



# Identifying behavioral responses to tax reforms: New insights and a new approach<sup>☆</sup>

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## ABSTRACT

We revisit the identification of behavioral responses to tax reforms and develop a new approach for graphical validation and representation of treatment effects. We show that the standard estimation strategy relies on an assumption of constant trend differentials. In the context of income taxation, this implies that differences in income trends across the income distribution should remain constant in the absence of tax reforms. Similar to pre-trend validation of differences-in-differences studies, we can validate this assumption by comparing the evolution of income in untreated parts of the income distribution. We illustrate our new approach by studying several tax reforms in Denmark. (JEL: C14 H30 J22).

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## 1. Introduction

Behavioral responses to taxes are key inputs in evaluations of tax distortions and the trade-off between equity and efficiency (Saez, 2001; Saez et al., 2012), and they serve as evidence on behavioral parameters in economic models more broadly (Chetty et al., 2011a,b). Researchers and policy makers use tax reforms to identify the strength of behavioral responses to various types of taxation, including income (Feldstein, 1995; Goolsbee, 2000; Gruber and Saez, 2002; Kleven and Schultz, 2014; Weber, 2014), wealth (Seim, 2017; Jakobsen et al., 2020) and corporate profits (Devereux et al., 2014).

While the large literature studying behavioral responses to taxation is diverse in nature, a common challenge is that treatment (e.g., marginal tax rates) is determined by the outcome of interest (e.g., taxable income) and, thus, endogenous. To overcome this challenge, the standard estimation strategy isolates exogenous variation in treatment using tax reforms and assigns treatment

status based only on pre-reform information (Gruber and Saez, 2002; Kleven and Schultz, 2014; Weber, 2014).<sup>1</sup> However, as this treatment assignment is a function of past outcomes with limited or no other sources of variation (e.g. spatial), almost any serial correlation in outcomes will violate the common trend assumption underlying differences-in-differences (DiD) studies; and serial correlation is a first order issue in the empirical tax literature as outcomes, such as wealth and income, are severely affected by secular trends and, in particular, mean reversion.

The additional challenge created by serial correlation is well known in panel data models and this is reflected in the solutions developed by the empirical tax literature. Serial correlation is dealt with either by modeling it as an autoregressive process, which can be controlled for by including functions of past outcomes in the regressions (Gruber and Saez, 2002; Kleven and Schultz, 2014), or – inspired by Arellano and Bond (1991) – by using further lags of pre-reform information to assign treatment status (Weber, 2014). However, by relying on these solutions, the empirical tax literature has diverged from modern reduced-form studies and lacks the ability to (graphically) validate identifying assumptions. Hence, researchers often find that estimation results are highly sensitive

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<sup>1</sup> The literature studying behavioral responses to income taxation often seek to estimate the elasticity of taxable income (ETI) and, hence, is often referred to as the ETI literature. However, this naming essentially refers to the choice of outcome variable and not the empirical strategy, which is the main focus in our paper. Applying the standard estimation strategy on hours worked yields an estimate of the more traditional (intensive margin) labor supply elasticity. Indeed, many papers in the ETI literature also consider measures of income that come closer to pure labor supply responses (see e.g. Kleven and Schultz, 2014).

to the exact specification (see e.g., [Kopczuk, 2005](#); [Neisser, 2018](#)) with no tools for choosing between them.

In this paper, we revisit the standard estimation strategy to identify behavioral responses to tax reforms and develop a new reduced-form approach that allows for clear validation of identifying assumptions.<sup>2</sup> Cast in the context of income taxation, we show that, in essence, the standard estimation strategy relies on an assumption that any trend differences in income across the income distribution remain constant in the absence of tax reforms. The assumption of constant trend differentials is equivalent to the common trend assumption underlying DiD studies, and we show how the assumption can be validated econometrically and graphically, in a way similar to the comparison of pre-trends in DiD studies.

One way to see the assumption of constant trend differentials of the standard estimate strategy is to consider the canonical study of the US Tax Act of 1986 by [Feldstein \(1995\)](#). [Feldstein \(1995\)](#) employs a simple DiD and estimates the reform effects by comparing the changes in taxable income for high and low-income individuals over a period, in which the tax reform lowered marginal tax rates more for high-income individuals than for low-income individuals. However, with today's knowledge, this estimate is likely to be biased as the underlying income trends for high and low-income individuals may be very different. To correct for underlying differences in income trends we can run the same DiD in a pre-period unaffected by tax changes and subtract the estimate from the reform DiD, thus in effect turning it into a DiDiD in time. This approach yields a causal estimate of the reform effect under the assumption that the trend differences estimated in the pre-reform DiD would remain constant in the absence of the reform. In its simplest form, this is what the standard estimate strategy does by controlling for past income, and we show how this insight extends to the use of further lags of pre-reform information to assign treatment status, as done by [Weber \(2014\)](#).

Our new approach enables a simple validation of the assumption of constant trend differentials. The key insight is that individuals treated similarly by a tax reform (e.g., individuals within a given tax bracket) are not a homogeneous group, but are drawn from a wider income range. Thus, within these groups we would expect differences in income trends due to, for example, mean reversion, but under the assumption of constant trend differentials we should observe no changes in these trend differentials within the untreated parts of the income distribution. Hence, we can validate the identifying assumption by non-parametrically comparing trend differentials in two periods: a reform period, where a reform changes tax rates differently for different groups, and a pre-reform period, where ideally the tax system was stable.

We illustrate our new approach using a number of tax reforms in Denmark with a particular focus on two reforms implemented in 2004 and 2009–10. The 2004 reform primarily reduced marginal tax rates in the middle of the income distribution, while the 2009–10 reform primarily reduced them at the top, with only minor changes in the bottom.

Applying our new estimation approach to the 2009–10 tax reform, we find significant negative correlations between initial income and subsequent income growth, which is consistent with mean reversion being the dominant (but not necessarily the sole) feature of the underlying income process in both the pre-reform and reform periods. Next, comparing the pre-reform period to

the reform period, we find that the income trend differentials remained stable for the untreated bottom part of the income distribution, while for the treated upper part of the income distribution we find significantly higher income growth. The changes in trend differentials are strongly increasing in initial income and somewhat larger in the medium run than in the short run. Taken together, we see this as compelling evidence of behavioral responses to the 2009–10 tax reform.

The behavioral responses translate into an average elasticity of taxable income with respect to the marginal net-of-tax rate of 0.227, ranging from 0.1 in the middle of the income distribution to 0.5 at the top.<sup>3</sup> We find that most of the responses are driven by income shifting from pension contributions to taxable income induced by the reform. Using a broader income measure that is unaffected by the shifting of income, we find an elasticity of 0.016.

Turning to the 2004 reform, we find close to the same average elasticities as with the 2009–10 reform when we apply the standard approach. However, applying our new approach and inspecting the trend differentials across the income distribution we find no changes around the changes in tax treatment. Instead, the elasticity estimate is driven by changes in trend differentials well within the control group and is most likely unrelated to the reform. Once we account for these changes, we obtain a precisely estimated zero response for both taxable and broad income. This illustrates the importance of careful validation of the underlying identifying assumptions.

The potential biases that we address are not new. The problems of mean reversion and secular differences in income trends were highlighted already by [Auten and Carroll \(1999\)](#) and discussed extensively in the large literature that followed (see e.g. [Saez et al., 2012](#)). Our contribution is to bring the empirical tax literature up to modern empirical standards by clarifying the assumptions underlying the standard estimation strategy and develop tools to (graphically and econometrically) validate these assumptions. In doing so, we extend the initial steps towards graphical validation of identifying assumptions taken by [Weber \(2014\)](#) and synthesize her work with the earlier approach by [Gruber and Saez \(2002\)](#). Our paper also relates to the recent work by [Kumar and Liang \(2020\)](#), who focus on estimation and validation using variation in tax rates within narrowly defined income groups, where mean reversion and differential income trends are less of a direct concern. In our paper, we discuss the use of tax variation both within and between income groups, but focus on the latter more “stereotypical” variation created by, for example, changes in top marginal tax rates.

The rest of the paper is organized as follows. Section 2 revisits the standard estimation strategies of behavioral responses to taxation and outlines our new estimation approach. Section 3 introduces the institutional setting and data used in our empirical application, while Section 4 presents graphical evidence on income responses to the 2004 and 2009–10 reforms. Finally, Section 5 concludes.

## 2. Estimating behavioral responses to taxation

In this section, we develop our new approach for estimation of behavioral responses to taxation. We will do so in three steps. First, we set up a simple theoretical framework in the context of income taxation and show the basic difficulties in estimating behavioral responses to different types of tax variation. Second, we revisit

<sup>2</sup> We lay out our new approach while remaining agnostic about the underlying anatomy of behavioral responses to tax reforms, including shifting across tax bases ([Slemrod, 1995](#); [Gordon and Slemrod, 1998](#); [Pirttilä and Selin, 2011](#)), anticipation effects and shifting across time ([Goolsbee, 2000](#); [Kreiner et al., 2016](#)), the endogeneity of the responses to the design of the tax system ([Slemrod and Kopczuk, 2002](#); [Kopczuk, 2005](#); [Fack and Landaïs, 2016](#)), and the presence of optimization frictions ([Chetty, 2012](#)).

<sup>3</sup> The marginal net-of-tax rate is defined as  $1 - \text{the marginal tax rate}$ . Hence, changes in the marginal net-of-tax rate are proportional to changes in the after-tax wage rate. Our finding of heterogeneous elasticities would normally invalidate the point estimates. This is less of a case in our setting, where we have close to a pure control group untreated by the tax reform.

and reinterpret the standard estimation strategies covering both the original approach by Gruber and Saez (2002) as well as the more recent work by Weber (2014). Finally, we lay out a new estimation approach that allows for graphical validation and identification of behavioral responses to tax reforms.

### 2.1. Theoretical framework

To fix ideas we start by setting up a simple model for the “supply” of taxable income.<sup>4</sup> Each individual  $i$  at time  $t$  maximizes the following quasi-linear utility function

$$u_{it}(c_{it}, z_{it}) = c_{it} - \frac{n_{it}}{1 + \frac{1}{\varepsilon}} \left( \frac{z_{it}}{n_{it}} \right)^{1 + \frac{1}{\varepsilon}}, \quad (1)$$

subject to a potentially non-linear budget set

$$c_{it} \leq z_{it} - T_t(z_{it}; x_{it}), \quad (2)$$

where  $c_{it}$  is consumption,  $z_{it}$  is taxable income and  $x_{it}$  is a set of other variables, such as the number of children, marital status or underlying components of taxable income that may affect individuals' tax liability  $T_t(z_{it}; x_{it})$  in addition to their taxable income. The last term of Eq. (1) captures the disutility associated with earning income, which is governed by the parameters  $n_{it}$ , and  $\varepsilon$ . These parameters can – as we show below – be interpreted as individuals' counterfactual income in the absence of taxation (potential income) and the elasticity of taxable income with respect to the marginal net-of-tax rate.<sup>5</sup>

As in other studies of taxable income responses, we think of  $z_{it}$  as being a function of a range of underlying margins, such as hours worked, choice between pecuniary and non-pecuniary job attributes, form and timing of compensation, tax avoidance, and evasion, all of which may be part of individuals' responses to tax changes. Similarly,  $n_{it}$  captures all other fixed or time-varying factors that affect taxable income in addition to the marginal net-of-tax rate.

Utility maximization yields the first-order condition

$$z_{it} = n_{it}(\tau_{it})^\varepsilon, \quad (3)$$

where  $\tau_{it} = 1 - T_t'(z_{it}; x_{it})$  is the individual marginal net-of-tax rate in year  $t$ . Rewritten as log differences, Eq. (3) becomes

$$\ln z_{it} - \ln z_{it-1} = \varepsilon(\ln \tau_{it} - \ln \tau_{it-1}) + \ln n_{it} - \ln n_{it-1} \quad (4)$$

$$\iff \Delta \ln z_{it} = \varepsilon \Delta \ln \tau_{it} + \Delta \ln n_{it}.$$

Econometrically, we are interested in the causal effect of a change in the marginal net-of-tax rate on taxable income captured by the elasticity  $\varepsilon$  in Eq. (4). However, a naive estimation of Eq. (4) is unlikely to give causal estimates as the marginal tax rate is a (deterministic) function of the dependent variable and, hence, the error term (the change in potential income,  $\Delta \ln n_{it}$ ).

To break this endogeneity problem, researchers, starting with Auten and Carroll (1999), have employed a simulated instrumental variable (IV) strategy using tax reforms.<sup>6</sup> The basic idea in this approach is to use predicted changes in marginal net-of-tax rates ( $\Delta \ln \tau_{it-k}^p$ ) driven only by changes in the tax system as an instrument for  $\Delta \ln \tau_{it}$ . Hence, we compute

$$\Delta \ln \tau_{it-k}^p = \ln(1 - T_t'(z_{it-k}; x_{it-k})) - \ln(1 - T_{t-1}'(z_{it-k}; x_{it-k})), \quad (5)$$

which compare individual marginal tax rates in period  $t$  and  $t-1$  given the in-period tax system, but holding fixed taxable income and other characteristics at their  $t-k$  levels. With  $k=1$ , we use information at the beginning of the period over which we consider the change in the tax system. This corresponds to the approach in most earlier studies (Auten and Carroll, 1999; Gruber and Saez, 2002; Kleven and Schultz, 2014). With  $k>1$ , we use lagged information corresponding to the approach suggested by Weber (2014).

This basic IV strategy only yields causal estimates of  $\varepsilon$  if the instrument given by the predicted changes in marginal net-of-tax rates is uncorrelated with the changes in potential income

$$\text{cov}(\Delta \ln \tau_{it-k}^p, \Delta \ln n_{it}) = 0. \quad (6)$$

Formally, this assumes that the instrument is mean independent (Angrist and Pischke, 2008). In addition we need the standard assumptions laid out by Angrist et al. (1996). In particular, we require the tax reform to affect taxable income only through the changes in marginal tax rates (the exclusion restriction) as well as a significant first stage relationship between the simulated and actual changes in the marginal net-of-tax rate ( $\text{cov}(\Delta \ln \tau_{it-k}^p, \Delta \ln \tau_{it}) \neq 0$ ). The exclusion restriction holds by definition in the above theoretical framework as individual behavior given by Eq. (3) depends only on the marginal net-of-tax rate.<sup>7</sup> However in practice, the exclusion restriction is non-trivial as tax reforms typically change a number of institutional details in addition to the marginal tax rates, and must be evaluated case-by-case according to the specific tax reform.<sup>8</sup>

### 2.2. Why the basic IV strategy is unlikely to work

To think about the independence assumption (6), it is useful to distinguish between two types of tax variations. Similar to Kumar and Liang (2020), we can break up the total simulated tax variation into variation between groups with different income levels (*between-income tax variation*) and variation within groups with the same income level (*within-income tax variation*). Specifically

$$\underbrace{\text{var}(\Delta \ln \tau_{it-k}^p)}_{\text{Total Simulated Variation}} = \underbrace{\text{var}(\Delta \ln \bar{\tau}_{it-k}^p)}_{\text{Between-Income Tax Variation}} + \underbrace{\text{var}(\Delta \ln \tau_{it-k}^p - \Delta \ln \bar{\tau}_{it-k}^p)}_{\text{Within-Income Tax Variation}}, \quad (7)$$

where  $\Delta \ln \bar{\tau}_{it-k}^p = E(\Delta \ln \tau_{it-k}^p | z_{it-k})$  is the average reform-driven change in the net-of-tax rate given initial ( $k=1$ ) or lagged initial ( $k>1$ ) income  $z_{it-k}$ .

The between-income variation is created by reforms that change marginal tax rates differently for different income groups (such as a reduction in a top tax), while the within-income variation is created by reform changes in marginal tax rates that differ among individuals with the same initial income level (such as tax credits for individuals with children or treatment of itemized deductions). We distinguish between these two types of tax variation, because the empirical strategies differ in the two cases. Below we focus on between-income variation, which is the stereotypical tax variation considered in the literature (Saez et al., 2012; Weber, 2014), and we delegate the treatment of within-income variation to Section 2.6.

With reforms that only create between-income variation, Eq. (6) will most likely not hold. To see this, note that the instrument

<sup>4</sup> It is custom in the ETI literature implicitly to ignore the “demand” side of taxable income. In part this goes back to the idea of the ETI being a sufficient statistic for the computation of the deadweight loss (Feldstein, 1995; Feldstein, 1999), which rests on the assumption that the “price” of taxable income is fixed. If instead, for example, an increase in hours worked reduces the hourly wage rate, using the ETI will underestimate the deadweight loss.

<sup>5</sup> In the main analysis, we ignore the possibility of income effects and assume a homogeneous elasticity ( $\varepsilon$ ) across the population. We return to both of these points in Section 2.6.

<sup>6</sup> The DiD used by Feldstein (1995) was, in practice, a Wald estimator and, hence, not fundamentally different from the simulated IV strategy used in later research.

<sup>7</sup> Alternatively, we can think of  $n_{it}$  as also capturing the effects of other institutional features on individual taxable income.

<sup>8</sup> Finally, we require the stable unit treatment value assumption (SUTVA). This assumption is also non-trivial in the case of tax reforms due to, for example, spillover or general equilibrium effects. The identification of such effects require other sources of variation than we consider here (see, e.g., Zidar, 2019).

$\tau_{it-k}^p$  is a function of two elements: the change in the tax schedule created by the reform and the individuals' (lagged) initial income,  $z_{it-k}$ , which in turn is correlated with initial potential income,  $n_{it-k}$ . Hence, if a tax reform changes marginal tax rates in a way that is correlated with income, and if the changes in potential income ( $\Delta \ln n_{it}$ ) are correlated with the initial level ( $n_{it-k}$ ), the instrument is not independent.

Most tax reforms do change marginal tax rates in a way that is correlated with income because most reforms aim at adjusting the balance between efficiency and redistribution of the tax system and, hence, adjust the progressivity of the tax schedule.<sup>9</sup> Similarly, there are at least two reasons why changes in potential income are correlated with initial income:

- **Mean reversion:** if part of  $n_{it-k}$  is driven by temporary shocks, individuals with higher  $z_{it-k}$  are more likely to experience a negative change in potential income. Hence,  $E(\Delta \ln n_{it}|z_{it-k})$  is decreasing in  $z_{it-k}$ .
- **Differential secular income trends:** e.g., secular increases in inequality, where individuals with higher  $z_{it-k}$  on average have larger growth in potential income for reasons other than tax reforms. In this case,  $E(\Delta \ln n_{it}|z_{it-k})$  is increasing in  $z_{it-k}$ .

These potential biases are well known in the literature and, in most studies with  $k = 1$ , mean reversion appears to be severe and the dominant source of bias. When the variation in marginal tax rates predominately comes from larger reductions at the top of the income distribution, mean reversion biases the estimate downward, which is consistent with the negative elasticity estimates usually obtained in the literature when using the basic IV strategy (Gruber and Saez, 2002; Kopczuk, 2005; Kleven and Schultz, 2014; Giertz, 2015). By choosing  $k > 1$ , we are likely to reduce the biased created by mean reversion – but not differential income trends – as any transitory component in potential income (by definition) will play out over time and, hence, not affect changes in potential income sufficiently far into the future (Weber, 2014).

### 2.3. Reinterpreting the standard estimation strategy

To deal with the likely violation of independence in Eq. (6), researchers have – again dating back to Auten and Carroll (1999) – included controls for initial income ( $z_{it-k}$ ) in the estimation, which changes the assumption of independence to

$$\text{cov}(\Delta \ln \tau_{it-k}^p, \Delta \ln n_{it}|z_{it-k}) = 0. \quad (8)$$

Looking only at a single reform period (changes from before to after a tax reform) this condition is fulfilled trivially in cases with only between-income variation. In these cases,  $\Delta \ln \tau_{it-k}^p = \Delta \ln \tau_{zt-k}^p$  is a constant for a given level of initial income, and Eq. (8) thus holds regardless of the changes in potential income. However, controlling fully non-parametrically for initial income as implied in Eq. (8) also absorbs all identifying variation in the tax instrument in the first stage.<sup>10</sup>

There are two ways to break this deadlock. We can abandon the non-parametric controls for initial income and assume a functional form of the relationship between  $\Delta \ln n_{it}$  and  $z_{it-k}$ . Or we can employ more time periods to obtain variation in  $\Delta \ln \tau_{it-k}^p$  conditionally on  $z_{it-k}$ . Auten and Carroll (1999) only had one period

available and, hence, they were only able to control for initial (log) income linearly. Most subsequent studies have had access to longer panels and have thus been able to use less parametric specifications.

With longer panels,  $\Delta \ln \tau_{it-k}^p$  only varies due to the time variation created by tax reforms. Hence, a sufficient condition for Eq. (8) to hold is that the dynamic process of potential income given initial income does not change systematically over time. Formally, we can write this condition as

$$E(\Delta \ln n_{it}|z_{it-k}) = g(z_{it-k}) + \delta_t, \quad (9)$$

where  $g(z_{it-k})$  is some time-independent function of initial income describing the dynamic income process, and  $\delta_t$  is a common income growth rate.<sup>11</sup>

Eq. (9) is an assumption of constant trend differentials across the income distribution over time in the absence of tax reforms. In the absence of changes in the tax schedule, changes in taxable income are solely driven by changes in potential income, and Eq. (9) states that, relative to the overall growth in the economy ( $\delta_t$ ), the differences in income growth across the distribution ( $g(z_{it-k})$ ) should be constant. This is the key identifying assumption underlying the standard estimation strategy as employed by, for example, Gruber and Saez (2002) and Kleven and Schultz (2014).

### 2.4. Graphical validation and identification of behavioral responses

With the above insight, the identification of income responses and the validation of the identifying assumption underlying Eq. (9) are straightforward. For identification, we compare the changes in income trends for the parts of the income distribution affected by tax changes to the changes in income trends for the untreated (or less treated) parts of the distribution, while for validation, we can compare the changes in income trends for different subgroups within the untreated parts of the distribution. Under the assumption of constant trend differentials, we should observe no changes in trend differentials within the untreated (or less treated) parts of the income distribution.

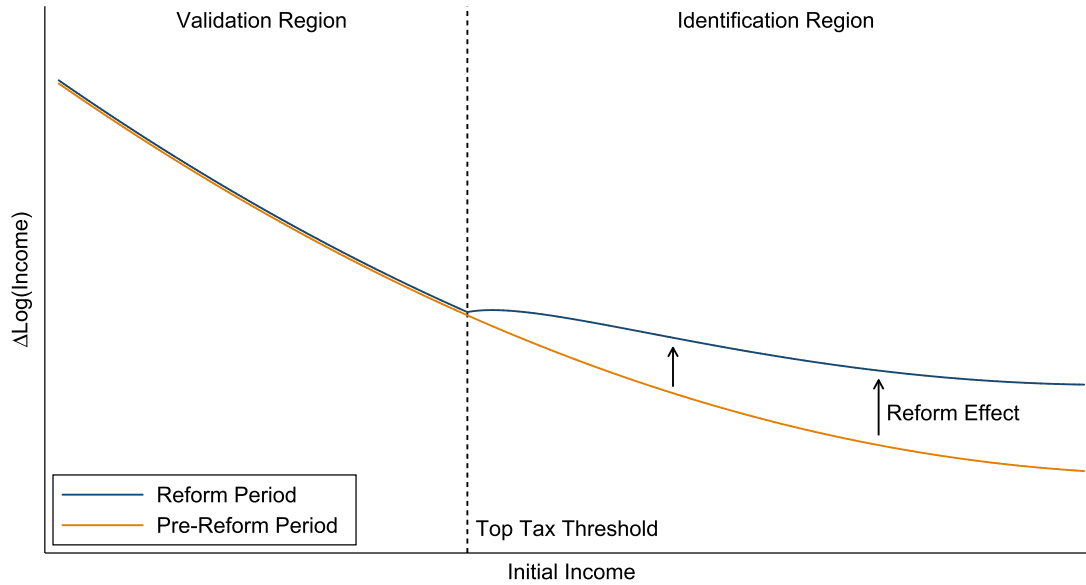
We illustrate our new approach in Fig. 1, which shows the growth in income across the income distribution for two time periods: a pre-reform period, where the tax schedule remains stable, and a reform period, where the top tax rate is reduced. In both the pre-reform and the reform period, we draw the growth in income as a decreasing function of initial income ( $z_{it-k}$ ). I.e., income trends differ across the income distribution with individuals at the top experiencing lower income growth, on average, than individuals with low initial income. This pattern is consistent with mean reversion being the dominant feature of the underlying income process. Importantly, we do not impose any functional form assumptions on the underlying income process, nor on the relative strength of mean reversion and differential secular income trends.

Next, we compare the trend differentials in the two periods, and for this purpose, we divide the income distribution into two regions. An untreated *validation region*, where the comparison of trend differentials serves the same validation role as the comparison of pre-treatment trend in DiD studies, and treated *identification region*, where we – under the identifying assumption – can interpret changes in trend differentials as behavioral

<sup>9</sup> A prominent example of this is the US tax act of 1986 (TRA86) studied by, for example, Feldstein (1995), which lowered marginal tax rates much more at the top of the income distribution than at the bottom.

<sup>10</sup> The first stage equation corresponding to Eq. (8) is  $\text{cov}(\Delta \ln \tau_{it-k}^p, \Delta \ln n_{it}|z_{it-k})$ , which is zero when  $\Delta \ln \tau_{it-k}^p = \Delta \ln \tau_{zt-k}^p$ .

<sup>11</sup> One way to think about Eq. (9) is to consider the a general process of potential income,  $\Delta \ln n_{it} = \tilde{g}_t(z_{it-k}, x_{it})$ , where the change in potential income can be a function of initial income and other covariates ( $x_{it}$ ), such as age, experience, children etc. and that this function may change over time. In this setting, there are two sufficient conditions for Eq. (9) to hold. First,  $\tilde{g}_t(z_{it-k}, x_{it})$  must be independent of time up to a constant:  $\tilde{g}_t(z_{it-k}, x_{it}) = \tilde{g}(z_{it-k}, x_{it}) + \delta_t$ . Second, the distribution of other (relevant) covariates conditional on initial income must be constant over time:  $F_t(x_{it}|z_{it-k}) = F(x_{it}|z_{it-k})$ . If the distribution of  $x_{it}$  is not constant, we can control for these in the estimations as discussed in Section 2.6.



**Fig. 1.** Illustration of the Identification and Validation Strategy. *Notes:* The figure shows  $E(\Delta \ln z_{it} | z_{it-k-1})$ , i.e., the changes in log income across the income distribution for two time periods: a pre-reform period, where the tax system remains stable and a reform period, where the top tax is reduced. The negative relationship between  $E(\Delta \ln z_{it} | z_{it-k-1})$  and  $z_{it-k-1}$  is consistent with mean reversion being the dominant - but not necessarily the only - feature of the underlying income process. Under the assumption that this pattern would have remained constant (relative to the overall growth in the economy) in absence of the reform, we can identify the reform effect from the differences between the reform and pre-reform periods for the population with initial income above the top tax threshold (the identification region). In contrast, we should observe no differences for the part of the population with initial income below the top tax threshold (the validation region). Thus, comparing the reform and pre-reform periods in this part of the income distribution acts as a placebo test for the validity of the identifying assumption.

responses to the reform. In our illustration, the trend differentials in the validation region follow the same pattern in the pre-reform period as in the reform period, while the trend differentials for the reform period lie above the pre-reform period. Thus, in our illustration we would find positive reform effects with clear validation of the identifying assumption.

Our graphical approach corresponds to the reduced-form of the IV estimation, and hence, the reform effect identified above is the intention-to-treat (ITT) effect of the reform, which relies solely on the assumption of independence given by Eq. (8). Given that the exclusion restriction also holds, we can obtain an estimate of the average elasticity of taxable income for the set of individuals, who stay within their income bracket and, hence, are treated as prescribed by the tax reform (the compliers), by scaling the ITT with the corresponding first stage.

In Fig. 1, we have drawn the reform effect as smoothly building up above the kink point, and there are two reasons why this is the empirically realistic scenario. First, with variation in potential income over time individuals close to the kink point are more likely to move out of their tax bracket. This translates into a smaller first stage and a smaller ITT even with a constant elasticity across the population. Second, abandoning the assumption of a constant elasticity, there are reasons to expect smaller behavioral responses around kink points. For example, in the presence of optimization frictions individuals may be unaware of the precise location of the kink point (Chetty, 2012; Rees-Jones and Taubinsky, 2020) and, hence, whether their marginal tax rate have changed following the reform. This translates into a lower effective elasticity and, hence, ITT close to the kink point.<sup>12</sup>

<sup>12</sup> With fixed potential income and perfect optimization, we would expect a sharp increase in income growth above the kink point. In this case we could in principle estimate the reform effect in a regression discontinuity design (RDD) locally around the kink point. The assumption of constant trend differentials is stronger than what is required for the RDD, but enables us to identify the elasticity of taxable income from observed behavior further away from the kink point.

## 2.5. A new estimation approach

Building on the discussion above, we layout the following three-step estimation procedure, which we implement in Section 4:

1. **Assignment of validation and identification regions:** Compare predicted between-income changes in marginal net-of-tax rates across the income distribution in the pre-reform and reform period. Identify validation region(s), where the marginal tax rates are (close to) stable in both periods, and identification region(s), where marginal tax rates change significantly in the reform period only.
2. **Comparison of trend differentials:** Compare income trends across the income distribution in the pre-reform and reform periods and examine if the assumption of constant trend differentials holds in the validation region(s). This can be done graphically as in Fig. 1 econometrically by estimating a flexible relationship between changes in taxable income and initial income in the two time periods and test for significant changes in trend differentials in the validation region.
3. **2SLS estimation:** Conditional on positive validation in step 2, we can obtain an estimate of the elasticity of taxable income by combining the Eqs. (4) and (9). This leads to the estimation equation

$$\Delta \ln z_{it} = \gamma_0 + f(z_{it-k}) + \gamma_2 D_{it}^{reform} + \gamma_3 \Delta \ln \tau_{it} + v_{it}, \quad (10)$$

where  $f(z_{it-k})$  is a flexible control function of initial income and  $D_{it}^{reform}$  is a dummy for the reform period, and where we instrument  $\Delta \ln \tau_{it}$  with  $\Delta \ln \bar{\tau}_{it-k}^p$ .<sup>13</sup> We can run this regression using either the entire sample, or using only particular parts of the

<sup>13</sup> By using  $\Delta \ln \bar{\tau}_{it-k}^p = E(\Delta \ln \tau_{it-k}^p | z_{it-k})$  as the instrument instead of the actual predicted tax changes,  $\Delta \ln \bar{\tau}_{it-k}^p$ , we only identify the elasticity from the between-income tax variation, which is the tax variation we can validate in our new approach.

validation and identification regions to investigate heterogeneity in the behavioral responses..

It is worth noting that as the regression in Eq. (10) fundamentally is the same as in the standard estimation strategy (Gruber and Saez, 2002; Kleven and Schultz, 2014), the estimated elasticities from our approach will not (necessarily) differ from the standard estimation strategy. However, without the validation provided by step 2, any aggregate elasticity from the standard estimation strategy may just as well be produced by changes in trend differentials within the validation region, which is a strong indication of violations of the identifying assumption. We illustrate this issue empirically in Section 4.

## 2.6. Extensions and additional considerations

*Using Lagged Initial Income:* In Fig. 1 above, we illustrated our new approach in a setting, where mean reversion was the dominant feature of the underlying income process, which most often is the relevant empirical setting when  $k = 1$ . With  $k > 1$  mean reversion becomes less severe, and picking a sufficiently high  $k$  we can hope to fully eliminate this source of bias (Weber, 2014). Hence, with  $k > 1$  and no secular income trends, the predicted tax changes  $\Delta \ln \bar{\tau}_{zt-k}^p$  may fulfill the basic independence assumption (6),  $\text{cov}(\Delta \ln \bar{\tau}_{it-k}^p, \Delta \ln n_{it}) = 0$ , without the need to control for initial income. We can validate the basic independence assumption using the same approach as described on Section 2.4. Under independence, the relationship between  $z_{it-k}$  and  $E(\Delta \ln z_{it-k} | z_{it-k})$  should be flat in untreated parts of the income distribution as illustrated in Fig. A.I in the Online Appendix. In our empirical application in Section 4, we focus on the baseline setting with  $k = 1$  and consider settings with  $k > 1$  in Online Appendix B.

*Adding Additional Controls:* Similarly to the standard estimation strategy, our new approach can incorporate additional controls such as changes in past income and more traditional controls for demographics etc. With additional controls, the assumption of constant trend differentials needs to hold conditional on the controls, which we can implement using the weighting strategy suggested by DiNardo et al. (1996) and adjust for changes in the distribution of  $\mathbf{x}$  using inverse propensity weighting.<sup>14</sup> Formally, we would estimate  $P(D_{it}^{\text{reform}} = 1 | \mathbf{x}, z_{it-k})$  and reweight the observations in the pre-reform period by  $P(D_{it}^{\text{reform}} = 1 | \mathbf{x}, z_{it-k}) / P(D_{it}^{\text{reform}} = 0 | \mathbf{x}, z_{it-k})$ . We implement this reweighting strategy for our empirical application in Fig. A.II in the Online Appendix.<sup>15</sup>

*Within-Income Tax Variation:* While our new approach focuses on the use of between-income variation, most tax reforms create both between and within-income tax variation. With within-income tax variation we do not face the same immediate problem of differential income trends due to mean reversion or secular income trends, as we can identify behavioral responses by comparing individuals with similar initial income (Kumar and Liang, 2020). Hence, we can – with a few adaptations – analyze this type of tax variation in a DiD framework either by splitting the population directly on the observables that drive the within-income variation (e.g. the number of children) or by using a tax simulator to compute the within-income change in marginal net-of-tax rate for each individual. Splitting the population directly on observables is arguably the

most transparent and elegant estimation strategy, but it typically requires that the tax variation is a simple function of a few variables.<sup>16</sup> Using a tax simulator is a “one-size-fits-all” approach that works irrespective of the source of the tax variation, but only by maintaining an element of black box in the estimation, where it is not clear what part of the reform variation is driven the estimates. We layout these points in Online Appendix C, where we also discuss related cases with disperse between-income tax variation.

*Heterogeneous Elasticities:* In practice, there may be a number of reasons why elasticities differ across the population. Behavioral responses to tax changes may, for example, be larger at the top of the income distribution due to income shifting, or they may be smaller close to tax thresholds due to optimization frictions such as imperfect knowledge of the exact location of the threshold etc. It is well-known that heterogeneous elasticities bias the elasticity estimates when there is no pure control group (with unchanged marginal tax rates), but only differences in treatment intensities. The same applies in our approach, but as we show in Section 4, it is often straightforward to investigate whether the elasticities are heterogeneous and, hence, to determine whether the estimates are likely to be biased.

*Income Effects:* Income effects are a particular reason why the (uncompensated) elasticities considered in our theoretical framework could vary across the population. Hence, as we lay out in Online Appendix D, we can use our new approach to investigate whether the heterogeneity in the elasticities follows the pattern predicted by the presence of income effects similarly to the detection of heterogeneous elasticities discussed above. In the case of a top tax reduction, we would expect the income effect to be larger for high-income individuals, who experience a larger mechanical reduction in tax payments. In contrast, individuals right above the top tax threshold experience only a marginal mechanical tax reduction. Hence, given the presence of income effects, we would expect the largest behavioral responses among the individuals right above the top tax threshold as these individuals almost exclusively respond to the change in the marginal tax rate (a positive substitution effect), while the behavioral responses of higher income individuals are more muted due to the negative income effect. However, the pattern of behavioral responses may be complicated by the presence of other sources of response heterogeneity, which make tax reforms in general less ideal to identify income effects compare to, for example, the study of lottery winnings (Imbens et al., 2001; Cesarini et al., 2017).

*Multiple Pre-Reform Periods:* We can use more pre-reform periods to validate the assumption of constant trend differential. In particular, additional pre-reform periods provide additional validation for the assumption of constant trend differentials further away from the validation region, where the comparison of trend differentials in the validation region are less informative. This is similar to the estimation of long-run treatment effects in DiD studies, where even small differences in pre-treatment trends become significant when extrapolated over long time periods (Kahn-Lang and Lang, 2020; Rambachan and Jonathan, 2020). The practical difficulty in using additional pre-reform periods is to find sufficiently long periods with a stable tax system. In our empirical application in Section 4, we consider changes in taxable income over 4-year periods. Hence, using two pre-reform periods ideally requires 8 years with no changes in the tax system. In Fig. A.III in the Online Appendix, we consider 2-year periods, which allows us to use two pre-reform periods as validation.

*Relationship with DiD Studies with Pre-Trend Corrections:* In some DiD studies researchers correct for differential pre-trends by

<sup>14</sup> An application of this control strategy can be found in Kawano et al. (2016).

<sup>15</sup> Simply adding controls to the regression Eq. (10) breaks the close link between the income definition relevant for the assessment of the constant trend differentials and the treatment status of individuals, as the identifying assumption with additional controls is constant trend differentials across the untreated part of the income distribution defined by residualized initial income. For this reason we prefer the weighting strategy.

<sup>16</sup> The large literature on the effects of the earned income tax credit (EITC) (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Kleven, 2020) is a prominent example of this approach.

**Table 1**  
Income Concepts in the Danish Income Tax System.

Income concept	Acronym	Main items included
Labor Market Income	LI	Salary, wages, honoraria, fees, bonuses, fringe benefits, business earnings
Broad Income	BI	LI + transfers, certain pension payouts, received alimony
Personal Income	PI	BI – labor market contribution, certain pension contributions <sup>a)</sup>
Capital Income	CI	Interest income, rental income, business capital income – interest on debt (mortgages, bank loans, credit cards, student loans)
Stock Income	SI	Dividends and realized capital gains from shares
Itemized Deductions	ID	Commuting, union fees, unemployment insurance contributions, other work expenditures, charitable donations, paid alimony
Taxable Income	TI	PI + CI – ID

a) Employer administrated contributions to annuity pension were completely tax deductible until 2010. After 2010 a cap of DKK 100,000 on contributions to expiring annuities was introduced, as described in the text.

estimating a linear trend on pre-reform data and subtracting this from the post-reform estimates (see, e.g., Jakobsen et al., 2020). Hence, the identifying assumption in these studies is also one of constant trend differentials. However, this approach is unlikely to work in the case of tax reforms when we assign individuals to treatment and control groups based on taxable income, as mean reversion creates very different income trends going towards and away from time of assignment. Moving away from the year of assignment, mean reversion tends to reduce the income of high-income individuals relative to low-income individuals, while income growth tends to be higher for the (to be) high-income individuals prior to assignment. Controlling for differential pre-trends in a standard DiD with this assignment of treatment and control groups will only exacerbate the bias created by mean reversion. This is the reason why we set up our new approach in *differences* instead of a standard DiD in *levels*.

### 3. Institutional setting and data

#### 3.1. Institutional setting

In Denmark, individuals are taxed according to a dual tax system with generally higher taxes on labor income (and transfers) than on capital income.<sup>17</sup> The system operates with six income concepts summarized in Table 1. The income concepts are labor market income (LI), personal income (PI), capital income exclusive of income from stocks (CI), stock income (SI), itemized deductions (ID) and taxable income (TI) with LI, PI, and TI constituting the main tax bases for labor income.<sup>18</sup> In addition, we have added broad income (BI) to the table, which we use to study different margins of response in our empirical analyses. Overall, these income concepts have remained stable over the time period 2000–12, which surrounds the 2004 and 2009–10 reforms we consider in our main application.

Table 2 shows the tax bases and associated marginal tax rates before and after the two reforms. The tax system consists of a number of flat elements levied on the entire base with only minor allowances (labor market contribution, regional taxes, bottom tax) combined with progressive elements created by the income thresholds of the middle and top tax brackets. The system creates three overall tax brackets, which we label *bottom*, *middle*, and *top*.

The tax rates shown in the table are cumulative such that a taxpayer in the top tax bracket is subject to the sum of all tax rates (except the tax on stock income). Hence, a top taxpayer faced a marginal tax of 63.0 percent on labor income before the 2009–10

reform and 56.1 percent after. This corresponds to a change in the net-of-tax rate of 17 log points. In contrast, the marginal tax rate for a bottom taxpayer is only reduced by 3 log points from 42.6 to 40.9 percent.

#### 3.2. Tax reforms

Fig. 2 illustrates the development in the marginal tax rates on labor income (Panel A) and income thresholds (Panel B) for the three overall tax brackets for a typical tax payer. There are a number of important points to take away from this figure. First, in the period leading up to the 2009–10 reform the income tax system was stable in terms of both marginal tax rates in each tax bracket and the bracket thresholds. Second, the 2009–10 reform affected the tax system in two ways. In 2009, the middle tax threshold was increased to be the same level as the top threshold, and in 2010 the middle tax bracket was abolished entirely.<sup>19</sup> Because the tax rates are cumulative, this affects all income above the (old) middle tax threshold, including income above the top tax threshold. Third, after the 2009–10 reform, the tax system is again relatively stable until 2012 with only minor changes in the top tax threshold. Taken together, these features come close to the stylized tax reform considered in Section 2 with a stable pre-period and reform that generates substantial between-income tax variation through the initial threshold increase and subsequent abolition of the middle tax.

In addition, it is worth noting that the 2009–10 reform introduced a cap on certain deductible pension contributions. Prior to the 2009–10 reform, all contributions to employer administrated annuity pensions were deducted from personal income and hence tax exempt at the point of contribution.<sup>20</sup> With the reform, the government introduced a cap of DKK 100,000 ( $\approx$  USD 15,000) on expiring annuities (typically with payouts over 10 years). Many employers reacted to the cap by automatically shifting contributions from expiring to non-expiring annuities, but as shown by Andersen (2018) many of the affected tax payers reduced overall pension contributions in response. In our analysis, this shifting of income will be captured as part of the behavioral responses to the 2009–10 reform, and for this reason, we consider income responses both with and without pension contributions in our empirical analysis in Section 4.

<sup>17</sup> This section extends the description of the Danish tax system provided by Kleven and Schultz (2014), who cover the tax reforms between 1984–2004.

<sup>18</sup> The duality is created by only taxing capital income as taxable income (TI), while labor income, in addition, is taxed as personal (PI) and labor income (LI). Only stock income (SI) is taxed completely separately from other income concepts. Capital income is negative for the majority of Danish taxpayers due to interest payments on mortgages and other loans, while the typical sources of positive capital income (e.g. stock income) are taxed separately. On top of the direct taxation of income, Denmark also levies a 25% VAT on close to all goods and services.

<sup>19</sup> The law abolishing the middle tax in 2010 was passed in parliament already in May 2009, which gave individuals more than half a year to shift income from 2009 to 2010 and thus avoid the middle tax. This created significant income shifting from 2009 to 2010 among certain groups of high-income earners (see Kreiner et al., 2016). We deal with these anticipation responses by primarily focusing on changes in income from 2008 to 2012. For the 2004 reform and the 2009 part of the 2009–10 reform, we are less worried about anticipation responses. The 2004 reform was initially passed in June 2003 with a gradual phase-in, but subsequently implemented fully in 2004. The increase in the middle tax threshold in 2009 (the part of the 2009–10 reform) was announced in September 2007.

<sup>20</sup> Employer administrated pensions in Denmark resemble the US 401(k) accounts with generally fully tax exempt contributions, while pension payouts are taxes as regular income (Chetty et al., 2014). Hence, top tax payers could often reduce their total taxes by shifting income from high-income employment periods to retirement as pensions contributions.

**Table 2**  
Tax Bases and Tax Rates over Time.

Tax type	Base <sup>e)</sup>	2004 Reform		2009–10 Reform	
		Pre-Reform Rate (%)	Post-Reform Rate (%)	Pre-Reform Rate (%)	Post-Reform Rate (%)
Labor Market Contribution <sup>a)</sup>	LI	8.0	8.0	8.0	8.0
EITC <sup>b)</sup>	LI	0.0	2.5	4.0	4.3
Regional Tax <sup>c)</sup>	TI	32.6	32.6	33.5	32.9
Bottom Tax	PI + [CI > 0]	5.5	5.5	5.5	3.7
Middle Tax	PI + [CI > 0]	6.0	6.0	6.0	0.0
Top Tax	PI + [CI > 0]	15.0	15.0	15.0	15.0
Marginal tax ceiling <sup>d)</sup>	–	59.0	59.0	59.0	51.5

a) Because labor market income enters the other tax bases net of the labor market contribution, the effective tax rate on labor income equals the statutory tax rate times (1 – the labor market contribution rate).

b) The Danish EITC is treated as an itemized deduction in taxable income (TI). Hence, the tax value of the earned income tax credit (EITC) is the EITC rate times the marginal tax on taxable income.

c) The regional tax includes municipal taxes and health contributions. The regional tax rate in the table is the average across all municipalities.

d) If the sum of all regional and national tax rates (excluding the stock income tax and the labor market contribution) exceeds the specified ceiling, the top tax is adjusted downward until the marginal tax rate equals the ceiling.

e) There have been a few minor changes to the tax bases over time, related to the treatment of capital income.

Compared to the 2009–10 reform, the 2004 reform comes after a more unstable pre-period with gradual declines in the marginal tax rates in all brackets and a gradual increase in the middle tax threshold, and instead of changing statutory tax rates, the main element of the 2004 reform is a significant increase in the middle tax threshold. Moving outside the 2000–12 window, the Danish tax system is affected by a number of other reforms. In Fig. 2, we also highlight the tax reforms in 1987 and 1994, which we analyze in Online Appendix E.

### 3.3. Data

We use administrative data for the full population of Denmark since 1980. The data combine several administrative registers (linked at the individual level via personal identification numbers) that contain detailed information on labor market history, education, earnings, and demographics with almost all income data being third-party reported (Kleven et al., 2011). However, our new approach does not rely on the availability of a rich set of control variables and is, therefore, implementable using data from tax authorities only. In addition, our new approach does not depend on the availability of large administrative data sets. In our empirical application in Section 4, we use the available statistics power to map out income trend differentials non-parametrically, but with less power we can implement our new approach in a much less granular manner as illustrated in Fig. A.IV in the Online Appendix.

Crucially, individual marginal tax rates are not observed in the data. Like most other papers in the empirical tax literature, we address this challenge by imputing marginal tax rates using a detailed tax simulator (similar to the NBER TAXSIM). For this purpose, we extend the model of the Danish tax system created by Kleven and Schultz (2014) and compute marginal tax rates as the increase in total household taxes due to an incremental increase in labor income.

For each reform, we construct a pre-reform period leading up to the reform and a reform period spanning the reform. As our baseline, we consider four year changes. For each period, we select individuals who are present in the tax data with positive income over the period considered and with initial income within a range around the tax variation created by the reform. For the 2009–10 reform we select individuals with personal income above DKK 250,000 (USD 37,000, 2019-level), while for the 2004 reform we include individuals with personal income starting already from DKK 200,000 (USD 29,000, 2019-level) as the main tax variation from this reform is located lower in the income distribution. However, the conclusions from our empirical analysis are not sensitive to the sample restrictions.

The sample restrictions leave us with over 2,000,000 individuals per period or approximately half of the Danish adult population, as shown in Table 3. In the table, we also show the descriptive statistics for the pre-reform and reform periods for the 2004 and 2009–10 reforms, respectively. These statistics are measured in the initial year of each period. Considering the 2004 reform, we see that the initial characteristics are stable from the pre-reform period (initial year 1999) to the reform period (initial year 2003). For the 2009–10 reform, we only include individuals with initial income above DKK 250,000, and hence, the samples are, on average, older, more likely to be married, and have higher income.

## 4. Graphical evidence of income responses

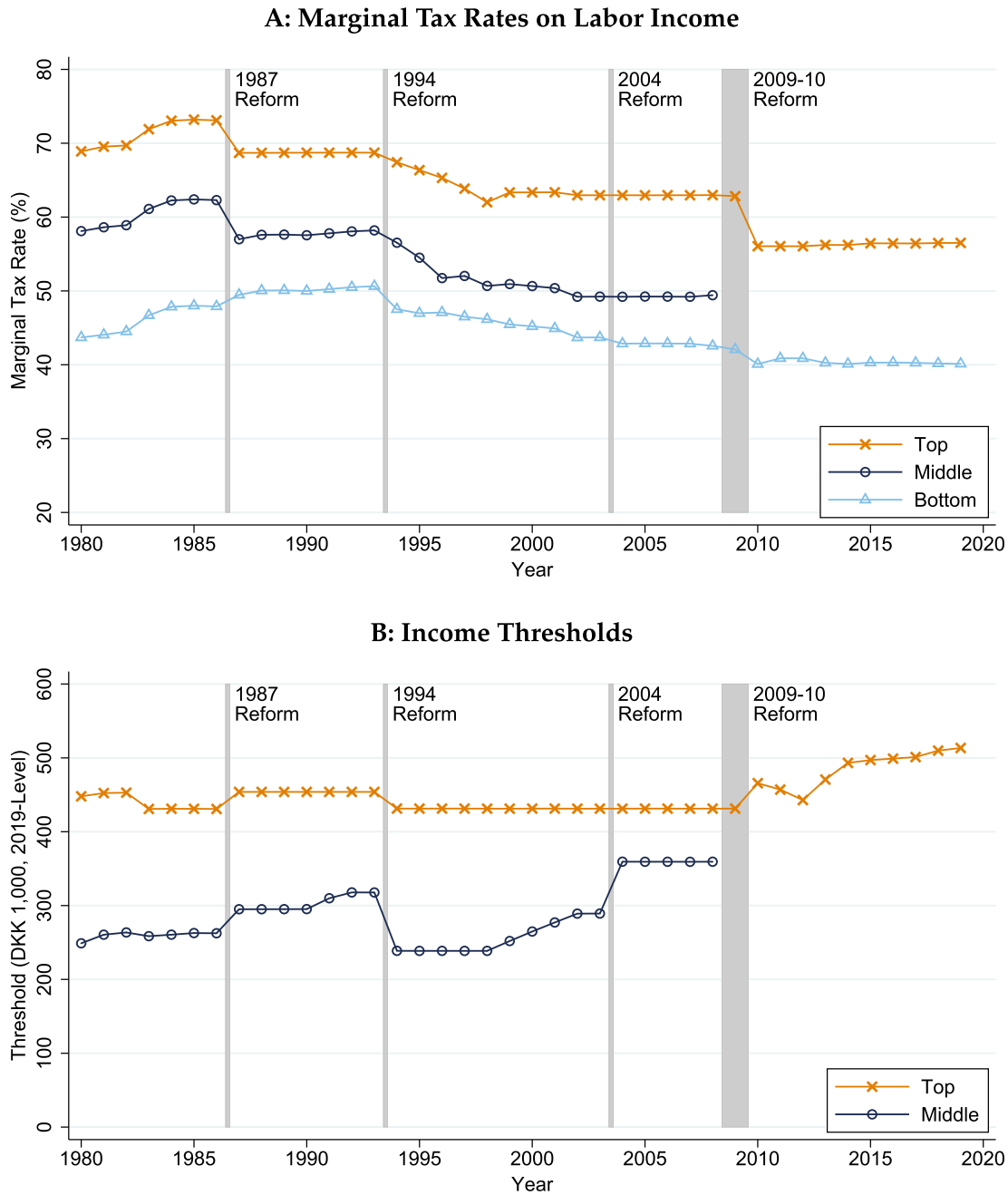
In this section, we turn to the empirical implementation of our new approach for studying income responses to taxation using the 2004 and 2009–10 reforms described in Section 3. We start by analyzing the 2009–10 reform as this is the largest and most salient of the two reforms and changes marginal tax rates in a way that comes close to the stereotypical reform considered in Section 2.

### 4.1. The 2009–10 tax reform

We implement our new approach following the steps layed out in Section 2.5. First, in Fig. 3, Panel A, we show the predicted changes in the marginal net-of-tax-rate across the income distribution for two periods.<sup>21</sup> In this step, we define the validation and identification regions. In the pre-reform period from 2004 to 2008, where the tax schedule was close to stable, we see close to no changes in marginal net-of-tax rates both across the income distribution and, as illustrated by the P10–P90 range, within a given income level. In the reform period spanning 2008 to 2012, the abolition of the middle tax increased the marginal net-of-tax rate by approximately 17 log points for the highest income individuals, while the average changes for individuals with incomes below DKK 350,000 are less than 5 log points.<sup>22</sup>

<sup>21</sup> Unless stated otherwise, we use personal income (PI in Table 1) as (taxable) income since this is the relevant income concept affected by the 2009–10 reform. We generally refer to taxable income responses as changes in the income concept subject to a specific tax change. Hence, it should not be confused with the legal concept of taxable income defined in the tax system (TI in Table 1).

<sup>22</sup> The spike around DKK 450,000 is caused by a small increase in the top tax threshold, which, together with the abolition of the middle tax, moved some individuals from the highest to the lowest tax bracket. The 2009–10 reform also created within-income tax variation due to the fact that the middle tax was based on couples' total income. Hence, differences in spousal income created differences in the income level at which individuals start paying the middle tax rate. We study this source of variation in Online Appendix C.



**Fig. 2.** Key Features of the Danish Tax Schedule 1980–2020. *Notes:* The figure shows the key features of the Danish tax system from 1980–2020. Panel A shows the marginal tax rates of the three (main) income tax brackets, while Panel B shows the income thresholds. The thresholds are deflated to 2019-levels using the implied wage indexes in the tax code. Hence, changes in the thresholds reflect active policy decisions. The figure highlights the four major reforms that we analyze. USD 1  $\approx$  DKK 6.8.

Second, in Panels B and C of Fig. 3, we investigate the income trend differentials across the income distribution in the pre-reform (2004–08) and reform (2008–12) periods to validate the assumption of constant trend differentials. To construct Panel B, we run the following regression separately for each period  $t$

$$\Delta \ln z_{it} = \beta_0^t + \beta_1^{t'} D_{it-1}^{inc} + v_{it}, \quad (11)$$

where  $D_{it-1}^{inc}$  is a vector of 50 initial income bin dummies based on initial income measured in 2004 or 2008. We exclude the dummy for the DKK 300,000 income bin, and hence, the coefficients  $\beta_1^t$  measure the differences in income growth (income trend differentials) between individuals with different initial income relative to the DKK 300,000 bin. Plotting  $\beta_1^t$  for the two periods produces the

empirical equivalent of our stylized Fig. 1 in Section 2. We consider changes in income over the full 4 year period to allow time for potentially gradual behavioral responses to materialize and to avoid short term income shifting affecting the estimates. In particular, we avoid capturing the anticipation effect of the 2010 abolition of the middle tax, which caused individuals to shift income from 2009 to 2010 as documented by Kreiner et al. (2016, 2017).<sup>23</sup>

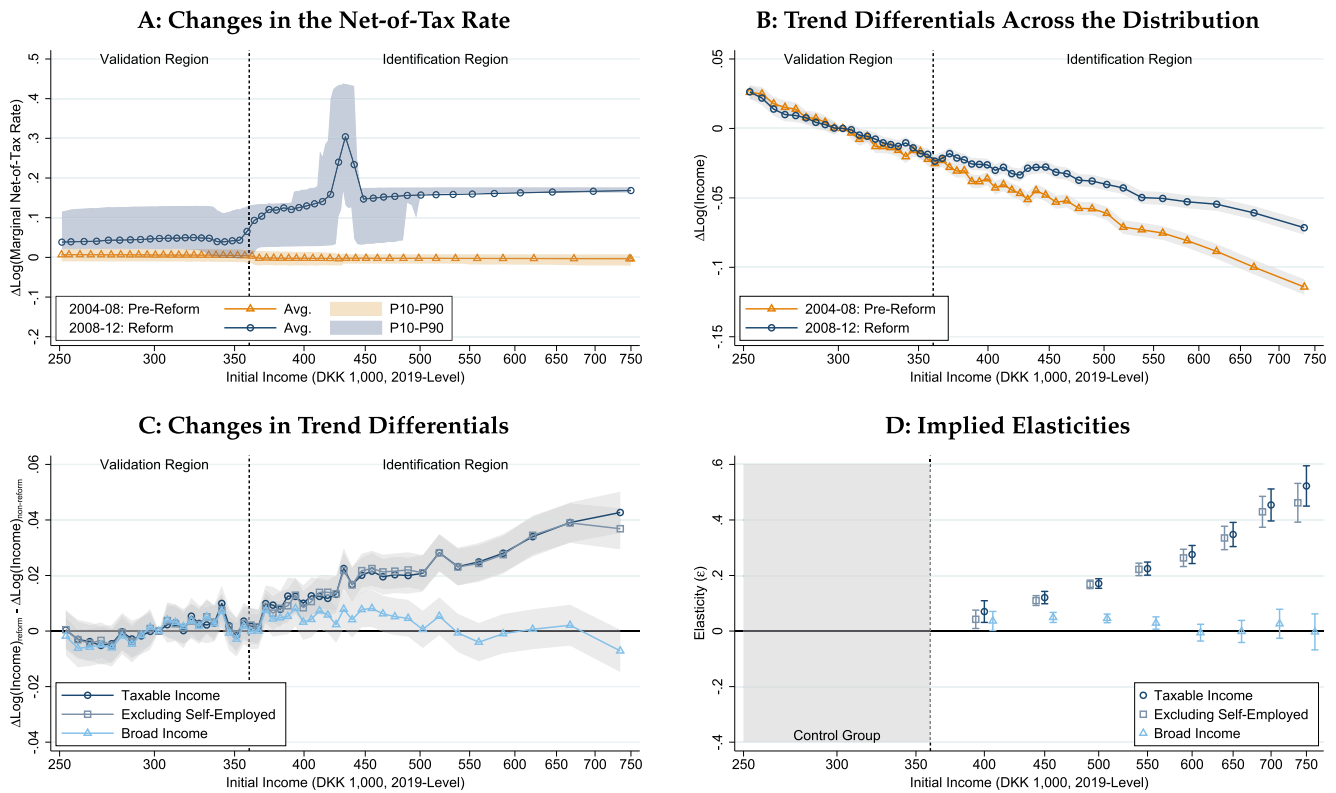
In Panel C we estimate the changes in trend differentials between the pre-reform and reform periods by running the regression

<sup>23</sup> In Fig. A.V and A.VI in the Online Appendix we consider income changes over 2 and 3 years to show how the 4-year effects on taxable income build up over time.

**Table 3**  
Sample and Initial Descriptive Statistics.

Panel A: 2004 Reform	Pre-Reform				Pre-Reform			
	P25	P50	Mean	P75	P25	P50	Mean	P75
Broad Income (DKK 1,000)	296	375	419	470	301	384	430	484
Personal Income (DKK 1,000)	279	350	389	437	276	345	386	433
Labor Income (DKK 1,000)	270	377	387	482	261	370	377	476
Capital Income (DKK 1,000)	-42	-16	-21	-0	-49	-21	-27	-2
Age	32	41	42	51	34	43	43	53
Self-Employed (%)	-	-	14	-	-	-	13	-
Married (%)	-	-	66	-	-	-	68	-
Number of Observations	2,240,332				2,295,922			
Panel A: 2009–10 Reform	Pre-Reform				Pre-Reform			
	P25	P50	Mean	P75	P25	P50	Mean	P75
Broad Income (DKK 1,000)	346	417	473	517	359	436	498	546
Personal Income (DKK 1,000)	313	373	419	457	322	386	434	473
Labor Income (DKK 1,000)	318	398	423	497	326	409	437	513
Capital Income (DKK 1,000)	-48	-24	-27	-3	-63	-31	-39	-4
Age	35	44	44	54	36	45	45	55
Self-Employed (%)	-	-	15	-	-	-	13	-
Married (%)	-	-	72	-	-	-	72	-
Number of Observations	2,097,862				2,240,833			

Notes: Monetary units are deflated to 2019-levels using the implied wage indexes in the tax code. Personal income, labor income, and capital income are defined in Table 1. Broad income corresponds to personal income before deductions. The variables are measured at the beginning of each 4-year period. For example, the pre-reform period in Panel A cover 1999–2003 and, hence, the variables are measured in 1999. USD 1 ≈ DKK 6.8.



**Fig. 3.** Identifying Income Responses Following the 2009–10 Tax Reform. Notes: Panel A shows the predicted changes in the log net-of-tax rate ( $\tau_{it-1}^p$ ) from 2004 to 2008 (the pre-reform period) and 2008 to 2012 (the reform period). The curves show the average changes within each income bin and the shaded areas show the 10th and 90th percentile ranges. Panel B shows the estimated income trend differentials using Eq. (11) for 2004–08 and 2008–12 relative to the average growth rate for incomes around DKK 300,000. Panel C shows the estimated changes in trend differentials based on Eq. (12) for different samples and income concepts. The use of different income concepts only affects the dependent variable (y-axis). Initial income always refers to personal income as defined in Table 1 and is measured pre-reform in 2004 and 2008. Panel D shows the implied elasticities over the identification region using the 2SLS local linear estimation described in Eq. (13). The gray shaded area illustrates the interval included as the control group in the elasticity estimations. Confidence bounds are based on robust standard errors. USD 1 ≈ DKK 6.8.

$$\Delta \ln z_{it} = \delta_0 + \delta_1 D_{it-1}^{inc} + \delta_2 D_{it}^{reform} + \delta_3 D_{it-1}^{inc} \times D_{it}^{reform} + v_{it}. \quad (12)$$

In this estimation, we pool the two periods in Panel B and include a vector of income bin dummies ( $D_{it-1}^{inc}$ ) interacted with a dummy for

the reform period ( $D_{it}^{reform}$ ). We exclude the interacted dummy for the DKK 300,000 income bin and, hence, the coefficients  $\delta_3$  capture the changes in income trend differentials across the income distribution relative to the change at this income level. We estimate

the changes in trend differentials for different groups and income concepts: *Taxable income*, which corresponds to the estimates from Panel B; *Excluding self-employed*, which excludes all individuals with income from self-employment; and *broad income*, which considers changes in taxable income before deductions, primarily consisting of employer administrated pension contributions.

From Panel B, we see the expected downward-sloping curves for both periods, which is consistent with mean reversion being the dominant (but not necessarily the sole) feature of the underlying income process. However, on top of this overall pattern, we see a marked difference between the pre-reform and reform periods. In the identification region, where individuals experience a large increase in the marginal net-of tax rate, the growth rate of taxable income in the reform period lies significantly above the growth rate in the pre-reform period (taking into account changes in the overall income growth rate). In contrast, we see essentially the same trend differentials in the two periods in the validation region, where individuals were largely unaffected by the tax reform.

Panel C isolates these changes in income trend differentials and is consistent with the identifying assumption of constant trend differentials as we observe no systematic changes in the validation region.<sup>24</sup> For the individuals in the identification region, in contrast, and we observe changes in trend differentials for taxable income that are strongly increasing in initial income, and assuming that the trend differentials across the entire income distribution would have remained constant in the absence of the reform, Panel C provides causal and non-parametric estimates of the taxable income responses to the 2009–10 tax reform. An assumption that appears reasonable given the constant trend differentials observed in the untreated part of the income distribution.

Finally, in Panel D of Fig. 3, we translate observed changes in trend differentials into income elasticities, but instead of estimating a single aggregate elasticity, we explore the heterogeneity across the income distribution by running a 2SLS local linear estimator centered at different points throughout the identification region.

Our implementation of the 2SLS local linear estimator follows a standard local linear regression. For each point of initial income in the identification region ( $h \in 400,000; 450,000; \dots$ ), we separately estimate a weighted 2SLS with the following second stage equation

$$\Delta \ln z_{it} = \gamma_0 + \gamma_1 D_{it-1}^{inc} + \gamma_2 D_{it}^{post} + \gamma_3 \Delta \ln \tau_{it} + \gamma_4 \Delta \ln \tau_{it} \times (z_{it-1} - h) + v_{it}, \quad (13)$$

where  $D_{it-1}^{inc}$  is a vector of DKK 10,000 income bin dummies.<sup>25</sup> In this equation we include the marginal net-of-tax rate both by itself and interacted linearly with initial income to capture both the level and slope of the income elasticity centered on  $h$ . Both of these terms are endogenous variables ( $\Delta \ln \tau_{it}$ ,  $\Delta \ln \tau_{it}(z_{it-1} - h)$ ) and we instrument these with the corresponding predicted between-income changes in the net-of-tax rate ( $\Delta \ln \bar{\tau}_{zt-1}^p$ ,  $\Delta \ln \bar{\tau}_{zt-1}^p(z_{it-1} - h)$ ).

For weights, we first assign individuals within the (less treated) validation region to a control group with a constant weight of 1 across all estimations. In the case of the 2009–10 reform, we select the entire validation region as illustrated by the shaded gray area in Panel D. For the treated individuals in the identification region

we assign, separately for each estimation, triangular weights ( $w$ ) within  $\pm$  DKK 50,000 of  $h$  computed as

$$w = \max(50,000 - |z - h|, 0) / 50,000. \quad (14)$$

Hence, with this weighting strategy we keep the control group constant and estimate heterogeneous elasticities by moving the treatment group up through the income distributions. Using this strategy, we estimate elasticities for taxable income that are increasing in initial income from below 0.1 for incomes around 400,000 to more than 0.5 for the top of the income distribution.<sup>26</sup>

Overall, Fig. 3 provides compelling evidence of behavioral responses to the 2009–10 tax reform, however, the substantial effect on taxable income could be driven by a range of factors from “real” labor responses reflecting unobserved efforts, occupational choice, hours worked etc., to avoidance or evasion. To separate out some of these factors, we start by excluding self-employed individuals, who typically have more room to change behavior through, for example, tax planning and retained earnings in the firm (see, e.g., le Maire and Schjerning, 2013). Consistently, we find slightly smaller effects when dropping individuals with income from self-employment, but overall the estimated trend differentials in Panel C and elasticities in Panel D are very similar to those found for the whole population. Next, we consider a broader income measure defined as our taxable income measure (personal income in Table 1) before deductions, which primarily consist of employer administrated pension contributions as discussed in Section 3. Looking at broad income, we find significantly smaller responses across the whole income distribution. Hence, most of the behavioral responses to the 2009–10 reform are likely driven by income shifting from pension contributions to taxable income. The responses in broad income are marginally significant for initial income around DKK 450,000, but insignificant at the top of the distribution.<sup>27</sup>

#### 4.2. The 2004 tax reform

Next, we turn to the 2004 tax reform, which we analyze following the same three steps as above. As mentioned in Section 3, the main feature of the 2004 reform was an increase in the middle tax income threshold, and in Panel A of Fig. 4, we see the largest changes in marginal net-of-tax rates for personal income between DKK 290,000 and DKK 360,000.<sup>28</sup> We define this interval as the identification region with validation regions both above and below.

In Panels B and C of Fig. 4 we examine the income trend differentials across the income distribution for the pre-reform period (1999–2003) and the reform period (2003–07).<sup>29</sup> Panel B reveals the expected downward sloping pattern consistent with mean rever-

<sup>26</sup> A finding of heterogeneous elasticities will normally invalidate the elasticity point estimates when a reform affects the entire income distribution with no pure control group but only differences in treatment intensities. Hence, in cases with no pure control groups, heterogeneity analyses such as the one above become particularly important as a way of validating the estimated elasticities. This is less of an issue in our setting, where the individuals in the validation region are only treated by small tax changes.

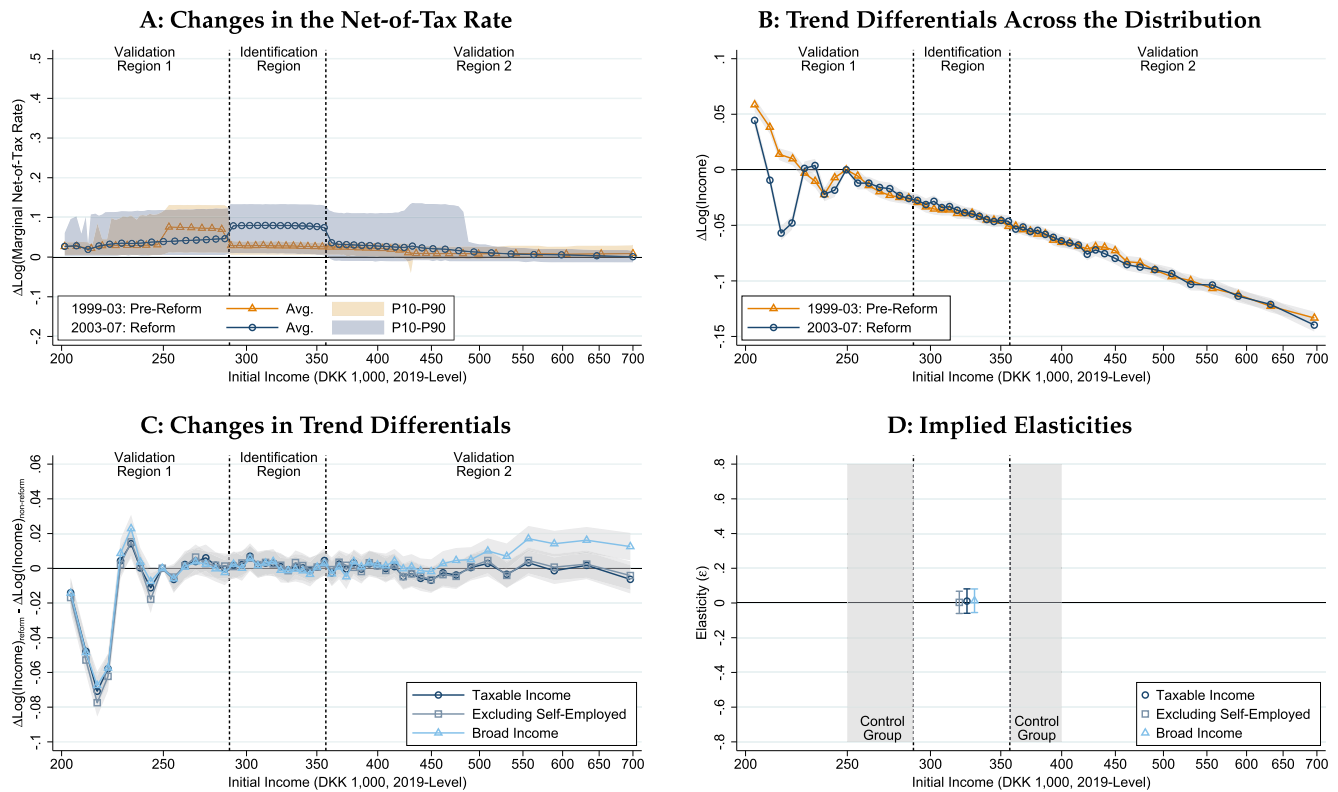
<sup>27</sup> Our finding that most of the behavioral responses are driven by avoidance is consistent with the findings in other papers that the scope for avoidance has significant influence on the estimated elasticities (Fack and Landais, 2016; Doerrenberg et al., 2017; Neisser, 2018). The heterogeneity in the responses in taxable income to the 2009–10 reform is consistent high-income individuals being more likely to be active savers and, hence, react more to the changed saving incentives (Chetty et al., 2014).

<sup>28</sup> Similar to the abolition of the middle tax in the 2009–10 reform, the increased threshold also created within-income tax variation due to the joint taxation of couples.

<sup>29</sup> It is worth noting that the reform period for the 2004 reform overlap with the pre-period for the 2009–2010 reform. Hence, strong behavioral responses to the 2004 reform risk confounding our analysis of the 2009–10 reform. In particular if they occur gradually. However, as we show below this is not the case.

<sup>24</sup> A standard F-test of  $\delta_3 = 0$  for taxable income in the validation region yields p-value of 0.0012. This finding of significant changes in trend differentials is driven by the dummy for incomes around DKK 340,000. Excluding this dummy yields a p-value of 0.1272.

<sup>25</sup> The income bin dummies run from DKK 250,000 and are capped at DKK 1,500,000, where the data becomes too thin for separate estimation of dummies. Hence, our regressions on the whole sample include 125 income dummies. Using instead the more fine grid DKK 1,000 dummies (1,250 dummies) does not change the results.



**Fig. 4.** Identifying Income Responses Using the 2004 Tax Reform. *Notes:* Panel A shows the predicted changes in the log net-of-tax rate ( $\tau_{it-1}^p$ ) from 1999 to 2003 (the pre-reform period) and 2003 to 2007 (the reform period). The curves show the average changes within each income bin and the shaded areas show the 10th and 90th percentile ranges. Panel B shows the estimated income trend differentials using Eq. (11) for 1999–2003 and 2003–07 relative to the average growth rate for incomes around DKK 250,000. Panel C shows the estimated changes in trend differentials based on Eq. (12) for different samples and income concepts. The use of different income concepts only affects the dependent variable (y-axis). Initial income always refers to personal income as defined in Table 1 and is measured pre-reform in 1999 and 2003. Panel D shows the implied elasticities over in the identification region using the 2SLS local linear estimation described in Eq. (13). The gray shaded area illustrates the interval included as the control group in the elasticity estimations. Confidence bounds are based on robust standard errors. USD 1  $\approx$  DKK 6.8.

sion, but notably we observe essentially no changes in income trend across the identification region from the pre-reform to the reform period. The only changes in trend differentials that stand out from Panel B are for initial income between DKK 200,000 and DKK 225,000, which is difficult to reconcile with being responses to the changes in taxation. The same is visible in Panel C where, in addition to taxable income, we also consider the changes excluding self-employed and changes in broad income. For broad income, we find slightly higher income trends for the top of the income distribution (above DKK 500,000), but similar to the change in income trends in the bottom, these are difficult to reconcile with the changes in taxation.

In Panel D, we translate the estimated trend differentials in Panel C into elasticities using the local linear 2SLS estimator described in Eqs. 13,14 above. However, as the reform creates only a narrow identification region, it is unfeasible to explore heterogeneity within this region so we only estimate a single point elasticity. A more relevant question when estimating elasticities from the 2004 reform is who to include in the control group. Based on the discussion above, and contrary to our analysis of the 2009–10 reform, we do not include the entire validation region. Instead, we select the gray income intervals immediately adjacent to the identification regions in Panel D as control groups. In this way, we avoid letting the changes in trend differentials observed at the very top and bottom of the income distribution affect the estimates, and from this we obtain a precisely estimated zero for both taxable and broad income. This result is not necessarily surprising given that the 2004 reform was smaller and, hence, less likely to prompt individuals to respond (Chetty, 2012).

The analysis of the 2004 reform illustrates a broader point about the selection of control groups. The assumption of constant trend differentials applies to the counterfactual situation, where the identification region were untreated by changes in the marginal tax rate. Constant trend differentials in the validation region provide support for this assumption, but similar to common pre-trends in DiD studies constant trend differentials in the validation region are neither necessary nor sufficient for the estimates to be causal (Kahn-Lang and Lang, 2020). Hence similar to other reduced-form methods, researchers have some discretion over what to consider relevant control groups. For the 2004 reform, we can either conclude that the constant trend assumption is violated, and hence, that any estimate is unlikely to be causal, or that there is support for the assumption locally around the validation region and accept an estimate of the elasticity around 0 as reported in Fig. 4. However, regardless of the conclusion, our approach increases the transparency of which choices are made.

#### 4.3. Results from the standard estimation strategy

Above, we analyzed the 2004 and 2009–10 reforms using our new approach, which allowed us to inspect the changes in trend differentials graphically and, hence, to validate the identifying assumption of constant trend differentials in the absence of tax reforms. To highlight the importance of such validation exercises, we examine the two reforms using the standard estimation strategy. More specifically, we run the following 2SLS regression separately for the 2004 and 2009–10 tax reforms

$$\Delta \ln z_{it} = \gamma_0 + \gamma_1' D_{it-1}^{inc} + \mu_2 D_{it}^{post} + \gamma_3 \Delta \ln \tau_{it} + v_{it}, \quad (15)$$

**Table 4**  
Estimates for the 2004 and the 2009–10 Reforms Using the Standard Estimation Strategy

	2004 Reform			2009–10 Reform		
	Taxable Income	Excluding Self-Employed	Broad Income	Taxable Income	Excluding Self-Employed	Broad Income
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Second Stage</i>						
$\Delta \log(\tau_{it})$	0.218 (0.046)	0.288 (0.041)	0.062 (0.039)	0.227 (0.009)	0.202 (0.007)	0.016 (0.007)
<i>First Stage</i>						
$\Delta \log(\tau_{it}^p)$	0.314 (0.005)	0.342 (0.005)	0.342 (0.005)	0.661 (0.002)	0.726 (0.002)	0.726 (0.002)
Positive Validation	÷	÷	÷	✓	✓	✓
Income Controls						
Time Periods	1999–2003 2003–2007	1999–2003 2003–2007	1999–2003 2003–2007	DKK 10,000 Bins 2004–2008 2008–2012	2004–2008 2008–2012	2004–2008 2008–2012
Observations	4,536,254	3,906,266	3,906,262	4,338,695	3,732,168	3,732,167

Notes: The table summarizes the results of the standard estimation strategy described in Eq. (15) for the 2004 and 2009–10 reforms. Positive validation refers to the graphical inspection of trend differentials in Fig. 3 and 4. Only for the 2009–10 reform we found support for the identifying assumption of constant trend differentials, and hence, only for this reform is the estimates using the standard estimation strategy likely to be causal. Robust standard errors in parentheses.

where  $D_{it-1}^{inc}$  is a vector of DKK 10,000 income bin dummies. This equation contains one endogenous variable ( $\Delta \ln \tau_{it}$ ), which we instrument with the corresponding predicted between-income changes in the marginal net-of-tax rate ( $\Delta \ln \tau_{it}^p$ ). We run the regressions on the same data and consider the same four year periods and outcomes as in Fig. 3 and 4 above. We present the results in Table 4.

Considering first the results for the 2009–10 reform in columns (4)–(6), we estimate an average elasticity of just above 0.2 for taxable income with (column 4) or without (column 5) self-employed individuals, while the elasticity for broad income is close to zero but still positive and significant. These estimates are well in line with the results from our graphical analysis above, which is not surprising given that we found that trend differentials were constant across the entire validation region in the case of the 2009–10 reform.

Next, considering the 2004 reform in columns (1)–(3), we find elasticities that are very similar to results from the 2009–10 reform. However, in our graphical analysis in Fig. 4 we saw no changes in trend differentials in connection with changes in tax treatment. Instead, the results we obtain from the standard estimation strategy are driven by the changes in trend differentials observed at the very top and bottom of the income distribution, which is less likely to reflect behavioral responses to the tax reform. Hence, in this case, the estimates from the standard approach are most likely biased.

In Online Appendix E, we analyze two additional tax reforms and essentially arrive at the same conclusion as for the 2004 and 2009–10 reforms. For a reform in 1987, we find support for the identifying assumption of constant trend differentials and positive responses for taxable income, while for the reform in 1994, we find clear violations of the identifying assumption. Summarizing the results from the standard approach for the four major Danish tax reforms since 1980, our point estimates are broadly consistent with the estimates found by Kleven and Schultz (2014). However, as highlighted by our new approach, we need to properly validate the assumption of constant trend differentials before assigning a causal interpretation to these estimates.

## 5. Conclusion

Behavioral responses to taxes are key inputs in the design of economic policy and serve as evidence on behavioral parameters

in economic models more broadly. In this paper, we revisited the identification of behavioral responses to tax reforms and developed a new approach that allows for graphical validation of key identifying assumptions and representation of treatment effects.

Considering stereotypical tax reforms, which change tax rates in one part of the income distribution, while keeping them constant in the rest of the distribution, we show that the standard estimation strategy employed by, for example, Gruber and Saez (2002) and Kleven and Schultz (2014), in essence, relies on an assumption that trend differences in income across the income distribution remain constant in the absence of reforms. Similar to the pre-trend validation of differences-in-differences studies, this identifying assumption of constant trend differentials can be validated by comparing the evolution of income in untreated parts of the income distribution over time.

We illustrate the importance of our new validation approach by studying a number of tax reforms in Denmark, with the 2004 and 2009–10 reforms as our main applications. Analyzing both reforms through the lens of the standard estimation strategy we find very similar average elasticities in the order 0.2 for taxable income and 0.01 for broad income. However, comparing the trend differentials for the pre-reform and reform periods, it is clear that only the results from the 2009–10 reform are likely to be causal. For this reform, we find that the income trend differentials remained stable in the untreated bottom part of the income distribution, which is consistent with the identifying assumption, while for the treated upper part of the income distribution, we find significantly higher income growth that is strongly increasing in initial income and somewhat larger in the medium run than in the short run. In contrast, we find no changes in trend differentials around the changes in tax treatment created by the 2004 reform. Instead, the elasticity estimates for this reform are driven by changes in trend differentials well within the control group, which are most likely unrelated to the reform. Once we account for these changes, we obtain a precisely estimated zero for both taxable and broad income.

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at <https://doi.org/10.1016/j.jpubeco.2022.104691>.

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