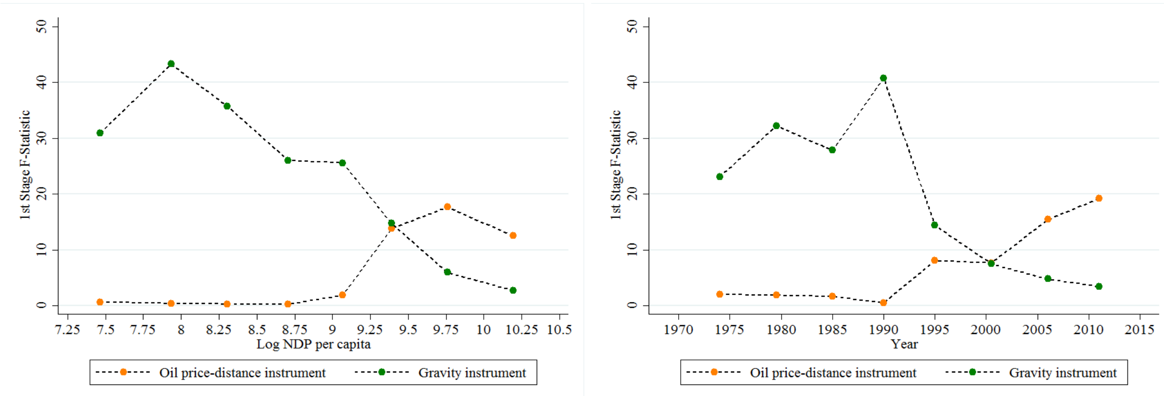
Figure A3: Trends by Initial Trade Openness in Developing Countries



Notes: These panels plot the time series of trade openness (top-left panel), effective tax rate on capital (bottom-left panel) and effective tax rate on labor (bottom-right panel). The sample is limited to low- and middle-income countries, according to the World Bank income classification in 2018. Within each panel, the orange line (green line) traces the evolution of the group which had relatively high (low) trade openness prior to 1995. Specifically, high (low) trade openness is defined as having average trade openness which lies above (below) the global average between 1965 and 1995. Trade openness is measured as the share of imports and exports in national domestic product; note that this share can exceed a value of 1. Each line plots the year fixed effects from an OLS regression in the relevant sub-sample of the outcome on country and year fixed effects. The inclusion of country fixed effects limits the influence of countries entering and leaving the sample. The fixed effects are normalized to equal the level of the outcome variable in the relevant sub-sample in 1965. The shaded area highlights the notable 1990-1995 period, which marks the beginning of the 'second wave' of globalization that featured a proliferation of bilateral and multilateral trade agreements (Egger, Nigai, & Strecker, 2019).

Figure A4: Strength of Individual Instruments Across Subsamples

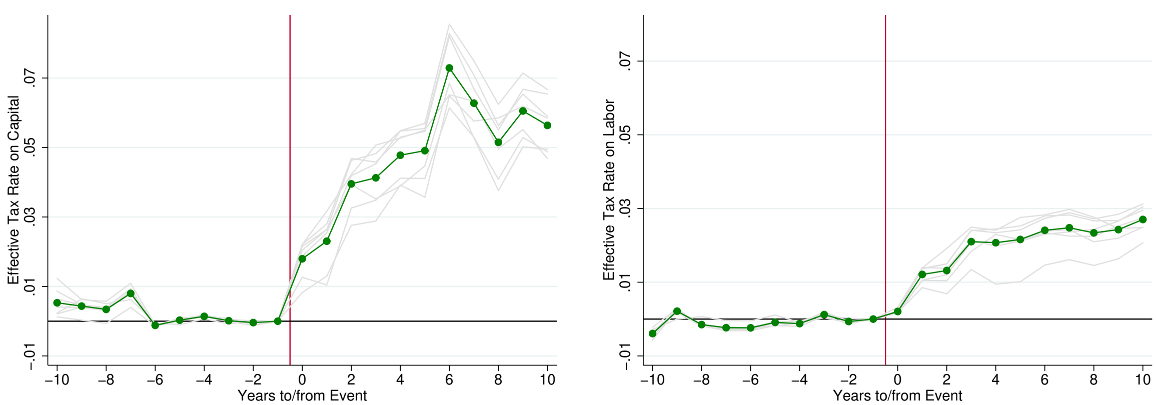
(a) Sub-samples of NDP per capita (b) Sub-samples of time-periods



Notes: These figures show the statistical strength of the instruments $Z^{oil-distance}$ and $Z^{gravity}$ in developing countries (low and middle-income countries based on World Bank classification in 2018, $N = 4970$). The outcome is the first-stage F-statistic from a regression of trade openness on each individual instrument, in subsamples of log NDP per capita (panel a) and years (panel b). The x-axis variable is partitioned into ten deciles, and the estimation is done in increments of one decile with a bandwidth of one additional decile of on either side. To maintain equal sample sizes, estimation centered on the first and the tenth decile are dropped. More details in Section 5.2.

Figure A5: Robustness of Trade Liberalization to Changing Events-Sample

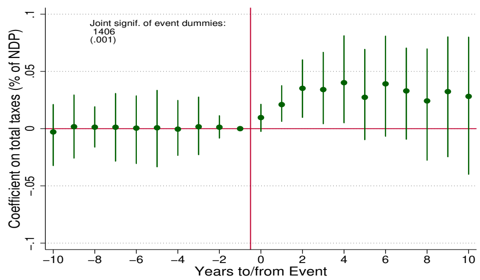
(a) Effective tax rate on capital (b) Effective tax rate on labor



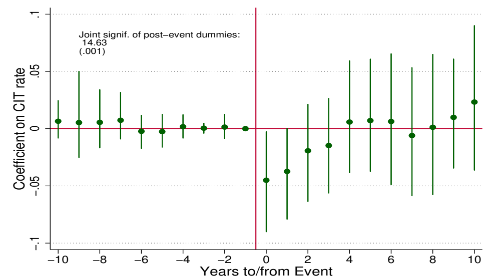
Notes: These figures show event study impacts of trade liberalization on the effective tax rate on capital (panel a) and the effective tax rate on labor (panel b). The solid green line displays the dynamic event-study coefficients β_e estimated in the full sample of 7 liberalization event-countries (Figure 5); the gray lines present the event-study coefficients estimated in samples that remove one event-country one at a time. More details in Section 5.1.1.

Figure A6: Mechanism Impacts in Trade Liberalization Event Studies

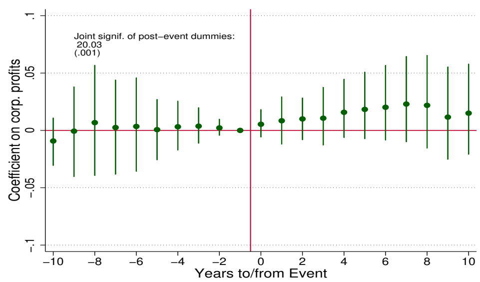
(a) Total taxes (% of NDP)



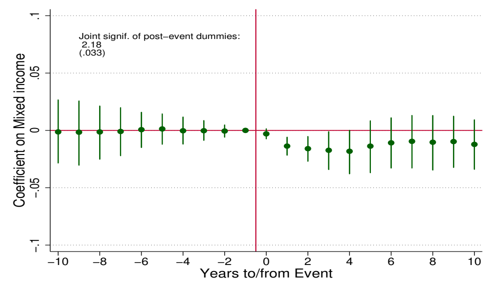
(b) Corp. income tax rate (first-diff.)



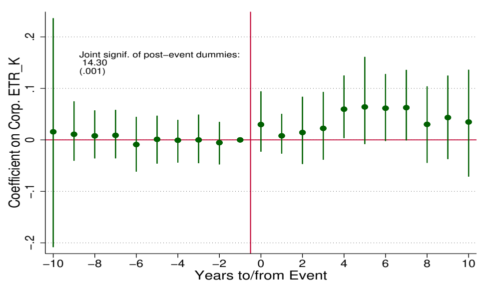
(c) Corporate income (% of NDP)



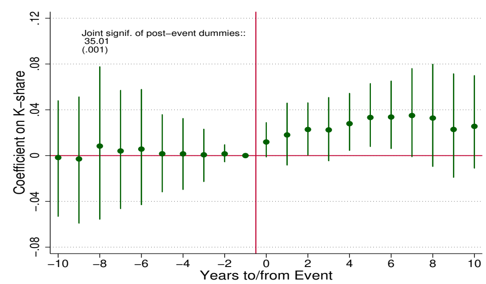
(d) Mixed income (% of NDP)



(e) Corporate ETR_K



(f) Capital income (% of NDP)



Notes: These panels show the impacts of the trade liberalization events on total taxes collected and mechanism outcomes. The panels are constructed using the method in Section 5.1, and similarly to Figure 5. Across panels, the outcome differs: panel a) is total tax revenue, as a percent of net domestic product (NDP); panel b) is the first-differenced statutory corporate income tax rate; panel c) is the corporate income share of net domestic product, where corporate income is the sum of corporate profits and employee compensation; panel d) is the mixed income share of net domestic product; panel e) is the average effective tax rate on corporate profits; panel f) is the capital share of net domestic product. In each panel, the top-left corner reports the F-statistic for the joint significance of post-event dummies, with the p-value reported in parentheses.

Table A1: Weights in Synthetic Control for Trade Liberalization Events

Treated Country	Event Year	Trade Openness	Weight	ETR_K	Weight	ETR_L	Weight	Reference
Argentina	1989	Bangladesh United States . .	97.3 2.7 % . .	Bangladesh Haiti Bolivia ...	41.6 % 14.1 % 13.4 % ...	Chile Togo Jordan ...	35.9 % 31.6 % 16.8 % ...	Goldberg and Pavcnik (2006)
Brazil	1988	Bangladesh United States Japan ...	59.8 % 32.2 6.1 % ...	Jordan Sudan Zimbabwe ...	35.7 % 21.2 % 12.7 % ...	Panama Guyana Chile ...	25.7 % 21.7 % 14.5 % ...	Goldberg and Pavcnik (2006), Dix-Carneiro and Kovak (2017)
China	2001	United States Bangladesh Dominican Rep. ...	36.2 % 36.0 % 12.2 % ...	Congo Nicaragua Gabon ...	41.8 % 26.3 % 14.2 % ...	Kuwait Pakistan Uganda ...	31.1 % 22.9 % 20.2 % ...	Brandt et al. (2017)
Colombia	1985	Bangladesh Iran Guatemala ...	50.7 % 22.6 % 12.5 % ...	Kuwait Gabon Sierra Leone ...	67.9 % 14.6 % 12.6 % ...	Paraguay Sudan Cameroon ...	45.5 % 15.0 % 11.5 % ...	Goldberg and Pavcnik (2006; 2016)
India	1991	United States Bangladesh . .	76.4 % 23.6 % . .	Uganda Bolivia Haiti ...	41.4 % 14.0 % 4.6 % ...	Lebanon Oman Jordan ...	37.9 % 17.6 % 16.2 % ...	Goldberg and Pavcnik (2006, 2016); Topalova et al. (2009)
Mexico	1985	Bangladesh Uruguay Spain ...	72.0 % 9.6 % 8.0 % ...	Sierra Leone Bahrain Bolivia ...	33.2 % 23.6 % 14.7 % ...	Tunisia Zimbabwe Uruguay ...	31.1 25.8 % 15.9 % ...	Feenstra and Hanson (1997); Goldberg and Pavcnik (2006, 2016)
Vietnam	2001	Thailand Ghana Venezuela ...	42.4 % 22.6 % 21.7 % ...	Korea Luxembourg Trinidad & Tob. ...	45.8 % 19.2 % 17.3 % ...	Bangladesh Myanmar Haiti .	72.8 % 22.6 % 4.6 % .	Goldberg and Pavcnik (2016), McCaig and Pavcnik (2018)

Notes: This table shows the seven treated countries and the three countries with the largest weight in the synthetic control group for each treated country and outcome (trade openness, ETR_K, ETR_L). For each outcome, the pool of possible donor countries consists of all non-treated countries with a balanced panel over all the pre-event periods that are used in the matching procedure.

Table A2: Synthetic Difference-in-Difference of Trade Liberalization

	Trade (1)	ETR_K (2)	ETR_L (3)
Panel A: *Synthetic control for each outcome separately*			
Post*Treat	0.064	0.0457***	0.020**
	(0.047)	(0.015)	(0.009)
Imputed treatment effect	0.070*	0.047***	0.020***
	(0.039)	(0.009)	(0.005)
Panel B: *Synthetic control for all outcomes jointly*			
Post*Treat	0.092*	0.033*	0.012
	(0.044)	(0.016)	(0.008)
Imputed treatment effect	0.101***	0.033***	0.012***
	(0.028)	(0.006)	(0.004)
Panel C: *Donor pool excluding major trading partners*			
Post*Treat	0.0733	0.0470***	0.0187**
	(0.0558)	(0.0151)	(0.00852)
Imputed treatment effect	0.0821**	0.0481***	0.0186***
	(0.0357)	(0.00912)	(0.00450)
Panel D: *Donor pool restricted to not-yet liberalized*			
Post*Treat	0.0544	0.0541***	0.0136
	(0.0582)	(0.0144)	(0.0088)
Imputed treatment effect	0.0625*	0.0548***	0.0135***
	(0.0348)	(0.0096)	(0.0051)
Panel E: *Donor pool restricted to same region*			
Post*Treat	0.0490	0.0341*	0.0074
	(0.0606)	(0.0192)	(0.0087)
Imputed treatment effect	0.0586*	0.0357***	0.0173***
	(0.0319)	(0.0124)	(0.0050)
Panel F: *Donor pool restricted to LMICs*			
Post*Treat	0.0768	0.0405**	0.0169*
	(0.0529)	(0.0167)	(0.00930)
Imputed treatment effect	0.0854**	0.0419***	0.0167***
	(0.0345)	(0.00846)	(0.00523)
N	294	294	294

Notes: This table shows the results from estimating the difference-in-difference effect and the imputed treatment effect - see Appendix C.2 for details. In Panel A, the synthetic control is created separately for each outcome (trade, ETR_K, ETR_L) and each liberalization country-event. In Panel B, the synthetic control is created for all three outcomes jointly for each country-event. In Panel C, the donor pool for each country-event excludes the 5 major import and export trading partners of the country, measured in terms of total volume of trade in the year immediately preceding liberalization. In Panel D, the donor pool excludes all countries that have already liberalized by the time of the event (based on Wacziarg & Welch, 2008). In Panel E, the donor pool is restricted to countries in the same region. In Panel F, the donor pool is all low and middle-income countries (LMICs), based on the World Bank income classification in 2018.* p<0.10 ** p<0.05 *** p<0.01.

Table A3: First-Stage and Reduced Form Regressions

	1^{st}-stage	Reduced form		1^{st}-stage		Reduced form	
	Trade	ETR_K	ETR_L	Trade	Trade$*\mathbb{1}$(High-inc.)	ETR_K	ETR_L
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Z^{gravity}$	0.068***	0.007***	0.003***	0.017	0.037***	0.016*	0.003
	(0.010)	(0.002)	(0.001)	(0.018)	(0.014)	(0.008)	(0.003)
$Z^{oil-distance}$	-0.115***	-0.017***	-0.013**	-0.089***	-0.023	-0.017**	-0.011***
	(0.036)	(0.006)	(0.006)	(0.015)	(0.014)	(0.007)	(0.003)
1^{st}-stage F-statistic	24.57			23.27	11.10		
1^{st}-stage Sanderson-Windmeijer Weak Instruments F-statistic	24.57			41.43	25.75		
1^{st}-stage Kleibergen-Paap F-statistic	24.57			14.39			
Sample	Developing countries only			Developing and developed countries			
N	4970	4970	4970	6544	6544	6544	6544

Notes: This regression table shows the first stage and the reduced form results. The sample is developing countries ($N = 4970$) in cols. (1)-(3), and developing and developed countries ($N = 6544$) in columns (4)-(7). Trade is exports and imports divided by net domestic product. Column (1) corresponds to the first-stage in developing countries, used in Tables 1-2-3. Columns (4)-(5) correspond to the first-stage in the full sample, which estimates heterogeneous effects by development level, and which is used in Table 5. We report several 1^{st}-stage statistics: the F-statistic of excluded instruments; the Sanderson-Windmeijer multivariate F-test of excluded instruments; and, the Kleibergen-Paap F-statistic. When there is only one endogenous regressor (column 1), these three F-statistics are equivalent. Note in columns (4)-(5) that there is only one Kleibergen-Paap F-statistic, which evaluates the overall strength of the first-stage, even though there are two first-stage regressions. Columns (2)-(3) and (6)-(7) report the reduced form regressions of the instruments on the effective tax rates for capital, ETR_K, and labor, ETR_L. Developing (developed) countries are low and middle-income countries (high-income countries) according to the World Bank income classification in 2018. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table A4: Trade Impacts on Effective Tax Rates in Different Samples

	Sample changes related to tax revenue data				Sample changes related to System National Accounts data			Sample changes related to time-periods and balancedness		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: ETR_K										
Trade	0.113***	0.123***	0.112**	0.075**	0.108**	0.094*	0.120***	0.092*	0.130**	0.103**
	(0.037)	(0.043)	(0.055)	(0.032)	(0.043)	(0.052)	(0.045)	(0.051)	(0.061)	(0.040)
Panel B: ETR_L										
Trade	0.064***	0.031***	0.094*	0.023**	0.039**	0.036*	0.050**	0.035**	0.066**	0.072***
	(0.018)	(0.010)	(0.053)	(0.011)	(0.015)	(0.018)	(0.020)	(0.014)	(0.030)	(0.022)
Modifications to benchmark sample in Table 1	Remove interpolated tax revenue	Only use HA tax data	Only use ICTD tax data	Only use OECD tax data	Remove composite SNA data	Only use SNA1968 data	Only use SNA2008 data	Only use pre-1994 years	Only use post-1994 years	Fully balanced panel 1965-2018
N	4612	2296	1194	1480	2783	1002	1781	2151	2819	2479

Notes: This table presents results from estimating the effect of trade on effective tax rates in different samples across developing countries. The estimation is identical to the benchmark IV model in column (2) of Table 1; across columns, the sample differs from that benchmark sample. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. The outcome is the effective tax rate on capital, ETR_K, in Panel A and the effective tax rate on labor, ETR_L, in Panel B. Trade is measured as the sum of exports and imports divided by net domestic product (NDP). In the first four columns, sample-changes are made to the tax revenue data: interpolated values are dropped in column 1; the only data-source is historical archives (HA) in column 2; the only data-source is ICTD in column 3; the only data-source is OECD in column 4. In the next three columns, sample-changes are made to the system of national accounts (SNA) data: in column (5), the composite SNA values are removed; in column (6), only data from SNA1968 are used; in column (7), only data from SNA2008 are used. In the final three columns, sample-changes are made regarding balancedness: in column (8), the quasi-panel between 1965 and 1993 is used; in column (9), the quasi-panel between 1994 and 2018 is used; in column (10), the fully balanced panel of countries between 1965 and 2018 is used. For more details on the interpolations, imputations and data-sources, see Section 3 and Appendix B.

Table A5: Robustness of Results for Total Taxes and Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total taxes (% of NDP)						
Trade	0.108*	0.091**	0.093***	0.103***	0.096***	0.176**
	(0.063)	(0.039)	(0.032)	(0.032)	(0.033)	(0.077)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel B: CIT rate (first-diff.)						
Trade	0.004	-0.008	-0.012*	-0.013*	-0.012*	-0.031*
	(0.011)	(0.009)	(0.007)	(0.007)	(0.007)	(0.016)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel C: $log(1+$CIT rate)						
Trade	-0.009*	-0.006	-0.009*	-0.010*	-0.009*	-0.027*
	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)	(0.015)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel D: Corp. income (% of NDP)						
Trade	0.225***	0.210***	0.180***	0.193***	0.183***	0.181**
	(0.052)	(0.046)	(0.043)	(0.044)	(0.044)	(0.090)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel E: Mixed income (% of NDP)						
Trade	-0.199***	-0.175***	-0.191***	-0.201***	-0.191***	-0.112
	(0.048)	(0.041)	(0.041)	(0.038)	(0.041)	(0.116)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel F: Capital share of NDP						
Trade	0.121***	0.112**	0.157***	0.170***	0.163***	0.111**
	(0.034)	(0.043)	(0.033)	(0.032)	(0.034)	(0.050)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Panel G: Corp. ETR_K						
Trade	0.237*	0.163	0.129*	0.149*	0.138*	0.399**
	(0.131)	(0.104)	(0.075)	(0.076)	(0.075)	(0.188)
1^{st} stage K-P F-stat	31.24	14.24	23.09	34.83	45.17	10.80
N	4970	3984	4970	4970	4970	4970
Modifications to IV in Panel B of Table 3	NDP weights	Include country-year controls	Include **1**(oil-rich)*year fixed effects	Winsorize trade at 5%-95%	Only use $Z^{gravity}$ instrument	Only use $Z^{Oil-Dist}$ instrument

Notes: This table presents robustness checks for trade's impacts on several outcomes in developing countries. Developing countries are low and middle-income countries according to the World Bank income classification in 2018. Trade is the sum of exports and imports divided by net domestic product (NDP). The outcome differs across panels, and the specification differs across columns: each cell is the coefficient from a separate IV regression. We report the 1^{st}-stage Kleibergen-Paap F-statistic separately for each IV regression. Panel A is total taxes as a % of NDP. Panel B is the first-differenced corporate income tax (CIT) rate. Panel C is the percent change from log of $(1 + \text{CIT rate})$. Panel D is the corporate income share of NDP. Panel E is the mixed income share of NDP. Panel F is the capital share of NDP. Panel G is the average effective tax rate on corporate profits. The different specifications across columns are the same as in Table 1 - please refer to that table for more details. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table A6: Heterogeneous Impacts of Trade in Developing Countries

	ETR_K (1)	ETR_L (2)	Corp. income (3)	Corp. ETR_K (4)
Panel A: Large Taxpayer Unit				
Trade	0.100**	0.039*	0.183***	0.088*
	(0.046)	(0.022)	(0.055)	(0.050)
Trade$*\mathbb{1}$(LTU)	0.024	0.041**	-0.000	0.129
	(0.056)	(0.020)	(0.049)	(0.132)
Implied coef. for	0.125***	0.081***	0.183***	0.218**
Trade with LTU	(0.041)	(0.019)	(0.040)	(0.095)
Panel B: Customs-Tax Integration				
Trade	0.099**	0.040*	0.182***	0.135*
	(0.043)	(0.024)	(0.052)	(0.077)
Trade$*\mathbb{1}$(Customs-Tax)	0.066	0.101*	0.005	0.228
	(0.124)	(0.059)	(0.111)	(0.262)
Implied coef. for	0.166	0.142***	0.187**	0.363*
Trade with Customs-Tax	(0.101)	(0.050)	(0.087)	(0.208)
Panel C: Value-Added Tax				
Trade	0.099**	0.040**	0.182***	0.130
	(0.044)	(0.020)	(0.054)	(0.096)
Trade$*\mathbb{1}$(VAT)	0.031	0.048**	0.002	0.103
	(0.060)	(0.022)	(0.053)	(0.112)
Implied coef. for	0.131***	0.089***	0.185***	0.234***
Trade with VAT	(0.045)	(0.021)	(0.043)	(0.083)
Panel D: International Accounting Standards				
Trade	0.102***	0.039**	0.185***	0.140*
	(0.038)	(0.017)	(0.050)	(0.081)
Trade$*\mathbb{1}$(IAS)	0.037	0.052**	-0.007	0.151
	(0.070)	(0.025)	(0.056)	(0.144)
Implied coef. for	0.140**	0.092***	0.178***	0.291***
Trade with IAS	(0.056)	(0.023)	(0.049)	(0.113)
N	4970	4970	4970	4970

Notes: This table estimates heterogeneous IV effects of trade in developing countries (low and middle-income countries according to the World Bank income classification in 2018). Trade is the sum of exports and imports divided by net domestic product (NDP). Outcomes differ across columns: column (1) is the effective tax rate on capital, ETR_K; column (2) is the effective tax rate on labor, ETR_L; column (3) is the corporate income share of NDP; column (4) is the average effective tax rate on corporate profits. We estimate

$$y_{ct} = \mu \cdot trade_{ct} + \kappa \cdot trade_{ct} \cdot \mathbb{1}(A)_{ct} + \theta \cdot \mathbb{1}(A)_{ct} + \pi_c + \pi_t + \epsilon_{ct}$$

where $\mathbb{1}(A)_{ct}$ is an indicator variable which takes a value of 1 in all years after the administrative reform has been implemented. We instrument for $trade_{ct}$ and $trade_{ct} \cdot \mathbb{1}(A)_{ct}$ using the two instruments (Section 5.2). The coefficient on $\mathbb{1}(A)_{ct}$ is also estimated, but is not reported in the table. In Panel A, the administrative reform is the existence of a large taxpayer unit (LTU); this variable is coded based on the USAID's 'Collecting Taxes Database' (website link) and country-sources. In Panel B, the administrative reform is the integration of the customs authority and the domestic tax authority in a single revenue agency; this variable is coded based on USAID's 'Collecting Taxes Database' (website link), the OECD Tax Administration Comparative Series (website link), and country-sources. In Panel C, the administrative reform is the implementation of a value-added tax (VAT); this variable is coded based on Keen and Lockwood (2010) and country-sources. In Panel D, the administrative reform is the adoption of internationl accounting standards (IAS); this variable is coded based on the IAS country-profiles (website link). At the bottom of each column and panel, we report the implied coefficient and estimated standard error based on the linear combination of the $trade_{ct}$ and $trade_{ct} \cdot \mathbb{1}(A)_{ct}$ coefficients. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Table A7: Additional Heterogeneity Impacts of Trade

Heterogeneity H_c :	Small population (1)	Capital openness (2)
Panel A: CIT rate (first-diff).		
Trade	-0.065***	0.006
	(0.016)	(0.038)
Trade$*H_c$	-0.025	-0.121**
	(0.064)	(0.054)
Implied coef. for Trade in H_c	-0.090	-0.115***
	(0.055)	(0.028)
1^{st}-stage Kleibergen-Papp F-statistic	7.01	9.96
N	6544	6017
Panel B: ETR_K		
Trade	0.294	0.456**
	(0.207)	(0.224)
Trade$*H_c$	-0.696	-0.410
	(0.511)	(0.296)
Implied coef. for Trade in H_c	-0.401	0.045
	(0.373)	(0.104)
1^{st}-stage Kleibergen-Papp F-statistic	7.01	9.96
N	6544	6017
Panel C: ETR_L		
Trade	0.155**	0.112
	(0.070)	(0.111)
Trade$*H_c$	-0.006	0.126
	(0.230)	(0.178)
Implied coef. for Trade in H_c	0.149	0.239**
	(0.199)	(0.095)
1^{st}-stage Kleibergen-Papp F-statistic	7.01	9.96
N	6544	6017

Notes: This table presents results from estimating heterogeneous effects of trade on outcomes in the full sample of developed and developing countries. Trade is the sum of exports and imports divided by net domestic product. We estimate an IV similar to equation (10), but where the interaction term H_c is an indicator for small population (column 1), or an indicator for capital openness (column 2). Small population takes a value of 1 if the country's population in 2018 was below 40 million. Capital openness takes a value of 1 if the country's average value of the Chinn-Ito index (Chinn & Ito, 2006) lies above the median value of all country-years. Both of these heterogeneity dimensions are therefore country-specific but time-invariant. The sample size is smaller in column (2) due to data-availability of the Chinn-Ito variable. The panels differ by outcome: panel a) is the first-differenced corporate income tax (CIT) rate; panel b) is the effective tax rate on capital, ETR_K; panel c) is the effective tax rate on labor, ETR_L. At the bottom of each column and panel, we report the implied coefficient and estimated standard error based on the linear combination of the $Trade$ and the $Trade$*H_c coefficients. We also report the 1^{st}-stage Kleibergen-Paap F-statistic. For more details on the IV, see Section 5.2 and 6.5. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level.

Appendix B Data & Construction of Effective Tax Rates

This appendix section provides an overview of the data sources used to create our tax revenue and national income series (Section B.1). Additionally, we discuss the methodology to measure effective tax rates (Section B.2).

B.1 Data sources

Tax revenue data Our tax revenue data draws from three key sources:

(i) **OECD Government Revenue Statistics** (website link): OECD revenue statistics take precedence in our data hierarchy as it contains all types of tax revenues already arranged in the OECD taxonomy of taxes. While it covers all OECD countries, it only covers a subset of developing countries which typically start in the early 2000s.

(ii) **ICTD Government Revenue Dataset** (website link): ICTD data covers many developing countries, but only begins in the 1980s. ICTD at times does not separate income taxes into personal vs. corporate taxes and often does not contain social security contributions.

(iii) **Archival data**: The main archival data collection corresponds to the digitization of the Government Documents section in the Lamont Library at Harvard University (website link). For each country, we scanned, tabulated and harmonized official data from the public budget and national statistical yearbooks, to retrieve official tax revenue statistics. Table B2 lists the main historical documents used in each country's time-series. In the case where the document is a statistical yearbook, the initial listed source is always a report produced by the finance ministry or the national tax authority. To complement hard-copy archival data, we retrieved countries' online reports, usually published by their national statistical office or finance ministry. We also used complementary sources, including offline archival Government Finance Statistics data from the IMF which covers the period 1972-1989. For social security contributions, we relied on two additional sources: the 'D61' statistic on social contributions in the household sector in SNA-1968 and SNA-2008, and data from Fisunoglu, Kang, Arbetman-Rabinowitz and Kugler (2011).

To increase the credibility of the tax revenue series based on newly digitized historical documents, we base our approach on the following criteria:

1. We seek to build long time-series from the archival records in order to overlap with pre-existing sources (OECD, ICTD, IMF). We use the overlapping years to inspect that the different sources provide similar estimates of the overall levels of taxes collected and to verify that they report the same set of taxes in place. If discrepancies exist when data sources overlap, we inspect the accuracy of each source with additional information. For this reason, switches in data-source rarely lead to a significant change in trend.

2. In historical time-periods where no overlap exists with pre-existing sources, we find academic publications and policy reports to compare the estimated overall levels of tax/GDP. When discrepancies exist, we investigate its causes (e.g. inclusion of non-tax revenues, differences in estimated GDP numbers).

3. We take note of instances where the overall tax take, or individual tax types, see sudden and large changes. We use additional sources to try to determine the proximate causes as they relate to policy changes, political transitions or economic shocks. We flag cases where we cannot find the proximate cause or where the political or economic events induce very significant volatility in the time-series.

4. We aim to be conservative in our inclusion of countries and time-periods. Specifically, we exclude countries in time-periods where data exists but where significant concerns remain about its reliability (and where it proves difficult to find corroborating sources). These instances are often in periods of significant political or economic change. For example, we exclude Afghanistan in the late 1970s and early 1980s; Cambodia in the late 1980s and early 1990s; Dominican Republic in the early 1960s; and, Namibia in 1990.

Table B2 summarizes our decisions as they relate to these four criteria in each country in our sample. The table emphasizes the uncertainty that exists for specific countries in specific time periods and we flag instances where we assess the data to be worthy of inclusion but where it should still be interpreted with caution and additional investigations would be helpful. We confirm that none of our main results change if we exclude these flagged instances (available upon request).

Moreover, the report in the supplementary appendix provides case-studies with additional details on our decisions. The report also provides links to the underlying historical documents in each country. The case-studies are currently limited to 67 countries, including the most populous ones, but will ultimately cover the entire sample. We invite comments from researchers to improve the accuracy of the series as we build the case studies and expand the data to recent years.

Equipped with the historical time-series, we have to construct long-run panels across sources. Below, we outline the guiding rules to harmonize across sources and to improve data quality for the measurement of each type of tax. We flag instances where we consider the series to be legitimate, but where harmonization proved more challenging due to coinciding economic or political changes. The main decisions and considerations related to the guiding rules are summarized in Table B2.

1. We first rely on OECD data whenever it exists. Archival data is initially second in priority, but we revise this based on whether ICTD data provides a long time series and separates personal from corporate income taxes. We also study if ICTD has the better match in overlapping time-periods with OECD data. When possible, we aim to use no more than two data sources per country.

2. We exclude country-years for communist/command economies. This implies that our panel size jumps in 1994, including when China and Russia first appear. The year 1994 is a few years removed from the dissolution of the Soviet Union but, as discussed below, arguably corresponds to China's establishment of a modern tax system (World Bank, 2008).

3. When none of the data sources separate PIT from CIT, we use academic sources and tax legislation to assign values.

4. To guard against omitting significant values of decentralized tax revenues, we use the OECD database on subnational government finance (link) to find the countries with significant state and local taxes, and we attempt to collect further data for these countries if necessary.

5. We linearly interpolate data when a given tax type is missing, but for no more than 4 years in a time-series and without extrapolation. We check for significant socio-economic changes that could cast doubt on the continuity of the tax revenue series and do not interpolate in such years.

6. We only use actual amounts of taxes collected, and do not rely on estimated values.

China's establishment of a modern tax system in 1994

In our benchmark setting, we only include formerly communist economies into our data starting in 1994. Given China's weight in the global economy, it is worth reviewing the reason for that choice. The tax revenue data for China covers most of our sample period although its quality improves markedly in the 1980s. Official statistics are available online: link here.

Prior to the 1980s, China had a command economy model of 'profit delivery,' in which the state directly received the revenues of profitable SOEs, and subsidized unprofitable ones. A corporate income tax first appears in China in 1983-84, but the majority of the base continues to be state-owned enterprises. In 1985, the tax system was further reformed into a 'fiscal contracting' system whereby firms negotiated a fixed lump-sum payment (regardless of economic outcomes), which cannot be split into labor versus capital taxes (nor into consumption taxes). We therefore exclude the 'pseudo'-CIT revenue dating from 1985 through 1993.

Rather, we consider that China's modern tax system began in 1994. The World Bank (2008) shows that, in 1994, China established for the first time a central tax administration; reformed the 'fiscal contracting' system; unified the PIT; created a VAT; and reduced 'extra budgetary' (non-tax) revenues. Thus from 1994 onward we can categorize tax revenue precisely by type, assign them to capital or labor, and estimate our ETRs.

National accounts data To compute factor incomes of net domestic product, we combine two main datasets from the United Nations Statistics Division. The first is the 2008 System of National Accounts (SNA) online data repository. The second is the 1968 SNA archival material. The 2008 and 1968 SNAs initially have different reporting classifications; to the best of our knowledge, our project is the first to harmonize national accounts across these two sources.

To estimate capital and labor factor incomes requires information on the 4 main sub-components that make up net domestic product (see equation 3). However, in some country-years where we have information on domestic product from an SNA dataset, there may not be data on all four sub-components at the same time. This is more frequently the case for the 1968 SNA than for the 2008 SNA and it is most frequent for mixed income (OS_{PUE}). In these cases, we first attempt to recover the value of the missing component using data from the other SNA dataset and national accounting identities with non-missing values for other components within the same country-year. For the remaining cases after applying this process, we impute values for the component. All of the regressions in Sections 5-6 include dummy variables for these composite cases; our main results also hold without the imputed values (Table A4). For the imputation, we follow the procedure from Blanchet et al. (2021). The World Inequality Database uses this procedure to impute consumption of fixed capital (depreciation) when it is missing in countries' series. For example, applying this procedure in our setting means that we model OS_{PUE} as a function of log national income per capita, a fixed country characteristic, and an AR(1) persistence term.

Table B1 summarizes the national accounts coverage in our dataset. The 'Complete SNA2008' row refers to country-years where all components of net domestic product are extracted from the 2008 SNA; similarly for the 'Complete SNA1968' row. The 'Composite' row counts instances where one component (or more) of net domestic product is initially missing from an SNA dataset and is retrieved from the other SNA dataset, is calculated via accounting identities, or is imputed.

Table B1: Main Data Sources

	Country-year obs.	%
Panel A: Tax revenue data		
OECD	2866	42.3%
Archives	2681	39.4%
ICTD	1249	18.3%
N	*6816*	*100%*
Panel B: Factor income data		
Complete SNA2008	2463	36.1%
Complete SNA1968	1362	20.0%
Composite	2991	43.9%
N	*6816*	*100%*

Notes: See Section B.1 for more details on the data-sources for tax revenue and factor income.

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Afghanistan	2003-2018	ICTD (2003-2018)		Exists HA series (1973-1978), but series is short and tax/GDP figures appear too volatile (could not find corroborating evidence); historical ICTD data exists (1982-1989), but no disaggregation by tax type	
Albania	1994-2018	ICTD (1994-2018)		Data begins in post-communist period; existing HA data nested in coverage in ICTD, so prefer ICTD source (and levels of tax revenues are comparable between sources)	Polackova (1996) tax/GDP estimate in 1993-1994 is slightly higher than ours, but likely includes non-tax revenues; World Bank (2020) tax/GDP matches our estimates from 1995 to 1998.
Algeria	1965-2017	HA (1965-2017)	*Annuaire Statistique de l'Algerie*	Taxe sur chiffre d'affaires is classified as unallocable between PIT and CIT in HA, but it is a tax on firms; 'Contributions diverses' is left as an excise tax in 1960s; interpolate 1967, 1970-1971, 1974.	
Argentina	1965-2018	HA (1965-1989); OECD (1990-2018)	*Sintesis Estadistica Mensual, Boletin Mensual de Estadistica*	Historically stable tax/GDP series, despite multiple political changes, until growth in tax take in 1990s when indirect tax expands; interpolate direct tax split (PIT vs CIT) between 1961-1969 and 1984-89, based on ratios on adjacent years; social security data from Alvaredo (2010) is comparable to OECD in overlapping years, so is preferred historical source.	Tax/GDP numbers comparable to historical time-series from Alvaredo (2010).
Armenia	1994-2018	HA (1994-2018)	Data provided by Statistical Committee of the Republic upon request	Independence in 1991, but official published revenue data begins in 1994; notable dip in social security in mid-2010s is genuine, results from several reforms (IMF, 2019; Asatryan, 2014).	Polackova (1996) tax/GDP estimate in 1993-1994 is slightly higher than ours, but likely includes non-tax revenues
Australia	1965-2018	OECD (1965-2018)			
Austria	1965-2018	OECD (1965-2018)			
Azerbaijan	1994-2018	ICTD (1994); HA (1995-2018)	Data retrieved from State Statistical Committee online data website	ICTD data is more accurate in 1994, from 1995 matches in trends and levels with HA data; independence in 1991, but unrest ensued until 1994 and limited government records (HA records unreliable, GDP numbers hard to corroborate); non-tax revenues are significant, especially since early 2000s; spike in CIT revenue in late 2000s reflects genuine economic shock (Aliyev and Gasimov, 2016)	
Bahamas	1973-2018	HA (1973-1989); OECD (1990-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	Historical HA data is based on IMF sources; social Initial difference between HA and OECD in overlapping years is due to social security contributions (missing in HA).	
Bahrain	1974-2018	HA (1974-1987); ICTD (1988-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	Historical HA data is based on IMF sources; change in CIT revenues in 1970s corresponds to nationalization and expropriation events (Kobrin, 1984) and there was no major change to oil production during this period (Ross and Mahdavi, 2015)	Comparable tax/GDP numbers in recent periods based on World Bank (2020), though the data in latter source stops in 2004.
Bangladesh	1976-2018	HA (1976-2000); ICTD (2001-2018)	*Budget Book, Statistical Digest of Bangladesh*	Independence in 1971, but reliable government data begins in 1976. Interpolate 1980-1981; very low direct taxes collected on firms prior to 1986 reform, and significant CIT drop in 2003.	Comparable tax/GDP numbers with ICTD in overlapping periods.
Barbados	1972-2018	HA (1972-1990); OECD (1991-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	HA data is based on IMF historical reports; use social security as reported in initial sources, corroborated with data from Fisunoglu et al. (2011)	
Belarus	1992-2018	ICTD (1993-2018)		ICTD data exists in 1991 but it is not disaggregated; decrease in CIT and increase in indirect taxes in early 2000s, may be due both to Russian financial crisis and to ICTD switching its source from IMF Article IV to IMF GFS [flagged]	Consistent tax/GDP when comparing to World Bank (2013), after adjusting ICTD for existence of social security contributions
Belgium	1965-2018	OECD (1965-2018)			
Belize	1982-2018	HA (1982-1989); OECD (1990-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	HA data is based on historical IMF data; interpolate 1986-1987; social security contributions missing in HA, we take it from Fisunoglu et al. (2011); social security started in 1979 (SSA, 2015)	
Benin	1965-2018	HA (1965-2018)	*Comptes de la Nation, Statistiques Finances Publiques*	Social security first implemented in 1970 (SSA, 2017); interpolate between 1988 and 1990.	Historical sources are hard to find. HA series comparable to historical IMF series in early periods (1960-1970), and dip in late 1980s exists across sources.
Bolivia	1965-2018	HA (1965-1989); OECD (1990-2018)	*Anuario Estadistico, Bolivia en Cifras*	Use historical data from Fisunoglu et al. (2011) for social security, which started prior to our time-coverage (SSA, 2017); unclear what 'complementaria' tax (1960-1970) refers to, we assign it equally to PIT and CIT; large decline in mid-1980s appears genuine (Kehoe et al., 2019)	Historical tax/GDP numbers comparable to Kehoe et al. (2019) and Sachs (1990), though larger than numbers reported in Thirsk (1997) in 1970s [flagged].
Bosnia and Herzegovina	1999-2018	ICTD (1999-2018)		War ends in 1995 but reliable data only starts in 1999; important role of local taxation (Fox and Wallich, 1997; Kandeva, 2001), compare data from ICTD with IMF GFS and Zorn et al. (1999) which suggest local tax sources are adequately covered.	Comparable tax/GDP numbers with World Bank (2020) after 2005, but higher tax/GDP reported in Ding and Sherif (1999) for historical period [flagged].
Botswana	1967-2018	HA (1967-1989); ICTD (1990-2003); OECD (2004-2018)	*Annual Statements of Accounts, Statistical Abstract, Statistical Bulletin, Financial Statistics*	OECD data is missing trade taxes, which we bring in from ICTD in overlapping years; GDP estimates differ in the pre-1990 period between IMF, World Bank and UN-SNA sources [flagged], we use World Bank source; CIT value in ICTD in 1990 appears too large, interpolate based on surrounding years [flagged]; 'mineral tax' in HA data appears to partly include CIT, predict CIT-share based on precise split between CIT and resource tax in other sources [flagged]; social security program starts in 1996 according to SSA (2017), however we observe contributions prior to that date (Fisunoglu et al., 2011) which may correspond to a non-contributory pension benefit (Arza and Johnson, 2006) [flagged]; large economic shocks in 1980s which affected public finances (O'Connell, 1988).	Comparable historical tax/GDP based on Takirambudde (1995), O'Connell (1988), Bonu (1995).

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Brazil	1965-2018	HA (1965-1989); OECD (1990-2018)	*Anuario Estatistico do Brasil*	Challenging to find reliable GDP data in historical periods, use reported national price index from Ayres (2019) prior to 1990 which in turns is based on Brazilian Institute of Geography and Statistics; OECD data appears to be high quality, including with respect to sub-national tax collection; corroborate sub-national taxes in HA data using detailed information from Afonso and Araujo (2004), which discusses local public finance since 1960s, and Varsano (1999); use Fisunoglu et al. (2011) prior to 1980 for social security contributions. Interpolate income tax between 1968 and 1971 and 1973 and 1975.	Historical tax/GDP numbers comparable to Afonso and Araujo (2004), Chelliah (1971) - both of which cover national and sub-national revenues.
Bulgaria	1993-2018	HA (1993-2018)	*Statistical Yearbook, Monthly Statistical Reviews*	Excess tax revenue' category in 1995-96 is contribution from previous year taxes collected, we check that it has not been double-counted [flagged]; some difference in social security contributions between UN-SNA data and Fisunoglu et al. (2011), prefer latter source as it compares to ICTD in overlapping years	Comparable tax/GDP numbers in early periods based on Bogetic and Hassan (1997), match World Bank (2020) in later years.
Burkina Faso	1965-2018	HA (1965-1999); OECD (2000-2018)	*Comptes Econonomiques de la Haute-Volta, Comptes Nationaux de la Haute-Volta, Les Comptes Economiques de la Nation*	Several periods of political instability (1974, 1980-1983, 1987) where interpolate data; large tax from property rights registration tax in late 1970s, appears genuine.	Comparable tax/GDP numbers with ICTD in overlapping periods.
Burundi	1965-2018	HA (1965-2018)	Annuaire Statistique, Bulletin Statistique	Data interpolated 1970-1973 but concerns remain about data quality (violence) [flagged]; IMF (1973) data suggests little change in composition of taxes, though change in overall tax take; historical IMF source lists a tax on property, which cannot be found in HA (which instead records a transaction tax)	
Cambodia	1994-2018	ICTD (1994-2018)		HA data exists from 1987 to 1993, but the data has quality concerns (given political transition), so prefer not to use that data; social security contributions begin in 1997 (SSA, 2018), we draw on data from Fisunoglu et al. (2011).	
Cameroon	1965-2018	HA (1965-1992); OECD (1993-2018).	*Note Trimestrielle sur la Situation Economique, Note Annuelle de Statistique*	Interpolate 1969-1970, 1989; classify the 'taxe unique' as an indirect tax, rather than direct firm tax, based on information from Gauthier et al. (2002); drop in overall revenue in 1980s confirmed to be mainly related to dwindling trade taxes (Gauthier and Gersovitz, 1997); decline in wealth taxes between 1968 and 1993 is not accounted for via additional sources [flagged]; social security contributions start in 1968 (SSA, 2017), we draw data from Fisunoglu et al. (2011); significant general volatility of revenues likely driven by reliance on volatile commodities (de Herdt, 2002).	Tax/GDP numbers comparable to Gauthier, Soloaga and Tybout (2002).
Canada	1965-2018	OECD (1965-2018)		Vaillancourt and Kerkhoff (2019) for additional information on the capital share of PIT	
Central African Republic	1965-2018	HA (1965-2007); ICTD (2008-2018)	*Bulletin de Statistique, Bulletin Mensuel de Statistique, Annuaire Statistique*	Political unrest in early historical periods create uncertainty around data [flagged]; the 'tax additionnelle' in the 1960s was a direct tax on firms rather than individuals (Mbounou-Ngopo, 2019); dips in tax collection in mid-1990s coincide with political transitions. HA data features change in terminology 1994-1997, which could erroneously be interpreted as a substitution from direct to indirect taxes; social security started in 1963 (SSA, 2019), we draw on Fisunoglu et al. (2011) for entire HA period.	Observed decrease in tax/GDP in recent periods also confirmed in IMF reports (IMF, 2016). Difficult to find historical sources to corroborate.
Chad	1965-2018	HA (1965-1982); ICTD (1983-2009); OECD (2010-2018)	*Bulletin Mensuel de Statistique, Budget General de l'Etat*	Military rule from 1975-1978 and civil war from 1979-1982, interpret data with caution during these periods [flagged]; social security program began in 1977 (SSA, 2017), draw on data from Fisunoglu et al. (2011) prior to OECD coverage; volatility in recent years is notable but appears to be genuine (found in both OECD and ICTD data).	Tax/GDP estimate in early historical period (1965) approximately 1.5 percentage points lower than reported in Lotz and Morss (1967).
Chile	1965-2018	HA (1965-1979); ICTD (1980-2018)	*Informe Economico Anual, Statistical Profile of Chile,*	The more recent data (1990-2018) is from Inter-American Development Bank; interpolate 1978-79; data quality is challenged during 1970-1973 period, the transition years for Allende [flagged]; use information from Mamalakis (1978) and Corbo (1989) to confirm HA data split between CIT and PIT, the extent of sub-national taxes, and the existence of social security contributions.	Social security data from Fisunoglu et al. (2011) in agreement with historical data from Cerda (2005). Historical tax/GDP ratio comparable to values reported in World Bank (1980).
China	1994-2018	HA (1994-2007); OECD (2008-2018)	Statistical Yearbook, online data from National Bureau of Statistics	See Appendix B for more details on sources and tax system; data exists prior to 1994, but we start the series after the transition away from central planning (conceptual difficulties with defining certain revenue sources as taxes in the pre-transition period); HA and OECD match well in overlapping years.	Tax/GDP comparable to values reported in Lou and Wang (2008) and ICTD.
Colombia	1965-2018	HA (1965-1989); OECD (1990-2018)	*Estadisticas Fiscales, Cifras Fiscales, Informe Financiero*	Good match in levels when sources overlap; special 'pro equity income tax', initially classified as unallocable between PIT and CIT in OECD data, is in fact a tax on corporate income (World Bank, 2014); concern if we are capturing all sub-national taxes in the HA period (based on Arroyo-Abad and Lindert, 2016) [flagged]; social security data missing in HA period [flagged].	McClure cites a tax/GDP in 1980 which is 3 percentage points higher than our HA estimate, though our estimates agree with Mitchell (2003) in the period 1965-1989.
Congo	1972-2018	HA (1972-1981); ICTD (1982-1998); OECD (1999-2018)	*Annuaire Statistique, Bulletin Mensuel de Statistique, Economic Survey*	Interpolate between 1977 and 1979; year of overlap between ICTD and OECD coincides with period of genuine drop in revenues, due to violence.	Historical tax/GDP values are broadly in line with Tait et al. (1979) for the 1970s.
Costa Rica	1965-2018	HA (1965-1987); OECD (1988-2018)	*Anuario Estadistico, Memoria Annual*	Use IMF historical data between 1974 and 1987, matches will in overlapping years with both HA and OECD; low CIT revenue collected in the 1980s is confirmed in Shome (1992); social security contributions are from Fisunoglu et al. (2011).	Comparable historical tax/GDP compared to Tait et al. (1979).

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Cote d'Ivoire	1965-2018	HA (1965-1989); OECD (1990-2018)	*Bulletin Mensuel de Statistique, Les Comptes de la Nation, Budget General de Fonctionnement,*	Interpolate 1977-1979, 1987-1989; in HA years where individual versus firm split exists for 'tax on benefices', assume same ratio in all other years where same tax does not have breakdown; OECD data appears to under-estimate PIT in recent years, use information from IMF and ICTD to adjust level [flagged].	
Croatia	1996-2018	ICTD (1996-2018)		HA data exists but has less complete coverage than ICTD by type of tax; ICTD also appears to have captured well sub-national taxes (IMF, 2020).	Comparable levels of tax/GDP in World Bank (2020).
Cuba	1990-2018	OECD (1990-2018)		HA data exists in some of the historical OECD periods, but prefer to draw all data from a single source	
Cyprus	1972-2018	HA (1972-2018)	*Annual Budget*	Historical IMF data between 1972 and 1989, then HA for remaining periods; supplement HA with Fisunoglu et al. (2011) for social security contributions; Lent (1977) confirms existence of corporate income tax in 1970s.	Comparable tax/GDP levels to Lent (1977).
Czech Republic	1993-2018	OECD (1993-2018)			
Democratic Republic of the Congo	1968-2018	HA (1968-1990); ICTD (1991-2018)	*Conjoncture Economique*	Some difference between sources in overlapping years, coincides with period of high inflation and significant seigniorage tax (De Herdt, 2002; Nachega, 2005) [flagged]; between 1977 and 1990, tax type called 'divers' which is initially unallocable between PIT and CIT [flagged]; likely that we capture local tax revenues in historical periods. Interpolate 1973, 1992-1995.	Trends in taxation by source in historical HA periods is consistent with Emziet (1997).
Denmark	1965-2018	OECD (1965-2018)		Specificity in how social security contributions are levied (through the PIT, unlike in many other countries)	
Dominican Republic	1968-2018	HA (1968-1989); OECD (1990-2018)	*Ejecucion Presupuesto*	Omission of indirect tax categories in HA in late 1970s and late 1980s - interpolate based on surrounding years [flagged]; given unrest in early 1960s, we begin our series in 1968.	Historical IMF data agrees with HA estimates in 1970s.
Ecuador	1973-2018	HA (1973-1989); OECD (1990-2018)	*Cuentas Nacionales del Ecuador*	Ministry of Finance (2016) data includes breakdown between resource and non-resource revenues in historical periods, which suggest that our HA data sometimes includes resource revenues [flagged] - use historical IMF data in limited sets of years to correct the direct firm income tax numbers in HA.	CEPAL (1991) corroborates tax revenue levels in 1979-1987, including the spike in 1985. Garcia and Uquilles (1992) confirm levels in 1989-1992 period.
Egypt	1965-2018	HA (1965-1989); ICTD (1990-2001); OECD (2002-2018)	*Annuaire Statistique*	HA and ICTD data match very well in overlapping year; for periods prior to OECD, use Fisunoglu et al. (2011) to measure social security contributions; interpolate 1965-67, 1970-1971, and 1973-1974; sharp drop in revenue in 1980s is genuine; difficult to ensure CIT does not in part capture resource revenues, as they grew in importance in 1970s [flagged]; in years 2002-2008, 'tax on movable capital revenues from Central Bank' appears to be unallocable between CIT and PIT, we assign shares based on information in Waterbury (2014).	Smith (1970) and Nyrop (1976) corroborate low level of PIT in 1960s and early 1970s. Mitchell (2003) differs on average by 10% from our tax/GDP estimates.
El Salvador	1965-2018	HA (1965-1989); OECD (1990-2018)	*Anuario Estadistico, Indicadores Economicos y Sociales*	Significant currency reform in 2001, interpret tax/GDP number with caution in that year [flagged], and we adjust the currency in prior years to be comparable; social security contributions began in 1959 (SSA, 2017), we use data from Fisunoglu et al. (2011) in years prior to OECD coverage, which was substantial (Grosh, 1990).	Estimates of tax/GDP in 1980s and 1990s very close to numbers reported in Cardemil et al. (2000).
Equatorial Guinea	1981-2018	ICTD (1981-2004); OECD (2005-2018)		Very limited direct taxes collected prior to early 2000s, corroborated in Same (2008); historically strong reliance on revenues from commodity exports (Human Rights Watch, 2017); social security first implemented in 1947 (SSA, 2017), we use data from Fisunoglu et al. (2011).	
Estonia	1993-2018	ICTD (1993-1994); OECD (1995-2018)		Russian presence only phased out by 1993; good match between sources in overlapping years	Our tax/GDP numbers in early transition period matches well with Polackova (1996).
Ethiopia	1965-2018	HA (1965-1992); OECD (1993-2018).	*Statistical Abstract*	Income tax only separates personal from business income starting in 1975; per Schwab (1970), income tax schedules prior to then included both individual and firm income - so we keep the initially unallocable category; our HA record of land use fees suggests we are capturing sub-national taxes; Chloe (1984) notes the reliance on commodity exports, which induces volatility in tax revenues; very limited quantity of social security contributions in comparison with Mengistu et al. (2017) [flagged]. Interpolate 1989, 2005.	Compares well with Mascagni (2016) for historical tax/GDP in 1960s-1980s, series are within one percentage point of each other.
Fiji	1972-2018	HA (1972-1989); ICTD (1990-2007); OECD (2008-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	HA data derives from historical IMF data; use Fisunoglu et al. (2011) for social security contributions, which began in 1966 (SSA, 2016); disappearance of property taxes between 1992 and 2010, though consistently missing in both ICTD and OECD sources [flagged]	
Finland	1965-2018	OECD (1965-2018)			
France	1965-2018	OECD (1965-2018)			
Gabon	1965-2018	HA (1965-1985); ICTD (1986-2018)	*Annuaire Statistique du Gabon, Tableau de Bord de l'Economie*	Confident that HA sources excludes resource revenues, comparison with historical IMF data; interpolate 1977-1980; several historical taxes unallocable between CIT and PIT, including 'impot general sur le revenue', for which historical sources are not informative (Abdel-Rahman, 1965); social security contributions began in 1963 (SSA, 2019), we draw on data from Fisunoglu et al. (2011).	Limited historical sources, though drop in revenue in 1980s is corroborated in Gaulme (1991) and Yates (1996).
Gambia	1972-2018	HA (1972-1987); ICTD (1988-2018)	*Estimates of Recurrent Revenue and Expenditure, Gambia Statistical Yearbook*	Start data in 1972, as currency change in 1971 introduces measurement challenges; at same time, continued macro-economic volatility means data prior to 1990 should be interpreted with caution [flagged]	Tax/GDP estimates in HA are comparable to Ansari (1982), about 18% lower on average. Jallow (2016) provides historical account of tax system, but no data on tax/GDP.

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Georgia	1994-2018	ICTD (1994-2018)		Social security contributions, in their current form, began in 1990 (SSA, 2010), we draw on data from Fisunoglu et al. (2011); interpolate social security between 1995 and 1997.	Barbone and Polackova (1996) have comparable tax/GDP estimates for 1995, once our series are adjusted for social security contributions.
Germany	1965-2018	OECD (1965-2018)			
Ghana	1967-2018	HA (1967-1999); OECD (2000-2018)	*Quarterly Digest of Statistics*	Very good match between sources in overlapping years, once account for social security contributions; remove revenue from government properties in HA historical periods (initially counted within 'tax on interest and profits); volatile patterns in 1980s confirmed in Darko-Osei and Telli (2017), coinciding with economic turbulence and IMF recovery and structural adjustment agreements.	Reasonable comparison for tax/GDP numbers in historical periods, as reported in Chelliah (1971), Lotz and Morss (1967), Killick (1978) and Darko-Osei and Telli (2017).
Greece	1965-2018	OECD (1965-2018)			
Guatemala	1965-2018	HA (1965-1989); OECD (1990-2018)	*Estadisticas de Finanzas Publicas de Guatemala*	OECD classifies solidarity tax as unallocable, since 1995, but it is a tax on corporations (PriceWaterhouseCooper, 2020); historical data on social security contributions from Fisunoglu et al. (2011) seem unreliable in this case, so we report such taxes from 1978 onward [flagged].	Tax/GDP estimates in early historical years (1965-1967) lie within one percentage point of numbers from Lotz and Morss (1967).
Guinea	1980-2018	ICTD (1980-2018)		Revenue movements in 1980s coincide with political transition and new fiscal regime under Conte (Yansane, 1990); Topouzis (1989) notes the rising importance of resource revenue, which may be captured inside our CIT category between 1985 and 1992 [flagged].	
Guyana	1972-2018	HA (1972-1986); ICTD (1987-1989); OECD (1990-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	HA is drawn from IMF sources; drop in revenue at end of 1980s is genuine, likely reflects economic turbulent times (consistent numbers across sources); initial difference across sources in overlapping years is due to ICTD data not accounting for social security contributions.	
Haiti	1975-2018	HA (1975-1989); ICTD (1990-2018)	*Tableau des Operations Financieres de l'Etat*	HA data draws on historical IMF reports; interpolate 1988-1989, though should be interpreted with caution given violence at time [flagged]; spike in 1987 is driven by collapse in underlying value of GDP; social security begins in 1965 (SSA, 2017), we draw on Fisunoglu et al. (2011); property and transaction taxes exist in HA, small in magnitude, but are missing from ICTD data [flagged].	Dioda (2012) estimates similar tax/GDP in 1990, Tanzi (2000) estimates slightly higher tax/GDP between 1993 and 1999.
Honduras	1973-2018	HA (1973-1989); OECD (1990-2018)	*Anuario Estadistico*	1974 is a complicated year due to missing types of taxes, so we interpolate it; jump in revenues around year of overlap between OECD and HA, but Herrera (1994) corroborates significant changes in tax performance at that time; use social security data from Fisunoglu in all years prior to OECD coverage.	Historical IMF data are comparable to HA numbers for tax/GDP in the 1980s.
Hungary	1994-2018	OECD (1994-2018)			
Iceland	1965-2018	OECD (1965-2018)		World Bank (2020) corroborates spike in non-recurrent property taxes observed in 2016; infer, based on Herd and Thorgeirsson (2001) and Karlsson (2014), assign portion of initially unallocable OECD revenue to individuals.	
India	1965-2018	HA (1965-2019)	*Monthly Abstract of Statistics, Indian Public Finance Statistics*	In HA, 'income tax other than CIT' is not exclusively a tax on individuals, we use additional information to assign this category to firms versus individuals (including Rao, 2005); HA data carefully records a comprehensive set of wealth and property taxes, including for land; reasonably confident HA captures sub-national taxes; social security contributions, very small in magnitude, appear to be reported inside individual income tax category [flagged].	Very comparable tax/GDP estimate in earliest periods with Rao (2005); also consistent with recent estimates in World Bank (2019).
Indonesia	1965-2018	HA (1965-1996); OECD (1997-2018)	*Statistik Indonesia*	Strong match for data in overlapping years between sources; 1983 reform collapsed multiple taxes (CIT, PIT, other direct income) into a single schedule, so we use shares of capital versus labor direct income taxes in 1983 and assign such shares until 1997 [flagged]; reasonable ability to exclude resource revenues in HA data. Interpolate 1968-1971, 1994.	Multiple sources estimate very low historical tax/GDP ratios (1960s and 1990s), between 2 and 8 percentage points (Prasetyo, 2018; Gillis, 1985; Amir et al., 2013), generally consistent with our estimates
Iran	1969-2018	HA (1969-2018)	*Annual Budget, Iran Statistical Yearbook*	Reasonable ability to exclude resource revenues; social security data in HA and in ICTD are unreliable, we instead draw data from UN-SNA, starting in 1996 [flagged]; data strictly based on central government, but no documentation suggests sub-national taxes are quantitatively important.	In overlapping period (1979-1989), our tax/GDP estimates and trend match very closely with Mazarei (1996). Generally limited studies in English on historical tax system in country.
Ireland	1965-2018	OECD (1965-2018)			
Israel	1972-2018	HA (1972-1992); OECD (1993-2018)	*Accountant General's Report, IMF Government Finance Statistics*	HA draws in part from historical IMF data; interpolate 1992-1994; some IMF data reported in 1970s seems approximate [flagged]; historical IMF misses property tax in some years, supplement with ICTD data.	Historical trends in 1980s and 1990s are corroborated in Brender (2007), though the level of tax/GDP is approximately 15% higher than our estimates.
Italy	1965-2018	OECD (1965-2018)			
Jamaica	1965-2018	HA (1965-1989); OECD (1990-2018)	*Statistical Yearbook of Jamaica, Abstract of Statistics*	Property taxes dip in 1997, but this appears genuine (in the OECD data, based on local public finance records); unallocable part of direct income taxes is significant in the 1980s, comprising a mix of taxes on dividends, interest and an 'education tax' since 1983 (Government of Jamaica, 1988), and we use additional information from Inter-American Center of Tax Administrators to assign it to firms versus workers;	Historical estimates of tax/GDP in 1960s and 1970s are 10-18% larger than ours (Chelliah, 1971; Shome, 1992).
Japan	1965-2018	OECD (1965-2018)			
Jordan	1973-2018	HA (1973-1989); ICTD (1990-2018)	*Annual Report, Yearly Statistical Series, Monthly Statistical Bulletin*	HA uses historical IMF data; at year of merge between sources, Abu-Hammour (1997) confirms a large increase in tax/GDP; non-tax revenues are significant in the country, but our sources can reasonably exclude them.	HA data matches very closely the numbers in Abu-Hammour (1997).

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Kazakhstan	1993-2018	ICTD (1993-1998); OECD (1999-2018)		Independence in 1991, but ICTD coverage begins in 1993; social security contributions began in 1991, we use data from Fisunoglu et al. (2011); volatility in indirect tax revenues is significant but plausible (World Bank, 2017).	
Kenya	1965-2018	HA (1965-2000); OECD (2001-2018)	*Statistical Abstract*	To assign initially unallocable direct tax between firms and individuals, we use information from Jetha (1966) and Wanjala (2006) on the income tax schedules; gradual increase in tax/GDP over the long run observed in Macha et al. (2018) and Omondi et al. (2014); possible that we are not capturing sub-national taxes in HA data [flagged]	Our tax/GDP estimates are systematically smaller by 2 percentage points in the 1960s and 1970s compared to other estimates (Wanyagathi, 2015; Kanji and Wanjala, 2005), but this may also be due to differences in the underlying estimate of GDP (our GDP estimates based on WID are larger than World Bank estimates).
Korea	1965-2018	HA (1965-1971); OECD (1972-2018)	*Korea Statistical Yearbook*	Good match in levels for years of overlap; interpolate from 1968 to 1971; overall low levels of revenue in the 1960s are genuine and reflect government policy; only after major tax reform in 1967 did tax collection start to significantly grow (Yoo, 2000); observed changes in capital taxes collected in 1970s are genuine (Kwack and Lee, 1992)	Tax/GDP estimate reported in mid-1960s is approximately 1 percentage point higher than our data.
Kosovo	2008-2018	HA (2008-2015); OECD (2016-2018)	Data retrieved from Department of Finance and General Services	Government data prior to 2008 is scarce; according to Koshutova (2004) and Kritzer (2005), pension system is funded through general taxation; level of CIT as a share of GDP is confirmed in Hernandez et al. (2019)	
Kuwait	1972-2018	HA (1972-1989); ICTD (1990-2018)	*Government Finance Statistics*	Interpolate 1975-1976; do not observe social security contributions in HA, but it is place in historical years (SSA, 2016), so we use data from Fisunoglu et al. (2011); large resource nationalization in 1975, firms' income tax data from 1972 to 1975 should be interpreted with caution [flagged]	Good historical match in tax/GDP and sources for 1972-1976 when compared to Nyrop (1977).
Kyrgyzstan	1994-2018	ICTD (1994-2018)		Data coverage of property and wealth taxes only begins in 1995, but we could not find a historical source to confirm if this reflects a policy implementation [flagged]	Estimates of tax/GDP in the 1990s are very close to data reported in Bokros and Dethier (1998) and Barbone and Polackova (1996).
Laos	1982-2018	ICTD (1982-2009); OECD (2009-2018)		While social security contributions have existed since 2001 (SSA, 2016), there is no data covering these contributions [flagged]; significant non-tax sources of revenue in the 1980s (Saignasith, 1997)	Historical estimates in Saignasith (1997) are larger than our data, but those figures also report for total revenues (rather than total taxes).
Latvia	1994-2018	OECD (1994-2018)		ICTD data exists prior to OECD coverage, but it is not disaggregated and difficult to reconcile with OECD numbers in overlapping years.	High levels of tax/GDP in 1994-1995 are corroborated in Polackova (1996).
Lebanon	1965-2018	HA (1965-2018); ICTD (1988-2001)	*Recueil de Statistiques Libanaises, Statistical Yearbook*	Series uses HA data, with ICTD data between 1988 and 2001; turbulent tax collection during civil war period (1975-1990), where information in Dimashkieh (1993) and Houry (1997) confirm the levels of taxes by type; social security contributions began in 1963, we use data from Fisunoglu et al. (2011); use information from Eken et al. (1995) to confirm level of CIT collected prior to 1993.	Historical estimates in Saleh (2004) are comparable to our series during turbulent period (1975-1990).
Lesotho	1965-2018	HA (1966-1981); ICTD (1982-2018)	*Statistical Bulletin*	Data missing in 1978-1981, but due to unrest we leave data empty (rather than interpolate); spike in revenue in 1977 is corroborated across data-sets; licensing fees constitutes large source of revenue in earliest periods (Cobbe, 1981).	Tax/GDP estimates from our data are 2 percentage points lower in the 1970s than the numbers reported in Cobbe (1981).
Liberia	1970-2018	HA (1970-1988); ICTD (2000-2018)	*Economic Survey, Quarterly Statistical Bulletin of Liberia, Statistical Bulletin of Liberia*	Important gap in coverage between 1988 and 2000 - a turbulent period during which revenues were collected but diverted from official use and GDP decreased by 90% (Atkinson, 1997); drop in revenue in 1973-1974 is genuine.	Levels of tax/GDP, both before and after the data-gap, are comparable to estimates in Davies and Dessy (2016).
Lithuania	1991-2018	HA (1991-1994); OECD (1995-2018)	*Lithuania Statistics Yearbook*	Social security begins in 1991, we use data from Fisunoglu et al. (2011).	High levels of tax/GDP in 1993-1994 is corroborated in Polackova (1996).
Luxembourg	1965-2018	OECD (1965-2018)			
Macedonia	1993-2018	ICTD (1993-2018)		Interpolate income taxes from 2003 to 2005; SSA (2005) confirms that social security contributions are significant in the country.	Excluding social security, tax/GDP levels are comparable to estimates since 2005 in World Bank (2020).
Madagascar	1965-2018	HA (1965-1989); ICTD (1990-2018)	*Inventaire Socio-Economique, Malagasy Budget, Budget General de l'Etat*	Interpolate 1969-1971, noting the political instability in 1972, 1974-1976, and 1981-1983; use social security data from Fisunoglu et al. (2011), starting in 1965 (SSA, 2018) though data from 1965 to 1969 may be estimated in original source [flagged].	
Malawi	1965-2018	HA (1965-2004); OECD (2005-2018)	*Malawi Statistical Yearbook, Compendium of Statistics, Economic Report*	Social security contributions missing in OECD, so we use data from Fisunoglu et al. (2011) for entire period, which were significant in more historical periods; spike in PIT revenue in 2001 is genuine, likely reflects tax enforcement reforms; Shalizi and Thirsk (1990) emphasize that direct income taxes were a significant share of total taxes in the 1960s.	Historical estimates of tax/GDP are slightly lower than in Chipeta (1998) and Shalizi and Thirsk (1990), though this may also be due to differences in GDP values.
Malaysia	1965-2018	HA (1965-1989); OECD (1990-2018)	*Economic Report*	Interpolate 1979-1980 and 1988, due to missing HA data; use social security contributions data from Fisunoglu et al. (2011), but unreliable in period from 1972 to 1980 [flagged]; total levels match well in overlapping years between OECD and HA, highlights that the stamp duty, which OECD classifies in as 'other tax', is classified as unallocable income tax in HA; drop in indirect taxes in late 1980s is genuine.	Limited existence of studies for historical comparison.
Mali	1965-2018	HA (1965-1979); ICTD (1980-1999); OECD (2000-2018)	*Comptes Economiques du Mali, Annuaire Statistique*	Levels match well in overlapping years between HA and ICTD, lends confidence to HA sources even though we cannot find multiple historical sources to corroborate (Founou-Tchuigoua, 1989); social security contributions begin in 1961 (SSA, 2019), we use data from Fisunoglu et al. (2011); OECD lists no corporate tax in 2000, though ICTD does [flagged].	Limited historical sources; our estimates are comparable to Founou-Tchuigoua (1989) for period 1981-1989.

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Mauritania	1986-2018	HA (1986-2006); OECD (2007-2018)	*Annuaire Statistique, Bulletin de la Direction de la Statistique et des Etudes Economiques*	Levels match well in overlapping years between HA and OECD; 'autres droits' in HA is listed as income tax in OECD, so we follow that assignment in HA; interpolate missing indirect taxes in 2000 and 2007-2008; limited historical sources, 1986 appears to be first year the government prepared a comprehensive budget statement (Handloff, 1990).	Limited historical sources; Oualalou and Jaidi (1986) discuss low overall levels of tax collection historically, and World Bank (2020) provides no data on the country.
Mauritius	1973-2018	HA (1973-1989); OECD (1990-2018)	*Digest of Statistics*	HA draws on historical IMF data; historical government publications match on levels to IMF data, but is less complete in years of coverage; use data from Fisunoglu et al. (2011) for social security contributions prior to OECD coverage.	Limited existence of studies for historical comparison.
Mexico	1965-2018	HA (1965-1979); OECD (1980-2018)	*Anuario Estadistico de los Estados Unidos Mexicanos*	Interpolate social security contributions 1973-1976; large increase in indirect taxes in early 1980s is genuine (driven by policy reforms), as is the drop in income taxes in 1980; initially unallocable income tax 'impuesto sobre la renta' pre-1970 (Aguilar, 2003), assign shares to firms and individuals based on data post-1970; unclear if we capture sub-national taxes pre-1970 [flagged].	Overall levels of taxes match the historical estimates in Martinez-Vasquez (2001) for period 1980-1999.
Moldova	1992-2018	ICTD (1992-2018)		ICTD misses revenue from property-transaction taxes, which we retrieve with HA sources; use social security data from Fisunoglu et al. (2011), starting in 1992.	Tax/GDP level comparable in 1993 to estimates reported in Barbone and Polackova (1996).
Mongolia	1994-2018	ICTD (1994-2018)		ICTD data goes back to 1986, but does not disaggregate income tax; according to IMF sources, capital gains tax is a tax on corporations; incorporate additional data on property-wealth taxes from HA sources, initially missing in ICTD; significant 'other tax' category between 1993 and 2011 (including stamp duties, royalties, land transactions), and important non-tax revenues.	Comparable tax/GDP estimates in World Bank (2020), though this is not surprising given similar initial data-sources.
Morocco	1965-2018	HA (1965-1999); OECD (2000-2018)	*Annuaire Statististique du Maroc*	Spikes in CIT in 1975 and 2009 appear genuine; 'droits d'enregistrement' in HA are classified as property taxes; social security contributions began in 1959, yet are not observable in HA, so we draw data from Fisunoglu et al. (2011); interpolate 1966, 1971-1973, 1995.	CIT levels in 1980s and 1990s are comparable to those reported by Ministry of Finance (2011).
Mozambique	1975-2018	HA (1975-2014); ICTD (2015-2018)	*Informacao Estatistica*	Interpolate 1991 and 2001; use information from Fjeldstad and Heggstad (2010) to assign income taxes to firms versus individuals in period 2003-2009, and Castro et al. (2009) for 1993-2007 period; HA sources for social security raise concerns, so use data from Fisunoglu et al. (2011); drop in revenue in 2014-2015 appears genuine, based on overlap of data between HA and ICTD.	Limited historical sources; our historical estimates for 1993-2007 are comparable to Lemgruber et al. (2010) and Castro et al. (2009).
Namibia	1991-2018	HA (1991-2018)	*Estimate of Revenue and Expenditure for the Financial Year*	Start data in 1991, given political instability in prior years (full independence achieved), though ICTD data exists in more historical years; perfect match in overlapping years with ICTD data for indirect taxes, though HA estimates of income taxes are 1 percentage point of GDP higher [flagged].	Comparable tax/GDP estimates from World Bank (2020).
Nepal	1976-2018	HA (1976-2005); ICTD (2006-2018)	*Statistical Yearbook of Nepal*	Match in overall tax/GDP level between HA and ICTD in overlapping years, but discrepancy in level by type of tax - this is because ICTD classifies 'excise on industrial product' as indirect tax while HA classifies as corporate income tax [flagged]; social security program began in 1962 (SSA, 2017), we use data from Fisunoglu et al. (2011); drop in PIT and CIT in 2005 are significant, have not found additional sources to corroborate; property-wealth taxes are a minuscule fraction of taxes in 2005 (HA), and disappear entirely in ICTD (2006) [flagged].	Overall good match in tax/GDP levels with World Bank (2020), though limited comparisons available in earliest historical periods.
Netherlands	1965-2018	OECD (1965-2018)			
New Zealand	1965-2018	OECD (1965-2018)		Exists initially unallocable income taxes (category 1300 in OECD classification), but as they are taxes on interest, dividends and withholding (on non-residents' passive investment income), we attribute them to capital rather than labor, minuscule in magnitude.	
Nicaragua	1965-2018	HA (1965-1989); OECD (1990-2018)	*Compendio Estadistico*	HA uses historical IMF data in period 1972-1989; multiple currency re-denominations and revaluations that affect tax/GDP estimates in late 1980s and early 1990s [flagged], but we corroborate with historical sources - including the spectacular spike in 1980s (Machado, 2010; Irvin and Croes, 1987; Gibson, 1996); social security policy began in 1956 (SSA, 2017), we use data from Fisunoglu et al. (2011).	Most importantly, comparable tax/GDP estimates in economic turbulent period of 1980s (Ocampo, 1990; Machado, 2010).
Niger	1965-2018	HA (1965-1999); OECD (2000-2018)	*Annuaire Statistique, Budget Annuel,*	Interpolate 1969-1972; use data from Fisunoglu et al. (2011) for social security contributions prior to OECD coverage, first policy implemented in 1967 (SSA, 2017); HA and OECD match in year of overlap, which could suggest low levels in HA in 1999 are genuine; tax 'sur un role' between 1975 and 1998 is assigned to PIT; use information from OECD in overlapping years with HA to assign 'autres recettes fiscales' in HA in 1999.	Limited existence of studies for historical comparison.
Nigeria	1965-2018	HA (1965-1991); ICTD (1992-2009); OECD (2010-2018)	*Annual Abstract of Statistics*	Interpolate personal income taxes between 1987 and 1990, interpolate overall taxes 2008-2009; important concerns about extent to which resource revenues are truly excluded in HA series, but match in levels is reasonable with ICTD in overlapping years [flagged]; personal income tax represents minuscule share of total taxes in 1980s, corroborated in IBFD (2016); drop in indirect taxes between 1965 and 1969 appears genuine, related to policy reforms.	Estimates of tax/GDP in early 1990s are approximately 1 percentage point lower than in Expo and Ndebbio (1996) and Baunsgaard et al. (2012).
Norway	1965-2018	OECD (1965-2018)		Corroborate significant component of corporate income tax which reflects variation in production of oil and gas.	

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Oman	1972-2014	HA (1972-1989); ICTD (1990-2014)	*Statistical Yearbook*	HA uses historical IMF data, general agreement with other historical HA sources but IMF data carefully and consistently excludes resource revenues; missing property-wealth taxes in ICTD [flagged], though HA series suggests very small in magnitude; additional information used to corroborate absence of personal income tax revenues (KPMG, 2013); sharp dip in 1990-2000 appears genuine; significant nationalization reforms in 1972, making data prior to those events hard to harmonize.	Tax/GDP levels from 2000 onward are comparable to estimates in Besley and Persson (2014).
Pakistan	1965-2018	HA (1965-2018)	*Detailed Statement of Revenue Receipts, State Bank of Pakistan Annual Report Statistical Supplement*	Combine several government publications to ensure we capture national and sub-national taxes in all periods, with the latter an important source of total taxes (Hasan, 1997); 'income tax other than corporation tax' is not entirely PIT (similar challenge in classification in India), use additional information from specific years to assign shares within this category to firms versus individuals.	Historical tax/GDP estimates in 1970s and 1980s are systematically larger, by 1-1.5 percentage points, than Hasan (1997, 2015) though this source only captures national taxes.
Panama	1973-2018	HA (1973-1989); OECD (1990-2018)	*IMF Government Finance Statistics, IMF Article IV Report*	HA uses historical IMF data; initially unallocable revenue between PIT and CIT in IMF, use additional information from Gomez and Sabaini (2005) and Shome (1994) to allocate; decline in revenue in mid-1980s is genuine, likely reflects political transition and violence.	Historical tax/GDP estimate comparable to estimates in CEPAL (1978) prior to 1975.
Papua New Guinea	1976-2018	HA (1976-1999); OECD (2000-2018)	*Compendium of Statistics, Summary of Statistics, Estimates of Revenue and Expenditure for the Year*	Social security contributions started in 1980 (SSA, 2016), use data fro Fisunoglu et al. (2011) prior to OECD coverage, though small in magnitude; volatile tax collection in mid-1970s, confirmed in Duc Thac and Lim (1984).	Historical levels and trends in tax/GDP are comparable to estimates reported in Duc Thac and Lim (1984) for period 1965-1997.
Paraguay	1965-2018	HA (1965-1989); OECD (1990-2018)	*Anuario Estadistico*	Decline in taxes in 1980s and uptick in early 1990s appear genuine, likely reflect political transition period; jump in social security contributions in 1998-1999 is genuine, reflects policy reforms (SSA, 2015); initial jump in tax/GDP in 2016-2017 was due to erroneous GDP value provided to WID, has been corrected.	Limited existence of studies for historical comparison.
Peru	1965-2018	HA (1965-1989); OECD (1990-2018)	*Anuario Estadistico*	Historical values (1968-1978) should be treated with caution, given strong changes in currency and macro-economic conditions [flagged]; use data from Fisunoglu et al. (2011) for social security contributions in pre-OECD periods; jump in total revenues in year of transition from HA to OECD is genuine (HA matches well the level of OECD in post-1989 years); OECD lists two types of wealth taxes levied at national level, while HA lists only one [flagged], though small in magnitude compared to local taxes which are captured in both HA and OECD; interpolate 1965-1967.	Limited existence of studies for historical comparison.
Philippines	1965-2018	HA (1965-1993); OECD (1994-2018)	*Philippine Statistical Yearbook, Annual Budget, Annual Report of the Commissioner of Internal Revenue*	Possible that HA fails to capture sub-national taxes, while OECD does [flagged], though such taxes are estimated to represent less than 5% of total taxes; uptick in tax collection starting in mid-1980s reflects important policy reforms (Reside and Burn, 2016).	Historical tax/GDP estimates are systematically within 1 percentage point of estimates in Reside and Burns (2016), though on average 15% smaller than Mitchell (2003).
Poland	1991-2018	OECD (1991-2018)			
Portugal	1965-2018	OECD (1965-2018)		Assign initially unallocable income tax (OECD category 1300) to individuals versus firms on the basis of information provided in OECD data and comparison with historical IMF reports.	
Qatar	2000-2018	ICTD (2000-2018)		Social security contributions do exist (Deloitte, 2019), we use data from Fisunoglu et al. (2011); IMF reports list 'other revenues' which are a significant source of total revenues, but this likely corresponds to resource revenues [flagged]; excise tax was introduced in 2019, corroborates indirect taxes listed in years of coverage.	
Romania	1991-2018	ICTD (1991-2018)		ICTD estimates for social security contributions are comparable to Fisunoglu et al. (2011), so we do not draw on additional data-sources.	Limited existence of studies for historical comparison.
Russia	1994-2018	HA (1994-1999); ICTD (2000-2018)	*HA draws on IMF sources. IMF Government Finance Statistics, IMF Article IV Report.*	Government statistics published during HA coverage should be interpreted with caution (Gale, 2005) [flagged]; complex property tax system in HA, but estimates of levels are corroborated in Martinez-Vasquez and Wallace (1999) and Chua (2003), and comparison with Treisman (2000) suggests HA series meaningfully captures sub-national taxes.	Historical estimates are comparable to numbers in various IMF reports.
Rwanda	1967-2018	HA (1967-1995); OECD (1996-2018)	*Situation Economique et Conjoncturelle, Bulletin de Statistique, Rapport sur l'Evolution Economique et Financiere, Statistical Yearbook*	Social security contributions begin in 1956 (SSA, 2017), we use data from Fisunoglu et al. (2011) prior to OECD coverage; interpolate 1990-1993, though concerns exists about data quality given unrest in country [flagged].	Tax/GDP estimates in early historical period (1966-1968) are very close to those reported in Cheliah (1971).
Samoa	1983-2018	ICTD (1983-2004); OECD (2005-2018)		Interpolate 1984; observe property tax in early ICTD years as well as in all OECD years, but small in magnitude.	Tax/GDP estimates approximately 2 percentage points lower (10%) than estimates in IMF (2006) for late 1990s.
Saudi Arabia	1994-2018	ICTD (1994-2018)		Non-tax revenues contribute significantly to overall revenues; social security contributions drawn from Fisunoglu et al. (2011). Interpolate 2006-2008.	Limited existence of studies for historical comparison.
Senegal	1965-2018	HA (1965-1984); ICTD (1985-1998); OECD (1999-2018)	*Bulletin Statistique et Economique Mensuel, Situation Economique et Sociale du Senegal*	Drop in revenue in HA in 1970s which is hard to account for, so replace with historical IMF data in that period [flagged]; Boye (1990) describes the tax system as generally stable and steadily growing in 1960s and 1970s, also confirms that PIT outweighs CIT in these periods and overall level of income taxes relative to GDP.	Estimates of historical tax/GDP significantly lower than estimates in Cheliah (1971) for period 1966-1968 [flagged].
Serbia	2000-2018	ICTD (2000-2018)		HA data exists in prior years, but series begins in 2000 given political transition.	Estimates of tax/GDP in mid-2000s is consistent with numbers reported in World Bank (2007).

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Seychelles	1980-2018	ICTD (1980-2007); OECD (2008-2018)		Social security program begins in 1971 (SSA, 2017), use data from Fisunoglu et al. (2011) prior to OECD coverage; PIT collections are practically zero in some years between late 1990s and early 2000s, which appears genuine.	Limited historical comparisons to other studies, but additional IMF data provides comparable estimates in 1980s.
Sierra Leone	1965-2018	HA (1965-1989); ICTD (1990-2018)	*Estimates of Revenue and Expenditure*	Use historical IMF data within HA series, between 1974 and 1989, and HA and IMF perfectly match on levels and type of tax in 1974; social security contributions begin in 2001 (SSA, 2017), prior to that observe payroll tax reported in ICTD and HA; interpolate split between PIT and CIT in 1994-1997.	Limited existence of studies for historical comparison.
Singapore	1965-2018	HA (1965-1999); OECD (2000-2018)	*Yearbook of Statistics*	In HA series, use additional information (Asher and Tayabji, 1980; Tanzi and Shome, 1992) to assign initially unallocable income taxes between PIT and CIT; use data from Fisunoglu et al. (2011) for social security contributions prior to OECD coverage; sharp dip in 1980s reflect genuine tax policy reforms (Joon Chien, 1996); based on overlapping years, suggests that HA is missing a tax on financial and capital transactions which is covered in OECD [flagged], but HA covers other, more significant, wealth-property taxes (Haq et al., 1996; Bird, 1991).	Comparable long-run series of tax/GDP as reported in World Bank (2020).
Slovakia	1994-2018	ICTD (1994); OECD (1995-2018)		ICTD and OECD data match perfectly in 1995.	
Slovenia	1995-2018	OECD (1995-2018)		ICTD and OECD data match perfectly in 1995, but ICTD data pre-1995 does not provide sufficient disaggregation by tax type; SSA (2016) confirms significance of social security contributions; drop in CIT revenue in mid-late 2000s appears genuine (World Bank, 2020).	Estimates of tax/GDP in 1990s are comparable with World Bank (2020).
Solomon Islands	1993-2018	ICTD (1993-2007); OECD (2008-2018)		ICTD data exists in 1980s but has no consistent dis-aggregation between PIT and CIT; interpolate in 1996; ICTD likely only covers national taxes [flagged].	Limited existence of studies for historical comparison.
South Africa	1965-2018	HA (1965-1989); OECD (1990-2018)	*Statistical Yearbook*	Higher level in OECD data than HA at year of transitioning data-source, which coincides with important indirect tax reform, which is also tax category where discrepancy lies, but no years of data-overlap to further investigate [flagged]; reasonable confidence in data's ability to exclude resource revenues, corroborated in additional sources (South Africa Revenue Services, 2015); Ndlovu (2017) refers to a pay-as-you-earn social security scheme dating back to 1963, which we do not separately observe in HA but which could be included under PIT category [flagged].	Comparable tax/GDP estimates in 1970s with Ndlovu (2017) and Koch et al. (2003), in 1990s with Central Reserve Bank, though lower estimates than in Glendlay (2008).
Spain	1965-2018	OECD (1965-2018)			
Sri Lanka	1965-2018	HA (1965-2018)	*Statistical Abstract of Ceylon, Statistical Pocketbook*	Social security begins in 1958, we use data from Fisunoglu et al. (2011); transaction and property taxes are prominent from 1982 onward, unable to confirm if absence pre-1982 is due to policy [flagged].	Ravinthirakumaran (2011) reports comparable tax/GDP numbers for the period 1977-2009.
Sudan	1972-2018	HA (1972-1980); ICTD (1981-2018)	*The National Accounts and Supporting Tables*	Challenging to assign initially unallocable income taxes to firms versus individuals in HA data; use some IMF data in HA coverage, but generally difficult to find reliable information in historical periods [flagged]; use data from Fisunoglu et al. (2011) for social security contributions.	Limited existence of studies for historical comparison.
Swaziland	1969-2018	HA (1969-1998); OECD (1999-2018)	*Estimates of Revenue and Expenditure, IMF Government Finance Statistics*	HA draws on historical IMF data between 1972 and 1989; spike in revenues in 1970s appears genuine and related to economic changes; use data from Fisunoglu et al. (2011) for social security contributions prior to OECD; taxes on international trade were substantial in 2000s, corroborated in Ayoki (2018).	Limited existence of studies for historical comparison.
Sweden	1965-2018	OECD (1965-2018)			
Switzerland	1965-2018	OECD (1965-2018)		Withholding tax on interest income in financial institutions is unallocable between PIT and CIT without further information.	
Syria	1965-2007	HA (1967-2007)	*Statistical Abstract of Syria*	HA data extends further back, but series starts in 1967 given political transition; drop in PIT in 2004 appears genuine, confirmed in ICTD data; large increase in CIT from 1980 to 1985 reflects changes in resource environment, but unclear if our series entirely excludes resource revenues [flagged]; social security program began in 1959 (SSA, 2018), we use data from Fisunoglu et al. (2011) for entire series.	Limited existence of studies for historical comparison.
Taiwan	1965-2018	HA (1965-2018)	*Statistical Yearbook*	Spike in taxes in 2000 appears genuine, reflects economic changes; no data from any of main sources on social security contributions, and official government records leave the reporting entry for such contributions blank; either social security is funded through other, general tax sources or social insurance schemes are decentralized and no centralized statistics exist (Chow, 2001) [flagged].	Limited existence of studies for historical comparison.
Tanzania	1965-2018	HA (1965-2018)	*Statistical Abstract, Financial Statement and Revenue Estimates*	Interpolate 1972, 1977, 1993-1995; PIT and CIT bundled in one reported category between 1965 and 1974, assume same split as reported in disaggregated data in 1975; uptick in revenue collection in early 1990s attributed to multiple reforms (IMF, 2009).	Estimates of tax/GDP are 1-2 percentage points lower than reported in Fjeldstad (1995) during 1986-1990 period, and lower than in Osoro (1993) for late 1980s period, though good match on levels and trends with IMF (2009) between 1986 and 2008; difference in estimate may partly be driven by differences in estimates of GDP value (WID and World Bank estimates differ by almost 25%) [flagged].

Table B2: Notes on tax revenue data series

Country	Years	Main data sources	Historical archive (HA) data sources	Comments on series	Comparison with other studies
Thailand	1965-2018	HA (1965-1999); OECD (2000-2018)	*Statistical Yearbook*	Good match on level of tax/GDP between 1960s and early 2000s with Jansen Khannabha (2009), approximately 1 percentage point lower on average in our series; good match in levels by tax-type with Matsumoto (2018). Interpolate 1972, 1980, 1985, 1987, 1994, 1996 - more so than in any other country [flagged]	
Timor-Leste	2006-2018	HA (2006-2018)	*Data retrieved from online annual budgets published by Ministry of Finance*	Data begins after independence; social security contributions began in 2016 (ILO, 2017); initially unallocable income tax is a withholding tax on personal income and hence classified under PIT; country strongly dependent on resource revenues (Doraisami, 2009; Scheiner, 2015).	Comparable tax/GDP with series reported in IMF (2019), though much smaller than estimates in World Bank (2020), the latter may include resource revenues.
Togo	1966-2018	HA (1966-1999); OECD (2000-2018)	*Annuaire Statistique*	Interpolate 1981-1982; 'transaction tax' in HA is classified as indirect tax, rather than property-wealth tax (Ghura, 1998); mid-1960s to late-1960s were were marked by political transition and coups d'etat, caution reliability of data and absence of historical estimates to corroborate [flagged]; social security began in 1968 (SSA, 2017), use data from Fisunoglu et al. (2011) prior to OECD coverage.	Overall tax/GDP levels are comparable to Ghura (1998) and Stotsky and Woldemariam (1997) during 1980s and 1990s.
Trinidad and Tobago	1965-2018	HA (1965-1989); OECD (1990-2018)	*Annual Statistical Digest*	Large increase in early 1970s seems to be driven by economic volatility and increased inflation with strong increase in CIT collection; dip in CIT collection in late 1990s may be genuine (appears in both OECD and official government records), but unclear nature of shock; historical IMF data corroborates levels of PIT and CIT in 1970s.	Close match in levels of tax/GDP during 1960s and 1970s with Lotz and Morss (1967) and Chelliah (1971).
Tunisia	1965-2018	HA (1965-1999); OECD (2000-2018)	*Annuaire Statistique*	Use data from Fisunoglu et al. (2011) for social security contributions prior to OECD coverage, which is otherwise missing in HA; initially unallocable income tax reported in HA in certain years, limited historical sources to specify allocation [flagged].	Tax/GDP levels comparable in late 1960s and early 1970s with historical IMF reports.
Turkey	1965-2018	OECD (1965-2018)		OECD data includes local taxes since late 1970s.	
Uganda	1965-2018	HA (1965-1991); OECD (1992-2018)	*Financial Summary and Revenue Estimates*	Use historical IMF reports to assign income tax between individuals and firms in HA periods; interpolate 1984, 1991; use data from Fisunoglu et al. (2011) for social security contributions prior to OECD coverage; large and sustained drop in tax collection in late 1970s likely driven by political transition.	Limited existence of studies for historical comparison.
Ukraine	1993-2018	ICTD (1993-2018)		HA is available from early 2000s onward, but comparable in levels with ICTD and we prefer to minimize total number of sources; ICTD numbers for social security contributions are corroborated in government documents, and dip in late 1990s is observed in additional sources (UN-SNA and Fisunoglu et al., 2011).	Polackova (1996) tax/GDP estimate in 1993-1994 is comparable.
United Kingdom	1993-2018	OECD (1993-2018)			
United States of America	1965-2018	OECD (1965-2018)			
Uruguay	1972-2018	HA (1972-1989); OECD (1990-2018)	*Anuario Estadistico*	Earlier HA series exist, but implied levels of tax/GDP are not comparable to historical estimates (Lotz and Morss, 1967), we begin series in 1972 when there is stronger consistency with other studies; very limited collection of PIT in historical periods is confirmed in IMF (1992); CIAT corroborates income tax split between PIT and CIT in 1990s.	Historical estimates of tax/GDP, centered on year of overlap between OECD and HA, match with data reported in IMF (1989) and OECD (1990).
Uzbekistan	1993-2018	ICTD (1993-2018)		ICTD includes social security contributions and which match well in levels with Fisunoglu et al. (2011), including the rise in collection in 2000s; interpolate 2013-2014.	Our tax/GDP estimates are slightly higher than Grigorian and Davoodi (2007) for 1998-2004 period, and slightly lower than Mokhtari and Ashtari (2012) for 2005-2010 period.
Venezuela	1965-2017	HA (1965-1979); ICTD (1980-1989); OECD (1991-2017)	*Anuario Estadistico*	Social security began in 1940s, we use data from Fisunoglu et al. (2011) for pre-OECD years; volatile yearly change in year of transition from HA to ICTD, no overlapping years of data to further investigate but similar yearly volatility persists in 1980s with ICTD coverage; assign most of non-CIT income taxes to PIT, given additional information in McClure (1992) and Zolt and Bird (2005). Interpolate 1976-1978.	Comparable levels and trends in tax/GDP with long-run estimates reported in Restuccia (2016) since early 1960s.
Vietnam	1994-2018	HA (1994-2002); OECD (2003-2018)	*Annuaire Statistique, Monthly Bulletin of Statistics, Vietnam Statistical Data in the 20th Century, Statistical Yearbook*	HA sources rely on IMF data; prior to Doi Moi reforms in late 1980s, revenues were largely generated from non-tax sources; social security contributions likely in place during HA coverage, but no data in HA series [flagged]; Bhattarai, Nguyen and Nguyen (2018) corroborate split between PIT and CIT between 1994 and 2010.	Close tax/GDP estimates reported in Cottarelli (2011) for the period between 2001 and 2008.
Yemen	1990-2012	HA (1990-1997); ICTD (1998-2012)	*Statistical Yearbook*	No data past 2012, given political unrest and violence; good match on levels between HA and ICTD in overlapping years; use data from Fisunoglu et al. (2011) for social security contributions.	Tax/GDP estimates are comparable to those reported in IMF (2002) for late 1990s period, generally limited historical comparisons.
Zambia	1965-2018	HA (1965-2018)	*Financial Report, Financial Statistics of Government Sector*	Use social security contributions from Fisunoglu et al. (2011) as it exists during full period, corroborated with HA estimates in specific years; limited comparison with ICTD data as it appears to include resource revenues in CIT numbers in certain years [flagged]. Interpolate 1986, 1990-1991.	Comparable tax/GDP estimates in 1990-2004 with DiJohn (2010) and Weeks et al. (2006), lower tax/GDP estimate than in Colclough (1988) for 1975-1985 period though this may be due to our omission of category 'miscellaneous capital receipts' in HA which we do not count as tax revenue.
Zimbabwe	1980-2018	ICTD (1980-2018)		Data coverage begins after independence; interpolate 1998; social security contributions began in 1989 (SSA, 2017), data matches well between ICTD and Fisunoglu et al. (2011); increase in unallocable income taxes between 2010 and 2018, but limited additional information to clarify allocation between individuals and firms; collapse in tax collection in late 2000s driven by economic conditions.	

B.2 Construction of ETR

By combining data on disaggregated tax revenues and national income components, we construct effective tax rates on capital and labor (equations 1 and 2 in Section 3.1). Here we provide further details on the definitions of ETR. Computing ETR_L and ETR_K requires the following information for country c in year t:

$$ETR_{L,ct} = \frac{T_{L,ct}}{Y_{L,ct}} = \frac{\lambda_{PIT,ct} \cdot T_{1100,ct} + \lambda_{socsec,ct} \cdot T_{2000,ct}}{CE_{ct} + \phi_{ct} \cdot OS_{PUE,ct}}$$

$$ETR_{K,ct} = \frac{T_{K,ct}}{Y_{K,ct}} = \frac{(1-\lambda_{PIT,ct}) \cdot T_{1100,ct} + (1-\lambda_{CIT,ct}) \cdot T_{1200,ct} + (1-\lambda_{assets,ct}) \cdot T_{4000,ct}}{(1-\phi_{ct}) \cdot OS_{PUE,ct} + OS_{CORP,ct} + OS_{HH,ct}}$$

For each type of tax j, there is a $\lambda_{j,ct}$ allocation of the tax to labor which may vary by country-year (and $1-\lambda_{j,ct}$ is the allocation to capital). The allocation for each type of tax is described in Table B3, where the types of taxes follow the OECD classification. In our benchmark assignment, these allocations are time- and country-invariant for all types of taxes, except for personal income taxes ($\lambda_{PIT,ct}$) which we discuss in detail below. Further, in our benchmark assumption, we assume that the labor share of mixed income, ϕ_{ct}, is fixed at 75% in all country-years ($\phi_{ct} = 0.75$). In robustness checks, we let ϕ_{ct} vary at the country-year level, based on either the ILO (2019) method or the country-year varying labor share in the corporate sector. In our benchmark assignment, replacing the invariant parameters with their fixed numerical values, we therefore have:

$$ETR_{L,ct} = \frac{T_{L,ct}}{Y_{L,ct}} = \frac{\lambda_{PIT,ct} \cdot T_{1100,ct} + T_{2000,ct}}{CE_{ct} + 0.75 \cdot OS_{PUE,ct}}$$

$$ETR_{K,ct} = \frac{T_{K,ct}}{Y_{K,ct}} = \frac{(1-\lambda_{PIT,ct}) \cdot T_{1100,ct} + T_{1200,ct} + T_{4000,ct}}{0.25 \cdot OS_{PUE,ct} + OS_{CORP,ct} + OS_{HH,ct}}$$

Below, we describe the parameter values in detail in Table B3, both for the tax revenue numerator and the national income denominator. We then provide more details on two key parameters: λ_{PIT}, the share of personal income tax revenue assigned to labor; and ϕ, the labor share of mixed income.

Table B3: Main Tax Revenue and National Accounts Concepts

Panel A: Tax Revenue

OECD revenue classification	type of tax j	incidence λ_j on labor	notes
1100	personal income tax (PIT)	$68\% \leq \lambda_{PIT} \leq 93\%$	Taxes on individuals (wages, capital income, capital gains). $\lambda_{PIT,ct}$ varies by country and year: see Section B.2 for details
1200	corporate income tax (CIT)	$\lambda_{CIT} = 0\%$	Taxes on corporate profits. Unallocable income taxes (OECD category *1300*) are split between PIT and CIT based on information from additional sources – see Table B2 for details
2000 / 3000	social security & payroll	$\lambda_{soc.sec.} = 100\%$	Includes all social security contributions as well as payroll taxes
4000	property & wealth taxes	$\lambda_{assets} = 0\%$	Includes property, wealth and financial transaction taxes
5000	indirect taxes	excluded	Includes trade taxes, value-added taxes and other sales taxes and excise taxes. We consider these taxes as prior to factor income returns, such that they can be excluded from factor income taxation (Browning, 1978; Saez and Zucman, 2019).
6000	other taxes	excluded	Rare in occurence and often quantitatively small
7000	non-tax revenue	excluded	Does not meet definition of taxation, can be quantitatively significant

Panel B: National Accounts

Natl. accounts acronym	national income component	benchmark allocation	notes
CE	compensation of employees	labor	Includes wages and salaries, employer and employee social contributions, and all payments from employers to their employees
OS_{PUE}	mixed income	$\phi = 75\%$ labor	'Operating surplus of private unincorporated enterprises' includes income from self-employment, household business owners, and informal or unincorporated enterprises
OS_{HH}	imputed rent	capital	'Operating surplus of households' is imputed rental income accruing to homeowners who live in their own home
OS_{CORP}	corporate profits	capital	'Operating surplus of corporations' includes all corporate income after paying employees and expenses, and can be thought of as corporate-sector capital income
OS_{GOV}	government operating surplus	—	$OS_{GOV} = 0$, by construction in national accounts
NIT	net indirect taxes	excluded	'indirect taxes, net of subsidies' usually comprise 8-15% of national income.
NFI	net foreign income	—	We treat domestic income without balancing the accounts to foreign earned income: many countries tax income earned domestically, regardless of citizenship, whereas net foreign income is taxed only with difficulty
CFC	depreciation	excluded	Factor income and our ETR are expressed net of 'consumption of fixed capital'

Labor share of personal income taxes: λ_{PIT} As discussed in Section 3.1, the level of PIT revenue that derives from capital versus labor income is rarely directly observed.[69] Thus, within personal income tax (PIT), an important parameter is the share of revenue assigned to labor, denoted λ_{PIT}. In the United States, Piketty et al. (2018) find that approximately 85% of PIT revenue is from labor and 15% from capital. To construct country-year specific $\lambda_{PIT,ct}$, we start from the US benchmark ($\lambda_{PIT} = 85\%$), to which we make two country-year specific adjustments:

(a) First, the location of the PIT exemption threshold in the income distribution impacts λ_{PIT}, since the capital income share is higher for richer individuals. We retrieve PIT exemption thresholds from Jensen (2022). We assume countries with a higher PIT exemption threshold have a higher λ_{PIT}. Since the US has a low exemption threshold with $\lambda_{PIT} = 85\%$, we similarly assign 85% of PIT to labor in countries for which the PIT covers half or more of the workforce (mainly high-income countries). For countries where the PIT covers 1% or less of the workforce (lowest-income countries), we assign a maximum PIT capital share of 30%. For PIT thresholds with a coverage between 1% to 50% of the workforce, we linearly assign λ_{PIT} between 70% and 85%.

(b) Second, we assume that countries where a dual PIT system is in place have a larger λ_{PIT}. Dual PIT systems set capital income taxation to a lower—often flat—rate, while labor income is taxed with progressive marginal tax rates. We compute the measure of the percent difference between the tax rate on dividends and the top marginal tax rate on labor income. Data on dividend vs wage income tax rates are taken from OECD Revenue Statistics and country-specific tax code documents. Since we only have dividend rates, we assume that 50% of capital income in PIT benefits from the lower rate (e.g., capital gains might not benefit). For this 50%, we multiply λ_{PIT} by the percent difference in dividend versus top marginal tax rates.

Labor share of mixed income: ϕ Section 3.1 noted the difficulty of estimating the labor share of mixed income (unincorporated enterprises). We assume a benchmark measure of $\phi = 75\%$. The implied capital share is lower than the 30% used in Distributional National Accounts guidelines (Blanchet et al., 2021). However, since the global average corporate capital share is 27%, assuming that the capital share of unincorporated enterprises is slightly lower appears reasonable (see Guerriero, 2019).

We implement two robustness checks. First, we set the labor share of mixed income equal to that of the corporate sector at the country-year level; specifically, $\phi_{ct} = \frac{CE_{ct}}{CE_{ct}+OS_{CORP,ct}}$. This procedure follows Gollin (2002).

[69] PIT revenue from capital income includes taxes on dividends and capital gains and on the capital share of self-employment income. OECD revenue data occasionally reports tax revenue from capital gains, which was on average 4% of PIT in the period 2010-2018 (7.5% in the US).

Second, we implement the ILO (2019) method which relies on harmonized household surveys and labor force surveys across many years in developing countries. To match our ETR sample coverage, we extend the ILO's sample with as much additional data as we can retrieve from complementary sources. Estimation of the relative labor income of self-employed is based on the observable characteristics of those workers and their comparison with employees. Relevant variables, including industry, occupation, education level and age, are used in a regression to uncover the determinants of labor income of employees. Given the estimated relationship between employee labor income and the explanatory variables, labor income is extrapolated to self-employed, generating a coefficient of relative earnings to employees, denoted γ_q. The method estimates a separate γ_q for different groups q of self-employed: self-employed workers; own-account workers; and, contributing family members. A correction procedure is implemented to reduce the bias from selection into self-employment. Total labor income in a given country-year is then determined as

$$Y_L^{ILO} = CE + \sum_q w_{emp} \cdot \gamma_q \cdot b_q$$

where CE is the total compensation of employees in national accounts, w_{emp} is the average employee wage (which relates CE to the total employee workforce), b_q is self-employed group q's count in the workforce, and γ_q is the q-specific earnings coefficient relative to the average employee wage.[70] Intuitively, ILO (2019) does not rely on the SNA observed values of mixed income; instead, it estimates factor shares through a regression framework which computes 'shadow wages' of self-employed based on their overlap in observable characteristics with employees. It provides country-year varying measures of ϕ based on variation across space and time in w_{emp}, γ_q and b_q.

The main challenge is that the estimation framework for γ_q is not disciplined by the country's actual values in SNA. In particular, nothing prevents $\sum_q w_{emp} \cdot \gamma_q \cdot b_q > OS_{PUE}$ - such that estimated labor mixed income is larger than the SNA actually observed entire mixed income. This would, implausibly, imply that $\phi^{ILO} > 100\%$. By extension, this also implies the need to correct values of other national account components, in order for the accounting identity to hold whereby the sum of these components add up to the observed SNA value of total national income Y. It is unclear which labor or capital components would have to be revised downwards, and to what extent.

By estimating labor's *share* of mixed income, the method can generate estimated values of the *level* of mixed income which are at odds with observed ones, and which require revisions to other national income components. To remedy this concern, we winsorize ϕ^{ILO} at 100%. In cases where γ_q and b_q are not from ILO (2019), we also winsorize ϕ^{ILO} from below with the lowest observed country-year value in ILO

[70] After expanding data-coverage through additional sources, in the remaining cases where b_q and γ_q are missing, we impute observations using the same procedure as in Section B.1 above. This is particularly relevant in the years before 1991.

(2019), which is 35%. This latter case occurs in 5% of the full sample. By imposing $35\% < \phi^{ILO} < 100\%$, we allow mixed income's labor share to be country-year varying and take on plausible values, while ensuring that SNA aggregate values remain intact and consistent with national accounting identities. In practice, while the ILO (2019) method generates important country-year variation, the global average value for ϕ^{ILO} does not differ much from our benchmark value $\phi = 75\%$.

Mixed income in China and the US We make minor mixed-income adjustments to the benchmark series for China and the United States. First, for China, Piketty, Yang and Zucman (2019) show that the income of many self-employed agricultural workers is attributed to employee compensation in the SNA 2008 data and not to mixed income (as in other countries). They conclude that Chinese national accounts systematically underestimate mixed income but overestimate compensation of employees. We therefore estimate mixed income according to ILO (2019) for China.

We use the factor shares from Piketty et al. (2018) for the US. The authors show that some large businesses (including listed firms) are organized as partnerships and are classified as non-corporate businesses and not corporations (as in other countries). The SNA of the US lists their income as mixed income (rather than corporate profits). The revised US series therefore (i) assumes a higher capital share of income for partnerships vs. other non-corporate businesses; and (ii) factors in the rising capital intensity of partnerships since the 1980s.

Comparison with pre-existing ETR series We compare our methodology to pre-existing ETR series. The main differences are summarized in Table B4.

In McDaniel (2020), updated from McDaniel (2007), there are two main differences with our benchmark methodology. First, the author assigns the capital-share in mixed income based on the observed factor share in the rest of the economy, while our benchmark assigns a fixed share (25%). Second, the author assumes that labor and capital in PIT are taxed at the same rate. This is the same assumption as in Mendoza et al. (1994), and differs from our benchmark where we create an allocation of PIT to capital that varies by country and year, $(1 - \lambda_{PIT,ct})$. These methodological differences are reasonably captured in our robustness checks. For the first difference, note that this choice effectively amounts to using the observed capital-share in the corporate sector to assign the capital share in mixed income (see equation 3 in McDaniel (2007)). This corresponds directly to one of our robustness checks (Panel B in Figure 2).

We can relate the second difference to our robustness check where we vary the capital share of PIT from 0% to 30% (Panel A in Figure 2). If labor and capital face the same tax rate, then the capital share of PIT increases in the capital factor share and in the share of capital that is taxable. Using the empirical measures for taxable shares established in the US (Piketty et al., 2018), and assuming both factors face the same rate, the capital share of PIT that would result at the 99^{th} percentile of observed capital factor shares in our full sample is $1 - \lambda_{PIT} = 0.305$. In other words, our robustness check which implements $1 - \lambda_{PIT} = 0.30$ constitutes a meaningful upper bound on the capital share in PIT that would result from any observed factor shares in our sample and assuming capital and labor pay the same rate and have taxable shares as measured in Piketty et al. (2018). Of course, under the assumption that both factors are fully taxable (unrealistic given empirical findings in Jensen (2022)) and face the same rate, the capital share of PIT would be equal to the observed capital factor share. Our benchmark methodology takes a step towards trying to measure the taxable factor shares as they vary across countries and time, with a $1 - \lambda_{PIT,ct}$ at the 99^{th} percentile that equals 0.32. Future work could improve on this measurement, by combining additional information from national accounts and tax records.

The ETR series that would result from applying the methodology in McDaniel (2020) to our sample is therefore reasonably bounded by our robustness checks which assign capital's share of mixed income based on the corporate capital share, and which vary the capital share in PIT between 0% and 30%.

In addition to these main differences, McDaniel (2020) considers property taxes paid by households as consumption taxes and property taxes paid by businesses as capital taxes, while our series considers all property taxes to be capital taxes. This difference is unlikely to be quantitatively significant. Finally, McDaniel (2020) uses tax data from national accounts, while we rely on various public finance sources.

In Panel B of Figure B1, we use McDaniel (2020)'s specific sample. The trends are similar between series. When weights are applied, our benchmark series is on average 18.75% higher in levels than McDaniel (2020). This wedge arises from the methodological differences (which we can account for in our robustness checks), and possibly from the differences in tax data-sources.

In Kostarakos and Varthalitis (2020), which applies the methodology in Carey and Rabesona (2004) to data from more recent years, there is one main methodological difference with our benchmark. In particular, the authors estimate relative labor income of self-employed on the basis of observable characteristics and a comparison with the wage of employees, while our benchmark method assigns a fixed labor share to mixed income. However, this alternative method corresponds closely to the method in ILO (2019), which we implement as a robustness check. Thus, this robustness check (Panel B of Figure 2) meaningfully captures the ETR series that would result from applying the methodology in Kostarakos and Varthalitis (2020) to our sample.

One additional difference is that Kostarakos and Varthalitis (2020) assume social security contributions are deductible from the taxable income of households while our method follows national accounts convention and assumes they are not. We confirm that implementing this change in our series does not meaningfully alter the results (available upon request). Finally, Kostarakos and Varthalitis (2020) draw their tax revenue data from a different source (Eurostat) than us.

In Panel A of Figure B1, we use Kostarakos and Varthalitis (2020)'s specific sample. The trends are similar between series. When weights are applied, our benchmark series is on average 14.2% lower in levels than Kostarakos and Varthalitis (2020). This wedge arises from the methodological differences (which we can account for in our robustness checks), and possibly from the differential treatment of social security contributions and from the different data-source for tax revenue.

Discussion of $\overline{ETR}_C^K$ Our measure of the backward-looking average effective tax rate on corporate profits, $\overline{ETR}_C^K$, is related to, but also distinct from, the forward-looking measures of the statutory tax burden in the corporate sector in developing countries (Section 2). There are two main reasons why these measures differ.

The first reason is that the measure of corporate profits in $\overline{ETR}_C^K$ is based on national accounts, which differs both empirically and conceptually from how corporate profits may be measured using tax data. Empirically, the data-sources for national accounts include corporate tax returns but also non-tax sources such as industrial censuses and surveys. The measure of corporate profits based on national accounts may therefore include profits which are not reported in tax returns. Indeed, the national account guidelines explicitly try to account for mis-reported profits and corporate profits are usually found to be larger in value when measured from national accounts than from tax returns (Lequiller & Blades, 2014). For this same reason, constructing an appropriately-weighted backward-looking firm-level effective tax rate based on taxes paid and profits reported in tax returns may not give the same value as $\overline{ETR}_C^K$. This firm-level measure is analyzed in Section 6; see also Dyreng et al. (2017) and Egger et al. (2009) for firm-level estimates in large samples focused on developed countries.

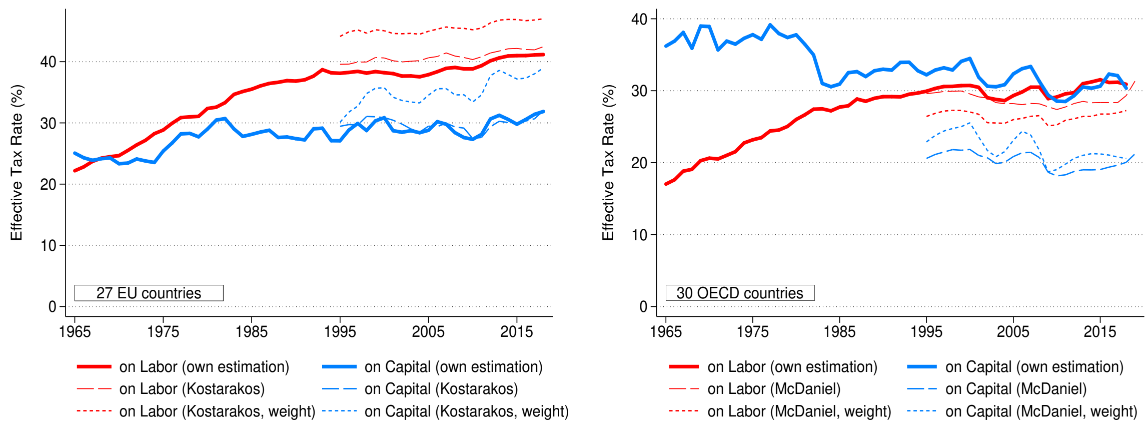
There are also conceptual differences, which are discussed in detail in IMF (2014), Lequiller and Blades (2014) and Ueda (2018). Consumption of fixed capital in national accounts adjusts for inflation and is estimated according to physical and economic laws of depreciation, whereas companies sometimes measure depreciation without regard to inflation and may shorten or lengthen the time of amortization according to tax advantages. In addition, inventory appreciation (the net gain in inventory) is usually accounted for in company profits but not in national accounts. Moreover, expenditure on intellectual property is counted as investment in national accounts, but may be listed as intermediate consumption by companies on their tax returns. Finally, some sources of property income (e.g. investment valuation increases; resource rents paid vs. received) and capital gains (e.g. sale of subsidiaries or currency transactions) are counted in company profits but not in national accounts.

The second reason is that the corporate statutory tax burden varies across firms due to economic variables, including sector, size and profitability (Devereux et al., 2004; Kumar & James, 2022). Changes in these economic variables will be reflected in $\overline{ETR}_C^K$, but may not be fully captured in the statutory measures.

Finally, $\overline{ETR}_C^K$ is related to the measure of CIT-efficiency in developing countries (IMF, 2014). We discuss how these measures compare in Section 3.3 and the supplementary appendix.

Figure B1: ETR Evolution and Existing Studies

(a) Kostarakos and Varthalitis (2020) (b) McDaniel (2020)



Notes: These graphs provide a comparison of our ETR estimations with the recent literature. The left-hand graph compares our estimations with Kostarakos and Varthalitis (2020), based on EU-27 members from 1995 to 2019. The right-hand graph compares our estimations with the updated dataset in McDaniel (2020) that includes 30 OECD countries from 1995 to 2018. This extension is based on McDaniel (2007) (Table B4), and covers the largest OECD countries, including the US, as well as Mexico and Turkey. The solid line represents the results using our ETR measures and weights, but based on the exact country samples in the respective studies. The long-dash line replicates the ETR measures from the two studies. The short-dash line extends their ETR series but using our country-year weights. For a discussion of the underlying differences in ETR, please see Section B.2 and Table B4.

Table B4: Effective Tax Rates: Pre-Existing Databases

Paper	**Time**	**Countries**	**Source**	**Methodological note on differences with our approach**
Mendoza et al (1994)	1965-1988	G7 members	OECD	Difference: All mixed income is allocated to capital income. Difference: Labor and capital in the PIT are taxed at the same rate
Carey and Rabesona (2004)	1975-2000	25 OECD biggest members	OECD	Difference: Mixed income allocation where self-employed pay themselves the annual salary earned by the average employee. Similarity: Labor and capital in PIT are not taxed at same rate, measure preferential tax treatment of pension funds and dividends. Difference: Social security contributions deducted from household income.
McDaniel (2007) (McDaniel 2020)	1950-2003 (updated: 1995-2018)	15 OECD biggest members (updated 30 OECD biggest members)	OECD	Difference: Mixed income imputed to capital based on rest-of-economy share. Difference: Labor and capital in PIT are taxed at the same rate
Kostarakos and Varthalitis (2020)	1995-2019	EU-27 members	Eurostat	Follows Carey and Rabesona (2004)

Appendix C Trade Liberalization Event Studies

C.1 Description of liberalization events

Our selection of trade events is determined by three criteria. First, the event is related to measurable policy reforms; this improves the transparency of the event-study design which is based on a well-defined policy event. Second, the policy reforms induced large changes in trade barriers; this increases the likelihood of observing sharp breaks in our macroeconomic outcomes around the event-time. Third, the event has been studied in academic publications; this allows us to rely on events for which the positive effects on openness have previously been established.

Selection of events These criteria led us to focus on the six trade liberalization events referenced in review articles by Goldberg and Pavcnik (2007, 2016) to which we add China's WTO accession event (studied in Brandt et al., 2017). Most of these selected events feature reductions in tariff rates: many of the countries did not participate in the early GATT/WTO negotiation rounds, making reductions in tariffs an available policy lever. The tariff reductions were large: Brazil cut tariff rates from 59% to 15%, India from 80% to 39% percent, and China from 48% to 20%. Mexico reduced tariff rates from 24% to 12% and import licence requirements went from covering 93% of national production to 25%; Colombia's tariffs were reduced from 27% to 10% and import requirements dropped from 72% of national production coverage to 1%. In the selected countries, "tariff reductions constitute a big part of the globalization process" (Goldberg & Pavcnik, 2016). The timing of the events and academic references are provided in Table A1.

Timing of events Below are narrative analyses for some of the events:

- **Brazil** The liberalization event of 1988 is detailed in Dix-Carneiro and Kovak (2017). The authors note: "In an effort to increase transparency in trade policy, the government reduced tariff redundancy by cutting nominal tariffs... Liberalization effectively began when the newly elected administration suddenly and unexpectedly abolished the list of suspended import licences and removed nearly all of the remaining special customs regimes."

- **Columbia** Similarly to Brazil, tariff reductions in Colombia in 1985 were driven by the country's decision to impose uniform rates across products and industries under the negotiation commitments to the WTO. Goldberg and Pavcnik (2007) note that this reform objective makes "the endogeneity of trade policy changes less pronounced here [in Colombia] than in other studies."

- **China** Brandt et al. (2017) note that trade openness reforms had gradually been implemented in China prior to the country's WTO accession in 2001, but that the tariff reductions implemented upon accession were large, "less voluntary" and largely complied with the pre-specified WTO accession agreements. Importantly, the potential accession to WTO contributed to the timing of privatization initiatives, in which the Chinese government restructured and reduced its ownership in state-owned enterprises. While the privatization efforts began in 1995 and were incremental, it is possible that additional sell-offs in the post-WTO years contribute to the observed medium-run trends in our outcomes.

- **India** The 1991 event in India occurred as a result of an IMF intervention that dictated the pace and scope of the liberalization reforms. Under the IMF program, tariff rates had to be harmonized across industries, which, like in Brazil and Colombia, led to a large average reduction in tariffs. Topalova and Khandelwal (2011) argue the Indian reform "came as a surprise" and "was unanticipated by firms in India." The reforms were implemented quickly "as a sort of shock therapy with little debate or analysis." The IMF program was in response to India's balance of payment crisis, which was triggered by "the drop in remittances from Indian workers in the Middle East, the increase in oil prices due to the Gulf War, and political uncertainty following the assassination of Rajiv Gandhi".

- **Vietnam** The 2001 reform in Vietnam was implemented as a broad trade agreement that did not involve negotiations over specific tariffs (McCaig & Pavcnik, 2018). The reform was driven by the American government's decision to reclassify Vietnam from 'Column 2' of the US tariff schedule to the 'Normal Trade Relations'. Column 2 was designed in the early 1950s for the 21 communist countries, including Vietnam, with whom the US did not have normal trading relations. McCaig and Pavcnik (2018) show that there are no differential trends between Vietnamese exports to the US vs other high-income countries.

These descriptions of reform timing do not suggest that the liberalization events were directly triggered by changes in domestic taxation or factor incomes.

Goldberg and Pavcnik (2007) note other cross-border reforms that occurred in the post-years of the liberalization events. Argentina's 1989 event and Brazil's 1988 event were followed by accession to Mercosur in 1991; India's 1991 event was followed by foreign direct investment liberalization in 1993; and Mexico's 1985 WTO accession was followed by a removal of capital inflow restrictions in 1989. These reforms occurred with some lag to the trade liberalization events, and may have contributed to the medium-run effects observed in Figure 5.

C.2 Event study methodology

Sample construction Our sample is constructed by applying a synthetic matching procedure to every treated country for each outcome of interest. The donor pool has to be fully balanced in all pre-event periods. To estimate the event study in equation (4) for a given outcome, the sample pools the seven treated countries and their synthetic control countries for 10 years before and after the events (yielding 294 observations). We also estimate the difference-in-differences (DiD) model:

$$y_{ct} = \beta^{DiD} \cdot \mathbb{1}(e \geq 0)_t \cdot D_c + \theta_t + \kappa_c + \pi_{Year(t)} + \epsilon_{ct} \quad (11)$$

which uses the same notation as equation (4). Moreover, we use the imputation method by Borusyak et al. (2021) to report average treatment effects comparable to β^{DiD} with a technique that deals with issues with two-way fixed effects and heterogeneous event timing. Details are provided in the supplementary appendix (link). All the DiD average treatment effects are reported in Table A2.

Simultaneously matching on main outcomes We test if our results hold with a more restrictive synthetic control, by using our three main outcomes—trade, ETR_K and ETR_L— to construct one synthetic control group per treated country. The composition of the control group is now held constant across regressions with different outcomes. The results are reported in Panel B of Table A2.

C.3 Alternative trade liberalization event study

We present results based on an alternative measure of trade liberalization events. We use the events from Wacziarg and Welch (2008), who extended the Sachs-Warner (1995) study to cover 141 countries at all levels of development between the early 1950s and 1998. When merged with our data, the sample covers 68 liberalization events that occurred between 1965 and 1998 in developing countries. A trade liberalization event is defined to occur when all five of the following conditions no longer hold: (i) average tariff rates are above 40%; (ii) non-tariff barriers cover at least 40% of trade; (iii) the black market exchange rate is at least 20% lower than the official exchange rate; (iv) there is a state monopoly on major exports; (v) there is a socialistic system in place. These conditions are broader than our main liberalization event criteria (Section 5.1 and C.1). At the same time, our main events are covered in this expanded event-sample (with the exception of China and Vietnam, whose events are after the end of the sample-period); this occurs because the reduction in tariff rates, one of our main event criteria, was the remaining event-condition to be satisfied in Wacziarg and Welch (2008). Based on a within-country event-study design, the authors find that the trade liberalization events led to higher trade openness, investment and GDP growth.

We estimate the effects of the liberalization events using the DiD model

$$y_{ct} = \beta^{DiD} \cdot E_{ct} + \theta_t + \theta_c + \epsilon_{ct} \quad (12)$$

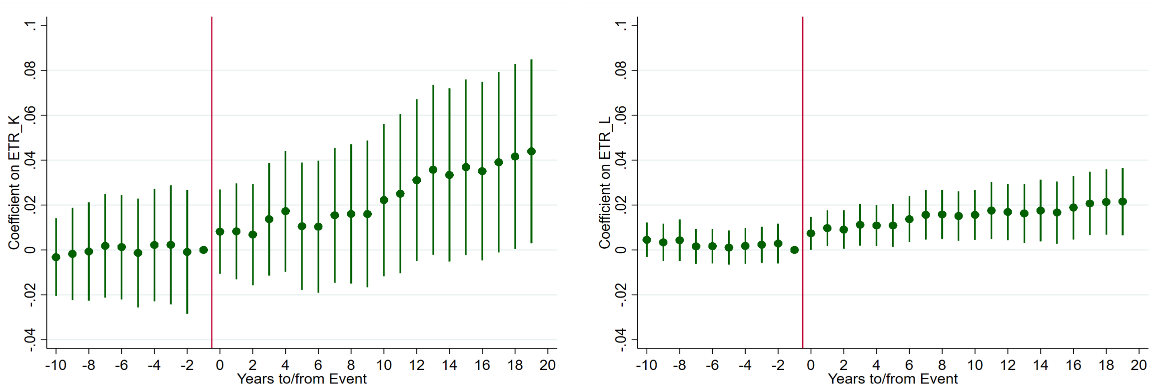
where y_{ct} is the outcome of interest in country c in year t, E_{ct} is the event indicator which takes on a value of 1 in all periods after a country has a liberalization event (and 0 otherwise), and θ_t and θ_c are year and country fixed effects, respectively. The standard error ϵ_{ct} is clustered at the country level. Given the large number of liberalization events ($N = 68$), estimation issues arising from heterogeneous treatment-timing are likely to be important. For this reason, we report and focus on the imputed treatment effects based on Borusyak et al. (2021). We do not use synthetic controls. We restrict the sample to developing countries between 1965 and 2008 (permitting a 10-year post-event horizon for all events, like in our main event-study in Section 5.1).

Panel A of Table C1 reports the β^{DiD} impacts on trade openness, ETR_K and ETR_L. Imputed treatment effects are more precisely estimated than the simple DiD coefficients. Despite being based on broader criteria, the trade liberalization events produce qualitatively similar results to the main event-study (Section 5.1), with positive impacts on openness and both ETRs, and a larger magnitude-impact on ETR_K than ETR_L. Figure C1 estimates the dynamic event-study regression with the method of Borusyak et al. (2021). Liberalized and control countries are on parallel trends until the event onset; both ETRs start to increase in the immediate post-event years.

The remaining panels conduct robustness checks similar to those for the main liberalization events (Section 5.1). In Panel B, the results are robust to estimating the effects in a fully balanced panel 10-years post-reform (matching the sample construction and post-event horizon for the main events). In Panel C, the results hold when the control group is formed within-region. To alleviate concerns over concurrent external reforms, Panel D shows the results are robust to excluding countries which have cross-border capital liberalization events at any point during the sample-period (based on Bekaert, Harvey and Lundblad, 2000). Finally, to alleviate concerns about confounding domestic reforms, Panel E shows the results hold when we exclude countries with concurrent domestic reforms (as coded in Wacziarg & Wallack, 2004).

Figure C1: Event-Study of Trade Liberalization Based on Wacziarg & Welch (2008)

(a) Effective tax rate on capital (b) Effective tax rate on labor



Notes: These graphs show the event-study impacts of the trade liberalization events from Wacziarg and Welch (2008) on the effective tax rate on capital (left panel) and labor (right panel). For more details, see Appendix C.3.

Table C1: Trade Liberalization Event-Study Based on Wacziarg & Welch (2008)

	Trade	ETR_K	ETR_L
	(1)	(2)	(3)
Panel A: *Benchmark*			
Post*Treat	0.030	0.016	0.006
	(0.048)	(0.017)	(0.006)
Imputed treatment effect	0.088*	0.039**	0.021***
	(0.049)	(0.016)	(0.005)
N	4032	4032	4032
Panel B: *Fully balanced panel, 10 year post-reform*			
Imputed treatment effect	0.110**	0.031**	0.018***
	(0.054)	(0.014)	(0.005)
N	3082	3082	3082
Panel C: *With region-year fixed effects*			
Imputed treatment effect	0.082**	0.040**	0.021***
	(0.040)	(0.016)	(0.005)
N	4032	4032	4032
Panel D: *Excluding countries with capital liberalization*			
Imputed treatment effect	0.101*	0.029*	0.014**
	(0.057)	(0.016)	(0.006)
N	2651	2651	2651
Panel E: *Excluding countries with domestic reforms*			
Imputed treatment effect	0.053	0.038**	0.017***
	(0.050)	(0.016)	(0.005)
N	3551	3551	3551

Notes: This table shows the results from estimating the difference-in-difference regression and the imputed treatment effect of the 68 trade liberalization events from Wacziarg and Welch (2008), between 1965 and 2008. The sample is low and middle income countries, based on the World Bank income classification in 2018. In Panel A, the post*treat coefficient corresponds to the β^{DiD} based on estimating equation 12. The imputed treatment effect is based on the method in Borusyak, Jaravel, and Spiess (2021). In Panel B, the sample is restricted to the fully balanced set of countries in the 10 years after the liberalization event. In Panel C, the estimation is augmented with region-by-year interactive fixed effects. In Panel D, the sample excludes all countries that have a capital liberalization reform at any point during the sample-period, based on Bekaert, Harvey and Lundblad (2000). In Panel E, the sample excludes all countries with domestic reforms which coincide in timing with their trade liberalization event, based on Wacziarg and Wallack (2004). Standard errors are clustered at the country level. For more details on the liberalization events, see Appendix C.3. * p<0.10 ** p<0.05 *** p<0.01.

Appendix D Instrumental Variables for Trade

In this section, we outline the construction of the two instrumental variables. Both instruments are drawn from Egger et al. (2019), who provide further details.

Instrument based on quantitative trade models The first instrument leverages the structure of gravity models in general equilibrium. These models permit calibration of country pair-year-specific trade costs from trade data, relying on three key assumptions: (i) producers are perfectly competitive and make zero profits or charge a constant markup; (ii) trade costs take the iceberg form; and (iii) aggregate expenditure and its allocation across products are separable. These assumptions imply that bilateral consumption shares towards country o by consumers in country c in year t, denoted π_{cot}, have multiplicative components that are exporter-year-specific (ψ_{ot}), importer-year-specific (ι_{ct}) and pair-year-specific (β_{cot}):

$$\pi_{cot} = \quad \psi_{ot} \quad \times \quad \iota_{ct} \quad \times \quad \beta_{cot}$$

The component ψ_{ot} is proportional to country o's supply potential and captures production costs and gross-of-tax factor income—and might be influenced by both capital and labor taxation. The component ι_{ct} depends on the consumer price index, which varies across years and countries.[71] β_{cot} captures trade frictions across country-pairs and time.[72] The product of the normalized shares gives the bilateral frictions of importing-exporting country-pairs at a point in time:

$$\frac{\pi_{cot}}{\pi_{cct}} \cdot \frac{\pi_{oct}}{\pi_{oot}} = \beta_{cot} \cdot \beta_{oct}$$

Finally, we use $\beta_{cot} \cdot \beta_{oct}$ to compute the average ct-specific costs of exporting and importing, which constitutes the instrument:

$$Z_{ct}^{gravity} = \sum_{o \neq c} [\beta_{cot} \cdot \beta_{oct}]$$

Note that all exporter-year and importer-year factors are removed from the instrument. This instrument is valid so long as the *distribution* of trade costs among country-pairs (not its level) is not influenced by the level of factor incomes or effective tax burdens. Constructing this instrument requires data on country-pair trade flows:

[71] Both ψ_{ot} and ι_{ct} may capture country-year-specific trade costs, but the pair-specific component β_{cot} is free of such country-year specific influence.

[72] Egger et al. (2019) exploit the multiplicative model structure about π_{cot} to recover measures of β_{cot}. They assume that transaction costs between domestic sellers and customers are zero, such that $\beta_{cct} = 1$. Both the importer-year component and exporter-year component can then be eliminated by normalizing import and export trade shares by the importer and exporters' consumption from domestic sellers.

we use UN COMTRADE data to construct a large sample of bilateral consumption shares.[73] First-stage regressions with $Z_{ct}^{gravity}$ are shown in Table A3.

Instrument based on global oil prices & transport distances The second instrument exploits spatial heterogeneity across countries in a way that interacts with oil price shocks. This instrument is based on global oil price changes over time and within-country transportation distances from cities to the nearest port.[74] The instrument is the variance of the product oil price p_t^{oil} $\times$ distance d_c^k across cities k in country c in year t:

$$Z_{ct}^{oil-dist} = \frac{1}{2}\sum_{k=1}^{3}[(p_t^{oil}d_c^k - p_t^{oil}\overline{d_c})^2]$$

where $\overline{d_c}$ is the average city-port distance in country c. This variance increases in countries whose main cities are far from the nearest port and far from each other, which implies a larger change to transportation costs following a global oil price shock in spread-out countries than in countries with concentrated populations. It is this transportation-cost shock that the instrument captures.[75]

This second instrument does not hinge on theoretical assumptions. Instead, this instrument is valid so long as the country-specific distribution of trade-costs, induced by the interaction between global oil price shocks and a country's fixed spatial concentration, is not correlated with contemporaneous changes in factor incomes and effective tax rates. First-stage results for $Z_{ct}^{oil-dist}$ are presented in Table A3.

[73] We augment our raw data from COMTRADE with data from Harvard Growth Lab, who harmonized importer- and exporter-reported trade flows to expand the coverage and improve the precision of country-partner-year trade flow estimates.

[74] For the former, we retrieve the OPEC Reference Basket benchmark world price of crude oil. For the latter, we measure road distances from the three largest cities (according to UN population statistics) to their nearest port, using SeaRates international shipping logistics calculators.

[75] Alternatively, one could measure the variance in distance and then multiply it by the global oil price. The distribution of the variance instrument $Z_{ct}^{oil-dist}$ across country-years would not change; the only impact would be a level-shift by the price. We consider the main approach to more closely capture the sensitivity of transport costs to spatial concentration, but results based on this alternative variance measure are similar.

Appendix E Additional Analyses of Tax Capacity

E.1 Firm-level analysis in Rwanda

This appendix provides details on the firm-level analysis in Rwanda (Section 6.3).

Data Our analysis draws on three administrative data sources from Rwanda, accessible at the Rwanda Revenue Authority (RRA), for the years 2015-2017. These data-sources can be linked through unique tax identifiers for each firm, assigned by the RRA for the purpose of collecting customs, corporate income and value-added taxes. The first data-source is the customs records, which contain information on international trade transactions made in each year by each firm. We use this data to measure each firm's direct imports. The second data is the firms' corporate income tax (CIT) declarations merged with the firm registry. These data contain detailed annual information on firms' profits, revenue and costs, as well as information on industry codes and geographical location. We use these data to measure firms' effective tax rate on profits. The third data-source is the business-to-business transactions database. These data are retrieved through the electronic billing machines (EBM) that all firms registered for VAT are legally required to install and use (Eissa and Zeitlin, 2014). For a given seller, EBMs record the transactions to each buyer identified by the tax firm-ID. We use this data to measure buyer-seller relationships.

When combined, these data allow us to construct the buyer-supplier relationships of the Rwandan formal economy and document firms' direct and indirect trade exposure. Importantly, since the network data is based on tax-IDs to link firms, this implies that we cannot observe transaction linkages with informal, non-registered firms. This sample selection on formal firms also features in most recent network studies, by virtue of relying on administrative data, including in Chile (Huneeus, 2020); Costa Rica (Alfaro-Ureña et al.); Ecuador (Adao et al., 2022); India (Gadenne et al., 2022); Turkey (Demir et al., 2021); and Uganda (Almunia et al, 2023a, 2023b).

Our sample is firms registered for CIT which report positive income during the years 2015-2017. Note that only a small number of firms are registered for CIT or VAT but not both, meaning that the overlap with the EBM transactions data is strong. However, restricting the sample to positive income is consequential, as a significant number of registered CIT firms are 'nil filers' that report zero income ('nil filers' are common in developing countries: Keen, 2012). We measure each firm i's yearly effective tax rate on corporate profits, corresponding to corporate ETR_i^K in equation (6), as the ratio of corporate taxes paid divided by net profit. Net profit is revenue minus material, labor, operational, depreciation and financial costs.

The EBM data is meant to improve the enforcement of corporate taxes and VAT, and the reporting of linkages is more comprehensive for the relatively larger firms that are registered for these tax bases. For smaller incorporated firms that are instead registered to simplified tax bases (flat-amount or turnover), the linkage reporting is less strong. Only a limited number of these firms are registered for VAT (due mainly to eligibility criteria). These firms are most likely to be recorded in the EBM data as clients in a particular transaction, making the coverage of their linkages less comprehensive. It is in principle also possible to measure ETR_i^K amongst these smaller, incorporated firms. However, the information on their tax returns regarding cost items is less detailed and additional assumptions on the relationship between turnover and profit are required, making the profit measure in the denominator of ETR_i^K less precise. With these data-challenges in mind, we can include these additional tax-registered firms in the analysis; we find qualitatively similar results (available upon request).

Exposure to trade To measure a firm's total exposure to trade, we follow Dhyne et al. (2021) who use similar administrative datasets as ours to measure trade exposure of Belgian firms. We define firm i's total foreign input share as the share of inputs that it directly imports (s_{Fi}), plus the share of inputs that it buys from its domestic suppliers l (s_{li}), multiplied by the total import shares of those firms:

$$s_i^{Total} = s_{Fi} + \sum_{l \in V_i} s_{li} \cdot [s_{Fl} + \sum_{r \in V_l} s_{rl} \cdot (s_{Fr} + ...)] \tag{13}$$

where V_i is the set of domestic suppliers of firm i, and V_l is the set of domestic suppliers of firm l. The denominator of the input shares is the sum of purchases from other firms and imports. Note that the definition of s_i^{Total} is recursive: a firm's total foreign input share is the sum of its direct foreign input share and the share of its inputs from other firms, multiplied by those firms' total foreign input shares. We limit the calculation to the inputs from a firm's immediate suppliers l as well as the suppliers to their suppliers r (adding more network-levels only marginally increases s_i^{Total}). In other words, s_i^{Total} reflects the direct import share of firm i's suppliers and the suppliers' suppliers, each weighted by the share of inputs that each firm buys from other domestic firms. We focus on firms' exposure to imports through their supplier network; in an extension, we find qualitatively similar results when we study firms' exposure to exports through their client network (results available).

Figure E1 displays a histogram of s_i^{Total} and s_{Fi} for all formal Rwandan firms. While just under 30% of firms import directly, 93% rely on trade either directly or indirectly through their suppliers. In the median firm, the total foreign input share is 48% (it is 39% for the median Belgian firm in Dhyne et al., 2021).

Impacts of trade exposure on ETR^K and size To visualize the association between trade exposure (s_i^{Total}) and ETR_i^K, we plot binned scatters of the variables against each other, after residualizing both against year fixed effects. In Figure E2, the dots correspond to equal sized bins of the residualized trade variable. The line corresponds to the best linear fit regression on the underlying firm-level data ($N = 18478$). Figure E2 reveals a positive and strongly significant association: firms that are more exposed to international trade, both through direct imports and through links to importers in the supply network, have higher effective tax rates on corporate profits.

We investigate the robustness of this association in Panel A of Table 4, where we estimate regressions of the form

$$ETR^{K}_{itg} = \mu \cdot s^{Total}_{it} + \Theta \cdot X_{it} + \pi_t + \pi_g + \epsilon_{itg} \tag{14}$$

where ETR^{K}_{itg} and s^{Total}_{it} are the corporate effective tax rate and trade exposure of firm i in year t in industry-geography group g, and π_t and π_g are fixed effects for year and industry-geography. ϵ_{itg} is clustered at the industry-geography level (robust to clustering at firm-level). Column (1) corresponds to the association in Figure E2. Column (2) adds 561 industry-geography interactive fixed effects between industry categories and geographical locations. In column (3), we add time-varying controls, including a firm's age, number of employees, and total number of clients and suppliers. In column (4), we include firm fixed effects (and cluster at the firm-level). The variation in trade exposure is now within-firm over time and can come, for example, from new linkages with suppliers that import directly or rely significantly on foreign inputs. The positive association between trade and ETR^{K}_{i} holds in these specifications.

In column (5), we employ an instrumental variable that creates trade shocks from changes in world export supply of country-product combinations in which a firm had a previous import relationship. Previous studies have used this strategy, arguing that the shocks are plausibly exogenous and vary significantly across firms because firms do not have all inputs in common (Hummels et al., 2014). Specifically, we follow the design in Dhyne et al. (2021) that extends the shift-share approach of Hummels et al. (2014) to a setting with shock pass-through via network linkages. To construct the direct import shock for firm i, we use information about the firm's product-country-level imports in year $t-1$ (the share variable capturing firm-specific shock exposure) and the aggregate shift in world export supply for each country and product:

$$logM^{D}_{it} = log \sum_{a,c} s^{a,M}_{ic,t-1} \cdot WES_{a,c,t} \tag{15}$$

where $s^{a,M}_{ic,t-1}$ is the share of imports of firm i in year $t-1$ of product a from country c, and $WES_{a,c,t}$ is the world export supply (excluding sales to Rwanda) of country c for product a. For firm i's suppliers, we construct the average of their import shocks, $logM^{S}_{it}$, using i's input share from each supplier in the previous year as the weights. We construct the weighted average of the trade shocks of the suppliers to the suppliers of firm i, $logM^{SS}_{it}$, using the recursive formula in (13). The 1^{st}-stage is:

$$s^{Total}_{it} = \beta_1 \cdot logM^{D}_{it} + \beta_2 \cdot logM^{S}_{it} + \beta_3 \cdot logM^{SS}_{it} + \kappa_t + \kappa_i + \epsilon_{it} \tag{16}$$

and the 2^{nd}-stage is equation (14). Standard errors are clustered at the firm-level.

In column (5), we find that an increase in the firm's trade exposure, when instrumented by the import shocks, causes an increase in the effective corporate tax rate ETR_i^K. The instruments are relevant, with a 1^{st}-stage Kleibergen-Paap F-statistic of 18.17. In additional regressions (not shown but available), we find that the results are robust to controlling for trade shocks to firm i's potential suppliers (firms that operate in the same industry and geographical area as i's current suppliers but are not currently supplying to i) and firm i's horizontal suppliers (firms that are suppliers to firm i's current clients).

Trade may positively impact ETR^K through its effect on size and we investigate this in Panels B and C of Table 4. In Panel B, we find, across the various specifications including the IV, that more exposure to international trade increases a firm's size. We proxy for size with annual revenue. Panel C reveals a positive within-firm OLS association between a firm's size and its effective corporate tax rate in the different specifications, though we cannot employ the IV due to the exclusion restriction.

We focus on firms' exposure to imports through their supply network, but firms may also be impacted by imports through their clients. In an extension, we find that increased output exposure to imports through the client network has positive effects on ETR^K (results available), though this average effect could mask heterogeneity across firms depending on the complementarity between imports and domestic inputs.

Because the estimation is within the corporate sector, this exercise cannot speak to the magnitude of trade's net impact on sector-level $\overline{ETR}_C^K$. We also found impacts of trade on tax policies at the corporate sector level (statutory CIT rate). These firm-level results on corporate ETR_i^K are therefore complementary to the country-level results on $\overline{ETR}_C^K$. An additional limitation is that the network linkage measures are derived from administrative data which, by construction, only exist for tax registered firms (Atkin & Khandelwal, 2020). This sample restriction implies that this firm-level regression is not suited to study the impacts of trade on the size of informal firms.

E.2 Type of trade analysis

We investigate whether trade has differential impacts on ETR and mechanism outcomes depending on the nature of the trade variation (Section 6.4). We use our two instruments to investigate the impacts of: (i) imports versus exports (of trade in both intermediate G-S and final G-S); (ii) trade in intermediate G-S versus final G-S (summed across imports and exports). We use UN's Broad Economic Categories (Rev. 5) to classify final versus intermediate goods-services (G-S), combining capital goods with the latter. For the imports versus exports IV, the two 1^{st}-stage regressions are

$$log(imp_{ct}) = \beta_1 \cdot Z_{ct}^{gravity} + \beta_2 \cdot Z_{ct}^{oil-dist} + \mu_c + \mu_t + \epsilon_{ct}$$
$$log(exp_{ct}) = \pi_1 \cdot Z_{ct}^{gravity} + \pi_2 \cdot Z_{ct}^{oil-dist} + \eta_c + \eta_t + \iota_{ct}$$

where $log(imp_{ct})$ and $log(exp_{ct})$ are the logs of total imports to NDP and total exports to NDP, respectively, in country c in year t. The log-transformation improves the 1^{st}-stage (results without logs are qualitatively similar). The 2^{nd}-stage is

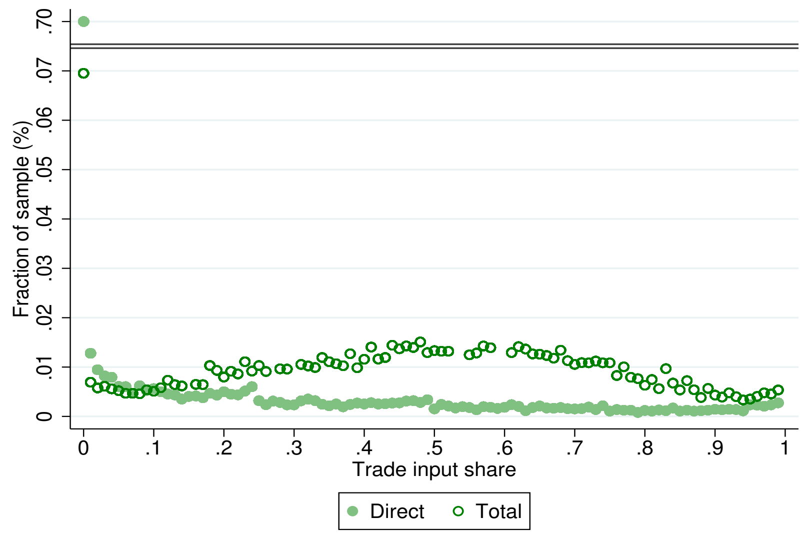
$$y_{ct} = \theta_1 \cdot log(imp_{ct}) + \theta_2 \cdot log(exp_{ct}) + \kappa_c + \kappa_t + \phi_{ct}$$

The set-up is similar for the second IV (intermediate G-S vs final G-S) where we replace $log(imp_{ct})$ and $log(exp_{ct})$ with the log of total trade in intermediate G-S to NDP and the log of total trade in final G-S to NDP. IV results for developing countries are in Panel A of Table E1, with 1^{st}-stage regressions in Panel B. Two comments are in order. First, the two IVs could in theory impact the different types of trade (Bergstrand and Egger, 2010). In practice, $Z^{gravity}$ significantly predicts all types of trade, while $Z^{oil-dist}$ significantly predicts imports and final G-S but not exports or intermediate G-S (Panel B). It is unclear if the instruments generate a strong overall first-stage. We gauge this by inspecting the Kleibergen-Paap F-statistics, which are not well above conventional threshold levels (10.18 and 7.39). Given this challenge, we limit our scope to studying whether the coefficient signs for the different types of trade are consistent with our simplified predictions (and whether they statistically differ from each other). Second, the exclusion restriction requires that the regressors add up to total trade openness. For this reason, we cannot implement an IV which focuses on the impacts of final versus intermediate G-S for, say, imports only. This also implies that, for a given outcome, the hypotheses in our two IVs (final versus intermediate G-S; imports versus exports) will be correlated. We accordingly adjust the p-values for multiple hypotheses testing using the Romano-Wolf method.

Focusing on the IV results, col. (1) shows exports increase ETR_K while imports decrease it; col. (2) shows trade in intermediate G-S increases ETR_K while trade in final G-S decreases it. In each IV, the coefficients imply a positive overall effect of trade openness on ETR_K even if the two trade-types had equal shares of NDP. In practice, many LMICs run trade surpluses (UNCTAD, 2014) and trade more in intermediate G-S than final G-S (Miroudot, Lanz and Ragoussis, 2009). We can statistically reject that the different trade-types have the same impact on ETR_K, at 10% for exports vs imports and at 1% for intermediate G-S vs final G-S. Similar patterns hold for ETR_L (cols. 3 and 4). The remaining columns focus on mechanism outcomes. Exports cause an increase in the corporate income share (μ_C in equation 6), while imports decrease it. Trade in intermediate G-S increases μ_C while trade in final G-S decreases it. Results are similar for the average corporate effective tax rate ($\overline{ETR}_C^K$ in equation 6).

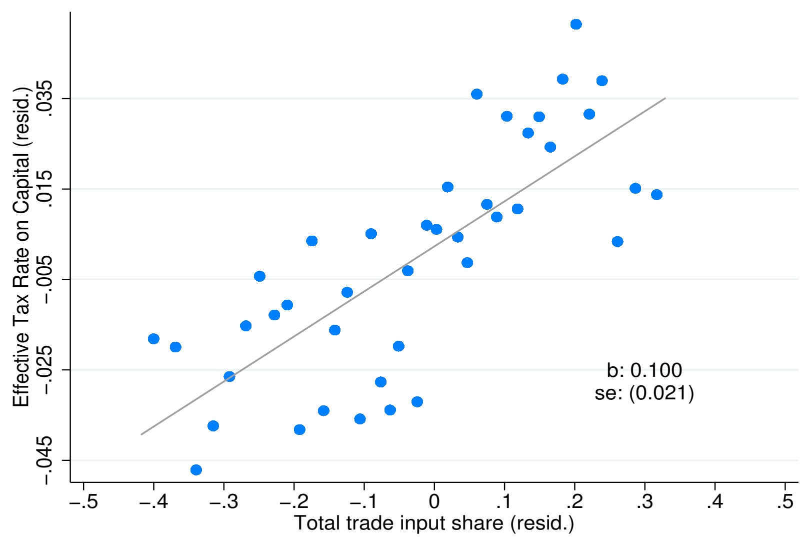
Since we only have 2 instruments, we cannot decisively conclude on the impacts for the 4 types of trade (imports of intermediate G-S, exports of intermediate G-S, imports of final G-S, exports of final G-S). Notwithstanding, the signs of the four estimated IV coefficients are consistent with imports of final G-S decreasing ETR_K and mechanism outcomes (μ_C,$\overline{ETR}_C^K$), and imports of intermediate G-S increasing them.

Figure E1: Rwandan Firms' Direct and Total Exposure to Trade in Imports



Notes: This figure shows the distribution of direct foreign input share, s_{Fi}, and total foreign input share, s_i^{Total}, for all corporate firms in Rwanda between 2015 and 2017. The measures are calculated annually, and the figure pools all firm-year observations. The horizontal line represent a scale break in the vertical axis. More details in Section E.1.

Figure E2: Rwandan Firms' Trade Exposure and Corporate Effective Tax Rate



Notes: This figure shows the firm-level association between total foreign input share, s_i^{Total}, and the corporate effective tax rate for all corporate firms in Rwanda between 2015 and 2017. The graph plots binned scatters of the variables against each other, after residualizing both variables against year fixed effects. The dots correspond to equal sized bins of the residualized trade exposure variable. The line corresponds to the best linear fit regression on the underlying firm-level data ($N = 18478$), which is also reported in column (1) of Table 4.

Table E1: Type of Trade Analysis in Developing Countries

Panel A: IV	ETR_K		ETR_L		Corporate Income		Mixed Income		Corporate ETR_K	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Export of G-S	0.406		0.184**		0.374**		-0.227*		0.475	
	(0.258)		(0.092)		(0.181)		(0.136)		(0.287)	
	[0.079]		[0.019]		[0.092]		[0.119]		[0.053]	
Import of G-S	-0.295*		-0.153***		-0.265**		0.136		-0.345**	
	(0.151)		(0.049)		(0.108)		(0.089)		(0.149)	
	[0.075]		[0.008]		[0.097]		[0.125]		[0.051]	
Intermediate G-S		0.270***		0.115***		0.252***		-0.162***		0.316***
		(0.100)		(0.042)		(0.072)		(0.060)		(0.101)
		[0.039]		[0.013]		[0.046]		[0.033]		[0.033]
Final G-S		-0.204***		-0.105***		-0.185***		0.096**		-0.239***
		(0.065)		(0.026)		(0.049)		(0.046)		(0.050)
		[0.037]		[0.006]		[0.019]		[0.119]		[0.006]
F-test: Equality of coefficients [p-value]	2.99 [0.086]	8.45 [0.004]	5.75 [0.018]	10.88 [0.001]	5.01 [0.027]	13.49 [0.000]	2.68 [0.104]	6.08 [0.015]	3.59 [0.060]	13.77 [0.000]
N	4572	4572	4572	4572	4572	4572	4572	4572	4572	4572

Panel B: 1st-stage	Import of G-S	Export of G-S	Intermediate G-S	Final G-S
	(1)	(2)	(3)	(4)
$Z^{gravity}$	0.277***	0.248***	0.274***	0.269***
	(0.037)	(0.058)	(0.035)	(0.055)
$Z^{oil-distance}$	-0.085***	0.013	0.019	-0.121***
	(0.014)	(0.019)	(0.013)	(0.023)
1st-stage F-statistic	131.83	21.29	65.03	82.09
1st-stage Sanderson-Windmeijer Weak Instrument F-statistic	35.70	33.25	51.78	55.50
1st-stage Kleibergen-Papp F statistic	7.39		10.18	
N	4572	4572	4572	4572

Notes: The sample is developing countries, which are low and middle-income countries according to the World Bank income classification in 2018. Panel A presents IV results, while Panel B presents 1^{st}-stage results. In Panel A's odd-numbered columns, imports and exports are the regressors while in even-numbered columns it is trade in intermediate goods and services (G-S) and trade in final G-S. Outcomes differ across columns in Panel A: in cols. (1)-(2), effective tax rate on capital, ETR_K; in cols. (3)-(4), effective tax rate on labor, ETR_L; in cols. (5)-(6), corporate income share of net domestic product; in cols. (7)-(8), mixed income share of net domestic product; in cols. (9)-(10), average effective tax rate on corporate profits. For details on the outcomes and the instruments, see Table 1 and 3. Relative to those tables, the drop in sample size in this table is due to availability of the type of trade classification. For each coefficient, we report in brackets the p-values which correct for multiple hypotheses testing, using the Romano-Wolf method. Multiple hypothesis testing is accounted for within each outcome between the two IV estimations (exports and imports; final G-S and intermediate G-S). At the bottom of each column in Panel A, we report the F-test for the equality of coefficients. In Panel B, cols. (1)-(2) correspond to the first-stage regression that instruments simultaneously for imports and exports; cols. (3)-(4) is the first-stage regression which instruments simultaneously for intermediate G-S and final G-S. In Panel B, we report the F-statistic of excluded instruments; the Sanderson-Windmeer multivariate F-test of excluded instruments; and, the Kleibergen-Paap F-statistic. * p<0.10 ** p<0.05 *** p<0.01. Standard errors in parentheses are clustered at the country level. For more details, see Section E.2.

Appendix F Capital Liberalization Events

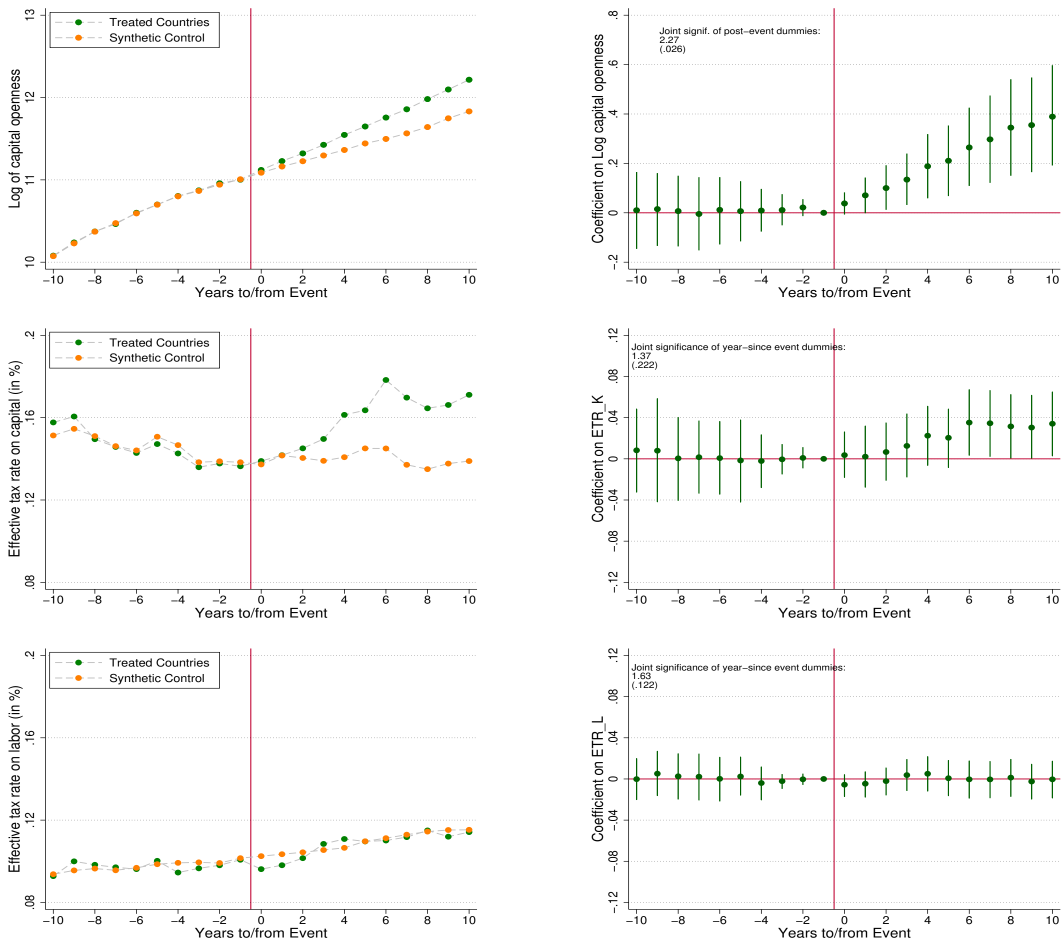
To attempt to investigate the impact of capital liberalization on effective tax rates, we draw on Chari et al. (2012). The authors measure capital liberalization events in 25 developing countries as the date when foreign investment in the domestic stock market was first allowed. They show that these events significantly increase foreign capital inflows, including foreign direct investment (FDI) and import of capital goods.[76] Compared to other policies aimed at lifting FDI restrictions, liberalizing the domestic stock market occurs at a precise point in time, is not marked by policy-reversal or net capital outflow, and is unambiguously related to capital liberalization (Eichengreen, 2001). We employ the empirical design of Section 5.1 and create a synthetic control country for each of the 25 treated countries and for each outcome. We measure capital openness as the total sum of the stocks of foreign assets and liabilities (Gygli et al., 2019). We find similar results when using alternative measures of capital openness, including portfolio equity assets and liabilities and the KOF financial globalization index (Gygli et al., 2019).

Figure F1 reports the event-study results. Relative to a stable pre-trend, we observe a sustained rise in capital openness precisely at the time of the event. ETR_K also increases, with a small lag to the timing of the capital liberalization event; in the medium-run, the positive effect on ETR_K is significant at the 5% level. There is no discernible effect on ETR_L. Similar to the reasoning for the trade tax-capacity mechanism, the inflow of foreign capital, as well as any subsequent increase in capital goods imports and aggregate investment, may positively impact ETR_K by contributing to general growth of firms or by causing an expansion of initially larger firms. Consistent with this interpretation, we find that the capital liberalization events led to increases in the corporate output-share and the average corporate effective tax rate (results not shown but available).

One important limitation is that the events considered here remove restrictions on capital *inflows* and are not informative of the impacts of increased capital *outflows*. In general, more work is needed to understand the determinants of policies which impact cross-border capital flows in developing countries and their effects on ETRs.

[76] FDI includes green field investments (building plants from scratch) and cross-border mergers and acquisitions (M&A). Chari et al. (2012) note that M&A is impacted by stock market liberalization, makes up to 40-60% of FDI in developing countries, and can trigger subsequent green field investments.

Figure F1: Event Study of Capital Liberalization Reforms



Notes: These panels show event-studies for capital liberalization reforms in the 25 developing countries of Chari, Henry, and Sasson (2012). The panels correspond to different outcomes: capital openness (top panels); effective tax rate on capital (middle panels); effective tax rate on labor (bottom panels). Capital openness is the total sum of the stocks of foreign assets and liabilities, in constant USD. We use the log transformation for this outcome; results where the total sum is expressed as a percent of GDP are similar. The left-hand graphs show the average level of the outcome in every year to/since the event, for treated countries and for synthetic control countries. The right-hand graphs show the estimated β_e coefficients on the to/since dummies, based on equation (4) but where the trade liberalization events are replaced with capital liberalization events. The bars represent the 95% confidence intervals. Standard errors are clustered at the country level and estimated with the wild bootstrap method. The top-left corners report the F-statistic on joint significance of the post-event dummies, with the p-value in parentheses. Details are in Appendix F.