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Taxing top incomes in the emerging world

Economic impact under the microscope

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Abstract: Rising levels of income inequality and tight government budgets have spurred discussions in many developing nations about how to appropriately tax high-income earners. In this paper, we study taxpayer responses to an increase in the top marginal tax rate in South Africa, drawing on exceptionally rich tax administrative data and a transparent empirical identification design. We establish that treated taxpayers strongly reduce their reported taxable income in response to the tax reform. Taxpayers' responses are driven by both reductions in broad income and increases in tax deductions. While regular labour earnings remain unaffected, we find a marked drop in non-monetary wage components and annual incentive and bonus payments. Linking individual to corporate tax returns, we show that part of the observed response reflects adjustments in real economic activity: South African firms, which employ treated workers, experience a decline in output after the reform.

Key words: income inequality, taxable income, high-income earners, South Africa, tax reform

JEL classification: H24, H31

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1 Introduction

Over recent years, rising levels of income inequality have ignited recurring policy discussions about the appropriate taxation of high-income earners across the developing world. High tax rates on the rich may counteract the increasing levels of pre-tax income inequality in many developing countries and, simultaneously, hold the promise to raise countries' tax revenue collection—thus contributing to overcoming low tax-to-GDP ratios and low levels of public good provision in many developing nations (Besley and Persson 2013). Opponents of increased progressivity, in turn, express concerns about the potential behavioural responses to high tax levies by the rich: high-income earners may hide income from tax authorities—which may be difficult to contain in environments with relatively weak tax administrative capacity. Marginal tax increases may, moreover, lower labour supply and effort provision, triggering economic repercussions that may be particularly detrimental in the developing world, where high-skilled labour is scarce.[1]

While being a key input into public and policy debates, there is to date little evidence on how high-income earners in less developed countries respond to income taxation. In this paper, we contribute to closing this research gap. Our testing ground is South Africa, where the South African government, in 2017, raised the personal income tax (PIT) rate on top incomes from 41% to 45%. The reform affected the very top of the income distribution—the top 0.6% of earners in the country. In this paper, we show that affected taxpayers responded strongly to the reform. Their taxable income sharply declined, despite a broad PIT tax base definition and careful crafting of the reform by legislators, who largely avoided opportunities and incentives for income shifting across time and across tax bases.[2]

We draw on exceptionally rich tax administrative data on the universe of South African taxpayers to study high-income earners' response to this PIT reform (National Treasury and UNU-WIDER 2023). Granular information on income sources allows us to put response behaviour under the microscope: we precisely identify not only the size of behavioural adjustments, but also their nature—both of which are decisive for social welfare and for policy design (Chetty 2009; Piketty et al. 2014; Slemrod 1995). We study adjustments in labour, investment, and business income, as well as in non-monetary income components and tax deductions. Information on labour income is drawn from pay as you earn (PAYE) data and is further decomposed into standard monthly earnings, annual bonuses, and incentive payments, as well as allowances and fringe benefits. In additional analyses, we link the PIT data to corporate income tax returns (National Treasury and UNU-WIDER 2021), which allows us to test for the real economic consequences of the reform on the output of firms with workers who are affected by it.

Empirical identification draws on a transparent identification strategy that compares changes in the reported taxable income (and its components) between treated top income earners and untreated taxpayers in lower tax brackets in a modified difference-in-differences (DiD) design. Mean reversion and secular trends imply that income trends may vary across the income distribution, violating the common-trend assumption in standard DiD-settings. We follow Jakobsen and Søgaard (2022) and address this challenge by using information on pre-reform income trends to granularly model trend differences across the income distribution. Assuming that these trend differences remain constant over time allows us to identify the tax reform effect (Jakobsen and Søgaard 2022). We validate the 'common trend differential' assumption in two ways: first, we show that trend differentials remained constant in untreated 'validation regions' of the income distribution throughout our sample frame; and second, we show that trend

[1] Real responses to tax increases may also be particularly large in the developing world, where public sectors are plagued by corruption and inefficiencies in public good and service provision, potentially undermining taxpayers' willingness to contribute funds to the state and to exert effort.

[2] Evidence suggests that a narrow tax base definition increases opportunities for tax evasion through over-reporting of tax expenditures (see e.g. Bachas and Soto 2021; Kopczuk 2005).

differentials remained constant across the full income distribution—including taxpayers in the treated region—in the pre-reform period.

Our estimates suggest that treated taxpayers strongly reduced their reported taxable income in response to the increase in the top tax rate. The estimated medium-run elasticity of taxable income (three years after treatment) is 1.2. In line with adjustment frictions, estimated short-run elasticities are smaller. We rule out that our estimates are in any meaningful way driven by anticipation effects, and we show that the estimates are robust to various sensitivity checks, including changes in the pre-treatment period used to model income trend differentials across the income distribution and defining treatment based on deeper lags of pre-reform income (Weber 2014). The effects are, moreover, found to be centred around the intensive margin: our estimates reject reform effects on the propensity of treated taxpayers to leave the PIT base.

In additional analyses, we assess the nature of the income response. We find marked responses in both broad income components and tax deductions. As the South African PIT system allows for few deductions, the increase in tax deductions contributes relatively little to the overall taxpayer response. Similar to prior evidence, we do not find significant adjustments in monthly 'standard' labour income. Other wage components respond sensitively: we observe declines in bonus and incentive pay as well as non-monetary remuneration components (such as cars, laptops, travel, mobile phones, or equity vesting)—both of which make up a significant fraction of top earners' compensation packages. Treated taxpayers, moreover, strongly reduce their reported investment income. Less prevalent business income responds more weakly to the reform (potentially reflecting high baseline evasion in that income domain).

In principle, all of these adjustments may reflect avoidance and evasion responses, as well as real changes in taxpayer behaviour. The literature commonly assigns changes in tax deductions and self-reported income components to reporting behaviour (e.g., Kleven 2016; Neisser 2021; Saez et al. 2012), while adjustments in third-party reported income are interpreted as real taxpayer responses. In the weaker institutional context of less developed countries, this distinction may be more blurry. Observed reductions in third-party reported incentive and bonus pay and non-monetary wage income may, for example, reflect that employees and their employers collude and shift compensation to non-taxable components of the remuneration package. Or they may engage in outright tax evasion by under-reporting fringe benefits—for example, the extent to which employees privately use cars or laptops—or annual incentive or bonus pay.[3]

Changes in real behaviour may accrue as well: leading employees, who are treated by the PIT reform, may have less incentive to exert effort and reach performance goals after the intervention, resulting in lower remuneration—which may be centred around non-standard wage components that may be less affected by downward wage rigidities. The latter effect has been documented for the developed world, but we are not aware of evidence for less developed countries (see Akcigit et al. 2022; Arnemann et al. 2023; Mertens and Montiel Olea 2018).[4]

Testing for real adjustments is particularly relevant in the developing world, where high-skilled labour and management resources tend to be scarce (see Barro and Lee 2013; Bloom et al. 2013; Hjort et al.

[3] Owner-managers, to the extent that they expect marginal PIT rates to drop again in the future, may also have incentives to keep income within the firm and distribute it through wage payments at a later point in time. Note, however, that owner-managers did not have incentives to distribute income in the form of dividends rather than as wages, as South African legislators, simultaneous to the PIT rate increase, raised the tax on dividend payouts. See Section 2 for details on the institutional background.

[4] A broad literature provides evidence consistent with firms shying away from nominal wage cuts to avoid reciprocal reductions in workers' effort provision (see Elsby and Solon (2019) for a literature review). Incentive pay is directly linked to performance and non-monetary wage components may be less salient and may therefore also be less downward-rigid than other, more standard wage components; they may thus be the first elements in workers' wage compensation package to be downward-adjusted when labour supply and performance is constrained.

2022) and reductions in labour supply and effort provision by key personnel may have significant negative economic repercussions. To shed light on possible real responses, we link our PIT return data to firm-level information and test for output reductions in treated firms—that is, firms with employees affected by the reform.[5]

Our evidence indeed points to real adjustments: while the sales of firms with and without treated employees emerged in parallel prior to the reform, there is a gradual decline in the output of treated firms relative to control firms after treatment, with the average output drop amounting to 4.6%. The effect emerges across firms in different size classes and is robust to granularly absorbing industry-specific and firm-size-specific trends in the dependent variable. A number of robustness checks further corroborates the interpretation of a real response in firm behaviour.

Our findings offer important policy insights. The South African government pursued two goals with the 2017 increase in the top marginal tax rate: it aimed (1) to increase PIT revenue collection; and (2) to lower the country's high level of after-tax income inequality. Our estimates suggest that, if at all, the goals were achieved at high efficiency costs: treated taxpayers sharply reduced their taxable income in response to the reform (in part reflecting real adjustments), rendering reform-related efficiency costs high. Our estimates place the new top tax rate on the wrong side of the Laffer curve—PIT revenue collection is estimated to have declined in the wake of the reform. Reported after-tax inequality, in turn, decreased. As *true* income likely did not drop at the same rate as *reported* income, the estimates reported in this paper are an upper bound of the effect on the country's *true* after-tax income inequality, however.[6]

Our paper contributes to an emerging literature that uses rich tax administrative data to assess taxpayers' behavioural responses to income taxation. There is a significant body of research on the estimation of the elasticity of taxable income (ETI), whose size determines the deadweight loss of taxation (Chetty 2009; Feldstein 1999) and shapes the equity–efficiency trade-off in optimal tax policy design (Saez 2001; Saez et al. 2012). Existing ETI estimates are largely set in the developed world, however (see e.g. Feldstein (1999), Gruber and Saez (2002), and Weber (2014) for the United States; Kleven and Schultz (2014) for Denmark; Miao et al. (2022) for Sweden; Doerrenberg et al. (2017) and Werdt (2015) for Germany; and Neisser (2021) and Saez et al. (2012) for recent literature reviews).

Evidence on less developed economies is scarce (Neisser 2021).[7] Given the pronounced institutional and economic differences between developed and less developed countries, existing work may not be externally valid for the developing world.

We also add to the literature by being the first to offer a granular picture of *high-income* taxpayers' response to increased marginal tax rates in a less developed country context. Understanding the tax responsiveness of high-income earners in the developing world is of particular interest as increasing income and wealth inequality have renewed policy and public interest in reforms that modify the marginal PIT rate schedule at the upper end of the income distribution (see Auten et al. 2016; Saez 2004; Saez et al. 2012). Our study offers granular evidence on the size and nature of high-income earners' tax re-

[5] As laid out in the paper, firms do not have an increased incentive to under-report sales.

[6] Note that several of the observed adjustments—including changes in tax deductions and investment income—likely, at least in part, reflect changes in evasion and avoidance behaviour. This type of response implies that individuals' true income levels may have been affected less than elasticities related to reported income suggest. Also note, however, that income under-reporting may involve monetary costs (e.g. for tax advisor services) or psychological costs from acting against the law and social norms. Therefore, even if 'true' after-tax income inequality did not change much, inequality of consumption or utility may have declined after the reform.

[7] The review paper by Neisser (2021) cites only two studies, which rely on testing grounds outside the developed world. Other studies for less developed countries include those by He et al. (2021), Kemp (2019), Kleven and Waseem (2013), Pillay (2021), and Tortarolo et al. (2020).

sponse. Among others, we are—to the best of our knowledge—the first to link individual- and firm-level data to show that increases in top income tax rates can reduce real economic activity (see e.g. Arnemann et al. (2023) for evidence from the developed world and Mertens and Montiel Olea (2018)).[8] Only a few papers provide estimates for the ETI of high-income earners outside of Europe and North America. One exception is Bergolo et al. (2022), who show that an increase in top labour income taxes led to a reduction in labour income of treated high-income earners in Uruguay—a country which is economically and institutionally significantly more developed than South Africa, however.[9] Another closely related study is that of Jouste et al. (2023), who investigate a top tax increase in Uganda, finding little indication for an income response by treated taxpayers (which contrasts much of the prior evidence for the developed world—see Neisser 2021). We rely on richer data—with a panel dimension and more granular income information—allowing for a more transparent identification design and for a more comprehensive empirical analysis. Next to country idiosyncratics, this may explain the difference in findings. Other existing evidence for the developing world focuses on behavioural responses of taxpayers in lower ranges of the income distribution (including recent work by Kemp (2019) and Pillay (2021) on South Africa).[10]

As sketched above, empirically identifying the elasticity of taxable income, moreover, involves the non-trivial challenge of adequately absorbing underlying income trends in the analysis. We rely on quasi-experimental variation in the top marginal tax rate and a transparent empirical identification approach that allows testing for underlying empirical identification assumptions (Jakobsen and Søgaard 2022). We consider this to be a plus relative to prior work, which is based on less granular and less transparent approaches and has produced a rather wide range of estimates. Specifically, much of the literature has pursued one of two approaches: DiD or bunching. DiD estimates struggle with absorbing underlying income trends and have been found to be sensitive to specification choices (see Neisser 2021).[11] Bunching estimates are often small, due to adjustment frictions on the side of taxpayers (see Kleven 2016).[12]

Last but not least, we consider our empirical testing ground to be of particular interest not only because South Africa is the largest economy on the African continent, but also because the South African government took care in avoiding behavioural responses in the design of the reform: the PIT base is broad, incentives to shift income from the PIT to the corporate income tax (CIT) system were neutralized by reform design, and there was little scope to avoid the increased tax rate by income shifting across time. Still, we find a quite substantial response in taxable income reporting to the reform, which contrasts with common wisdom that the ETI is small with well-designed tax systems and tax reforms (e.g. Saez et al.

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[8] The prior evidence for the GDP effect of top marginal tax rate changes in the United States is mixed: while Zidar (2019) rejects a major effect of top income taxes on GDP growth or employment, Mertens and Montiel Olea (2018) find that lowering top marginal income taxes does exert GDP and employment effects. More loosely related, Akcigit et al. (2022) show that individual inventors adjust their activity in response to US state income taxes.

[9] Uruguay is classified as a high-income country under the World Bank classification. In 2021, the World Bank logged the GDP per capita at around US$7,000 in South Africa and around US$18,000 in Uruguay. Another more loosely related paper by Londoño-Vélez and Ávila Mahecha (2021) uses Colombian tax data and information from the Panama Papers to study the impact of taxation on taxable wealth.

[10] The latter evidence may lack external validity for the behaviour of taxpayers at the upper end of the income distribution. Consistent with theoretical considerations, prior evidence for the developed world suggests that high-income earners can draw on more options to adjust their taxable income, translating into higher tax responsiveness (see e.g. Saez et al. 2012; Neisser 2021).

[11] Kemp (2020) presents tax elasticity estimates based on bracket creep for South Africa. These DiD estimates are subject to the challenges discussed in the main text. Bracket creep is also a rather non-salient phenomenon, which may dampen taxpayers' behavioural adjustment.

[12] This is particularly true in labour supply contexts. A range of optimization frictions may attenuate bunching and are difficult to observe and model. These frictions include aspects such as hours constraints, search costs, inattention, and uncertainty. Any evidence of sharp bunching in earnings likely results from tax evasion or tax avoidance rather than real labour supply responses (see Kleven 2016).

2012). We show that this does not necessarily hold true in developing country contexts and at the upper end of the income distribution.

The rest of the paper is structured as follows. Section 2 discusses the institutional background; Sections 3 and 4 describe our data and estimation strategy. The results are presented in Sections 5 and 6. Section 7 concludes.

2 Institutional background

2.1 Background

Our empirical testing ground is South Africa. The country is an upper middle-income economy with a GDP per capita of around US$7,000 in 2023. Its tax-to-GDP ratio exceeds that of other less developed countries, but still falls short of developed country levels. In 2020, South Africa's tax-to-GDP-ratio was 25.2% (relative to an average of 16% on the African continent and an average of 33.5% in the OECD).[13] PIT contributes 36% to total tax revenue collection in South Africa, rendering it the largest government revenue source (followed by value-added tax (VAT), which contribute 24% (OECD 2022b)). Compared to other countries, South Africa relies relatively heavily on the PIT for revenue collection (Inchause et al. 2015). Fiscal capacity in South Africa is higher than in other countries on the African continent, but the country still struggles with common institutional challenges such as corruption and state capture, including in the tax administrative domain (see Nugent 2018).

South Africa has, moreover, experienced stagnant economic development over the last decade. It has run deficits in government budgets since the financial crisis and the public debt-to-GDP ratio has risen from 27% in 2009 to 69% in 2021 (see IMF 2023). The South African society is one of the most unequal on the globe (Leibbrandt et al. 2018).[14] The Gini coefficient is 0.69; the top 20% of the population earn more than 60% of aggregate income (compared to a median of 47% for similar emerging markets). The bottom 40% of the population, in turn, receive only 7% of income (compared to 16% for other emerging markets—cf. IMF 2020). While the extent of informal employment in South Africa is considerable (International Labour Organization 2023), the formal sector share among the top income earners—studied in our paper—is very high (Jacobs et al. 2023), close to 98%, rendering them well represented in our tax administrative data.

2.2 Personal income taxation in South Africa

Individual taxpayers in South Africa are subject to a progressive PIT specified under the Income Tax Act 58 of 1962. Taxation is residence-based and income is filed individually (Maboshe and Woolard 2018). Tax years run from the beginning of March to the end of February in the following year. We will, in the following, refer to the tax year from March in year t (e.g. March 2015) to February in year $t+1$ (e.g. February 2016) as the tax year $t+1$ (e.g. 2016).

The calculation of taxable income in the South African PIT system is generally described as simple. There is no household taxation—that is, there are no provisions for joint taxation of couples and no deductions for children (Maboshe and Woolard 2018). The number of tax expenditures is small. Pen-

[13] Information on GDP per capita was obtained from the World Bank; information on tax-to-GDP ratios are taken from OECD statistics.

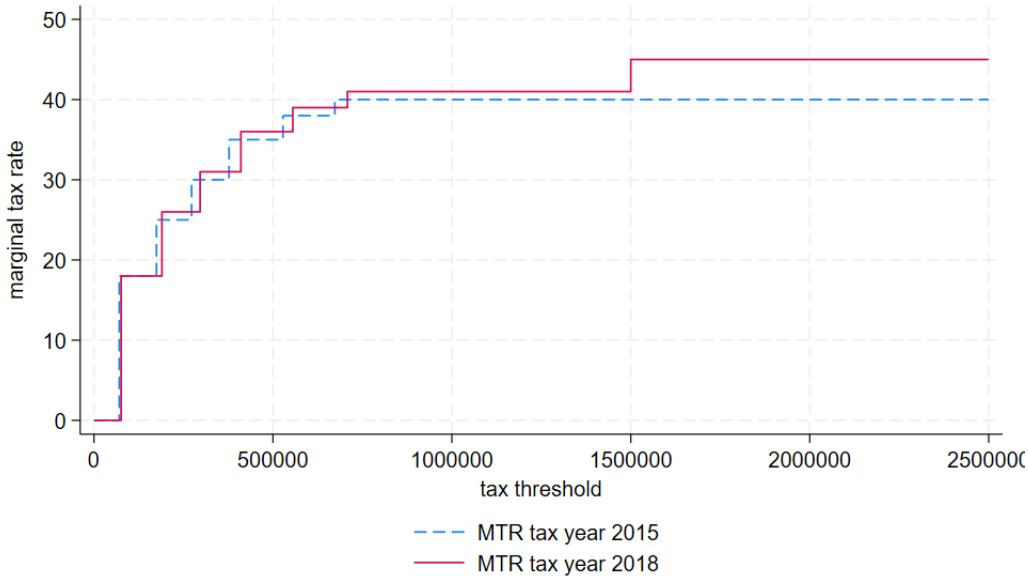
[14] Despite democratization and several reforms after the end of the apartheid regime, South Africa's income inequality has remained 'stubbornly' high (Bhorat et al. 2009; Leibbrandt et al. 2010). This contrasts with many other emerging markets, which have managed to reduce their level of income inequality since the 2000s.

sion fund contributions are deductible and there is a tax credit for medical expenditures. A number of smaller deduction items are negligible in terms of uptake and size. Importantly, the South African PIT system does not account for many internationally common deductions such as child-related tax deductions, student loan interest deductions or mortgage interest deductions, household service tax incentives, commuter tax allowances, certain educational expenses, or other labour-related expenses, which have in part been associated with tax avoidance and evasion behaviour (see e.g. Harju et al. 2021; Paetzold and Winner 2016). See Appendix A for more details on the calculation of PIT revenues.

Both labour and capital income are subject to PIT in South Africa. This includes labour income from dependent employment, self-employment income, income earned by non-incorporated businesses, as well as interest income and capital gains.[15] Dividend income is subject to a flat tax that is withheld by the dividend-paying firms and directly transferred to the South African Revenue Service (SARS).

Figure 1 depicts the marginal tax rate schedule of the South African PIT system in the tax year 2015 (prior to the increase in the top marginal income tax rate) and the tax year 2018 (after the increase in the top marginal income tax rate). In 2015, income below R70,700 was exempted from taxation;[16] higher incomes were subject to increasing marginal tax rates, with six income tax brackets ranging from 18% to 40% in 2015 (SARS 2023a). The top marginal tax rate was levied on incomes from R673,101 upwards (US$40,972[17]). Income tax brackets, rebates, and thresholds are shifted upwards on a regular basis, to (partially) adjust for inflation and bracket creep (e.g. National Treasury 2017a). The next section describes reforms to the PIT schedule after 2015.

Figure 1: Marginal tax rate schedule



Note: the figure shows the marginal tax rate schedule for the pre-reform tax year 2015 (March 2014–February 2015) (dashed blue line) and the post-reform tax year 2018 (March 2017–February 2018) (red line). The tax threshold is given in South African rand (R1.5 million corresponds to approx. US$91,000).

Source: authors' compilation based on data from SARS (2023b).

[15]Note that only 40% of net capital gains realized are included in taxable income and taxed according to the PIT rate of the respective tax bracket. The maximum effective tax rate on capital gains is 18% and individuals are entitled to certain annual exclusions (PWC 2023b). In addition, interest income from a South African source earned by any natural person is exempt up to a certain threshold. In the tax year 2020, the threshold was R23,800 for a person younger than 65 and R34,500 for a person 65 or older (SARS 2023a).

[16]This applies for taxpayers under the age of 65. Older taxpayers benefit from higher levels of tax-exempt income.

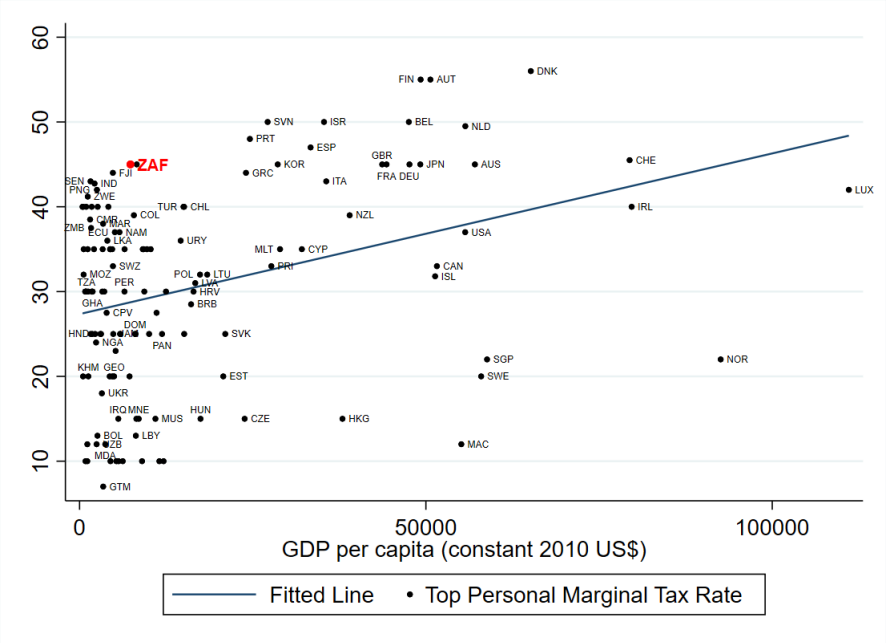
[17]We apply the average exchange rate of 2020 throughout this paper: 0.06087 ZAR/US$ (exchange rates.org 2024).

2.3 Tax policy reform

In this paper, we study the effect of an increase in the top marginal tax rate on taxpayer behaviour. The tax rate change was announced in the finance minister's budget speech on 22 February 2017, and came into effect for the following tax year starting on 1 March 2017. It introduced a new top marginal tax rate of 45%, levied on incomes above R1.5 million (approx. US$91,305).

The reform came mostly as a surprise to the public. Prior to February 2017, there had been little discussion about the reform in the policy domain or within the general public (National Treasury 2017a; National Treasury 2017b). The scope for behavioural changes by taxpayers in anticipation of the reform was thus small. The government underscored that it pursued two aims with the reform: first, to increase tax revenue collection in South Africa and expand the provision of much-needed public goods and services; and second, to reduce the high level of after-tax inequality in the country. The latter goal reflects a high awareness in government policy and public debates of the staggering economic inequality in the country. Its reduction has been explicitly stated as a primary target by the South African government for many years, as laid out in the National Development Plan 2030. A specific goal set out in the plan is to reduce income inequality to a Gini coefficient of 0.6 in 2030 (National Development Plan 2030).

Figure 2: Top marginal tax rate and country development: international comparison



Note: the figure shows countries' top personal marginal tax rates plotted against GDP per capita (constant 2010 US$). South Africa (ZAF) is highlighted in red. The top personal marginal tax rates are collected from PWC (2023a) and GDP per capita is obtained from the World Bank.

Source: authors' illustration based on data from PWC (2023a) and the World Bank.

How does the new top marginal tax rate in South Africa compare to those of other countries at similar stages of economic development? Figure 2 draws on country-level data on economies' top marginal income tax rate and their economic development as measured by GDP per capita. The figure shows that top marginal tax rates in the PIT scheme are positively correlated with countries' economic development. The graph is closely linked to Jensen (2022), who documents a positive correlation between countries' economic development and their income tax-to-GDP ratio. He shows that economic development raises the fraction of incomes subject to third-party withholding, which, in turn, improves countries' ability to enforce income taxation. Figure 2 suggests that better enforcement extends to the upper end of the income distribution and allows economically more developed countries to levy higher taxes on high-income earners. The figure indicates that South Africa's top marginal tax rate is high, compared to other

countries at the same development stage—already before, but even more so after the marginal tax rate increase in tax year 2018.[18]

During our data frame, there were three other reforms of the PIT system, which we account for in our empirical analysis.

First, in 2016, the South African government changed pension-related tax deductions in the PIT system. The reform became effective in March 2016 and affected tax years from 2017 onwards. The aim of the reform was to simplify and harmonize the pension-related deduction system, making pension-related deductions fairer and providing better incentives for retirement saving (see Redonda and Axelson 2021 for details). In the following, we employ adjusted measures for tax deductions and taxable income, which abstract from any income and deduction components that might have been affected by the pension reform. Appendix B presents the key features of the reform and how we adjust taxpayer income to ensure that our estimates are unaffected by it.

A second potential confounding tax policy shock is a global increase in marginal income tax rates across the PIT schedule in March 2015 by 1 percentage point (with an exception of the very first income tax bracket). The first treated tax year is 2016. We will account for this reform in our empirical analysis and show that it does not materially impact our main estimates of interest.

A third reform that is of relevance for our analysis is that the South African government, in February 2017, parallel to the announcement of the increase in the top marginal PIT rate, also increased the flat tax on dividend income from 15% to 20%. The purpose of this reform was to neutralize taxpayers' incentives to shift income from the PIT to the CIT base to shield it from the increased 45% PIT rate (a behavioural response that has been documented in other countries—see, for example, De Mooij and Nicodéme 2008; Gordon and Slemrod 2000; Thoresen and Alstadsaeter 2010). No matter whether income is earned as personal or corporate income, it is subject to a tax rate around 5 percentage points higher after the reform. Individuals in the treated region thus face no incentive for (additional) income shifting across tax bases.[19] For individuals in the control region (that is, individuals who are unaffected by the top marginal PIT increase), the incentive to earn PIT income increased, in turn. We will account for this point in our empirical analysis by showing that excluding all individuals who at least once received dividends within our data frame does not change our estimates of interest.

3 Data

Our analysis draws on rich tax administrative data for South Africa. We utilize a variety of data sources for our project. The primary data source is what we refer to as the 'individual panel' (National Treasury and UNU-WIDER 2023). The data is provided by SARS and National Treasury of South Africa (see Appendix C for details). It combines the entirety of PIT returns submitted to SARS with the complete collection of PAYE payroll accounts. The latter consists of employment income records submitted by employers to SARS. PAYE tax-registered companies are required to file tax certificates for all employees earning more than R2,000 per year (around US$122) (Pieterse et al. 2018).[20] Covering the tax years

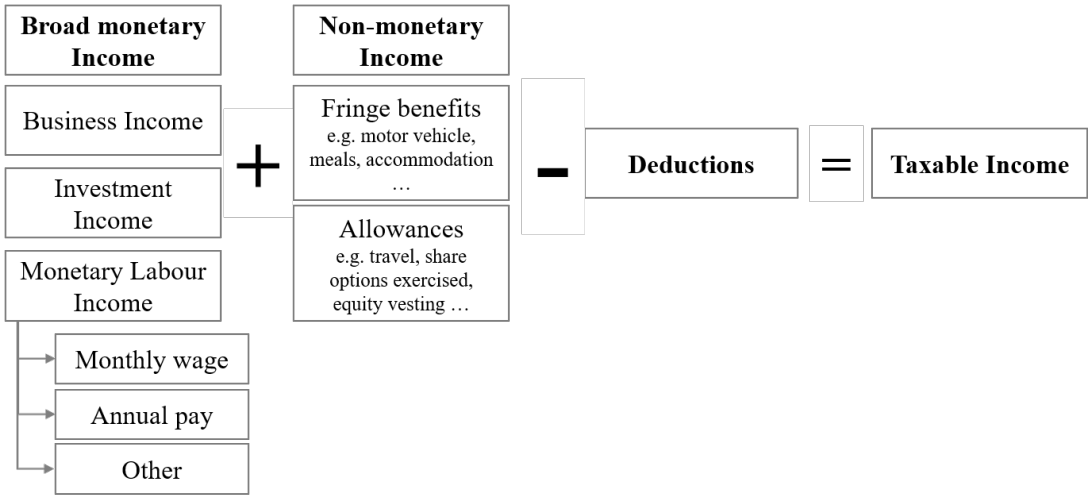
[18]As laid out in the previous section, South Africa also featured a broad PIT base definition and took great care to design the reform to minimize behavioural responses, which may have increased the 'enforceability' of the top tax increase.

[19]Individuals could, in principle, still try to shift income from wages to capital gains by keeping money in a business to then sell at a higher profit and pay the lower capital gains tax rate of 18%. We consider this type of shifting to be of second-order importance as it involves significantly higher hurdles and uncertainties than shifting between wage and dividend income.

[20]IRP5 certificates are also filed by financial services companies for clients receiving other forms of income, such as pension fund or retirement annuity income.

from 2011 to 2020, this data allows deriving the assessed taxable income and tax payments within the PIT framework.[21] Overall, the dataset encompasses information about roughly 14.9 million taxpayers annually.[22] In addition to the taxpayers' total assessed income and tax payments, the data provides an intricate breakdown of the composition of taxpayers' income, subject to PIT (see Figure 3).

Figure 3: Income categories



Note: the figure illustrates the income categories used in our analysis.

Source: authors' illustration.

This breakdown accounts for standard broad monetary income—monetary income from dependent work as well as business income and investment income (e.g. interest, royalties, or rental income). In some analyses, we further distinguish between labour income in the form of monthly wages, annual payments from incentive and bonus programmes, and commission payments. Moreover, we account for non-monetary income components, such as fringe benefits (e.g. motor vehicles or accommodation) and allowances (e.g. income associated with exercised stock options or equity vesting). The data also includes information on tax deductions in the PIT system.[23] This fine-grained data allows us to determine the anatomy of taxpayer responses to the studied PIT reform. See Table D1 for standard descriptive statistics.

In additional analyses, we link information on individuals' income reporting under the PIT scheme to the universe of CIT returns (National Treasury and UNU-WIDER 2021)—modelling firm–employee relations through the PAYE information. This allows us to shed light on whether the increase in the

[21]Specifically, we rely on data from IRP5/IT3(a) (payroll) or ITR12 (tax return) forms. Note that taxpayers are not required to fill in tax returns (ITR12 forms) if they only have employment income from one source, do not have investment income above the exempt thresholds, do not utilize additional deductions, and have an income below the compulsory submission threshold. There is also a compulsory submission threshold that has increased from R120,000 to R350,000 within our sample frame.

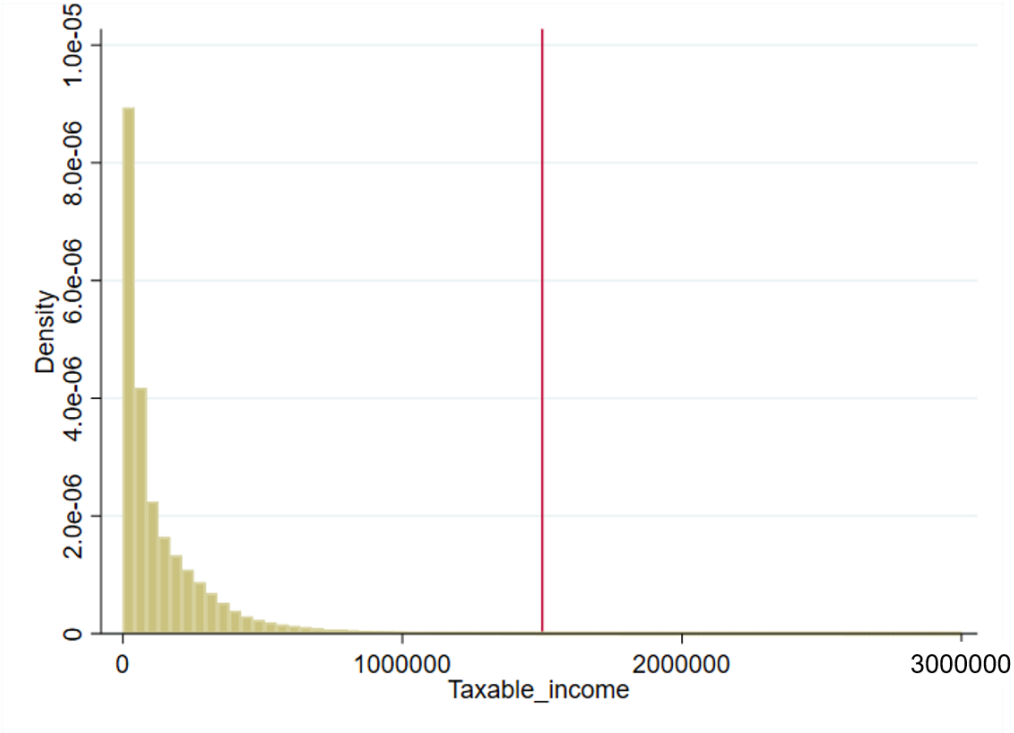
[22]There are two tax years with strong outliers in the reported income distribution, where individual taxpayers at the upper end of the income distribution reported incomes up to R7.8 trillion (approx. USD 475 billion), driving the macro aggregates. We winsorize these observations from our sample for the descriptive statistics reported in this section, and for the empirical analysis to come. To avoid any ad hoc modification of the sample, we use the highest taxable income reported in 2013—our first sample year—and winsorize all observations, where taxpayers, in real terms, in later years earned incomes higher than the top 2013 income. We apply the same procedure to the other income categories.

[23]Tax deductions tend to be low and their relative importance (as a fraction of gross income) declines across the income distribution (see Figure B1 in Appendix B).

top marginal tax rate impacted corporate performance and output, thus triggering a reduction in real economic activity in treated firms.[24]

Figures 4–6 provide a first descriptive perspective on the data. Figure 4 depicts the income distribution for tax year 2018 (the first treated tax year). The vertical line indicates the treatment threshold at R1.5 million. The figure illustrates that the reform treated only taxpayers at the very upper end of the income distribution—approx. 87,000 individuals or 0.6% of all taxpayers. Still, the tax revenue collected by the group of treated taxpayers is significant, amounting to R99.9 billion, or 23% of PIT revenue collection in 2017.[25]

Figure 4: Income distribution



Note: this figure shows the density distribution of taxable income (in rand) in tax year 2018. The vertical red line indicates the threshold for the new top tax bracket at R1.5 million.

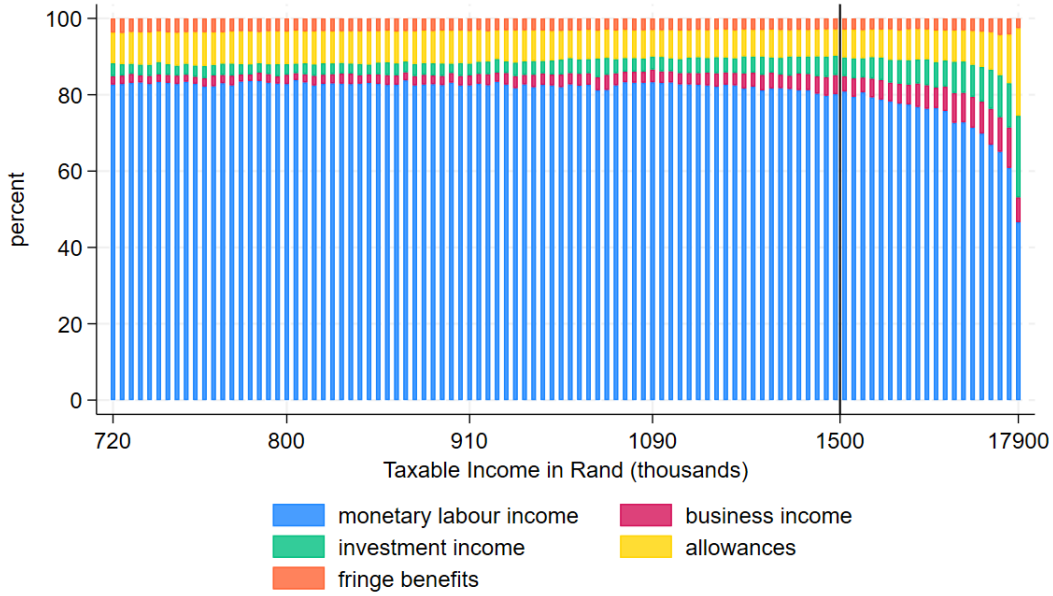
Source: authors' illustration.

Figure 5 offers a perspective on the income composition at the upper end of the income distribution. The graph zooms in on taxpayers with incomes above R700,000, which corresponds to the top marginal income tax threshold *before* the new top marginal tax rate of 45% was introduced for incomes above R1.5 million. This is also the set of taxpayers who will enter our main empirical analyses (see below for further details; Figure D2 shows an analogous graph for the full income distribution). Several aspects stand out: first, the majority of PIT income at the upper end of the income distribution is monetary labour income. This is consistent with prior findings of Leibbrandt et al. (2010), Sulla and Zikhali (2018), and Ebrahim and Axelson (2019), who show that income inequality in South Africa mostly relates to differences in labour income.

[24]While reductions in third-party reported labour income also point to real economic adjustments, we cannot fully exclude that responses along these lines do not root in collusive behaviour of employers and employees. Firms, in turn, lack reform-induced incentives to under-report sales in their public accounts.

[25]Zooming in around the threshold shows very mild evidence of bunching both before and after the reform (see Figure D1).

Figure 5: Income composition: top income taxpayers



Note: the figure displays the income composition of taxpayers included in our estimation sample for tax year 2016. The *x*-axis represents taxable income in thousands of rand, while the *y*-axis depicts the percentage share of the respective income category.

Source: authors' illustration.

Second, top earners in South Africa earn a relevant fraction of their labour income in the form of fringe benefits and allowances.[26] Business and investment income make up a relatively small fraction of income subject to PIT. This pattern, in part, relates to the fact that a large fraction of capital income—namely domestic dividend income—is not recorded in the data as it is subject to a separate withholding tax.[27] The low fraction of income earned from self-employment is consistent with low self-employment rates for South Africa in macro statistics (16% according to the WorldBank 2021) and may, in itself, also reflect tax non-compliance of business owners.[28]

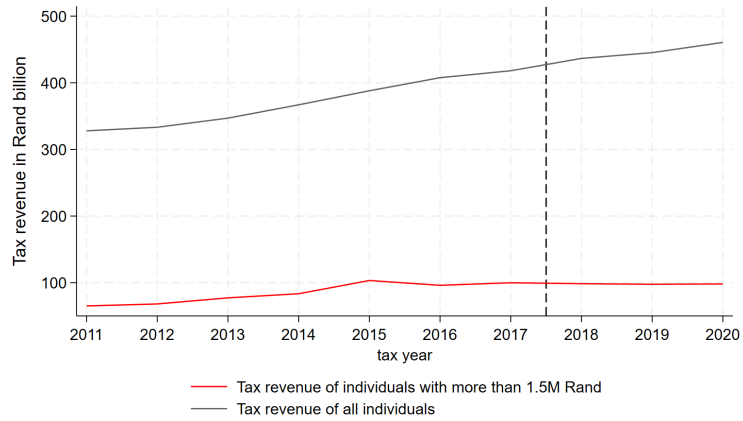
Figure 6(a) depicts the PIT revenues collected from the full population of taxpayers and from taxpayers earning more than R1.5 million, who are treated by a rise in the top tax rate. The figure does not point to a significant increase in tax revenue collections from treated taxpayers in tax year 2018, despite the significant increase in the top marginal income tax rate. Consistent with this finding, Figure 6(b) shows that the pre-tax income earned by income earners in the top 0.5% and top 1% of the income distribution declines after the reform, consistent with behavioural responses to the reform. The same holds true for after-tax income, pointing to a decline in income inequality at the top of the income distribution after the reform.

[26]Allowances and fringe benefits at the upper end of the income distribution include the exercising of share options or vesting of equity.

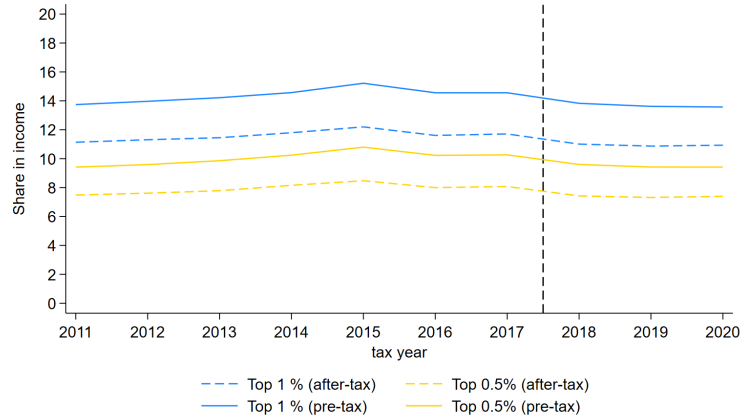
[27]Other forms of capital income—interest income, capital gains, or rents—are taxable under the PIT.

[28]At the upper end of the income distribution, the fraction of self-employment income increases (see Figure D3). But even for taxpayers who receive self-employment income, it often makes up a relatively small fraction of overall reported taxable income.

Figure 6: Evolution of PIT revenue collection and income inequality



(a) Tax revenue collection PIT



(b) Pre- and after-tax income inequality

Note: panel (a) depicts aggregated tax revenue collected in tax years 2011–20, in billion rand, separately for all individuals (black line) and only for individuals affected by the reform, with real income above R1.5 million (red line). Panel (b) illustrates inequality measures for South Africa spanning the tax years 2011–21. The blue and red lines depict the share in after-tax income for the wealthiest 1% and 0.5% of the population, respectively. The vertical dashed lines indicate the timing of the tax reform.

Source: authors' illustration.

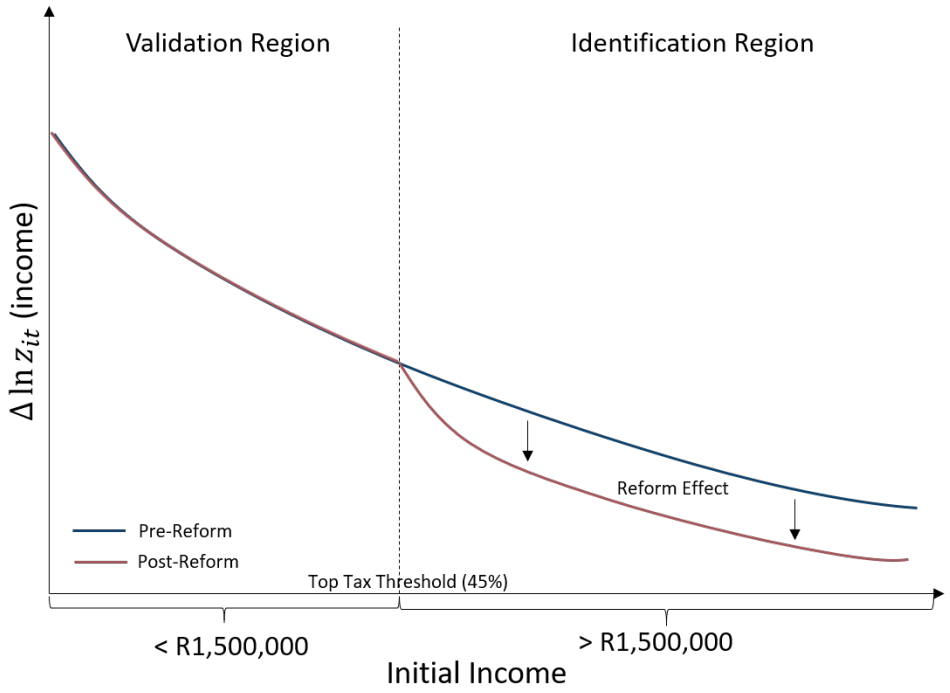
4 Empirical identification

We pursue a DiD-style approach to identify the elasticity of taxable income. Following seminal work by Feldstein (1999), the idea is to compare taxpayers who are treated by changes in the marginal tax rate schedule to untreated taxpayers, who face unchanged (or less-changed) marginal tax rates. The obvious challenge with this identification design is that taxpayers at different points in the income distribution may differ in their underlying income trends for reasons unrelated to the policy reform. Secular trends like skill-biased technological change or globalization may lead to differences in income growth for higher and lower incomes. Moreover, income trends may be shaped by mean reversion: taxpayers may be subject to idiosyncratic temporary income shocks, implying that taxpayers at the upper end of the income distribution experience systematically lower income growth than taxpayers at the lower end of the distribution. The literature has tried to address this challenge by controlling for lagged income, but ETI estimates have been found to be sensitive to specification choices (see e.g. Jakobsen and Søgaard 2022; Neisser 2021).

We follow Jakobsen and Søgaard (2022) and opt for a transparent empirical strategy to identify the ETI that allows testing for the underlying empirical identification assumptions. Specifically, we draw on a long panel of administrative taxpayer data that allows for fine-grained modelling of differences in income trends across the income distribution in the period *prior to the studied tax policy reform*. Under the assumption that the modelled trend differentials remain constant over time, relative shifts in income trends in the treated region can be interpreted as the treatment effect.

Figure 7 offers a graphical depiction of the empirical identification strategy. The horizontal axis depicts taxpayers' initial pre-reform taxable income. Taxpayers with taxable income above the R1.5 million threshold (indicated by the vertical line) are treated by the increase in the top marginal tax rate. Taxpayers below that threshold remain untreated. The empirical approach models the changes in taxpayers' income across time for the pre-reform period (blue line) and the reform period (red line). In our base analysis, we account for three-year differences in taxpayers' income reporting to account for adjustment frictions, implying that effects require some time to emerge. The base analysis accounts for the income difference between 2013 and 2016 (pre-reform period) and between 2017 and 2020 (reform period). Note that tax year 2020 ends in March 2020 and hence prior to the outbreak of the COVID crisis. We will present additional results, however, where we assess the robustness of our findings to looking at shorter timeframes and to shifts in the pre-reform and the reform period.

Figure 7: Illustration of the identification strategy



Note: this figure is an illustration of the identification and validation region strategy following Jakobsen and Søgaard (2022).
Source: authors' illustration.

Figure 7 illustrates that, while income trends in the treated region—for incomes above R1.5 million—are affected by the policy reform, the same does not hold true for incomes below the R1.5 million threshold. This region in the income schedule, therefore, serves as a validation region that graphically allows testing for the assumption that differentials in income trends do not systematically differ in the pre-reform and reform periods. Complementary to this analysis, we run placebo tests where we reject significant changes in income trend differentials in the pre-reform period *across the full income distribution*, including the treatment region.

Formally, our estimation model reads

$$\Delta \ln z_{itn} = \gamma_0 + \gamma_1' D_{i,t-n}^{inc} + \gamma_2 \, D_t^{reform} + \gamma_3' \, D_{i,t-n}^{inc} \times D_t^{reform} + v_{itn} \tag{1}$$

where the dependent variable is the change in income ($\Delta \ln z_{itn}$, namely broad income, taxable income, or individual income components) of taxpayer i between period t and $t-n$, with $n=3$ in the base analysis. Income trend differentials are modelled non-parametrically by percentile dummies, denoted by $D_{i,t-n}^{inc}$. Percentiles are determined based on period $t-n$, that is the years 2013 (for the pre-reform period) and 2017 (for the reform period). D_t^{reform} is a dummy variable indicating the reform period and capturing common income shocks across time. $D_{i,t-n}^{inc} \times D_t^{reform}$ allows the income trends to differ (relative to the base percentile) in the pre-reform and the reform periods. If trend differentials are constant across time, relative changes in income trends remain unchanged in the validation region (for incomes below R1.5 million). In the treatment region, we expect income reporting to drop in the reform period in response to the increase in the top marginal tax rate. v_{itn} is the error term. We account for serial correlation by clustering standard errors at the individual level.

To determine the elasticity of taxable income, we further estimate a slightly modified model version of the following form:

$$\Delta \ln z_{itn} = \beta_0 + \beta_1' D_{i,t-n}^{inc} + \beta_2 D_t^{reform} + \beta_3 \Delta \ln(1-\tau_{itn}) + \varepsilon_{itn} \tag{2}$$

where the regressor of main interest is $\Delta \ln(1-\tau_{itn})$, which represents the change in the marginal net-of-tax rate of taxpayer i between years t and $t-n$. To account for the fact that the actual change in taxpayers' marginal net-of-tax rate $\Delta \ln(1-\tau_{itn})$ is a function of reported income, we follow the literature and rely on an instrumental variable approach, using $\Delta \ln(1-\tau_{it-k}^p)$ as an instrument, which models the *policy-induced* change in the net-of-marginal tax rate, based on constant income in year $t-k$. Formally,

$$\Delta \ln(1-\tau_{it-k}^p) = \ln\left(1-T_t'\left(z_{it-k}\right)\right) - \ln\left(1-T_{t-n}'\left(z_{it-k}\right)\right) \tag{3}$$

In the base analysis, we set $k=n+1$—that is, we define treatment based on taxpayer income in the year prior to the reform. In robustness checks, we follow Weber (2014) and account for $k>n+1$—that is, we define treatment based on deeper lags.

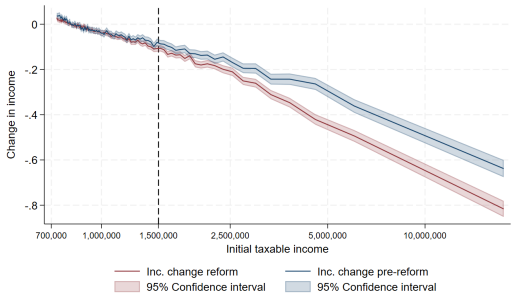
5 Results

5.1 Baseline estimates

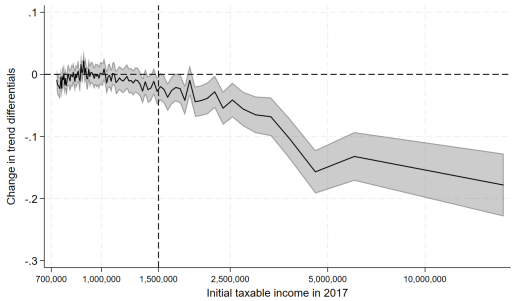
The baseline estimates for the models in Equations (1) and (2) are depicted in Figure 8 and Table 1. Figure 8(a), consistent with the illustration in Figure 7, shows that there are no systematic differences in the relative differentials in the taxable income growth across percentiles of the taxable income distribution in the validation region (for incomes below R1.5 million, left of the dashed line) in the pre-reform period and the reform period. In the treatment region (taxpayers with taxable income above R1.5 million, right of the dashed line), taxable income growth rates are significantly lower in the reform relative to the pre-reform period, consistent with treated taxpayers lowering their taxable income in response to the increase in the top marginal tax rate.[29] Figure 8(b) plots the trend differentials between pre-treatment period and treatment period in the validation and the treatment region, again illustrating the reduced taxable income growth in the treatment region after the increase in the top marginal tax rate. Figures 8(c) and 8(d) show an analogous result pattern for taxpayers' broad monetary income before deductions.

[29]The figure shows that there is a strong negative correlation between income level and income changes, consistent with mean reversion in income. This pattern is comparable to trends documented in other countries.

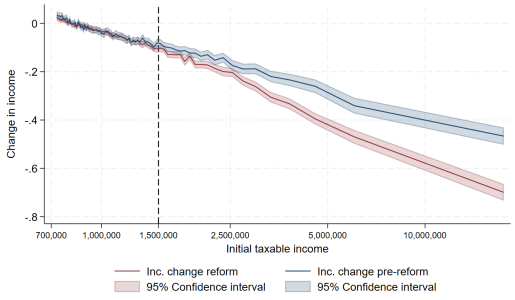
Figure 8: Income trends and trend differentials



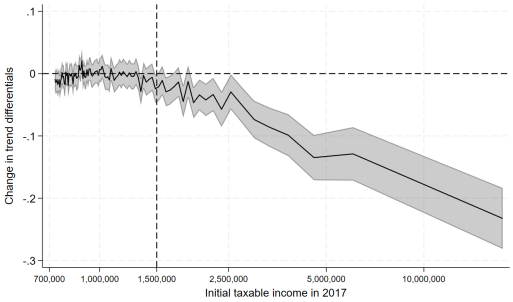
(a) Taxable income: trends



(b) Taxable income: trend differentials



(c) Broad monetary income: trends



(d) Broad monetary income: trend differentials

Note: panels (a) and (c) show the estimated trends for taxable income (a) and broad monetary income (c) for 2013—16 (in blue) and 2017—20 (in red) relative to a base category (incomes of around R800,000). Panels (b) and (d) show the estimated changes in trend differentials based on Equation (1) for taxable income (b) and broad monetary income (d).

Source: authors' illustration.

Table 1 presents the corresponding tax elasticities. The first row depicts the taxable income elasticity, and the second row the elasticity for broad monetary income. Column (1) presents the reduced form estimate, where we regress the change in taxpayers' income on the *policy-induced change in the net-of-marginal tax rate*.[30] Column (2) presents the instrumental variable estimates, where we instrument the *actual* change in the net-of-marginal tax rate by changes induced by the policy reform. In line with intuition, the IV estimates—which normalize the reduced form estimate by the first-stage coefficient—turn out larger in size. Both estimates point to sizable taxpayer responses. The IV regressions yield ETI estimates of 1.16 and 1.23 for taxable and broad monetary income.[31]

These ETI estimates are large relative to many prior studies (cf. Neisser 2021), suggesting that high-income earners in South Africa responded sensitively to the change in tax incentives. Like in previous work, we find that adjustments in tax deductions add to the observed response in taxable income reporting (see the fourth row of Table 1). But given that the South African PIT system is characterized by a broad tax base and few tax deductions (see Section 2), the quantitative relevance of deduction adjustments for the overall taxable income response is relatively limited.

[30]For the pre-reform period, this change is 1 percentage point or in relative terms 2.5% for all taxpayers in the estimation sample. For the reform period, the change is zero for taxpayers in the control region (with taxable income below R1.5 million in 2017) and 4 percentage points or 9.76% for treated taxpayers with incomes above R1.5 million in 2017.

[31]Figure E3 shows that similar findings emerge when we use gross income as the dependent variable.

Table 1: Baseline estimates for the ETI

Dependent variable	(1) Reduced form	(2) Instrumental variable
Taxable income	0.7444***	1.1618***
	(0.0439)	(0.0697)
Observations	517,227	517,227
Broad monetary income	0.7903***	1.2324***
	(0.0448)	(0.0710)
Observations	516,640	516,640
Non-monetary income	1.1023***	1.6102***
	(0.1431)	(0.2096)
Observations	411,803	411,803
Deductions	–0.7383***	–1.0657***
	(0.1666)	(0.2403)
Observations	181,928	181,928

Note: the table depicts the estimates β_3 of Equation (2) for different dependent variables. Taxable income, broad monetary income, non-monetary income, and deductions are all defined in Section 2.2. Column (1) presents estimates from a reduced form equation, Column (2) depicts instrumental variable estimates.

Source: authors' calculations.

Our estimates also point to significant adjustments in broad income, in both monetary and non-monetary income (see the second and third rows of Table 1). There are different potential explanations for this pattern. One is that high-income taxpayers in weaker institutional contexts may have many options available to engage in avoidance and evasion behaviour—even if the tax system design limits opportunities to evade taxes through elevated tax deductions (see e.g. Carrillo et al. (2017) for related evidence pointing to the substitutability of evasion channels in less developed country contexts). Another is that taxpayers at the upper end of the income distribution may lower their real economic activity in response to the reform—such responses may be shaped by cultural aspects (e.g. Bandiera et al. 2020) and might be particularly large in the developing world, where public sectors are plagued by corruption and inefficiencies in public good provision, undermining taxpayers' willingness to contribute funds to the state and exert economic activity. Taxpayers at the upper end of the income distribution may, moreover, adjust their labour supply more flexibly to tax incentives than lower-income workers. While 'standard' labour supply (hours worked) among male workers has been shown to be largely insensitive to income taxes, increases in marginal tax burdens may disincentivize work effort by high-skilled taxpayers at the upper end of the income distribution and lower their willingness to go 'the extra mile' to achieve performance goals (e.g. Arnemann et al. 2023). In general, non-standard wage components like incentive and bonus pay and non-monetary compensation may also be less subject to downward rigidities. We will assess the quantitative relevance of these channels in greater depth below.

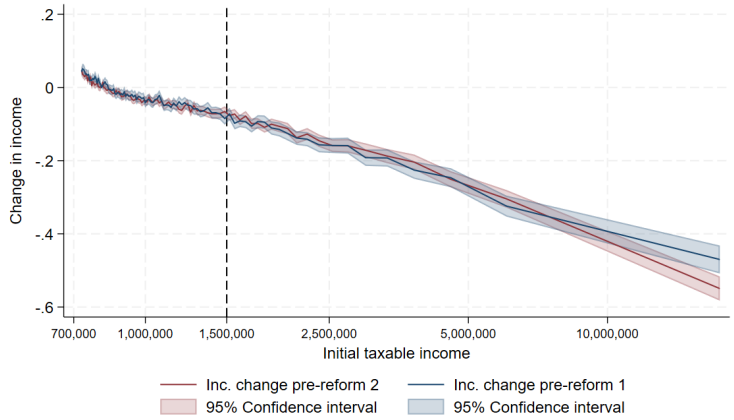
5.2 Robustness and validity checks

We run several validity and robustness checks to corroborate these baseline findings. As sketched above, the underlying empirical identification assumption is that income trend differentials across taxpayers remain constant over time. As outlined previously, there are two strategies to test for this assumption: first, as shown in Figure 8, trend differentials remain constant between the pre-treatment period and treatment period in validation regions, which are unaffected by the reform. Second, Figure 9 depicts placebo tests, which document constant trend differentials across the *full* income distribution in the pre-reform period.

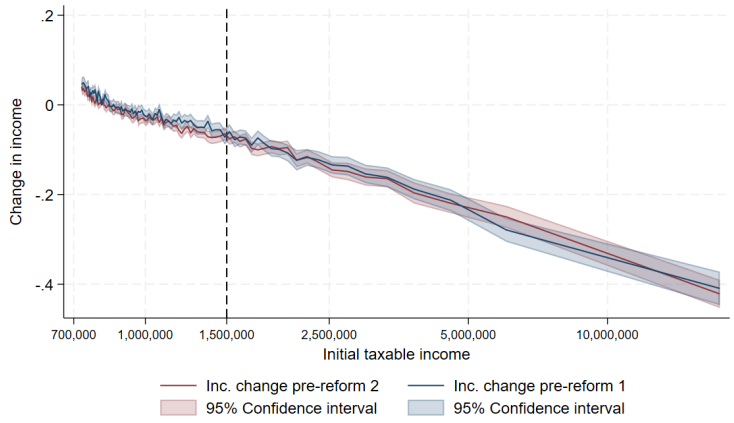
Figure 9 compares two-year income differences in the pre-reform period between 2011 and 2013 (blue) and 2014 and 2016 (red), using taxable income and the broad monetary income measures as the dependent variable. The figure does not point to major changes in differences in taxable income growth across the income distribution prior to the 2017 tax reform, hence corroborating the constant trend differential assumption. In Appendix E, we present three further robustness checks: first, we show that economic

and stock market development in South Africa were largely flat throughout our sample period, dampening concerns that our estimates might pick up underlying breaks in macroeconomic trends, which may exert a heterogeneous effect on individuals across the income distribution. Second, we present placebo tests for periods further back in time.[32] Third, we present a robustness check where we modify the estimation approach to model *pre-treatment period changes* in relative income trends and extrapolate them to the treatment period. This leaves the pattern of our findings unchanged.

Figure 9: Placebo tests



(a) Taxable income



(b) Broad monetary income

Note: comparison of two unaffected periods 2011–13 (blue) and 2014–16 (red) using taxable (a) and broad monetary income (b) as the dependent variable.

Source: authors' illustration.

In a further set of robustness checks, we follow Weber (2014) and define treatment not based on 2017 (and hence with a one-year lag to the policy reform) as in our baseline specification, but rather based on deeper lags. Specifications (1) and (2) of Table 2 presents estimates for $k = n+3$, which resemble our baseline findings.[33] We, furthermore, shed some light on effect dynamics: specifications (3) and (4) re-estimate our baseline model with one-year differences, accounting for income changes between

[32]The choice of time period for the placebo test involves a trade-off: on the downside, moving back in time increases the propensity for structural changes in income trend differentials. This may render the pre-reform periods a relatively poorer match to model counterfactual trend differentials for the treated group in the treatment period. On the upside, moving back in time comes with the advantage that we can rule out that income trends are impacted by other PIT-related reforms between 2016 and 2017.

[33]Similar results are also obtained for $k = n+2$ (not reported).

2017 and 2018 as the reform period, and income changes between 2013 and 2014 as the control period. The estimates suggest that taxpayers adjust to the reform quickly: two-thirds of the estimated three-year effect had already built up in the first post-treatment year.

Table 2: Robustness checks

	Weber $k = n+3$		One-year difference	
	(1)	(2)	(3)	(4)
	Reduced form	IV estimates	Reduced form	IV estimates
Tax. inc.	0.6614***	1.2257***	0.5740***	0.7674***
	(0.0491)	(0.0923)	(0.0326)	(0.0441)
Observations	352,337	352,337	586,699	586,699
	Shift control to 2012–15		Shift treat to 2016–19	
	(5)	(6)	(7)	(8)
	Reduced form	IV estimates	Reduced form	IV estimates
Tax. inc.	0.9931***	1.5535***	0.6617***	1.0465***
	(0.0462)	(0.0738)	(0.0418)	(0.0672)
Observations	498,438	498,438	466,188	466,188
	Shift treat to 2015–19, control to 2011–15		Exclude dividend earners	
	(9)	(10)	(11)	(12)
	Reduced form	IV estimates	Reduced form	IV estimates
Tax. inc.	0.9953***	1.7054***	0.6171***	0.9315***
	(0.0483)	(0.0849)	(0.0733)	(0.1123)
Observations	415,930	415,930	249,882	249,882

Note: the table shows several robustness checks. Uneven specifications present reduced form estimates, even specifications IV estimates. Specifications (1) and (2) depict results with a lag length of $k = n+3$; Specifications (3) and (4) estimate models with a one-year difference in income growth (2013-2014 and 2017-2018). Specifications (5) to (10) shift treatment and control periods as indicated by the column headings; Specifications (11) and (12) re-run the baseline estimates, excluding dividend earners.

Source: authors' calculations.

Table 2, moreover, presents robustness checks where we assess the sensitivity of our findings to changes in the definition of treatment and control group. In a first step, we re-estimate our baseline model shifting back the pre-reform period from 2013–16 by one year to 2012–15. This increases the time gap between the pre-reform and treatment periods (potentially increasing dissimilarities in income trends), but allows us to determine if our findings are driven by any of the adjustments in the income tax schedule between 2015 and 2016. The findings are robust to this modification, as shown in columns (5) and (6) of Table 2. In specifications (7) and (8), we re-estimate our baseline model, shifting the treatment period to 2016–19 (instead of 2017–20). This accounts for potential anticipation effects of the reform. As laid out in Section 2.3, the reform was announced on 22 February 2017, and hence six days before the end of tax year 2017. Taxpayers' scope for income adjustments in tax year 2017 was thus limited. In line with this notion, we find that estimates turn out similar to our baseline findings.

In columns (9) and (10), we shift the treatment period to 2015–19 and define an analogous four-year control period in 2011–15. This modification can be understood as an attempt to estimate a joint effect of the global marginal income tax increase in tax year 2016 by 1 percentage point and the increase in the top marginal tax rate by 4 percentage points on income above R1.5 million in 2018. The global marginal tax increase in 2016 affects individuals in both the treatment and control region of the top tax reform two years later. If income responses are homogeneous across the income distribution, effects are hence absorbed by the D_t^{reform}-regressor in Equations (1) and (2). If taxpayer responses (as suggested further below) increase along the income distribution, our baseline estimates are a lower bound to the

true effect.[34] Consistent with this interpretation, the coefficient estimates in specifications (9) and (10) turn out slightly larger than our baseline estimates.[35]

In an additional robustness test, we show that our findings are robust to dropping all taxpayers from the estimation who received dividend income. While reform design—as spelled out in Section 2.3—neutralized incentives of treated taxpayers to shift income from wages to dividends, this provides some further hedging against concerns that taxable income reporting in the PIT base may be affected by the changes in dividend taxation, which were implemented simultaneously to the increase in the top marginal tax rate. To do so, we draw on third-party reported information by firms on dividends paid to their shareholders. The data is available from 2016 to 2020 and is linked to the population of PIT returns. We drop all taxpayers from the data that received any dividend payment within this data frame. Again this yields large estimates for the taxable income elasticity (cf. columns (11) and (12) of Table 2).

Finally, Table 3 shows that our estimates for the ETI decrease moderately when we winsorize the dependent variable, the change of taxable income across periods ($\Delta \ln z_{itn}$). Our administrative data stems from tax returns or third-party PAYE reports submitted to SARS; as the information is tax-relevant, we consider the data to be reliable (extreme outliers in the level of taxable income are winsorized throughout the analysis, see Section 3). Even when winsorizing the dependent variable at the 5% level, the ETI estimates remain large and statistically significant. This indicates that our findings are not (solely) driven by taxpayers with large income adjustments.

Table 3: Winsorizing of dependent variable

	(1)	(2)
Dependent variable	Reduced form	Instrumental variable
Taxable income (base)	0.7444***	1.1618***
	(0.0439)	(0.0697)
Taxable income winsorized at 0.5%	0.7085***	1.1059***
	(0.0401)	(0.0638)
Taxable income winsorized at 1%	0.6791***	1.0599***
	(0.0379)	(0.0601)
Taxable income winsorized at 5%	0.5006***	0.7812***
	(0.0275)	(0.0438)

Note: the table depicts estimates for β_3 of Equation(2), winsorizing the dependent variable (taxable income) at different thresholds. Column (1) presents estimates from reduced form equations, column (2) depicts instrumental variable estimates.
Source: authors' calculations.

5.3 Anatomy of the response

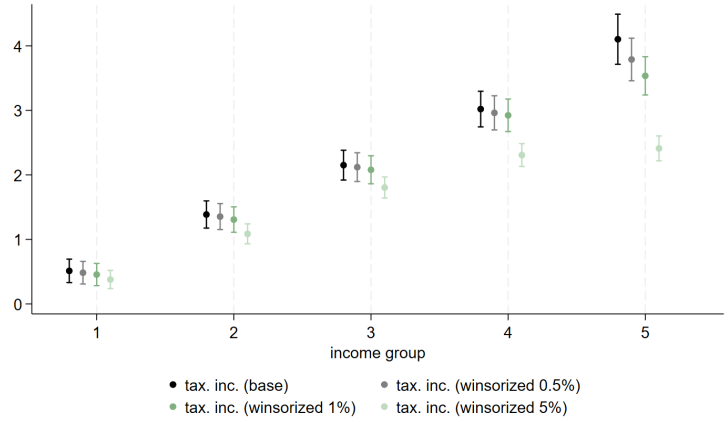
Next, we study the anatomy of the response. Figure 10 provides a first piece of evidence and (in line with Figure 8(b)) documents that the ETI steeply increases with taxable income. There are various potential explanations for this pattern. Taxpayers may systematically differ in their preference structure: if taxpayers at the upper end of the income distribution, for example, have lower costs (such as feelings of guilt) when evading taxes, their response to the policy reform might be stronger. Alternatively, taxpayers

[34]Relative to the true income trend differential, estimates for the income trends are too negative in the pre-reform period (2013–16) at the upper end of the income distribution, implying that the reform effect—the change in the relative income growth at the upper end of the income distribution from the pre-reform to the reform period is estimated too small.

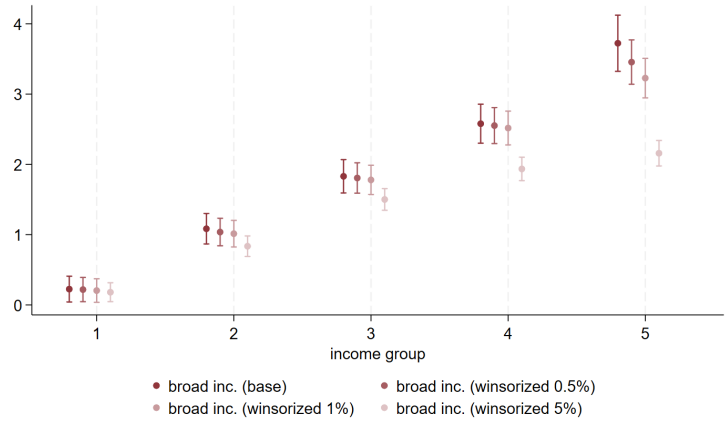
[35]This interpretation is also consistent with the placebo tests presented in Figures 9 and E2. Figure 9 accounts for tax year 2016 (i.e. the year of the global marginal tax increase) and, for the highest income group, shows smaller income growth in 2014–16, relative to 2011–13. We do not see an analogous pattern when we compare income growth between 2011 and 2013 with 2013 and 2015. Note that, consistent with this line of argumentation, the estimates in specifications (5) and (6) also turn out larger than our baseline estimates.

at the upper end of the income distribution may earn more income from sources that are easier to tax-adjust—for example, income that is not subject to third-party withholding, including self-employment income and some forms of investment income (cf. Figure 5). A further alternative explanation, which is consistent with the observed pattern, is that taxpayers misperceive their marginal for their average tax rate (as suggested by recent research by Rees-Jones and Taubinsky 2019). As individuals at the upper end of the treatment region (measured by income) experience a stronger increase in their average tax rate, they might also show stronger behavioural responses.

Figure 10: Results by income groups



(a) Taxable income



(b) Broad monetary income

Note: the figure allows the ETI estimate to vary within the treatment region, accounting for five groups (percentiles 81–84, 85–88, 89–92, 93–96, and 97–100). Estimates in different colours come from specifications where the dependent variable is non-transformed and winsorized at the 0.5%, 1%, and 5%-level.

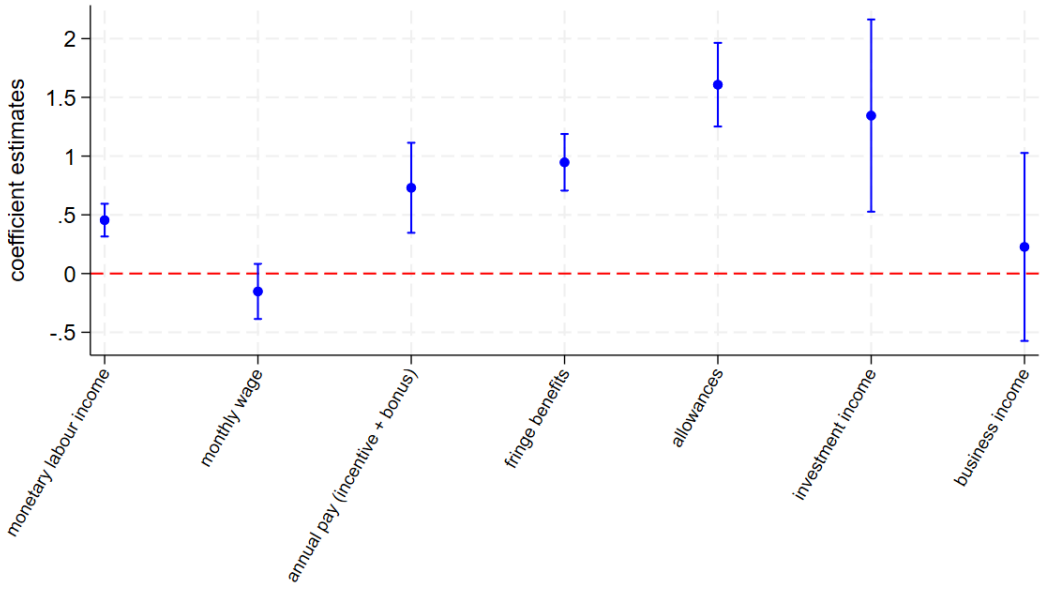
Source: authors' illustration.

In additional analyses in Figure 11, we offer a more fine-grained perspective on the adjustments in different income components. Specifically, we re-estimate the reduced form model in Equation (2) separately for labour income, investment income, and business income. In the labour income domain, taxpayers at the upper end of the taxable income distribution do not only earn regular monthly income but also, to a relevant extent, 'annual' income related to bonuses and incentive pay (see Figure 12), as well as non-monetary income.[36] Figure 11 shows a stark picture: while there is a zero response for

[36]Figure 12 shows that the majority of labour income earned by high-earning individuals comprises regular monthly earnings (blue), with the fraction decreasing across the income distribution. While it makes up around 75% of the income earned by individuals with R1.5 million, the share drops to around 50% for the very top earners. Annual payments—like incentive pay

monthly labour income, the results point to a high tax elasticity of annual payments—that is, incentive and bonus payments. This is consistent with prior evidence (see e.g. Arnemann et al. 2023) that has documented negative effects of top personal income taxes on the pay and labour supply of top employees in the developed world.

Figure 11: Reform effect by income category



Note: the figure displays the coefficient estimates when re-estimating our baseline reduced form model in Equation (2) separately for different income categories: total monetary labour income, monthly wages, annual pay (incentives and bonuses), fringe benefits and allowances, investment income, and business income (accounting for all taxpayers with non-zero business income). In the specifications pertaining to the labour income components, we restrict the sample to individuals, who are workers aged 15–65. We disregard PAYE records that display inconsistencies (e.g. reported employment spells outside the considered tax years).

Source: authors' illustration.

Allowances and fringe benefits are also adjusted flexibly in response to the reform, potentially reflecting that non-monetary income components can more easily be replaced with non-taxable income or benefits that are not subject to taxation.

Figure 11 also indicates that investment income responds strongly to the reform. While parts of investment income—namely interest income earned on bank accounts in South Africa—are subject to third-party reporting, many other sources of investment income are not (e.g. foreign investments or domestic investments such as rentals), offering scope for taxpayers to effectively evade taxes.

Strikingly, the figure points to a small and insignificant response of business income to the reform.[37] As stressed above, the self-employment rate in South Africa tends to be low, which may in itself reflect evasion behaviour and may explain the weak taxpayer response (if individuals already evaded tax on a large share of their business income prior to the reform, the scope for expanding evasion schemes may be limited).

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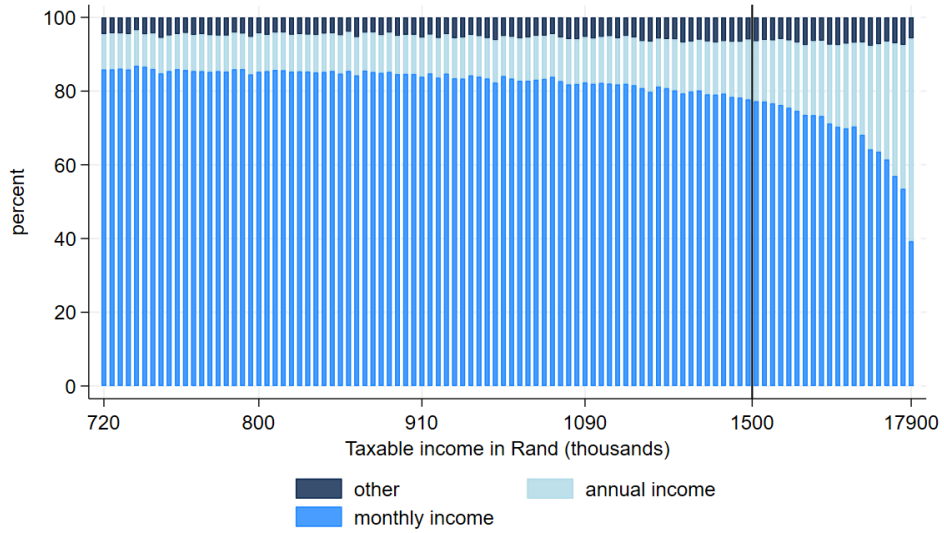
and bonuses—(light blue) are an important income component throughout the treatment region, but their relative importance increases for higher-earning individuals.

[37]Limiting the analysis to taxpayers who derive a substantial portion of their income from business activities does not alter the findings.

Figure 12: Composition of monetary labour income



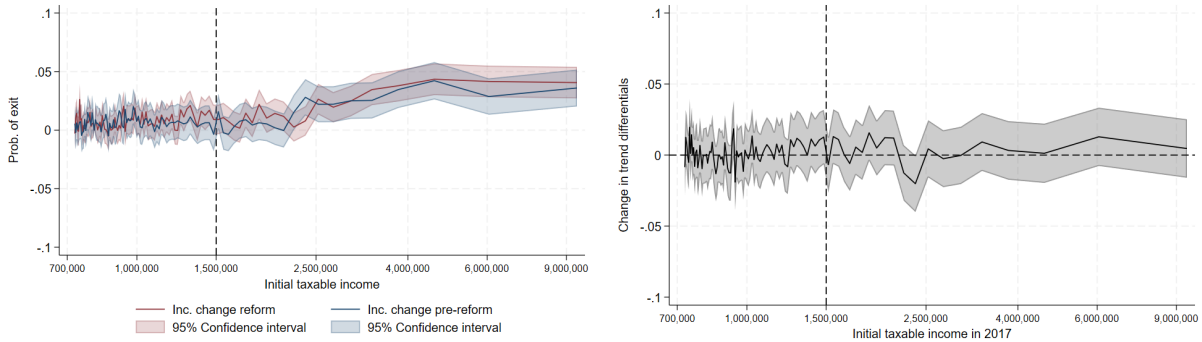
Note: the figure displays the composition of monetary labour income of taxpayers included in our sample for tax year 2016. The horizontal axis represents taxable income in thousands (rand), while the vertical axis depicts the percentage share of the respective income category, accounting for regular monthly wage income, annual pay, and commission income.

Source: authors' illustration.

5.4 Extensive margin: taxpayer exits

The analysis so far has tested for intensive margin responses to the reform. Figure 13 assesses whether the increase in the top marginal tax rate triggered extensive margin adjustments, with taxpayers leaving the PIT system. There are three potential underlying rationales for this response: first, taxpayers may leave the country to live in other (lower-tax) jurisdictions. While being a drastic response, South Africa has a non-negligible underlying emigration rate from the country, which might make emigration responses more likely (see e.g. Halstein 2021). Second, individuals may, in response, stop working and earning income or, evasion-driven, stop reporting income to the authorities. Third, individuals may incorporate businesses and shield income from higher PIT by declaring it as corporate income. As laid out in Section 2.3, the South African government neutralized this incentive by simultaneously raising the tax rate on dividend income.

Figure 13: Probability of exit



(a) Pre-reform and reform period

(b) Differential pre-reform and reform period

Note: the figure depicts estimates for the main estimation model in Equation (2), using exits from the PIT return information as the dependent variable. See the main text for further explanation.

Source: authors' illustration.

We re-estimate our baseline model, testing for reform effects on the propensity of treated individuals to exit from PIT taxation. Taxpayer observations enter the data if taxpayers submitted a PIT tax return in the base years of the analysis—2013 and 2017, respectively, for the pre-reform and the reform period. The dependent variable then takes on the value of 1 if individuals did exit the PIT data three years later (by not submitting a PIT tax return) and 0 otherwise. The findings do not point to a statistically significant reform-induced increase in the likelihood of exiting the PIT system. Also note that analogous findings emerge when we pursue a traditional DiD estimation strategy (see Appendix F for details).

5.5 Effects on revenue collection and inequality

As highlighted above, the South African government pursued two goals with the studied reform: first, it aimed to increase PIT revenue collection in the country. Conditional on taxpayers' income reporting, an increase in the top marginal tax rate mechanically raises the revenue collection from top income earners. Reductions in taxable income reporting in response to the reform, as estimated in the previous section, lowered tax collections from affected taxpayers.

In Table 4 we use our data to simulate the mechanical and the actual impact of the reform on PIT revenue collection. To do so, we compare the PIT revenue collection from treated taxpayers in the pre-reform year 2017 (see column (1))[38] to simulated tax revenue collections under the revised PIT schedule—accounting for the increased top marginal tax rate. In a first step, we abstract from behavioural responses ('mechanical' effect, see column (2)). In a second step, adjustments in income reporting are taken into account ('overall' effect, see column (3)). Behavioural adjustments are modelled based on the ETI estimates in Figure 8(b) and E3. In the absence of behavioural responses, tax revenue collection from high-income earners would have increased by R5.46 billion. In turn, taking the strong estimated taxpayer response into account suggests that PIT revenue collection *dropped* by R6.48 billion in response to the policy reform, putting the new top tax rate on the wrong side of the Laffer curve.

Table 4: Reform effects on aggregated tax revenue and after-tax income inequality

	Pre-reform	Simulation post-reform	
		Mechanical effect	Overall effect
	(1)	(2)	(3)
Tax revenue (treated)	103.5294	108.9918	97.0499
Gini coefficient	0.6111	0.6105	0.6074
Top 0.5%	7.89%	7.66%	6.94%
Top 1%	11.54%	11.30%	10.58%

Note: the table shows the simulated reform effects on tax revenue collection and after-tax income inequality. All inequality measures refer to after-tax income defined as taxpayers' gross income minus their tax liability. Column (1) depicts revenue collection and income inequality in the pre-reform period based on the gross income, taxable income, and the tax schedule for tax year 2017. Column (2) depicts the 'mechanical effect' of the reform, relying on the unchanged gross income and taxable income from tax year 2017 but the adjusted tax schedule (for tax year 2020) to obtain after-tax income. Column (3) depicts the 'overall' reform effect, accounting for the tax schedule in 2020 and simulating post-reform incomes by adjusting the 2017 income values based on the ETI from Figure 8(b) and E3.

Source: authors' own calculations.

In Table 4 we also simulate the reform effect on after-tax income inequality in the country. Both the mechanical and the behavioural effects work towards a reduction in reported after-tax inequality. We determine the reform effect on several inequality measures: the income earned by the top 1% or top 0.5% of income earners and the Gini coefficient. Again, we start from the pre-reform inequality measures in 2017 and then simulate the estimated behavioural responses and apply the revised tax schedule, accounting for the increased marginal income tax rate at the top. We find a moderate drop in all inequality measures. When we account for income reporting responses, the findings suggest that the share of total after-tax income earned by the top 1% (0.5%) dropped from 11.54% to 10.58% (from 7.89%

[38]We determine taxpayers' tax liability based on their taxable income reporting and the PIT schedule for 2017.

to 6.94%). Note, however, that, as illustrated in the previous section, part of the reform response may reflect avoidance and evasion adjustments in income reporting rather than real responses to the policy change. Our simulated decline in *reported* after-tax income inequality should thus be interpreted as an upper bound to the decline in *actual* true after-tax income inequality.

6 Real response

Finally, we assess whether the reform had negative real economic repercussions —that is, if treated taxpayers reduced their labour supply and work effort in response to the higher marginal tax, resulting in a reduction in real economic output. The decline in employer-reported incentive and bonus pay and non-monetary compensation is consistent with a real taxpayer response. But as laid out above, related adjustments might also reflect collusive tax evasion by employers and their employees in response to the reform (e.g. by understating the fraction of the private use of company cars, laptops, and cell phones, or private elements of business travel).

In this section, we therefore present a second piece of evidence. Specifically, we link the PIT return data to CIT information and document that firm output of treated enterprises—that is, firms with employees who are treated by the PIT rate increase—experience a drop in sales output, consistent with less effort provision and labour input by key employees in South African firms. As firms do not have a direct incentive to adjust their sales *reporting* in response to the PIT reform, this serves as an indication that part of the observed reform effect reflects a real economic adjustment.

6.1 Data

We empirically link the PIT-related information used in the main analysis to the universe of corporate tax returns in South Africa through PAYE information. This allows us to determine whether employees working for a given firm are treated by the PIT reform (that is, have taxable income above R1.5 million).

We draw on the universe of CIT returns (National Treasury and UNU-WIDER 2021). The data is linked to firms' company accounts and thus also includes information on firms' sales. Our sample is restricted to firms with PAYE information in the tax year 2017 (the year prior to the studied reform)—that is, with at least one worker for which the firm sends a PAYE report to SARS. The analysis accounts for tax years 2014–20 (where tax year 2020 ends in February 2020 and is thus unaffected by the COVID crisis).

As our sample period coincides with the implementation of various measures in South Africa and worldwide aimed at constraining income shifting by multinational companies to tax haven countries, we exclude these firms from our analysis to avoid picking up potentially confounding trends (this sample restriction is not decisive for any of the results presented below). Specifically, we drop all firms that are parent firms in South Africa (with ownership links to foreign countries) or subsidiary firms of foreign multinational companies located in South Africa, as identified following the broad multinational firm definition from Kilumelume et al. (2021). The analysis also disregards large entities with more than 100 workers.

Treatment status is determined based on individuals' real taxable income being greater than R1.5 million in tax year 2017. Among firms with PAYE information in 2017, 6.6% of businesses are treated by the reform in the sense that they employ at least one person who is treated by it.

6.2 Methodology

We assess the impact of the reform on firm outcomes based on a static and dynamic DiD strategy.[39] Formally, the static model reads:

$$y_{it} = \alpha_0 + \alpha_1 TREAT_i \cdot POST_t + \rho_i + \delta_t + \varepsilon_{it} \tag{4}$$

where y_{it} represents firm output, namely log-transformed sales, $TREAT_i$ indicates firms' treatment status (defined as a binary variable or as the fraction of employees with taxable income greater than R1.5 million in the year prior to the reform), ρ_i is a full set of firm fixed effects, and δ_t is a full set of time fixed effect, absorbing time-constant heterogeneity in firms' output and common shocks to firms' sales over time. Standard errors account for serial correlation—that is, we allow for clustering at the firm level.

The key assumption of the described strategy is the common-trend assumption: in the absence of the intervention, the sales of treated and control firms would have developed in parallel. Potential concerns include that treated and control firms systematically differ in key characteristics such as industry affiliation and firm size, implying that they might be on different underlying sales trends. We account for this concern in two ways: first, we run specifications where we allow for potential differences in sales trends across industries by augmenting the vector of regressors by a full set of two-digit industry fixed effects interacted with time dummies, which non-parametrically models industry differences in outcome trends. Analogously, we determine the demi-deciles (i.e. 20 quantiles) of the firm size distribution (determined by firms' number of workers) and allow outcome trends to differ across firms in different firm size classes. Second, we present estimates from dynamic DiD models, which allow us to assess whether the outcome trends of treated and control firms emerged in parallel prior to the reform, consistent with the common-trend assumption. Specifically, we estimate a model of the following form:

$$y_{it} = \delta_0 + \sum_{\ell \neq -1} \delta_1^{\ell} D_{it}^{\ell} + \rho_i + \delta_t + \varepsilon_{it} \tag{5}$$

where the variable definition corresponds to Equation (4) and D_{it}^{ℓ} indicates the relative time to treatment for treated firms, accounting for four leads (from $\ell = -4$, i.e. the pre-reform year 2014) and two lags (to $\ell = 2$, i.e. the post-reform year 2020).

6.3 Results

Specifications (1) and (2) in Table 5 present our baseline static DiD estimates of the model in Equation (4). Specification (1) suggests that sales of treated firms drop by 3.6%, on average, after treatment. In specification (2), we rely on a continuous treatment measure, the fraction of treated employees in the firm. The estimates suggest that a 10 percentage-point increase in this fraction lowers firm sales by 1.1%. The output effect, while statistically significant, is thus quantitatively relatively moderate. Specifications (3) and (4) re-estimate the baseline models, additionally absorbing firm size and industry-related shocks to firm sales over time. This leaves our estimates largely unaffected.

This finding is robust to a number of robustness checks. In specifications (5) and (6) of Table 5, we show that the estimates remain unchanged when we apply a broader treatment definition, defining workers as treated if they earn real income above R1.5 million in any of the pre-treatment years 2015–17. Table G1 shows that similar findings to the baseline estimates emerge when the dependent variable is the (less well-covered) value-added of firms.

[39] DiD-type strategies are commonly applied to estimate the impact of tax policy changes on firms' real behaviour (see e.g. Giroud and Rauh 2019). Issues related to mean reversion, which complicates empirical identification strategies in the personal income domain, have not been documented (and are theoretically not indicated) with respect to real activity in the corporate domain.

Table 5: Reform effects on firm outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	Binary	Fraction	Binary	Fraction	Binary	Binary
Treat	–0.0361***	–0.1126**	–0.0460***	–0.1319***	–0.0396***	–0.0497***
	(0.0107)	(0.0493)	(0.0113)	(0.0496)	(0.0095)	(0.0101)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	NO	NO	YES	NO
Size–year FE	NO	NO	YES	YES	NO	YES
Industry–year FE	NO	NO	YES	YES	NO	YES
Treatment def.	2017	2017	2017	2017	2015–17	2015–17
Observations	568,804	568,804	568,804	568,804	568,804	568,804

Note: ***, **, and * indicate statistical significance at the 1, 5, and 10% level, respectively. Standard errors are shown in brackets and account for clustering at the firm level. The dependent variable is the log of firm sales. Specifications include full sets of firm fixed effects and full sets of year fixed effects. Specifications (3), (4), and (6) additionally account for full sets of firm-size-decile–year fixed effects and full sets of two-digit industry–year fixed effects. In specifications (1), (3), (5), and (6), the treatment variable is binary (= firms are coded as treated if at least one of their workers is treated by the reform; in specifications (2) and (4), the treatment variable is the fraction of workers who are treated by the increase in the top marginal tax rate. Specifications (1)–(4) define the treatment variable based on the pre-reform year 2017; specifications (5)–(6) define it based on a three-year pre-reform period: 2015–2017.

Source: authors' calculations.

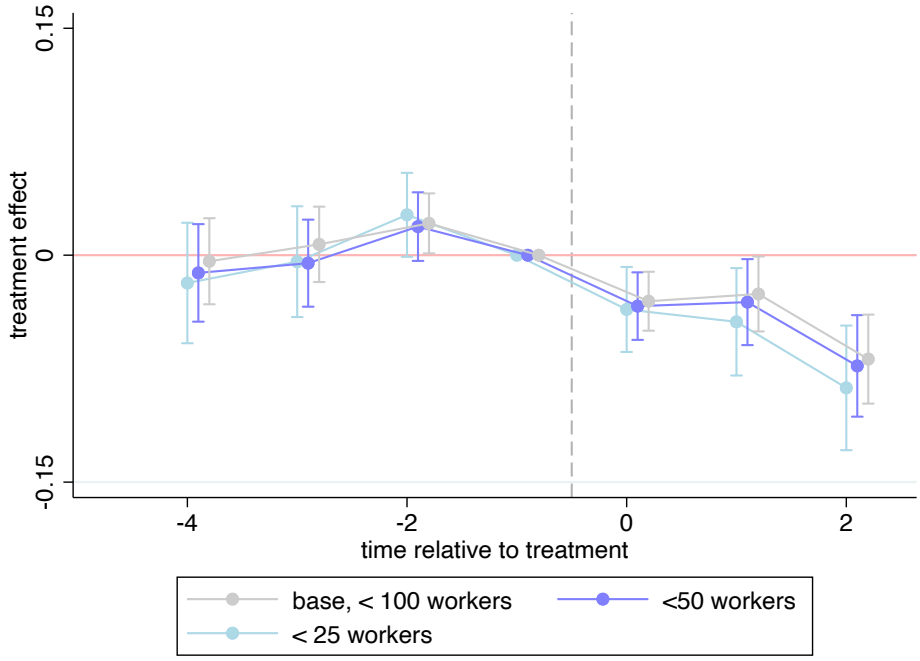
As described above, we also estimate the event study model depicted in Equation (5). The results are presented in Figure 14. The figure yields two insights: first, sales of treated and control firms emerge in parallel prior to the reform. Second, after the reform, the effect builds up gradually in the post-reform period and increases in size—consistent with reduced effort of key employees, which then gradually lowers firms' output. Figure 14 shows that similar (slightly larger) relative reform responses emerge when we restrict the sample to firms with fewer than 50 and firms with fewer than 25 workers.

We interpret these results as an indication that the reform had negative real economic effects. One caveat to this interpretation is that, while firms have no direct incentive to under-report sales, manager-owners might find it more attractive to shift firm activity to the shadow economy after the reform. If they did, this would lower both sales and taxable labour income. Such diversions are mostly feasible in smaller, organizationally non-complex entities that are not subject to tight scrutiny by the country's tax authority. In Table G1 we accommodate these concerns by showing that our estimates remain largely unchanged when we drop small firms and firms with many treated workers from the data (in the latter firms, coordination frictions when setting up (new) evasion schemes and the risk of detection by whistle blowers tend to be lower).[40] Moreover, we find similar results to the baseline estimates when we restrict our data to firms that are registered for VAT.[41] The latter check follows the notion that VAT constrains sales under-reporting by creating a paper trail on transactions (see e.g. Pomeranz 2015). For firms integrated into formal value chains, this limits options for sales shifting to the informal sector (see e.g. de Paula and Scheinkman 2010).

[40]Our data, unfortunately, does not allow us to identify owner-managers of firms.

[41]In order to identify the relevant firms, we merge our data with VAT data (National Treasury and UNU-WIDER 2022).

Figure 14: Effect on sales: different company sizes



Note: the figure depicts estimates of the estimation model in Equation (5) for samples of firms with fewer than 100/50/25 workers. All models include full sets of firm fixed effects and industry–year and size–year fixed effects, as discussed in the main text.

Source: authors' illustration.

7 Conclusion

The responsiveness of the tax base to changes in the tax rates is a crucially important policy parameter, yet the evidence regarding this responsiveness is very scarce outside of OECD countries. In many developing and emerging economies, inequality is a key concern, and one way to fight inequality is by increasing the tax burden on high-income individuals. This may be a costly strategy, however, if the reported incomes of the high-income group are significantly reduced in response to the tax increase. Existing estimates for the developed world may have little external validity for developing nations—where weak institutional capacity may increase the ability of taxpayers to evade and avoid taxes and erode their willingness to contribute funds to an often corrupt and ineffective public sector. Reliable evidence is needed to quantify the extent of this possible efficiency–equity trade-off in such a setting.

This paper contributes towards filling this gap by examining the behavioural consequences of a recent tax reform in South Africa, where the marginal tax rate paid by the top income earners in the country (approximately 0.6% of all income earners) was raised from 41% to 45%. The South African PIT system generates substantial revenue, being responsible for close to 40% of total tax revenue. The tax base is broad, with few deduction possibilities and the reform design largely avoided incentives to shift income across tax bases and across time.

We estimate the elasticity of taxable income, drawing on detailed administrative data from SARS covering all formal sector income earners for the years 2011–20. The estimation approach follows Jakobsen and Søgaard (2022) and, in transparent manner, tackles the possible mean reversion issue, known to be problematic in tax responsiveness studies. We provide extensive checks to demonstrate that the approach is valid in our setting.

Our results indicate that the taxpayers subject to the tax increase drastically reduced their taxable income reporting in response to the reform. The estimated elasticities are in the range of 1.1–1.2, implying that the country would be above the revenue-maximizing top tax rate. The anatomy of income changes, in line with prior findings, suggests that part of the taxpayer response relates to adjustments in tax reporting behaviour: we find strong adjustments in tax deductions and in broad income components like investment income, which are commonly associated with avoidance and evasion behaviour. Our findings, interestingly, also point to a marked response in labour-related broad income components. While standard monthly earnings do not respond in a statistically significant manner, we document sizeable reductions in other forms of employment income, such as fringe benefits and bonus and incentive pay. One interpretation of this finding is that—in the weaker enforcement context of less developed nations—employers and employees collude and under-report income or divert it to non-taxable components.[42]

Another interpretation is that the top tax increase reduces labour supply and effort provision of top earners—which may have relevant adverse economic consequences in less developed countries, where high-skilled labour is scarce. To further assess the relevance of the latter concern, we link the PIT information to firm-level data and—consistent with a real response—show that firms, which employ workers that are treated by the reform, experienced a significant drop in output after treatment, even conditionally on granularly absorbing industry and firm-size trends.

Our study adds to a broader understanding of the equity–efficiency trade-off when taxing high-income earners. We show that the efficiency costs of taxation can be large in less developed country contexts—even if the tax base, as in South Africa, is broad and even if careful reform design limits tax base and intertemporal income shifting. Our findings suggest that policy-makers in the developing world need to carefully balance equity goals against efficiency losses when taxing top income earners in their economies.

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[42]The results in the current paper are also related to the findings of Carrillo et al. (2017), who demonstrate that simply having access to third-party information did not improve tax compliance in the Ecuadorian context because of a lack of ability to act on the information.

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Appendix A: PIT system

Since 2001, South African residents have been individually taxed on their global income. The South African government transitioned from a territorial to a residence-based tax system to broaden the tax base. Tax credits are granted for income from non-South African sources, while non-residents are taxed solely on their South African-sourced income. Progressive tax rates apply to both residents and non-residents. Income taxes are collected at the central level, and there are no local income taxes. Employers must register formal employees for PIT, withholding it at the source. Employers are legally obligated to issue tax certificates (IRP5) for employees to complete their income tax returns. The IRP5 includes all remuneration (including allowances and fringe benefits), and a separate certificate (ITR12) is used for additional income sources exceeding exempt thresholds, or for claiming extra deductions. However, individuals are only required to file ITR12 returns if they have more than one form of employment, investment income above exempt thresholds, foreign capital income, or if additional deductions to taxable income are claimed (OECD 2022a). Progressive tax rates are applied to taxable income, which is determined by summing all taxable revenue sources to gross income (normal income, business income, allowances, fringe benefits, lump sum income, investment income, activity income) and subtracting any deductions. Before the final tax liability is determined, credits are given (SARS 2023). The income tax period in South Africa spans from 1 March to the end of February the following year. There is a tax-free allowance, increasing for taxpayers above the age of 65. To encourage savings, interest income up to a certain amount (again increasing for taxpayers above 65) from domestic sources is tax-exempt. Other income sources, trusts, and companies are subject to a presumptive tax system (OECD 2022a).

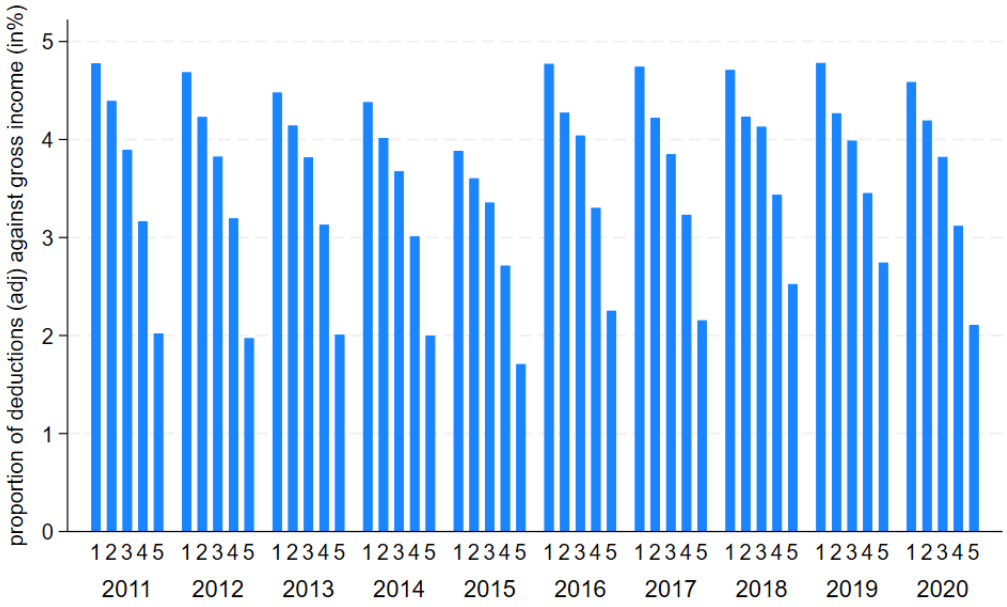
Appendix B: Pension reform and adjustment of the taxable income measure

The South African government enacted a pension reform which became effective in March 2016 and affected tax years 2017 onwards. The aim of the reform was to simplify and harmonize the pension-related deduction system (Redonda and Axelson 2021), making pension-related deductions fairer and providing better incentives for retirement saving. The following key changes were implemented: Before March 2016, contributions by employers and employees to different funds (pension, provident, and retirement annuity funds) were treated differently. Employee contributions to pension and retirement annuity funds were tax deductible (up to a certain threshold, namely pensions funds up to 7.5% of their retirement-funding employment income and retirement annuity funds up to 15% of their non-retirement-funding employment income), while employee contributions to provident funds were not deductible. Employer contributions to pension and provident funds were not treated as fringe benefits and, therefore, not part of employees' taxable income, while employer contributions to retirement annuity funds were classified as fringe benefits and hence taxable. In 2016, the system became more generous and was harmonized as employee contributions to all three funds were made deductible up to 27.5% of either taxable income or gross remuneration (whichever is higher), with a cap of R350,000 (Redonda and Axelson 2021).

To avoid that our findings are affected by the described changes in the pension-deduction scheme, we—in our empirical analysis—use two income measures that abstract from pension deductions in the South African PIT scheme. The first is a measure for monetary income, which comprises monetary labour income, business income, and investment income earned by taxpayers. The second is a measure for taxable income, which abstracts from any income and deduction components that might be affected by the reform. Specifically, we make two adjustments: first, we add employee provident fund contributions to our taxable income measure after 2017 to accommodate that they were not deductible beforehand. Second, we add taxpayers' retirement annuity contributions as well as pension fund contributions back to the taxable income measure for the whole sample frame as the 2017 reform raised the limits for this deduction substantially, thus increasing tax-deductible contributions after the reform. The sketched adjustments neutralize any effect on the taxable income variable in our empirical analysis. In addition, we adjust fringe benefits and deductions for the same reasons. We only look at deductions unrelated to retirement contributions, and fringe benefits are adjusted by subtracting employer provident and pension fund contributions from tax year 2017 onwards, since they were not included before the reform.

We only consider deductions which are not related to retirement contributions. Figure B1 depicts the ratio of tax deductions relevant in our empirical analysis as a fraction of taxpayers' gross income per quintile of the income distribution (1–5), across our data frame (years 2011–20). It illustrates that tax deductions tend to be low and their relative importance (as a fraction of gross income) declines across the income distribution (around 2–5% in the treated region for taxpayers with taxable income >R1.5 million).

Figure B1: Deductions relative to gross income: quintiles in the treatment group



Note: the figure shows the share of deductions (excluding pension deductions) on gross income for the treatment group (taxable income >R1.5 million) over tax years 2011–20. The treatment group is divided into five quintiles based on income levels.

Source: authors' illustration.

Appendix C: Data

This data appendix is created as per UNU-WIDER requirements for users of the National Treasury Secure Data Facility (NT-SDF).

C1 Data access

The data used for this research was accessed from the NT-SDF. Access was provided under a non-disclosure agreement, and our output was checked so that the anonymity of no firm or individual would be compromised. Our results do not represent any official statistics (NT or SARS). Similarly, the views expressed in our research are not necessarily the views of the NT or SARS.

Data used: Individual Panel provided by Christopher Axelson similar to Individual Panel 2023 (National Treasury and UNU-WIDER 2023), CIT firm-level panel (cit_panel_v5) (National Treasury and UNU-WIDER 2021), year-by-year IRP5 job-level data (v5), and VAT data (vatafp_2008_2022_e5_v1) (National Treasury and UNU-WIDER 2022). Date of first access for this project: 15 September 2022. Last accessed: 5 April 2024.

C2 Software

Our analysis was conducted using Stata 18. User-written programs used include reghdfe (Correia (2017)).

C3 List of variables used

Individual Panel: `taxable_income tax_year tax_liability business_income investment_income normal_income allowances_sc fringe_benefits_sc pr_ee pf_ee pr_er pf_er ra deductions`.

In addition, we created the following variables used for the analysis: `gross_income` as the sum of business income, normal income, investment income [`monetary_income`] and allowances, fringe benefits (adjusted) [`non-monetary_income`]; `after_tax_income` as the difference between gross income and tax liability; `reform_dummy`, a dummy variable turning 1 in tax year 2018; `marginal_tax_change_sim`, the simulated change in marginal tax rates based on pre-reform income; `marginal_tax_change`, the actual marginal tax change an individual experienced; and `pct`, which indicates the percentile of each individual in our sample for each tax year, based on real taxable income.

IRP5 job-level data: `taxyear taxrefno payereferenceno amt3601` (monthly wage) `amt3605` (annual payment). In addition, we create the following variables used for the analysis: `workers` (number of workers per firm).

CIT firm-level panel: `taxrefno taxyear x_int_fininst x_int_conx_int_oth x_int x_labcost y_int cit_taxable_income g_sales imp_mic_sic7_2d imp_mic_sic5_2d`
In addition, we create the following variables used for the analysis: `value added`, defined as the sum of labour costs, taxable income, and net interest paid.

C4 Cleaning and sample notes

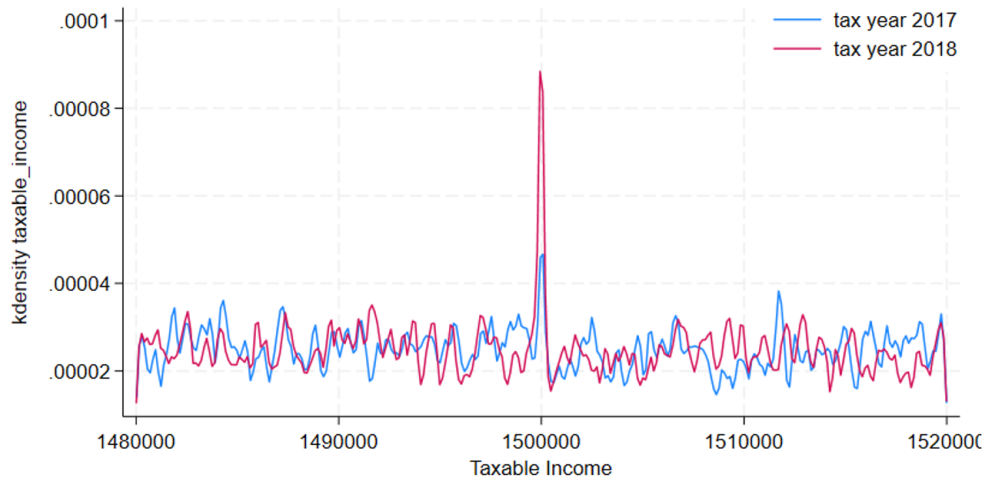
Individual Panel: We exclude individuals for which tax liability is greater than taxable income. We winsorize the variables taxable income, normal income, business income, investment income, fringe benefits, and allowances with the maximum value of the respective real income category in tax year 2013 (the beginning of our sample period). We use the CPI Index (normalized to March 2017) for deflation. We adjust for the pension reform by adding provident and pension employee fund contributions and retirement annuity contributions to taxable income and by subtracting provident and pension fund employer contributions from fringe benefits.

CIT Firm-level Panel: Our sample is restricted to firms with PAYE information in tax year 2017—that is, with at least one worker for which the firm sends a PAYE report to SARS. As our sample period coincides with the implementation of various measures in South Africa and worldwide, aimed at constraining income shifting by multinational companies to tax haven countries, we exclude these firms from our analysis to avoid picking up potentially confounding trends. Specifically, we drop all firms that are parent firms in South Africa (with ownership links to foreign countries) or subsidiary firms of foreign multinational companies located in South Africa, as identified following the broad multinational firm definition in Kilumelume et al. (2021) [`ITR14_c_foreign_broad_cons ITR14_c_foreign_strict_cons SA_MNE_const SA_MNE_CbCR_const`]. The analysis, furthermore, disregards large entities with more than 100 workers.

Appendix D: Descriptive statistics

Figure D1 presents the density of taxable income across the R1.5 million threshold in the pre-reform period 2017 and in the post-reform period 2018. We see mild bunching in both years—likely reflecting round number bunching. In line with a treatment response, there is more mass on the kink in the marginal tax rate schedule after the reform. But quantitatively, this translates into a limited response—consistent with many other prior estimates, which yielded small ETIs based on bunching estimators (see e.g. Kleven 2016).

Figure D1: Density zoomed in around threshold

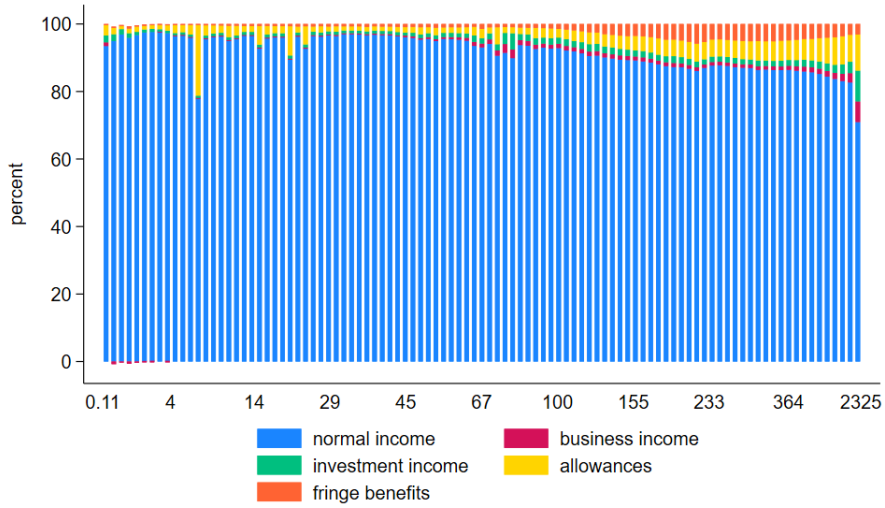


Note: the figure shows the density distribution zoomed in around the highest tax bracket threshold (R1.5 million) in tax year 2017 (blue line) and tax year 2018 (red line).

Source: authors' illustration.

Figure D2 complements Figure 5 in the main text, showing the composition of broad income for the full income distribution (while Figure 5 focuses on taxpayers with incomes >R700,000). The figure suggests that taxpayers at the lower end of the income distribution earn the vast majority of their income in the form of normal labour income.

Figure D2: Income composition full distribution (2016)



Note: the figure shows the income composition of taxpayers for the full income distribution for tax year 2016. The x-axis represents taxable income in million rand, while the y-axis depicts the percentage share of the respective income category.

Source: authors' illustration.

Table D1 presents summary statistics for various income concepts utilized in our empirical analysis. The overview exclusively covers our specified sample period (2013, 2016, 2017, 2020), during which we analyse differences between 2013–16 and 2017–20, considering our defined sample population (highest tax bracket until tax year 2017 and the two highest tax brackets from tax year 2018 onwards). It is worth noting that the minimum taxable income appears lower than expected (falling below the highest tax brackets). This discrepancy arises because our sample is restricted based on taxable income, while we employ taxable income (adjusted for the pension reform) as the dependent variable, as detailed in Appendix B.

Table D1: Summary statistics

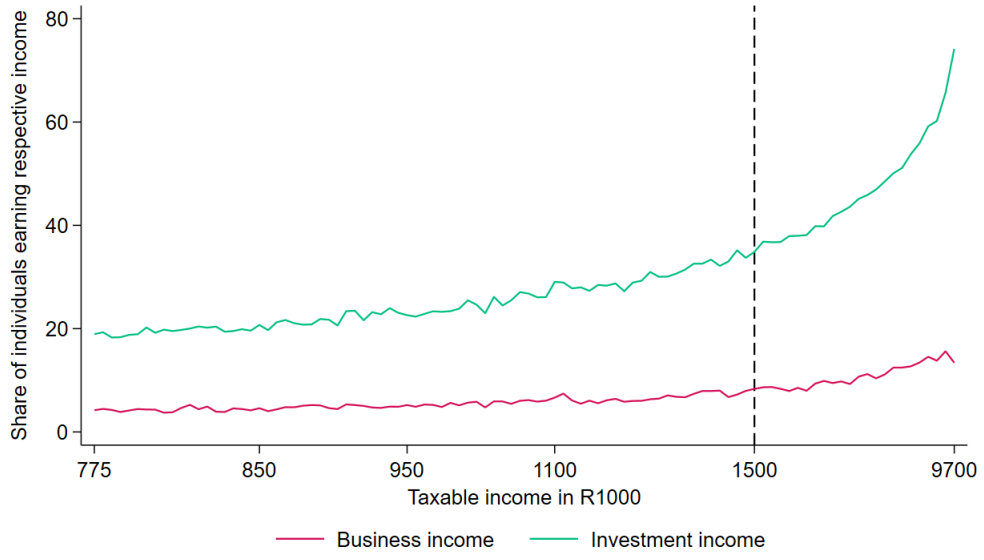
	Observations	Mean	Std. deviation	Min.	Max.
Monetary income	1,692,586	1218494	1,820,103	–1.25e+07	2.40e+08
Monetary labour income	1,692,586	1,070,983	1,543,445	0	2.40e+08
Business income	1,692,586	59,811.43	484,287.8	–3.85e+07	4.85e+07
Investment income	1,692,586	87,699.02	845,318	–4667191	8.09e+07
Non-monetary income	1,692,586	155,172.1	1.45e+07	–1.33e+10	6.30e+08
Fringe benefits	1,692,586	29,165.72	1.45e+07	–1.33e+10	3.14e+07
Allowances	1,692,586	126,006.4	1,258,067	0	6.30e+08
Deductions	2,503,993	20,780.19	127,162.8	0	1.00e+08
Taxable income	1,692,586	1,375,150	4,841,808	105,026	2.86e+09

Note: monetary income consists of monetary labour income, business income, and investment income. Non-monetary income consists of fringe benefits (adjusted for the pension reform) and allowances. Deductions and taxable income are also adjusted for the pension reform. The observations include our sample income period and timeframe (2013, 2016, 2017, and 2020). The variables are winsorized to top incomes in tax year 2013. Missing values are replaced with zeros.

Source: authors' calculations.

Figure D3 complementarily depicts the fraction of taxpayers per income percentile with non-zero business and investment income, respectively, at the upper end of the income distribution (below and above the R1.5 million mark). The figure indicates that the propensity to observe business and investment income increases with taxable income. At the very upper tail of the income distribution, almost all taxpayers (around 80%) observe investment income.

Figure D3: Business and investment income 2016



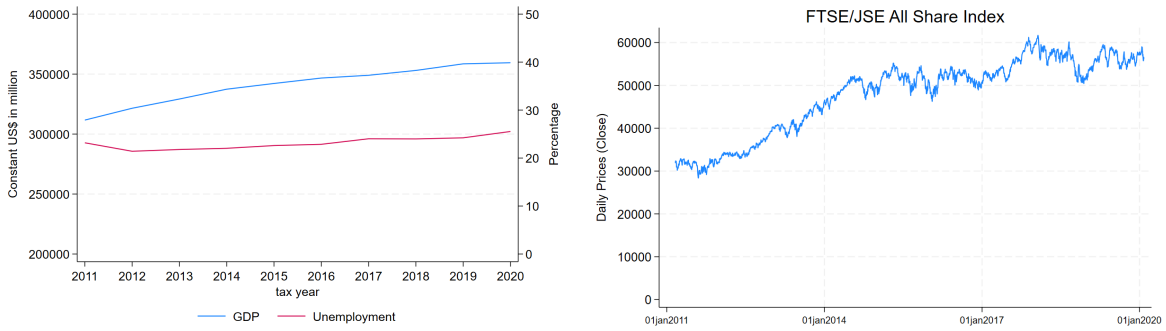
Note: the figure depicts the share of individuals who earn positive business (red) or investment (green) income.

Source: authors' illustration.

Appendix E: Specification checks

We run a number of specification checks to assess the validity of our empirical design. In Figure E1, we show that economic development was steady within the sample frame and did not show any major breaks, as indicated by GDP, unemployment rate, and stock market development.

Figure E1: Economic development



(a) GDP and unemployment

(b) FTSE/JSE All Share Index

Note: panel (a) depicts the GDP development (million US$ (constant)) and the level of unemployment (%) over our sample period. Panel (b) illustrates daily closing prices of the JSE All Share Index on the Johannesburg Stock Exchange.

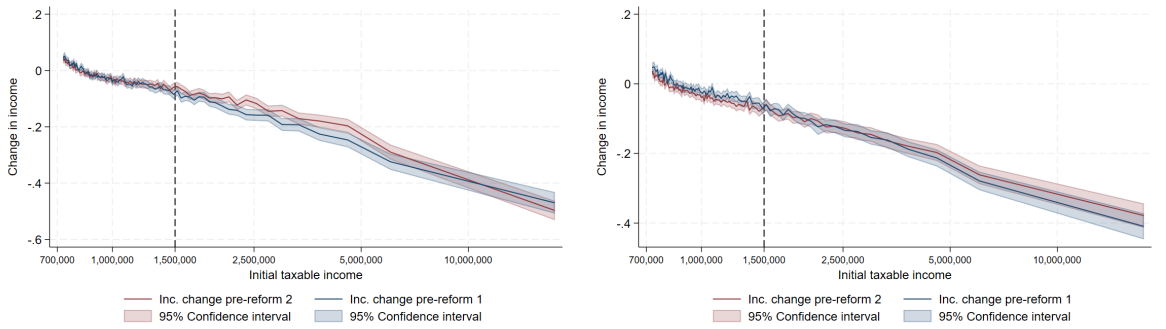
Source: authors' illustration.

We run placebo tests, based on information about income trends in the pre-reform periods of 2011–13 and 2013–15 (complementary to the placebo tests presented in the main text). Similar to our main findings, relative trend differentials remain largely unchanged across the two periods (see Figure E2). In an additional check, we further relax our empirical identification assumption by modelling changes in trend differentials during the pre-reform period and extrapolate them for the reform period. Specifically, we augment our baseline estimation model to include three trend differentials, each accounting for two-year differences in taxpayer income: 2011–13, 2014–16 (both pre-reform) and 2017–19 (reform period). The estimation model is:

$$\Delta \ln z_{it} = \delta_0 + \delta_1 D_i^{inc} \cdot TIME_t + \delta_3 \mu_t + \rho \Delta(1-\tau_{it}) + \varepsilon_{it} \tag{6}$$

where D_i^{inc} is a full set of percentile dummies and $TIME_t$ is a linear time trend which is interacted with the percentile dummies, allowing differences in trend differentials to vary over time (identified through changes in trend differentials in the pre-reform period).

Figure E2: Placebo test: pre-reform and reform period



(a) Taxable income

(b) Broad monetary income

Note: this figure presents placebo tests where we compare two-year income differences in the pre-reform period between 2011 and 2013 (blue) and 2013 and 2015 (red), using taxable income (a) and broad monetary income (b) as the dependent variable.

Source: authors' own calculations.

In line with the comparable income trend differentials modelled in Figure 9, the estimates for the coefficient of interest for ρ in specifications (1) and (2) (where we do and do not include the additional set of regressors $D_i^{inc} \cdot TIME_t$, see Table E1) turn out to be comparable: the coefficient estimates do not statistically differ, in the sense that 95% confidence bounds overlap, nor if the variable is broad monetary income (upper row) nor if it is taxable income (lower row).

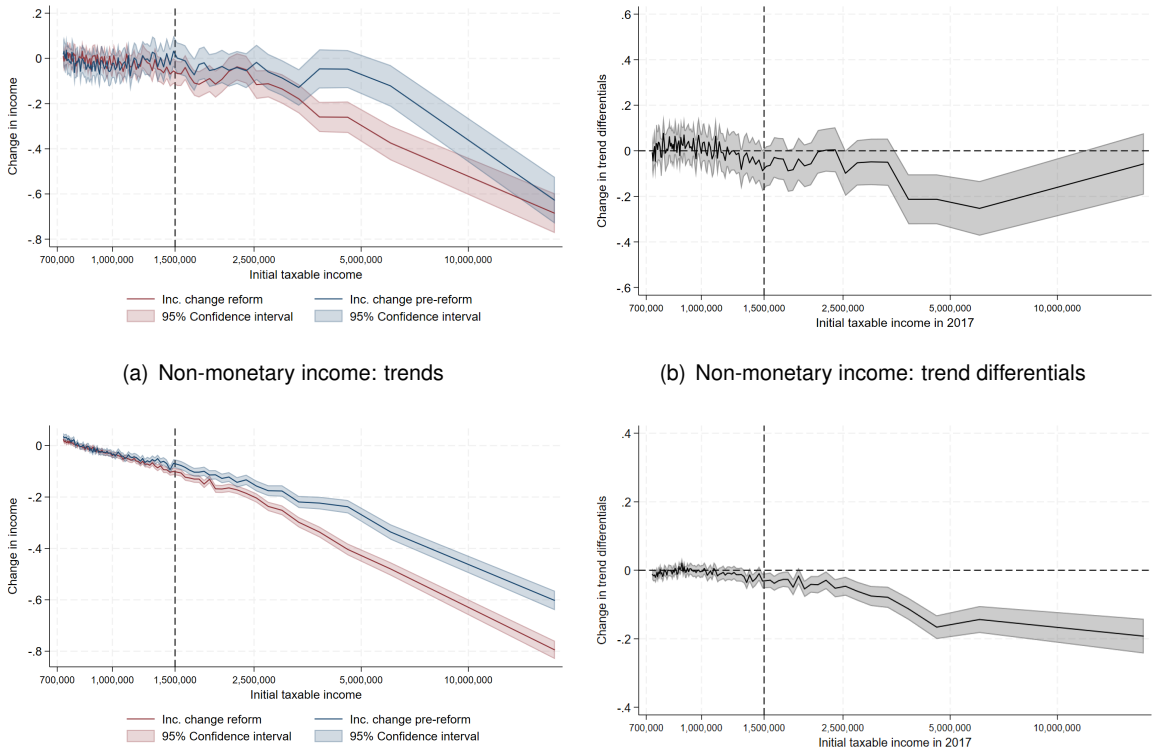
Table E1: Modelling pre-reform trends

	(1) Without time trend	(2) With time trend
Broad income	0.7470***	0.6669***
	(0.0291)	(0.0654)
Observations	757,177	757,177
Taxable income elasticity	0.7613***	0.6156***
	(0.0292)	(0.0648)
Observations	757,961	757,961

Source: authors' calculations.

Complementary to Figure 8 in the main text on the PIT reform effect on taxable income and broad monetary income, Figure E3 re-estimates our baseline model using non-monetary income separately as well as overall gross income (defined as the sum of monetary and non-monetary income).

Figure E3: Income trends and trend differentials



(a) Non-monetary income: trends

(b) Non-monetary income: trend differentials

(c) Gross income: trends

(d) Gross income: trend differentials

Note: panel (a) and (c) show the estimated income trend differentials for non-monetary (a) and gross income (c) for 2013—16 (blue) and 2017—20 (red) relative to the average growth rate for incomes around R800,000. Panels (b) and (d) show the estimated changes in trend differentials based on Equation (1) for non-monetary (b) and gross income (d).

Source: authors' illustration.

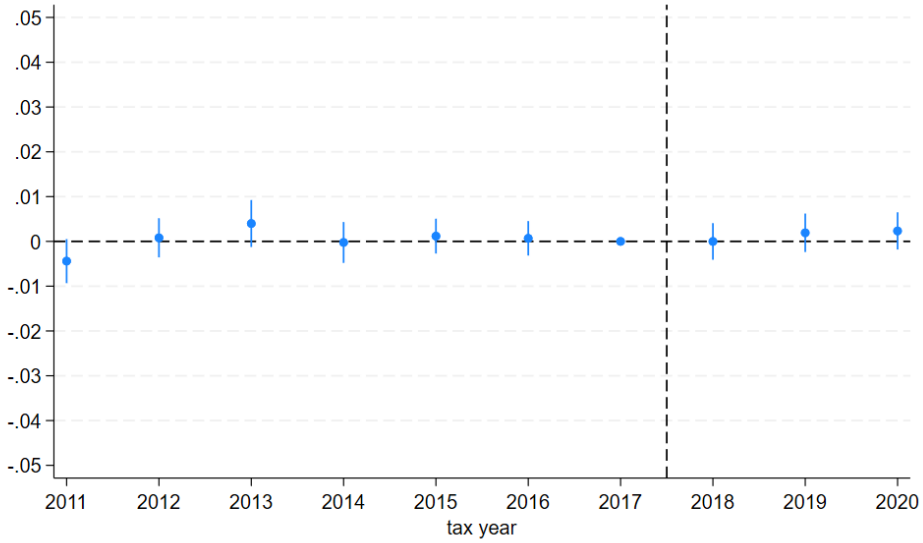
Appendix F: Exits

The rise in the top marginal tax rate may have prompted significant adjustments in the extensive margin, leading taxpayers to exit the PIT system. To assess whether the reform resulted in an increase in high-income earners leaving the South African tax system, we calculate exit fractions—indicating the propensity of individuals to leave our sample. This involves determining the ratio of taxpayers exiting our data to those present in our data per percentile for each year. Subsequently, we employ a dynamic DiD approach to estimate the treatment effect:

$$y_{it} = \delta_0 + \sum_{\ell \neq -1} \delta_1^{\ell} D_{it}^{\ell} + TREAT_i + \delta_t + \varepsilon_{it} \tag{7}$$

where y_{it} is the exit fraction in percentile i and tax year t, δ_t are year fixed effects. A percentile is treated if it is larger or equal to the 81st percentile. D_{it}^{ℓ} indicates the relative time to treatment for treated percentiles, accounting for seven leads (from $\ell = -7$, i.e. the pre-reform year 2011) and two lags (to $\ell = 2$, i.e. the post-reform year 2020). Figure F1 illustrates that we do not find any effects on the extensive margin.

Figure F1: Event study estimates of the exit fraction



Note: the figure presents event study estimates of the exit fraction.

Source: authors' illustration.

Appendix G: Firm-level analysis

Table G1 contains a number of further robustness checks to the firm-level analysis in the main part of the paper. One presumption might be that our findings are driven by smaller enterprises and responses by owner-managers who are treated by the reform. As illustrated in the main text, there were no incentives to shift income between wages and dividends for treated owners-managers after the reform. And while treated individuals had incentives to under-report their income in the PIT base, this does not hold true for firm output in the CIT base (the reporting of which had no direct implication for owner-managers' PIT liability).

Exceptions might be very small firms, which are dominated by treated owner-managers. Here, treated individuals may have a stark position within the firm, relative to other stakeholders, which might allow them to adjust firm operations to hide income from the tax authorities. Specifically, they might—in response to the reform—decide to move part of their business activity from the official sector to the black market, resulting in lower sales and, in consequence, lower wage and income tax payments. But even in small firms, incorporation and the accounting duties that come with it may hinder this type of evasion. Specifications (1)–(4) assess the quantitative relevance of such concerns by showing that our findings remain largely unchanged when we drop small firms (with fewer than 10 workers) and firms with a large share of treated workers (>75% of the workforce) from the data. Similar results, moreover, also emerge when we restrict the estimation data to firms that are registered for VAT. This robustness check follows the notion that VAT undermines sales under-reporting by creating a paper trail of transactions (see e.g. Pomeranz 2015).

We used the firm data at hand to construct the value-added of the firm as the sum of compensation for the labour and capital production factors (labour costs and profits before interest and taxes, defined as firms' taxable income plus net interest paid). Information on labour costs and interest paid and received stems from firms' balance sheets. The results are presented in specifications (7) and (8) of Table G1 and align with our baseline findings.

Table G1: Reform effects on firm outcomes: robustness analysis

Dep. var.	Sales						Value-added	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat	–0.0421***	–0.0543***	–0.0425***	–0.0472***	–0.0247**	–0.0371***	–0.0739***	–0.0886***
	(0.0132)	(0.0120)	(0.0114).	(0.0102)	(0.0111)	(0.0099)	(0.0107)	(0.0098)
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES
Size–year FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry–year FE	YES	YES	YES	YES	YES	YES	YES	YES
Treatment def.	2017	2015-17	2017	2015–17	2017	2015–17	2017	2015–17
Sample	Workers > 10	Workers > 10	Share < 0.75	Share < 0.75	VAT	VAT	ALL	ALL
Observations	218,667	218,667	567,249	567,249	483,003	483,003	478,242	478,242

Note: ***, **, and * indicate statistical significance at the 1, 5, and 10% level, respectively. Standard errors are depicted in brackets and account for clustering at the firm level. The dependent variable is the log of firms' sales (specifications (1)–(6)) and the log of firms' value-added (specifications (7) and (8)). Specifications include full sets of firm fixed effects, full sets of firm-size-decile–year fixed effects and full sets of two-digit industry–year fixed effects. The treatment variable is binary (= firms are coded as treated if at least one of their workers is treated by the reform; specifications with uneven numbers account for the pre-treatment year 2017 when defining treatment status; specifications with even numbers account for the years 2015–17 when defining treatment status (treated in any year). Specifications (1) and (2) drop firms with fewer than 10 workers from the data; specifications (3) and (4) drop treated firms with a share of treated workers larger 0.75 from the data; specifications (5) and (6) only keep firms that are registered for VAT in the data; specifications (7) and (8) keep all firms but use the log of value-added as the dependent variable.

Source: authors' calculations.