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Estimating Taxable Income Responses with Elasticity Heterogeneity*

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Abstract: Previous research on estimating the elasticity of taxable income (ETI) only allowed one source of heterogeneity. By additionally introducing elasticity heterogeneity, we show that existing methods yield elasticities that are biased and local. We propose a novel instrument based on tax rate changes across the entire taxable income distribution to overcome these shortcomings. We illustrate the empirical importance of elasticity heterogeneity by using the NBER tax panel for 1979-1990. Reconciling the remarkable divergence of estimates in the previous literature, we find that a positive elasticity heterogeneity bias explains previous high estimates, whereas overweighting of inelastic taxpayers accounts for previous low estimates. Estimation based on the new instrument yields a global ETI of around 0.7.

Keywords: elasticity of taxable income, elasticity heterogeneity, tax reforms, panel data, preference heterogeneity

JEL classification: D11, H24, J22

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1. Introduction

Taxable income responses to tax reforms capture multiple behavioral margins indicative of deadweight loss, e.g., labor supply, work effort, job location, tax avoidance and evasion. Following the seminal work of Feldstein (1995, 1999), a large body of literature emerged regarding estimation of the ETI defined as the elasticity of taxable income with respect to the marginal net-of-tax rate at the observed taxable income level.[1] This literature has generated a wide range of strikingly different estimates for similar methods applied to the same reform. For instance, extensive research on the impact of the tax cuts in the Tax Reform Act of 1986 (TRA86) has produced ETI estimates ranging from 0.2 to 3 (e.g., Feldstein, 1995; Auten and Carroll, 1999; Mofitt and Wilhelm, 2000; Gruber and Saez, 2002; Kopczuk, 2005; Weber, 2014). Even for a given method, the estimates are highly sensitive to using closely related specifications, e.g., Weber (2014) found ETI estimates between 0.8 and 1.5. Saez et al. (2012, Table 2) is another example with panel estimates of the ETI between 0.1 and 1 across similar specifications. Because the ETI has crucial implications for optimal tax design, the large variability of estimates is a puzzle that must be resolved (Giertz, 2009). Assessing the reliability and relevance of previous methods and estimates is essential for effectively analyzing implications of tax policy.

Methodologically, the previous taxable income literature focused primarily on quasi-experimental identification by making use of reform-induced differential changes in tax rates across groups or taxpayers with different levels of taxable income. However, a basic behavioral model was adopted that allowed one-dimensional unobserved preference heterogeneity to capture only variation in taxable income that is separable from tax rates. Thus, the ETI was assumed to be homogeneous, even as elasticity heterogeneity, due to skill or ability differences, is central in the optimal taxation literature (e.g., Mirlees, 1971) and is a key component in structural models of labor supply. Estimation-wise, Navratil (1995) noted that elasticity heterogeneity between treated and control groups receiving different tax rate changes would lead to substantial bias. Such heterogeneity also raises another issue: which taxpayers and tax rate changes contribute to the identification? Understanding this issue is crucial for evaluating the applicability of estimates in terms of generalizations.

In this paper, we introduce elasticity heterogeneity in the estimation of the ETI in the standard IV setting in first-differences and make four contributions.[2] First, we show that instruments used in the literature are generally invalid because they are endogenously determined by elasticity heterogeneity, which is a source of bias distinct from trend heterogeneity (non-parallel trends) that has received substantial attention. Second, we demonstrate that previous instruments, after appropriate corrections to account for bias, yield local elasticities representing weighted averages of taxpayer elasticities that are analogous to the local average treatment effects (LATE) in the treatment effects literature. These elasticities are typically far from being representative for the taxpayers reacting to the tax structure changes generated by the tax reforms in the data. Third, we propose potentially valid

[1] We will refer to the "marginal tax rate at the observed taxable income level" as "the observed tax rate", and the "net-of-tax rate" is one minus the tax rate. See Saez et al. (2012) for a review of the literature.

[2] While allowing elasticity heterogeneity, we do not recover its entire distribution. There are also other issues with the standard model that we do *not* particularly address, e.g., income shifting, distinguishing substitution and income effects, dynamic adjustment costs and frictions, and savings over the life cycle.

instruments, including a synthetic instrument for estimating a global ETI similar in spirit to the average treatment effect on the treated (ATT). The proposed synthetic average net-of-tax change instrument weighs net-of-tax changes at different income levels by the respective observed probability density, conveniently summarizing and exploiting the change in the entire tax structure. Finally, we explore the empirical implications of elasticity heterogeneity using the NBER tax panel for 1979-1990. Because this panel data set has been frequently used, it is ideal for quantifying the different sources of divergence across various methods. Just by allowing a second heterogeneity dimension, we not only reconcile the remarkable divergence in previous estimates but also obtain robust estimates.[3]

By way of background, the previous literature primarily used instrumental variable regression of the change in the log of taxable income on the change in the log of observed net-of-tax rate. Instruments are required because the observed tax rate is a function of taxable income; therefore, the change in observed net-of-tax rate is endogenous to the change in taxable income. Auten and Carroll (1999) and Gruber and Saez (2002) introduced the most widely used instrument which is the net-of-tax change constructed holding real taxable income fixed at the base-year taxable income level prior to the tax rate change. While the early panel study by Feldstein (1995) grouped taxpayers into two base-year income groups, subsequent studies typically made use of the entire continuous variation in base-year income. It is well-known that instruments are invalid if they are correlated with heterogeneous income trends due to mean reversion or widening income distribution. Various solutions have been implemented. Gruber and Saez pooled differences from multiple years with the purpose to primarily utilize *income-by-year* variation in tax rate changes by controlling for trends correlated with base-year income and year non-interacted. Others constructed instruments based on lagged income (Weber, 2014) or mid-year income (Blomquist and Selin, 2010) instead of base-year income. Burns and Ziliak (2017) grouped base-year net-of-tax change by demographic characteristics.

In our setting with elasticity heterogeneity, taxable income is endogenously determined by preferences and the tax structure. Therefore, taxpayers with different taxable income, ceteris paribus, have different elasticities, generating the elasticity heterogeneity bias that Navratil (1995) was concerned about for grouped instruments. We show that controlling for trends in base-year income as suggested in the literature only accounts for trend heterogeneity, whereas controlling for *year-specific* trends in base-year income is needed to account for elasticity heterogeneity. Identification then relies on the remaining variation in tax rate changes within income levels and years. Such variation exists because tax structure changes differ widely, e.g., across demographic characteristics such as state of residence. While the conventional belief has been that, holding income fixed, any remaining variation in tax rates across demographic groups must be limited, we demonstrate substantial variation for TRA86 using the NBER-TAXSIM model.[4]

[3] Blomquist et al. (2014) developed a non-parametric method that allows *general* heterogeneity of arbitrary dimensionality. Adding just a second dimension is a parsimonious exploration of the importance of heterogeneity. Furthermore, their setting in levels does not nest the standard first-difference setting.

[4] Net-of-tax changes at fixed taxable income levels, such as the first-dollar net-of-tax change, are also valid instruments exploiting such variation, but our average net-of-tax change instrument is stronger and has advantages that we soon describe.

From the literature on LATE, it is well-known that taxpayers with observed net-of-tax changes that highly correlate with the instrument (i.e., compliers) contribute more to the identification. We show that, after correcting for bias, the base-year net-of-tax change yields a local ETI estimate that overweighs relatively inelastic taxpayers who do not switch tax brackets and hence have identical observed and base-year tax rate changes. This is quite contrary to the prevailing view that such estimates capture the elasticities of more elastic high-income taxpayers.

The new synthetic average net-of-tax change instrument that we propose accounts for the fact that each taxpayer may switch tax brackets and is potentially affected by each of the possibly differential tax rate changes across the income distribution. We show that it yields a consistent global ETI that is representative of responses to tax structure changes in the data and that is the best approximation of the average ETI on the taxed. Furthermore, the reduced-form estimate of this instrument represents a policy elasticity (Hendren, 2016) that, compared with the standard ETI, is more indicative of behavioral responses and the marginal deadweight loss for nonlinear tax structures (Blomquist and Simula, 2016).

Our primary empirical finding is that the synthetic instrument yields an IV estimate of the global ETI of around 0.7.[5] This estimate is robust to controlling for year-specific trends in base-year income and demographics to account for potential remaining trend and elasticity heterogeneity with respect to these variables.

We find that the standard base-year net-of-tax change instrument yields an IV estimate of 0.26.[6] To illustrate the empirical significance of elasticity heterogeneity, we decompose this instrument into two components: (1) a part subject to elasticity heterogeneity bias that varies purely by base-year income and year and (2) a remainder that is stripped off income-by-year variation and free from such bias. We obtain estimates between 1 and 1.4 when only utilizing income-by-year variation and 0.2 when only exploiting the remaining variation. The discrepancy between the estimates reveals a positive elasticity heterogeneity bias. In comparison, the difference between the bias-corrected local base-year ETI estimate of 0.2 and our global ETI estimate of 0.7 demonstrates how unrepresentative a local ETI can be.

We also find that using instruments that vary across base-year income *groups* and years lead to similar biased estimates as using instruments that vary continuously across base-year income levels and years. However, using multiple instruments or instruments grouped by demographic characteristics alleviates the two opposing impacts of bias and localness.

Saez et al. (2012, p.28) offered two explanations for divergence across estimates in the literature. First, they argued that using continuous instruments, which also capture minor taxpayer-level tax rate changes, leads to lower estimates because taxpayers are less likely to respond to such changes. Second, they suggested that trend heterogeneity could account for much of the sensitivity in estimates across various methods. We find compelling evidence of alternative explanations. We show that the grouping estimates (1 to 3 in, e.g., Feldstein, 1995) were larger than the subsequent ungrouped estimates (0.2 to 1.5 in, e.g., Gruber and Saez, 2002; Weber, 2014) mainly because income-grouping discards substantial variation due to tax

[5] We use two-year differences as in Weber (2014) and drop differences with base-years in 1979 and 1980 to allow assessing two-year lagged-income instruments along her suggestions.

[6] Using a slightly different sample and specification (see the previous footnote), Gruber and Saez (2002) obtained a two-year difference estimate of 0.33.

structure changes, and therefore, suffers from a larger elasticity heterogeneity bias. We also show that the discrepancies between the ungrouped estimates are primarily due to the local ETI estimates being representative of different subsets of taxpayers.

Our analysis provides some general methodological insights – it shows that treatment effect heterogeneity can lead to severely biased and local estimates. These issues could be as severe as violations of the parallel trend assumption. Furthermore, if there is variation in treatment intensity within groups, using a within-group strategy can both serve as a diagnostic test and provide a solution.

The next section outlines our model with two-dimensional heterogeneity. Section 3 discusses issues with instruments in the literature and proposes solutions. Section 4 describes the data. Section 5 reports estimation results that reconcile previous estimates. The final section concludes.

2. Theory

The decision problem is such that the taxpayer chooses taxable income Y and consumption c to maximize utility $u(Y, c)$ subject to a budget constraint $c(Y)$. We assume a standard iso-elastic quasi-linear utility function:

$$u(c, Y|\beta_i, \alpha_i) = c - \frac{\exp(\alpha_i)^{-\frac{1}{\beta_i}}}{1+\frac{1}{\beta_i}} Y^{1+\frac{1}{\beta_i}}, \tag{1}$$

with a vector of preference parameters $\boldsymbol{e} = (\beta_i, \alpha_i)$ where subscript i indexes taxpayers. Under locally nonsatiated preferences, all net income is consumed in our static model.

The budget constraint depends on the tax (and transfer) structure according to:

$$c(Y) = Y - T(Y) + c_0, \tag{2}$$

where $T(Y)$ expresses net taxes as a function of taxable income and where c_0 is net income from sources other than taxable income. We assume that $T(.)$ is exogenous to c_0. Without loss of generality, the tax structure can be described by the (marginal) net-of-tax rate function $t(Y) = dc(Y)/dY = -dT(Y)/dY$. We work with the natural logarithms of Y and t:

$$y = \ln Y, \tag{3}$$

$$\tau(y) = \ln t(y). \tag{4}$$

To fully characterize a general budget constraint, we need c_0 and the sequence of net-of-tax parameters $\boldsymbol{\tau} = \left[\tau_j = \tau(y_j)\right]_{y_j \geq 0}$ consisting of the net-of-tax rate at the first dollar, the second dollar, and so on. Government policy decides $\boldsymbol{\tau}$ which is allowed to vary across taxpayers.

Utility maximization under a budget constraint with progressive tax rates yields a first-order condition that determines the (log of) observed taxable income y^*. Plugging y^* back into $\tau(.)$ yields the (log of) observed net-of-tax rate τ^*. We get the following system of simultaneous equations:

$$y^*(\beta_i, \alpha_i|\boldsymbol{\tau}) = argmax_y\, u[y, c(y)] = y(\tau^*|\beta_i, \alpha_i) = \beta_i\tau^* + \alpha_i, \tag{5}$$

$$\tau^*(\beta_i, \alpha_i|\boldsymbol{\tau}) = \tau(y^*|\boldsymbol{\tau}). \tag{6}$$

A consequence of quasi-linear utility is that we implicitly abstract from income effect that depends on c_0. The Slutsky condition with a positive substitution effect then implies $\beta_i \geq 0$. From the point of view of Eq. (5), $\beta_i = dy^*/d\tau^*$ represents the (both uncompensated and compensated) elasticity of taxable income with respect to the observed net-of-tax rate (ETI), whereas α_i represents potential taxable income without taxes (in which case $\tau^* = 0$).

We introduce unobserved preference heterogeneity through the error terms b_i and a_i and we let β and α be population-average parameters[7] according to:

$$\beta_i = \beta + b_i, \tag{7}$$

$$\alpha_i = \alpha + a_i, \tag{8}$$

with $E(b_i) = E(a_i) = 0$. Preference heterogeneity captures differences in taste for taxable income and reflects taxable income differences between taxpayers with the same tax structure. The error terms b_i and a_i capture variation in taxable income that are non-separable and separable from the tax rate, respectively. While we allow β_i to vary across taxpayers, we assume that it is constant for each taxpayer. We do not make any distributional assumptions. Most empirical work on taxable income allowed one-dimensional preference heterogeneity through α_i. The optimal taxation literature also typically assumed just one source of heterogeneity, but in this case, it is skill or ability heterogeneity that leads to heterogeneity in β_i (e.g., Mirlees, 1971; Saez, 2001). On the other hand, empirical work on labor supply that are structural regarding nonlinear tax structures typically allowed several error terms, but restricted to follow certain parametric distributions.[8]

Beginning with linear tax structures containing only one net-of-tax rate $\tau(y) = \bar{\tau}$, the first-order condition in Eq. (5) becomes: $y^* = \beta_i\bar{\tau} + \alpha_i$. Now, $\beta_i = dy^*/d\bar{\tau}$ and $E_e(y^*|\bar{\tau}) = \beta\bar{\tau} + \alpha$. Therefore, $\beta = E_e(\beta_i) = dE_{\boldsymbol{e}}(y^*|\tau^*)/d\tau^*$ represents the mean ETI which can be estimated by regressing y^* on $\bar{\tau}$ assuming statistical independence of $\boldsymbol{\tau}$ from $\boldsymbol{e}$ (Wald, 1947).

The taxable income literature handles tax nonlinearities by linearizing tax structures at observed taxable income levels. A rationale for this procedure is that the desired choice is the same on the linearized and nonlinear tax structures (Hausman, 1985; Mofitt, 1990). The first-order condition in Eq. (5) is a correlated random coefficient model (Hastie and Tibshirani, 1993). Making use of Eq. (7), we obtain $y^* = \beta\tau^* + b_i\tau^* + \alpha_i$. The problem of estimating β by regressing y^* on τ^* is that τ^* is correlated with the disturbance term $b_i\tau^* + \alpha_i$ as both are functions of $\boldsymbol{e}$.[9] Because τ^* is decreasing in y^*, the simultaneity bias is negative.

With panel data, taxpayer-specific separable heterogeneity can be differenced away. Let subscript t index years, and drop superscript * for observed variables for notational convenience. Then:

[7] For presentational simplicity, we will be sloppy on distinguishing population and sample quantities.
[8] Typically, error terms followed normal or extreme-value distributions, e.g., in Hausman-type methods (Burtless and Hausman, 1978; Hausman, 1985) and in discrete-choice methods (Dagsvik, 1994; van Soest, 1995; Hoynes, 1996; Keane and Mofitt, 1998).
[9] This model has similarities with the canonical return-to-schooling model in Card (2001), where y^* was earnings, τ^* was schooling, β_i was marginal return to schooling, and α_i was ability.

$$\Delta y = y_{iT} - y_{it}, \tag{9}$$

$$\Delta\tau = \tau_{iT}(y_{iT}) - \tau_{it}(y_{it}), \tag{10}$$

with $T = t + Dt$ where Dt is difference length and $\tau_{it}(.) = \tau(.|\boldsymbol{\tau}_{it})$. We let $\Delta\boldsymbol{\tau} = \boldsymbol{\tau}_{iT} - \boldsymbol{\tau}_{it}$ denote parameters of tax structure change.

We introduce dynamics in the preference errors in order to capture common panel complications. For clarity, let $\beta_{it} = \beta_i$ be fixed over time. On the other hand, let $\alpha_{it} = a_{it}^p + \alpha_{it}^v$ where superscripts p and v index permanent and transitory income components, respectively. We specify preference change according to $\alpha_{iT}^p - \alpha_{it}^p = g^p(\alpha_{it}^p) + \alpha_{it}^{p\epsilon}$ and $\alpha_{iT}^v - \alpha_{it}^v = g^v(\alpha_{it}^v) + \alpha_{it}^{v\epsilon}$ where g^p and g^v are systematic trends and $\alpha_{it}^{p\epsilon}$ and $\alpha_{it}^{v\epsilon}$ are error terms with $E(\alpha_{it}^{p\epsilon}) = E(\alpha_{it}^{v\epsilon}) = 0$.[10] In this setting, the vector of preference errors becomes $\boldsymbol{e} = (\beta_i, \alpha_{it}^p, \alpha_{it}^v, \alpha_{it}^{p\epsilon}, \alpha_{it}^{v\epsilon})$ and taxable income change becomes:

$$\Delta y = \beta_i \Delta\tau + \Delta\alpha, \tag{11}$$

$$\Delta\alpha = g(\boldsymbol{e}) = g^p(\alpha_{it}^p) + g^v(\alpha_{it}^v) + \alpha_{it}^{p\epsilon} + \alpha_{it}^{v\epsilon}, \tag{12}$$

Widening income distribution driven by such factors as trade or technological change leads to permanent income trends $g^p(\alpha_{it}^p)$ that is increasing in α_{it}^p. Mean reversion where taxpayers with high transitory income revert toward lower income levels leads to transitory income trends $g^v(\alpha_{it}^v)$ that is decreasing in α_{it}^v.

We can rewrite Eq. (11) as $\Delta y = \beta\Delta\tau + b_i\Delta\tau + \Delta\alpha$. The problem of estimating β by regressing Δy on $\Delta\tau$ is that $\Delta\tau$ is correlated with the disturbance term $b_i\Delta\tau + \Delta\alpha$ as both are functions of $\boldsymbol{e}$. Without reform ($\Delta\boldsymbol{\tau} = \mathbf{0}$), $\Delta\alpha$ is positively correlated with Δy and negatively correlated with $\Delta\tau$, because some taxpayers increasing their income switch to tax brackets with higher tax rates. This leads to a first-difference version of the negative simultaneity bias.

It is well known from Wooldridge (1997) and Heckman and Vytlacil (1998) that estimation with instrumental variables could yield consistent estimates for correlated random coefficient models. Let z denote the instrument. In Proposition 1, we decompose the IV estimate β^{IV} into a local ETI β^{LATE}, which is a weighted average ETI, a bias term $bias^b$ due to elasticity heterogeneity, and a bias term $bias^a$ due to trend heterogeneity. The elasticities are local in the same sense as the local average treatment effects (LATE) in the treatment effects literature (Imbens and Angrist, 1994; Angrist and Imbens, 1995). We also specify the instrument that recovers the global ETI (β^{ATT}) analogous to the average treatment on the treated (ATT) although this instrument is not generally implementable. We defer the discussion of feasible instruments and more intuition to the next section.

Proposition 1. Suppose tax structure change $\Delta\boldsymbol{\tau}$ is statistically independent of preferences $\boldsymbol{e}$.

a) For the first-difference specification in Eq. (11), the 2SLS-estimator yields an IV-estimate β^{IV} consisting of a local ETI β^{LATE}, an elasticity heterogeneity bias term $bias^b$, and a trend heterogeneity bias term $bias^a$ according to:

[10] Our specification encompasses the case where permanent income grows at a constant rate according to: $\alpha_{iT}^p = \alpha_{it}^p + g^p + \alpha_{it}^{p\epsilon}$; and the case where transitory income is serially correlated according to: $\alpha_{iT}^v = g^v\alpha_{it}^v + \alpha_{it}^{v\epsilon}$; where g^p and g^v are constants.

$$\beta^{IV} = \frac{Cov(\Delta y, z)}{Cov(\Delta\tau, z)} = \beta^{LATE} + bias^b + bias^a, \quad (13)$$

$$\beta^{LATE} = \frac{Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})]}{Cov[E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})]} = \int \beta_i w_{\beta_i}^{LATE} d\beta_i, \quad (14)$$

$$bias^b = \frac{E_{\Delta\boldsymbol{\tau}}[Cov(\beta_i \Delta\tau, z|\Delta\boldsymbol{\tau})] - \beta^{LATE} E_{\Delta\boldsymbol{\tau}}[Cov(\Delta\tau, z|\Delta\boldsymbol{\tau})]}{Cov(\Delta\tau, z)}, \quad (15)$$

$$bias^a = \frac{E_{\Delta\boldsymbol{\tau}}[Cov(\Delta\alpha, z|\Delta\boldsymbol{\tau})]}{Cov(\Delta\tau, z)}, \quad (16)$$

b) For the instrument:

$$z^{ATT} = E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}), \quad (17)$$

the IV estimate is the weighted average ETI on the taxed according to:

$$\frac{Cov(\Delta y, z^{ATT})}{Cov(\Delta\tau, z^{ATT})} = \beta^{ATT} = \int \beta_i w_{\beta_i}^{ATT} d\beta_i, \quad (18)$$

where $\beta^{ATT} E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ is the best linear approximation of $E_{\boldsymbol{e}}(\Delta y|\Delta\tau)$ given $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ in a minimum mean-square-error sense.

The weights $w_{\beta_i}^{LATE}$ and $w_{\beta_i}^{ATT}$ are given by Eqs. (A5) and (A7) in Appendix A with $\int w_{\beta_i}^{LATE} d\beta_i = \int w_{\beta_i}^{ATT} d\beta_i = 1$. Proof: See Appendix A.

We refer to any weighted average of β_i as an aggregate ETI. For a given instrument, β^{LATE} represents the aggregate ETI that could be recovered. It is identified by exogenous variation in changes in shapes of entire tax structures $\Delta\boldsymbol{\tau}$. The weight given to each value of β_i is $w_{\beta_i}^{LATE}$ which depends on the degree of compliance, i.e., the correlation between observed net-of-tax change $\Delta\tau$ and the instrument z due to variation in $\Delta\boldsymbol{\tau}$.

Instrument relevance requires z to be correlated with $\Delta\tau$. The two bias terms reflect correlations due to variation in preferences $\boldsymbol{e}$ conditional on $\Delta\boldsymbol{\tau}$. They are nonzero when z is correlated with either elasticity heterogeneity β_i or separable income trends $\Delta\alpha$. Relevance is achieved without violating the exclusion restriction if z is correlated with only tax structure variables.[11] Using the terminology of the treatment effects literature, $\Delta\tau$ measures treatment intensity and z measures treatment intention. Furthermore, β^{LATE} indicates the external validity of β^{IV}, wheras $bias^b$ and $bias^a$ indicate the internal validity of β^{IV}.

Suppose we could group $\Delta\tau$ by $\Delta\boldsymbol{\tau}$ and construct $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ without error. Then, the OLS-estimate of $E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau})$ or Δy on $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ would provide the average ETI on the taxed β^{ATT} analogous to the weighted ATT interpretation of regression estimates in the treatment effects literature when treatment intensity is continuous. With binary $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$, β^{ATT} would reduce to the simple ATT. The second part of Proposition 1 shows that using $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ as an instrument rather than as the main regressor also yields β^{ATT}. In practice, tax structure changes vary in a complicated way across group- and taxpayer-level characteristics such as demographics. Because $\Delta\boldsymbol{\tau}$ is high-dimensional, $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ cannot be constructed without

[11] This is similar to using arguably exogenous institutional characteristics as instruments for schooling in the return-to-schooling application.

error. However, we will suggest feasible instruments that are scalar functions of parameters in $\Delta\boldsymbol{\tau}$ to recover an aggregate ETI. To obtain a good approximation of β^{ATT}, we will suggest an instrument that mimics $E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$.

Different tax reforms yield different collections of $\Delta\boldsymbol{\tau}$. Because the weighting function $w_{\beta_i}^{LATE}$ in Eq. (14) depends on $\Delta\boldsymbol{\tau}$, β^{LATE} and β^{ATT} are mixtures of preference and tax parameters. Slemrod and Kopzcuk (2002) demonstrated this insight for the case without elasticity heterogeneity. For a given tax reform, a consistently estimated β^{ATT} accounts for the reform-specific compliance of each taxpayer and is in this sense representative for the taxpayers reacting to the tax structure changes in the data. On the other hand, β is a pure preference parameter. However, it is only representative for tax rate changes to linear tax structures or for the case where taxpayers never switch tax brackets.

The previous literature imposed a homogeneous elasticity in which case $\beta^{IV} = \beta^{LATE} = \beta^{ATT} = \beta$. With this abstraction, it is difficult to assess how local an estimate is. Such a restriction also implies $bias^b = 0$ and implicitly assumes away potential elasticity heterogeneity bias. Thus, all focus was on addressing the trend heterogeneity bias.

3. Different instruments

The most widely used instrument for panel data, introduced by Auten and Carroll (1999) and Gruber and Saez (2002), is the net-of-tax change constructed holding real taxable income fixed at the (pre-reform) base-year (taxable) income level y_{it}:

$$\Delta\tau_0 = \tau_{iT}(y_{it}) - \tau_{it}(y_{it}). \tag{19}$$

Across base years, tax structures often change due to tax reforms. Such changes are sometimes identical for all taxpayers in each given base year. However, *across* base-year income levels and years, even such reforms typically lead to differential tax rate changes.

The literature recognized that the base-year instrument in Eq. (19) can utilize income-by-year variation in tax rate changes. However, tax rate changes could also vary *within* a base-year income level and year, in reforms that lead to differential tax structure changes across taxpayers.[12] We can gain insights by disentangling the two sources of variation. Let $c(y_{it})$ be a smooth function of base-year income such as a polynomial or a spline and $\boldsymbol{\mu}_t$ be a vector of year dummy variables. We can then regress the instrument on the base-year function for each year separately, and decompose it into a prediction and a residual. Let $\hat{c}(y_{it})$ be a local polynomial fit and μ_t represent year-fixed effects. Then:

$$\Delta\tau_0 = z(y_{it}, \mu_t, \Delta\boldsymbol{\tau}_{it}) = \hat{c}(y_{it})\boldsymbol{\mu}_t + \hat{\varepsilon}, \tag{20}$$

$$\Delta\hat{\tau}_0 = z(y_{it}, \mu_t) = \hat{c}(y_{it})\boldsymbol{\mu}_t, \tag{21}$$

$$\Delta\tau_0^{\varepsilon} = z(y_{it}, \mu_t, \Delta\boldsymbol{\tau}_{it}|y_{it}, \mu_t) = \hat{\varepsilon}. \tag{22}$$

[12] With base-year income being continuous, we can think of a base-year income level as observations within a small income band around this income level.

The predicted net-of-tax change $\Delta\hat{\tau}_0$ is conceptually the expectation of $\Delta\tau_0$ conditional on base-year income and year and it varies only across income-year interactions.[13] Income-by-year variation can be utilized in other ways, e.g., by directly employing year-specific income functions as instruments, i.e., $\boldsymbol{z} = c(y_{it})\boldsymbol{\mu}_t$. The residualized net-of-tax change $\Delta\tau_0^{\varepsilon}$ captures the remaining variation in tax rate changes that is uncorrelated with year-specific base-year trends. While conventional belief is that this variation is limited, we later demonstrate substantial variation for TRA86 using the NBER-TAXSIM model.

In a setting with only one cross-section of differences, Feldstein (1995) used a method that corresponds to employing a binary instrument with $z = 1(y_{it} > \bar{y})$ where $\bar{y}$ is the top-income tax-bracket cutoff.[14] It has been previously noted that income-grouping in this way discards potential gradual minor variation in tax rate changes across multiple tax brackets. We additionally note that variation due to differential tax structure change is also discarded. Thus, income-grouped base-year instruments rely only on income-by-year variation.

In Figure 1, we provide a stylized TRA86 example with tax structures containing two tax brackets/segments $s = 1{,}2$. There is one pre-reform tax structure and two post-reform tax structures $k = A, B$. Net-of-tax rates are τ_t^s before the reform and τ_{kT}^s after the reform and we have net-of-tax changes $\Delta\tau_k^s = \tau_{kT}^s - \tau_t^s$. Two types of taxpayers are each observed twice in each bracket before and after the reform. For clarity, no taxpayer switches brackets. Four pre-reform observations with y_t^s and post-reform observations with y_{kT}^s generate the four differences $\Delta y_k^s = y_{kT}^s - y_t^s$ indicated by the arrows in the figure.

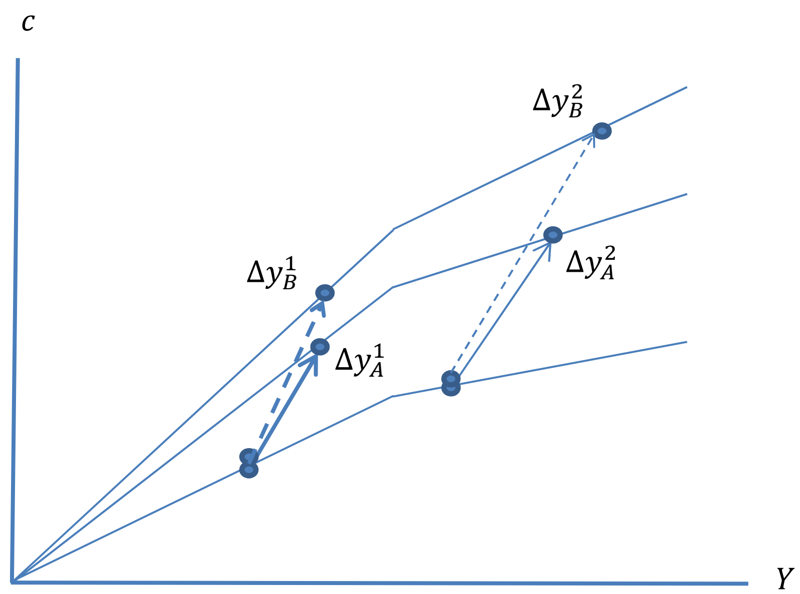

Figure 1. Stylized TRA86 example

[13] We reserve the term “predicted net-of-tax change” for $\Delta\hat{\tau}_0$ although some authors use it for $\Delta\tau_0$.

[14] Tax reforms can also be exploited with repeated cross sections and aggregated time-series. Lindsey (1987), Feenberg and Poterba (1993), Slemrod (1996), and Saez (2004) grouped individuals by their observed income. As Saez et al. (2012) noted, changes in group composition over time (e.g., due to stochastic trends $\alpha_{it}^{p\epsilon}$ and $\alpha_{it}^{v\epsilon}$) could be an issue without panel data.

In this example, $\Delta\tau_0 = \Delta\tau_k^s$, $\Delta\hat{\tau}_0 = \Delta\hat{\tau}_0^s = \Delta\bar{\tau}^s = 0.5(\Delta\tau_A^s + \Delta\tau_B^s)$, and $\Delta\tau_0^\varepsilon = \Delta\tau_{0,k}^\varepsilon = 0.5(\Delta\tau_k^s - \Delta\tau_{-k}^s)$ where $-k = B$ if $k = A$ and $-k = A$ if $k = B$. Predicted net-of-tax change $\Delta\hat{\tau}_0$ groups taxpayers by brackets (s). Taxpayers in different brackets receiving different average slope rotations are compared, i.e., $\Delta\tau_A^1$ and $\Delta\tau_B^1$ are related to $\Delta\tau_A^2$ and $\Delta\tau_B^2$. Residual net-of-tax change $\Delta\tau_0^\varepsilon$ groups taxpayers by tax structure changes (k). Taxpayers in the same bracket receiving different slope rotations are compared i.e., $\Delta\tau_A^1$ is related to $\Delta\tau_B^1$ and $\Delta\tau_A^2$ is related to $\Delta\tau_B^2$.

Using the predicted net-of-tax change as instrument yields Feldstein's (1995, Table 2) difference-in-differences estimator:[15]

$$\beta^{Feldstein} = \frac{\Delta\bar{y}^2 - \Delta\bar{y}^1}{\Delta\bar{\tau}^2 - \Delta\bar{\tau}^1}, \tag{23}$$

where both $\Delta\bar{y}^s$ and $\Delta\bar{\tau}^s$ are means over differences conditional on tax bracket. During the reform period, tax cuts increased net-of-tax rates by 42% ($\Delta\bar{\tau}^2$) for the top-income treated group and by 25% ($\Delta\bar{\tau}^1$) for the high-income control group. At the same time, income increased by 72% ($\Delta\bar{y}^2$) for the treated group and by 21% ($\Delta\bar{y}^1$) for the control group. The difference in income change of 51% divided by the difference in net-of-tax change of 17% yields his ETI estimate of 3. Under the assumptions of homogeneous trends $\Delta\bar{\alpha}^s = \Delta\bar{\alpha}$ and elasticities $\beta^s = \beta$, $\beta^{IV} = (\beta^2\Delta\bar{\tau}^2 + \Delta\bar{\alpha}^2 - \beta^1\Delta\bar{\tau}^1 - \Delta\bar{\alpha}^1)/(\Delta\bar{\tau}^2 - \Delta\bar{\tau}^1) = \beta$.

However, the differential income change between the groups could reflect heterogeneous trends unrelated to tax policy ($\Delta\bar{\alpha}^2 \neq \Delta\bar{\alpha}^1$ and in Eq. (16), $bias^a \neq 0$). Even with parallel trends, the additional income change of the treated group could reflect differential responses to the common (25%) net-of-tax change ($\beta^1 \neq \beta^2$ and in Eq. (17), $bias^b \neq 0$), as noted by Navratil (1995) and Saez et al. (2012, p.26). Because base-year income is determined by the first-order condition $y^* = \beta_i\tau^* + \alpha_i$,[16] the group assignment is endogenous and ceteris paribus, taxpayers with different elasticities have different base-year income. Rather than being a potential pitfall, consistency is unlikely and the bias is not limited to grouped instruments.

If another cross-section of pre-reform differences with the same two groups of taxpayers experiencing no tax rate changes ($\Delta\bar{y}^s = \Delta\bar{\alpha}^s$) is available, we could account for trend heterogeneity by subtracting the pre-reform difference from the post-reform difference for each group before applying Eq. (23). More generally, Auten and Carroll (1999) and Gruber and Saez (2002) suggested controlling for trends in base-year income, and in the case with pooled differences, also controlling for macro-economic shocks correlated with the timing of reforms. However, utilizing income-by-year variation conditional on general trends across years does not address elasticity heterogeneity which interacts with observed net-of-tax change that is year-specific.

For the net-of-tax changes in Feldstein's (1995) example, assuming parallel trends, we can show that the IV estimate never yields an aggregate ETI, i.e., $\beta^{IV} \notin (\beta^1, \beta^2)$. In Table 1,

[15] Without taxpayers switching brackets, the first-stage estimate is one and therefore by Eq. (13), $\beta^{IV} = \beta^{Feldstein} = Cov(\Delta y, \Delta\tau)/Var(\Delta\tau)$, which yields Eq. (23).

[16] For the simultaneous Eqs. (5) and (6), we can derive $dy^*/d\beta_i = \tau^*/[1 - \beta_i \partial\tau(y^*)/\partial y] \neq 0$. The bias depends on the correlation between β_i and α_i and vanishes only for a certain one-to-one mapping between the parameters implying one-dimensional heterogeneity.

we provide a few simple simulation examples with different elasticities across the two groups. We set mean ETI to 0.7 in each column in line with the global ETI that we estimate later. We find that when base-year income and elasticity are positively correlated, the elasticity heterogeneity bias is positive. The distribution of elasticities in column (5) most closely resembles the picture that will emerge from our empirical results and yields a positively biased ETI estimate of 1.9 not far from Feldstein's estimate.

Table 1. Simple simulations of elasticity heterogeneity bias

	(1)	(2)	(3)	(4)	(5)
β^1	1.0	0.8	0.7	0.6	0.4
β^2	0.4	0.6	0.7	0.8	1.0
$\beta^{Feldstein}$	-0.5	0.3	0.7	1.1	1.9

Notes: For the example in Figure 2, let $\Delta\bar{\tau}^1 = 0.25$, $\Delta\bar{\tau}^2 = 0.42$, and simulate $\Delta\bar{y}^s = \beta^s \Delta\bar{\tau}^s$. Now, for instruments that vary purely by base-year income and year, $\beta^{IV} = \beta^{Feldstein}$ is given by Eq. (23).

With elasticity heterogeneity, consistency can only be achieved in the special case where the control group is untreated, which is rare as new policy programs are often simultaneously introduced, some of which affects everybody.[17] While there is an awareness of the pitfall of treatment effect heterogeneity in the general policy evaluation literature,[18] potential bias has rarely been addressed.

The residualized variation is uncorrelated with income-by-year variation. We show in the following proposition that controlling for year-specific base-year trends yields consistent estimates. This can be done either by including year-specific income functions as covariates or by using the residualized net-of-tax change in Eq. (22) as instrument.

Proposition 2. The base-year net-of-tax change $\Delta\tau_0$ in Eq. (19) is an invalid instrument because it correlates with year-specific trends in base-year income. However:

a) Controlling for trends in base-year income overcomes the trend heterogeneity bias.
b) Controlling for year-specific trends in base-year income overcomes biases due to trend and elasticity heterogeneity and yields consistent estimates.

Proof: See Appendix A.

We now turn to the issue of external validity, i.e., which taxpayers and tax rate changes contribute to the identification of the local ETI by complying with the instrument. Taxpayers may be affected by tax rate changes at other income levels than the base-year income level, e.g. by responding to a base-year tax rate change and switching to an adjacent tax bracket. Despite this, it is tempting to believe that the consistent base-year ETI, obtained using the residualized instrument to correct for bias, is representative by making use of the tax rate change at different income levels for taxpayers with different base-year income. We show that

[17] For certain reforms, e.g., the American Taxpayer Relief Act of 2012 with an isolated tax rate change in the top-income tax bracket investigated by Kawano et al. (2016), this may be a reasonable approximation. But even then, implicit tax code revisions due to inflation lead to bracket-creep effects (Saez, 2003) in the control group.

[18] Eissa and Liebman (1996) is an example from the labor supply literature. Lone mothers with children were affected by EITC+TRA86, and lone mothers with children were only affected by TRA86. As they noted, a comparison of the two groups cannot yield the effect of EITC unless both groups responded equally to TRA86.

this local ETI in fact overweighs relatively inelastic taxpayers. The reason is that unlike switchers, non-switchers fully comply with the base-year instrument as their observed and base-year net-of-tax changes are identical, and non-switchers have lower elasticities than switchers.

Proposition 3. The base-year net-of-tax change $\Delta\tau_0$ in Eq. (19) gives greater weight to non-switchers who, ceteris paribus, have lower elasticities than switchers.
Proof: See Appendix A.

Unfortunately, the prevailing view is that existing ETI estimates are more representative of elastic taxpayers in the top end of the income distribution who experienced larger tax rate changes (e.g., Saez et al., 2012).

To illustrate Proposition 3, in Figure 2, we provide a similar example as in Figure 1 with tax structures containing two tax brackets $s = 1,2$. The difference is that all four taxpayers have the same base-year income in the first bracket. After the reform, the two taxpayers of type $g = 1$ stay in the first bracket, whereas the two taxpayers of type $g = 2$ switch to the second bracket, giving the differences indicated by the arrows in the figure. Assuming that there are no separable trends, we get $\Delta y_k^g = \beta^g \Delta\tau_k^g$. Thus, the switchers have higher elasticities, i.e., $\beta^2 > \beta^1$, as they have larger income changes and smaller net-of-tax changes. A completely inelastic taxpayer would never switch tax bracket.

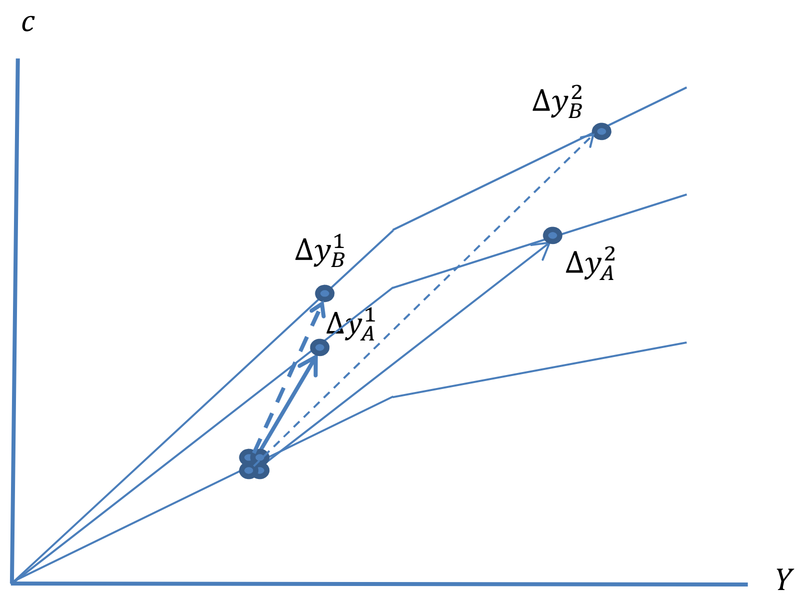

Figure 2. Compliance of non-switchers and switchers

The non-switchers comply with the first-bracket base-year net-of-tax change $\Delta\tau_k^{s=1}$ that equals the observed net-of-tax change $\Delta\tau_k^{g=1}$. On the other hand, the switchers comply with the second-bracket adjacent net-of-tax change $\Delta\tau_k^{s=2}$ as $\Delta\tau_B^{g=2} - \Delta\tau_A^{g=2} = \Delta\tau_B^{s=2} - \Delta\tau_A^{s=2}$. Thus, the base-year ETI $\beta^{IV} = (\Delta y_B^1 - \Delta y_A^1)/(\Delta\tau_B^1 - \Delta\tau_A^1) = \beta^1$ is identified by the least elastic taxpayers.

Several studies in the literature proposed alternative instruments similar in spirit to $\Delta\tau_0$ but replacing y_{it} with a different measure of income. Weber (2014) showed that net-of-tax change instruments based on lags of base-year income $y_{i,t-l}$ mitigates mean reversion in the limit as l increases, since $y_{i,t-l}$ becomes independent of temporary income. In our application, we use the two-year lagged net-of-tax change:

$$\Delta\tau_{-2} = \tau_{iT}(y_{i,t-2}) - \tau_{it}(y_{i,t-2}). \tag{24}$$

To account for widening income distribution, Weber included a spline in lagged base-year income as covariates to proxy permanent income trends. Blomquist and Selin (2010) made similar remarks about mean reversion and used mid-year income to construct the instrument. However, along the lines of Proposition 2, even such alternative instruments are correlated with elasticity heterogeneity, an issue that can be addressed by controlling for year-specific trends in the measure of income used. In the example in Figure 1 without switchers, grouping by base-year, lagged or mid-year income yield identically biased estimates.

Several grouping methods in the labor supply literature exploit variation in tax rate changes across demographic characteristics. In the EITC-application by Eissa and Liebman (1996), single mothers were grouped by whether they had children. In the labor supply application by Blundell et al. (1998), grouping was based on cohort-education interactions conditional on the non-interacted trends. Burns and Ziliak (2017) provided a recent taxable income application that grouped the base-year net-of-tax change instrument by state-cohort-education interactions conditional on the non-interacted trends. Even at the level of demographic groups, variation in tax rate changes may stem from either group-level differences in income or tax structure change. Because variation in income between groups is much smaller than between taxpayers, the elasticity heterogeneity bias is smaller for these methods. Conditioning on income-year trends still provides a remedy for remaining bias.

While variation in tax structure changes across demographic characteristics is less contaminated than variation in income, heterogeneous preferences across these characteristics may still threaten identification. Consistency can be achieved by controlling for year-specific *demographic* trends. Identification is then based on the remaining variation within demographic groups due to, e.g., interaction between demographic characteristics.

Rather than isolating exogenous variation from invalid instruments, we can directly construct instruments as functions of only parameters of tax structure change. For instance, we can use the net-of-tax change at some taxable income level y_j that is fixed across taxpayers as an instrument:

$$\Delta\tau_j = \tau_{iT}(y_j) - \tau_{it}(y_j). \tag{25}$$

The first-dollar net-of-tax change is an example and its level version has been widely used in the literature on estimating tax price impact on charitable contributions, 401(k) contributions, capital gains realization, and labor supply.[19] While valid, estimates from fixed-income instruments also represent largely local elasticities. For instance, relative to high-income taxpayers, low-income taxpayers are more likely to comply with a first-dollar net-of-tax change.

[19] See Triest (1998) and Keane (2011) for a review of the labor supply literature.

To estimate a global ETI, we need to account for the fact that that each taxpayer may switch tax brackets and is potentially affected by each of the possibly differential tax rate changes across the income distribution. One method is to use multiple $\Delta\tau_j$ at different y_j as instruments. Our preferred method is, however, to construct a single synthetic average net-of-tax change instrument that is a weighted average of $\Delta\tau_j$ across $y_j = j * 1000$ USD:

$$\sum p_j \Delta\tau_j = \sum_{j=0}^{200} p(y_j)[\tau_{iT}(y_j) - \tau_{it}(y_j)]. \tag{26}$$

Empirically, we approximate the income distribution by 201 intervals. The weights can be chosen in sophisticated ways to obtain elasticities representative for different taxpayers and types of tax reforms. We suggest using the empirically observed unconditional probability density function p_j as weighting function. We show that this choice yields an IV estimate of the global ETI representing the best approximation of the average ETI on the taxed. Furthermore, the reduced-form estimate of Δy on $\sum p_j \Delta\tau_j$ represents a weighted average elasticity with respect to changes in net-of-tax parameters across the income distribution. Additionally, making use of all available variation in tax rate changes due to tax structure changes improves instrument strength and precision.

Proposition 4. Among linear combinations of net-of-tax changes, the weighted average net-of-tax change $\sum p_j \Delta\tau_j$ in Eq. (26) has the following favorable properties:

a) It yields an IV estimate that is the best approximation of the average ETI on the taxed β^{ATT} in Eq. (18).
b) The reduced-form estimate of Δy on this instrument represents a policy elasticity that is a weighted average elasticity with respect to the set of $\Delta\tau_j$ across y_j.

Proof: See Appendix A.

The average net-of-tax rate conveniently summarizes tax structure change. It also has an intuitive interpretation: for a given tax structure and income distribution, varying tax rates so that $\sum p_j \Delta\tau_j$ changes by 1% also mechanically (before any behavioral response) alters observed net-of-tax change by 1%. In the examples in Figures 1 and 2, $\sum p_j \Delta\tau_j = 0.5(\Delta\tau_k^1 + \Delta\tau_k^2)$, which only varies across differences with different tax structure changes, as for the residualized instrument in Eq. (22).[20]

Our reduced-form equation relates taxable income change to the entire policy vector of tax structure change. In contrast, it is not entirely clear how a tax policy can, ceteris paribus, implement a net-of-tax change at the observed or base-year income for every taxpayer. Our reduced-form estimate can be interpreted as an average elasticiy for an across-the-board tax rate change that accounts for bracket-switching.[21] Hendren (2016) called such elasticities,

[20] The literature sometimes used "average net-of-tax rate" as a synonym for the participation net-of-tax rate up to the endogenously determined observed income level. On the other hand, the participation net wage between zero and *fixed* hours of work used by Eissa and Hoynes (2004) is free from such endogeneity.

[21] With progressive tax rates, an across-the-board tax rate change of 1% lead to observed tax rate changes of less than 1% due to bracket switching.

which varies across tax reforms, "policy elasticities".[22] Hendren (2016) and Blomquist and Simula (2016) showed that for nonlinear tax structures, such elasticities better capture behavioral responses and the marginal deadweight loss, which is an efficiency measure comparable across tax reforms. For linear tax structures, it is well-known from Feldstein (1999) that the ETI, which in this case equals the policy elasticity, is a sufficient statistic for efficiency analysis. While there are strong arguments for estimating policy elasticities, in the remainder of the paper we focus on the ETI for meaningful comparisons with estimates in the previous literature.

4. Data

We use data from the NBER panel of tax returns from 1979 to 1990, also known as the Continuous Work History File, which is the same data as used in a series of papers, e.g., by Gruber and Saez (2002), Kopczuk (2005), and Weber (2014). It contains detailed administrative information on taxes and income variables, and it includes a limited set of demographic variables. Gruber and Saez provided a detailed description of the data.

An important source of variation in tax structure changes during the sample period comes from the Tax Reform Act of 1986 (TRA86) which simplified the tax structure by reducing the number of tax brackets from 15 in 1986 to five in 1987 and just two in 1988, with the top marginal tax rate declining from 50% to 28%. The act also eliminated the second-earner deduction and income averaging, and it increased the personal exemption from USD 2,160 in 1986 to USD 3,800 in 1987 and the standard deduction from USD 3,760 in 1987 to USD 5,000 in 1988. The Economic Recovery Tax Act of 1981 provides another source of variation in this data set.

We construct tax structures for taxpayers by computing marginal tax rates accounting for federal, state, and payroll tax rates at different income levels using NBER-TAXSIM. We vary earnings in steps of USD 1,000, keeping income from other sources fixed. Deductions/itemizations that vary between taxpayers are accounted for in the construction of the tax structures.

We use two of the most common measures of taxable income previously used in the literature: actual taxable income (almost exactly as technically defined in the tax forms) and broad income. Broad income is a comprehensive definition of income that includes, among other things, wage income, interest income, dividends, and business income. Taxable income consists of broad income minus a number of deductions. We use the constant definition of taxable income, as applicable to the year 1990, and include all adjustments that can be computed from the data for all sample years. Our taxable income measures are identical to the ones used by Gruber and Saez (2002) and Weber (2014).

One margin of behavioral response to tax reform is to shift income between different sources, e.g., between taxable income and other components of broad income. If income composition is endogenous to tax structure change, estimates based on taxable income could

[22] In general, policy prediction of behavioral responses from preference estimates requires knowing the error-term distribution and simulations are needed to account for tax nonlinearities (Keane, 2011). Blundell and Shephard (2012) simulated a policy elasticity.

be misleading, but those based on broad income would still be largely valid. The taxable income estimates do, however, incorporate the effects of tax avoidance, which is also a margin of interest for policy and efficiency evaluations. In the presence of income shifting, Chetty (2009) showed that for linear tax structures, the marginal deadweight loss depends on a weighted average of the taxable income and broad income elasticities.[23]

Our sample selection criteria are similar to the ones in Gruber and Saez (2002). We drop filers that changed filing status and observations with missing values. In the baseline specification, we also truncate our data from below by dropping observations with less than USD 10,000 (1990 price level) to avoid issues with filing thresholds. Following Weber (2014), we use two-year differences for the differenced variables.[24] Because two-year lags in base-year income are used to construct some of the instruments and control functions, we use data with base years from 1981 to 1988 with 28,386 observations.

In Table 2, we report variable means and standard deviations for our sample. The first section reports the statistics for the dependent variables: changes in taxable and broad incomes; and for base-year taxable and broad incomes. In the second section of rows, we first report statistics for the main observed net-of-tax change regressor $\Delta\tau$. We then report the statistics for the instruments: base-year net-of-tax change $\Delta\tau_0$, its predicted component $\Delta\hat{\tau}_0$, its residualized component $\Delta\tau_0^{\varepsilon}$, and the second-lag net-of-tax change $\Delta\tau_{-2}$. The instruments were described in Eqs. (19) to (22) and (24).

Table 2. Sample statistics

	Logged variables		Non-logged variables	
Variable	Mean	Std. dev.	Mean	Std. dev.
Taxable income change	-0.078	0.589	586	34,143
Broad income change	-0.029	0.297	383	35,747
Base-year taxable income	10.295	0.622	37,325	42,166
Base-year broad income	10.756	0.518	55,262	50,740
Observed net-of tax change	0.029	0.137	1.708	8.061
Base-year net-of tax change	0.030	0.080	1.678	4.461
Predicted net-of tax change	0.028	0.035		
Residualized net-of tax change	0.002	0.070		
Second-lag net-of-tax change	0.026	0.075	1.480	4.303
Average net-of-tax change	0.060	0.107	3.432	7.066
Net-of-tax change at USD 10,000	0.056	0.137	3.298	9.201
Net-of-tax change at USD 25,000	0.061	0.133	3.378	8.633
Net-of-tax change at USD 50,000	0.068	0.117	3.743	7.345
Net-of-tax change at USD 100,000	0.068	0.121	3.751	7.150
Single-filing singles			0.279	0.448
Joint-filing couples			0.668	0.471
Other filing status			0.053	0.224
No children			0.570	0.495
One child			0.155	0.362
Two children			0.187	0.390
At least three children			0.087	0.282

Notes: Monetary outcomes are in USD (1990 price level). Changes in tax rate variables are in percentage points.

[23] Doerrenberg et al. (2014) showed similar consequences when deductions are endogenous.
[24] The overall picture that emerges from our results is the same when using three-year differences.

In the third section of rows, we report the statistics for our preferred synthetic (density-weighted) average net-of-tax change instrument $\sum p_j \Delta\tau_j$ and the net-of-tax changes Δy_j at the four fixed income levels y_j equaling USD 10,000, 25,000, 50,000, and 100,000, respectively. These instruments were described in Eqs. (25) and (26). While we report logarithmic versions of the variables used in the regressions in the second and third columns, we report the non-logarithmic versions in the fourth and fifth columns to give a better sense of magnitudes, when applicable. In the fourth section of rows, we report the shares of taxpayers with different filing status and number of children (dependents).

The mean taxable income is USD 37,325, which is considerably lower than the mean broad income of USD 55,262. The standard deviations are similar in magnitude to the means. The mean changes in these variables are small, but the standard deviations of the changes are almost as large as their level versions. The mean of the main regressor, observed net-of-tax change, is 1.708 percentage points, which is similar to the means of the base-year and second-lag instruments. The standard deviation of the main regressor is about five times its mean. In comparison, the standard deviations of the instruments are roughly half of that of the main regressor.

The standard deviation of the residualized instrument (0.070) is double that of the predicted instrument (0.035). Therefore, the main variation in the base-year instrument does not come from variation across but rather from variation within base-year (taxable) income levels and years. The means of the fixed-income instruments are between 3.298 and 3.751 percentage points and are increasing in income. The standard deviations are all higher than the means, indicating that there is plenty of variation in tax structure changes. For the synthetic instrument, the mean is 3.432 percentage points, and the standard deviation is about twice the mean. The demographic variables show that most of the sample consists of married joint filers and filers without children.

In Figure 3, we explore the amount of variation in tax structure changes by plotting marginal tax rate changes across base-year income levels for a joint-filing family with two children claiming standard deductions in California, New York, and Texas. We do so for the difference generated by TRA86 with base-year in 1986. The figure illustrates that the state of residence alone is a significant source of variation in the tax structure.

In Figure 4, we explore the state-by-year reduced-form relationship between changes in taxable income and the average net-of-tax rate, by plotting our differences grouped by state-year interactions. We state-demean and year-demean all observations. The tax rate changes (measured along the x-axis) correspond to yearly across-the-board weighted averages of the type of tax rate changes shown in Figure 3. Clearly, changes in taxable income and the tax structure, as measured by the average net-of-tax change, are positively correlated.

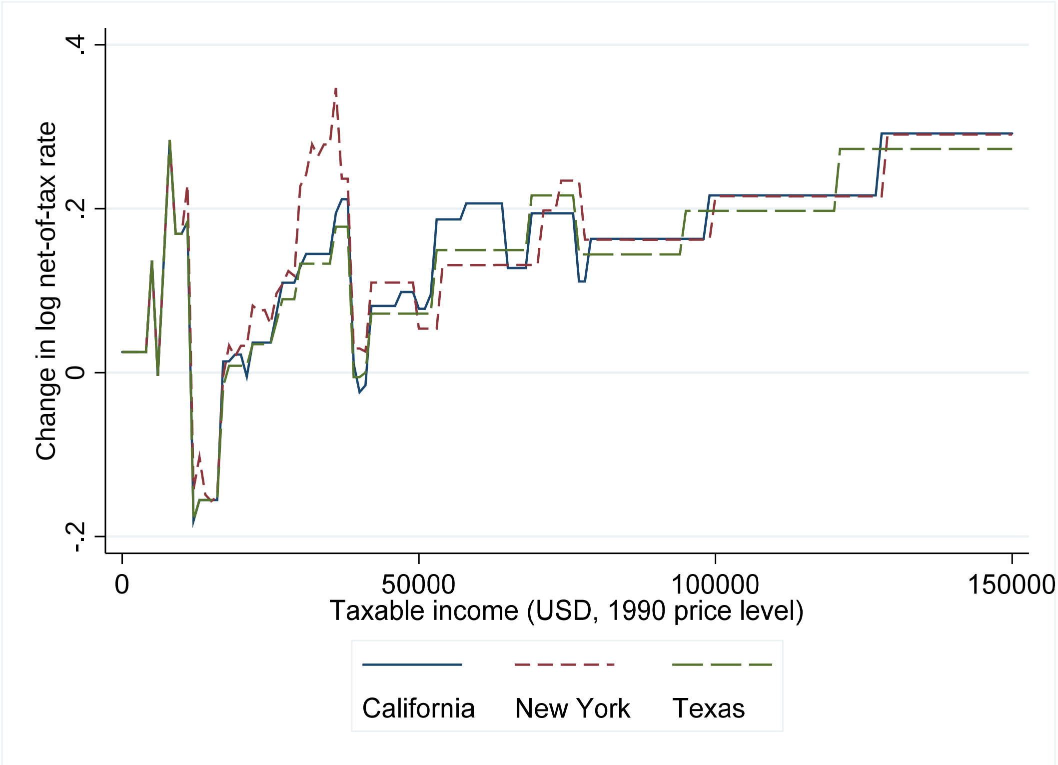

Figure 3. Marginal net-of-tax changes 1986-1988 for a joint-filing family with two children

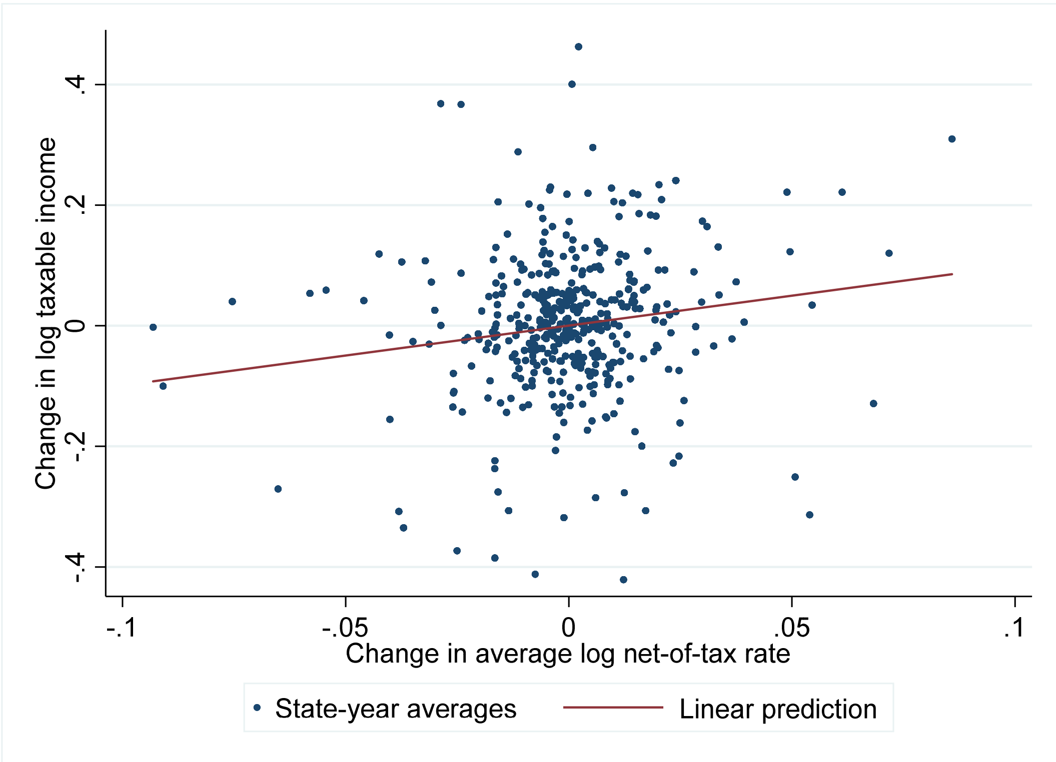

Figure 4. State-year relationship: changes in taxable income and the average net-of-tax rate

Note: The observations are state-demeaned and year-demeaned before the group-averaging

5. Empirical results

In Table 3, we report structural IV estimates of the ETI. In the first section, we begin with the base-year net-of-tax change instrument $\Delta\tau_0$, We continue with the predicted net-of-tax change instrument $\Delta\hat{\tau}_0$, and then we use year-specific base-year income splines as instruments to utilize the same source of income-by-year variation in tax rate changes more flexibly. Thereafter, we use year-specific top-5% income dummy variables (fixed cutoff at USD 83,118) as instruments to provide an example of income-grouped instruments. In the final rows of the first section, the instruments are the income-by-year residualized net-of-tax change $\Delta\tau_0^{\varepsilon}$, and then the second-lag net-of-tax change $\Delta\tau_{-2}$. In the second section, we report estimates using our preferred synthetic (density-weighted) average net-of-tax change instrument $\sum p_j\Delta\tau_j$ and the net-of-tax changes $\Delta\tau_j$ at four fixed income levels with y_j equaling USD 10,000, 25,000, 50,000, and 100,000, respectively. We also report estimates using all four $\Delta\tau_j$ as instruments. See Eqs. (19) to (26) for a description of the instruments.

The raw ETI estimate from the base-year instrument reported in column (1) of Table 3 is negative. It turns positive when including year-fixed effects in column (2). It increases to a statistically significant estimate of 0.263 in column (3) which includes five-piece splines in base-year income and base-year income lagged two years to control for trend heterogeneity. It is then stable as additional demographic covariates in the form of dummy variables for state of residence, filing status, and number of children, are included in column (4). These estimates are of the same magnitude as the two-year difference estimate of 0.33 by Gruber and Saez (2002, Table 6) using the same instrument. The discrepancy arises because we drop base-years in 1979 and 1980 to be able to use second-lag instruments and spline covariates, and because we use splines with five rather than ten pieces to be able to year-interact them in columns (5) and (6) without losing too many degrees of freedom. We discuss the estimates in columns (5) and (6) shortly.

With spline covariates included in columns (3) and (4), the predicted instrument yields estimates of approximately 1.3, whereas the year-specific spline instruments yield estimates of approximately 1.0. For the related year-specific top-5% grouped instrument, the estimates are approximately 1.4, which are within the range of Feldstein's (1995) grouping estimates (between 1 and 3) based on one cross section of differences. Pooling multiple differences and using a base-year control function to account for trend heterogeneity does not lead to markedly lower estimates, unlike previously thought. Furthermore, we do not find any large divergence in estimates from instruments that are continuous in or grouped by base-year income. This result challenges another conventional belief, namely that estimates from continuous instruments capturing minor taxpayer-level tax rate changes across the income distribution are lower due to taxpayers being less likely to respond to such tax rate changes.

Table 3. Structural IV estimates of the ETI

Instrument	(1)	(2)	(3)	(4)	(5)	(6)
Base-year net-of-tax change	-0.064	0.096	0.263**	0.262**	0.183*	0.129
	(0.065)	(0.072)	(0.077)	(0.076)	(0.079)	(0.082)
Predicted net-of-tax change	-0.753**	-0.655**	1.247**	1.285**		
	(0.120)	(0.205)	(0.278)	(0.280)		
Year*Base-year spline	-0.811**	-0.660**	0.970**	0.962**		
	(0.113)	(0.171)	(0.248)	(0.247)		
Year*Dummy for top 5% base-year income	-0.835**	-0.477	1.487**	1.444**		
	(0.138)	(0.327)	(0.426)	(0.425)		
Residualized net-of-tax change	0.170*	0.178*	0.215**	0.213**	0.183*	0.130
	(0.076)	(0.077)	(0.078)	(0.077)	(0.079)	(0.082)
Second-lag net-of-tax change	-0.198	0.196	0.527*	0.467	0.115	-0.063
	(0.135)	(0.206)	(0.258)	(0.259)	(0.307)	(0.365)
Average net-of-tax change	0.622**	0.834**	0.819**	0.696**	0.678**	0.691**
	(0.084)	(0.099)	(0.097)	(0.093)	(0.093)	(0.095)
Net-of-tax change at USD 10,000	0.354**	0.398**	0.380**	0.276**	0.220*	0.223*
	(0.096)	(0.101)	(0.098)	(0.096)	(0.097)	(0.099)
Net-of-tax change at USD 25,000	0.765**	1.049**	1.060**	0.919**	0.904**	0.922**
	(0.098)	(0.117)	(0.116)	(0.111)	(0.110)	(0.113)
Net-of-tax change at USD 50,000	1.009**	1.356**	1.300**	1.182**	1.189**	1.205**
	(0.106)	(0.135)	(0.128)	(0.124)	(0.123)	(0.123)
Net-of-tax change at USD 100,000	0.697**	1.425**	1.302**	1.330**	1.361**	1.346**
	(0.137)	(0.202)	(0.185)	(0.186)	(0.178)	(0.177)
All four fixed-income instruments	0.643**	0.851**	0.832**	0.727**	0.723**	0.741**
	(0.087)	(0.103)	(0.100)	(0.097)	(0.097)	(0.099)
Year	No	Yes	Yes	Yes	Yes	Yes
Base-year spline + second-lag spline	No	No	Yes	Yes	No	No
Demographics	No	No	No	Yes	No	No
Year*base-year spline + Year*second-lag spline	No	No	No	No	Yes	Yes
Year*demographics	No	No	No	No	No	Yes

Notes: Each cell reports an estimate from one regression. Two-year differences are used. The splines each contain five pieces. The top 5% cutoff is USD 83,118 (1990 price level). Demographic covariates include fixed effects for state of residence, filing status, and number of children. Standard errors reported within parentheses are clustered at the taxpayer level. * p<0.05; ** p<0.01.

As discussed in detail in Section 3, because base-year income is endogenously determined by elasticity heterogeneity, estimates from instruments based on income-by-year variation are subject to bias. We now turn to the residualized instrument which is uncorrelated with year-specific trends in base-year income and, therefore, consistent under the identifying assumption of statistical independence of tax structure changes, as shown in Proposition 2. The residualized estimates are about 0.20, ranging from 0.170 without covariates in column (1) to 0.213 with all general covariates in column (4). The fact that the estimates are robust to controlling for demographic trends is reassuring for instrument validity.

The base-year estimate of 0.26 lies between the estimates from the predicted and residualized instruments of 1.3 and 0.21, respectively. Interpreting the bias-corrected estimate of 0.21 as the consistent base-year ETI, the elasticity heterogeneity bias in the estimate of 1.3 is large. On the other hand, the estimates of 0.21 and 0.26 are not statistically significantly different from each other. Therefore, elasticity heterogeneity bias is not the primary issue for the base-year instrument. The reason is that the residualized variation contributes more to the identification because the standard deviation of the residualized instrument is higher than that of the predicted instrument, as shown in Table 2.

The starkly different estimates from using income-by-year variation versus the residualized variation are useful for interpreting divergent results in the literature. As an example, consider the estimates on the 1993 tax rate increase reported in Table 2 of Saez et al. (2012). The base-year instrument yielded estimates of 0.143 and 0.237, while a corresponding income-grouped instrument yielded estimates of 0.564 and 0.732. As we already showed, estimates from continuous and grouped instruments varying by only base-year income and year are similar. However, income-grouping also removes the residualized variation that is free from the positive elasticity heterogeneity bias affecting the base-year instrument. This fact can explain much higher grouped estimates.

Another way to address elasticity heterogeneity bias is to control for year-specific trends. The base-year estimate decreases to 0.183 when including year-specific splines in column (5) of Table 3. This estimate is similar to the residualized estimates of 0.213 and 0.183 with general and year-specific splines in column (5) and (6), respectively. Additionally including year-specific demographic covariates to control for elasticity heterogeneity across demographic characteristics in column (6) lowers the estimates further.

The second-lag instrument yields considerably higher estimates than the base-year instrument, once splines have been included. Standard errors are three times larger, and the estimates are more volatile to the set of covariates used. With the wider sets of covariates in column (4), the estimate is approximately 0.5 and lower than the preferred estimate of approximately 0.8 in Weber (2014). However, in both cases, replacing base-year income with lagged income yield higher estimates. Our estimates are lower than those by Weber because we use a single lagged-income instrument whereas she used multiple lagged-income instruments. We return to a deeper discussion of her estimates shortly.

By Proposition 4, the synthetic instrument yields a global ETI estimate. Without covariates in column (1) of Table 3, the estimate is 0.622. It increases to 0.834 when controlling for time effects in column (2). The fact that adding splines in column (3) to control for trend heterogeneity does not change estimates supports instrument validity. The relative stability of estimates indicates that the underlying tax structure changes that are used to construct the instrument are uncorrelated with trends in base-year income. The estimates decrease a bit to 0.696 as additional demographic covariates are included in column (4). However, they are remarkably robust to addressing potential remaining elasticity heterogeneity bias by adding year-specific splines and demographic covariates with an estimate of 0.691 in column (6).

In general, precision is much higher when using the synthetic instrument than when using the other instruments, because it exhaustively uses the available variation in tax rate changes across the income distribution. The higher global ETI estimate of around 0.70 compared to the local base-year ETI estimate of 0.21 suggests that the global estimate gives weight to all taxpayer responses to tax rate changes. The large difference indicates that taxpayers that switch tax brackets and react to tax rate changes at other income levels than the base-year income level have substantially higher elasticities. Thus, the base-year ETI, which overweigh inelastic taxpayers by Proposition 3, is quite unrepresentative of the tax reforms in the data, and understanding the set of compliers for different instruments is of first-order importance.

The fixed-income instruments yield ETI estimates between 0.223 and 1.346 with all covariates in column (6). The estimate increases as the fixed income level increases. Thus, the substantial variation in elasticities is positively correlated with base-year income. According to the simple simulations in Table 1, the elasticity heterogeneity bias should be positive in this case, which is consistent with our estimation results.

The estimates from the synthetic instrument represent weighted averages of these local ETI estimates. Using several fixed-income instruments as an alternative way to exploit multiple tax rate changes yields an estimate of 0.741 in column (6), which is close to the estimate from the single synthetic instrument of 0.691.

With multiple lagged-income instruments, Weber (2014) obtained ETI estimates between 0.8 and 1.5. She interpreted the higher estimates compared to base-year estimates as the consequence of lagged-income instruments accounting for the trend heterogeneity bias better. We offer a different explanation: Her specifications are related to our multiple-instrument specification and therefore yield similar higher estimates. The reason is that a more representative weighting of taxpayers with different elasticities (compared to the base-year instrument) is achieved. She did, however, not control for year-specific trends in lagged income to account for elasticity heterogeneity bias.

Variation in tax rate changes across demographic groups could potentially be less contaminated by preferences than the variation within demographic groups. Using such group-level variation could therefore serve as a sensitivity test. In comparing grouped and ungrouped estimates, it is, however, important to keep in mind that a discrepancy may reflect both differences in bias and localness of the ETI estimates.

We construct groups based on state of residence, filing status, number of children, and the double and triple interactions between these variables. In Table 4, we report estimates using net-of-tax change instruments averaged within each group and year. We control for the non-interacted general trends. The base-year specification is similar in spirit to the specifications used by Burns and Ziliak (2017).

Table 4. Estimates from instruments grouped by demographic characteristics and year

Grouping	(1) Base-year	(2) Predicted	(3) Residualized	(4) Second-lag	(5) Synthetic
State	1.521**	1.478*	1.537**	1.058*	0.905**
	(0.402)	(0.645)	(0.425)	(0.472)	(0.343)
Filing	0.740	-0.044	0.902	0.784	-2.646
	(0.479)	(0.864)	(0.551)	(0.484)	(3.642)
Children	1.038**	0.787	1.056**	1.000**	0.938**
	(0.322)	(0.766)	(0.325)	(0.329)	(0.348)
State-Filing	0.847**	1.246*	0.760**	0.749*	0.530*
	(0.263)	(0.539)	(0.273)	(0.363)	(0.255)
State-Children	0.806**	1.351*	0.720**	0.874**	0.825**
	(0.233)	(0.580)	(0.239)	(0.335)	(0.214)
Filing-Children	0.618	0.118	0.682	0.855*	1.224*
	(0.328)	(0.777)	(0.349)	(0.361)	(0.515)
State-Filing-Children	0.709**	1.733**	0.571**	1.050**	0.633**
	(0.209)	(0.552)	(0.219)	(0.341)	(0.190)

Each cell reports an estimate from one regression. Two-year differences are used. All regressions include five-piece splines in base-year and second-lag base-year incomes, as well as fixed effects for state of residence, filing status, and number of children. Standard errors reported within parentheses are clustered at the taxpayer level. * p<0.05; ** p<0.01.

While ETI estimates using filing groups are imprecise, the estimates from our synthetic instrument in column (5) vary between 0.530 and 1.224, which are comparable to their ungrouped equivalent of around 0.7. Standard errors are large; the estimate of 0.633 using the triple interaction is the most precise with standard errors that are double those of the ungrouped estimate.

Turning to columns (1) to (4), the base-year, predicted, and residualized estimates are now closer to each other. This can be explained by base-year income differences within years being smaller across groups than within groups, which leads to a smaller elasticity heterogeneity bias. Except for grouping by filing status, all estimates lie between 0.571 and 1.537, and they are not that different from estimates using the synthetic instrument. This indicates that the grouped estimates are less local than the ungrouped estimates.[25]

In the first section of Table 5, we report reduced-form and first-stage estimates for taxable income, in addition to the structural IV estimates. In the second section, we report estimates for broad income. For taxable income, by Proposition 4, the reduced-form estimate of the synthetic instrument yields a policy elasticity of 0.455, which is roughly two-thirds of the IV estimate of the ETI of 0.696. The first-stage estimate is 0.653. To get more clarity, consider changing tax schedules in the same way as they vary in the data. The estimate of the policy elasticity implies that when such changes lead to higher observed net-of-tax rates at the base-year income level by 1%, taxable income increases by 0.46%.

[25] Suppose that differences in tax rate changes between groups are smaller in magnitude than the differences within groups. In this case, some elastic taxpayers who switch brackets due to the within-group variation will not do so due to the between-group variation. The underweighting of elastic taxpayers due to bracket switching would then be less severe when identification is based on the between-group variation.

Table 5. Reduced-form, first-stage, and broad income estimates

	(1) Base-year	(2) Predicted	(3) Residualized	(4) Second-lag	(5) Synthetic
Dependent variable: taxable income change					
IV	0.262**	1.285**	0.213**	0.467	0.696**
	(0.076)	(0.280)	(0.077)	(0.259)	(0.093)
Reduced form	0.183**	1.656**	0.148**	0.099	0.455**
	(0.052)	(0.357)	(0.053)	(0.053)	(0.055)
First stage	0.697**	1.288**	0.695**	0.211**	0.653**
	(0.015)	(0.074)	(0.015)	(0.017)	(0.017)
Dependent variable: broad income change					
IV	0.082*	0.021	0.086*	0.172	0.205**
	(0.041)	(0.181)	(0.041)	(0.137)	(0.052)
Reduced form	0.058*	0.027	0.060*	0.036	0.133**
	(0.028)	(0.233)	(0.029)	(0.028)	(0.032)
First stage	0.701**	1.283**	0.697**	0.210**	0.650**
	(0.015)	(0.074)	(0.015)	(0.017)	(0.017)

Each cell reports an estimate from one regression. Two-year differences are used. All regressions include five-piece splines in base-year and second-lag base-year incomes, as well as fixed effects for state of residence, filing status, and number of children. Standard errors reported within parentheses are clustered at the taxpayer level. * p<0.05; ** p<0.01.

For broad income, the base-year and residualized instruments yield IV estimates of approximately 0.08. The second-lag estimate of 0.172 is larger but not statistically significant. The synthetic instrument yields a broad income IV estimate of the global ETI of 0.205. Like for taxable income, the synthetic instrument yields the highest estimate indicating that the other estimates underweight elastic taxpayers. It also yields a broad income reduced-form estimate of the policy elasticity of 0.133. From the point of view of efficiency analysis, our policy elasticity estimates of 0.455 for taxable income and 0.133 for broad income are the most indicative of the marginal deadweight loss.

6. Conclusion

We introduced elasticity heterogeneity in the estimation of the ETI in the standard IV setting in first-differences. We showed that elasticity heterogeneity, in addition to trend heterogeneity, is an important source of bias. Instruments used in the literature are invalid because they are endogenously determined by elasticity heterogeneity. In particular, income-by-year variation in tax rate changes is contaminated. Controlling for year-specific trends in base-year income provides a solution. Identification based on variation in tax rate changes within income levels and years due to tax structure changes can also be exploited by using a class of instruments based on fixed-income tax rate changes.

We also explicitly derived the weights given to different taxpayer elasticities. Most instruments do not yield an ETI estimate that is representative for the taxpayers reacting to the tax rate changes in the data. In particular, we showed that after correcting for bias, the local ETI estimate from the base-year net-of-tax change instrument overweighs inelastic taxpayers.

We proposed a new synthetic average net-of-tax change instrument that weighs net-of-tax changes at different income levels by the respective observed probability density. Such weights yield a global ETI that is the best approximation of the average ETI on the taxed. Furthermore, the reduced-form estimate of this instrument provides a policy elasticity that, compared to the ETI, is more indicative of behavioral responses for nonlinear tax structures.

We explored the implications of our theoretical analysis using the NBER tax panel for 1979-1990. The synthetic average net-of-tax change instrument yielded an IV estimate of the ETI of around 0.7. The estimate was robust to controlling for trends in base-year income and demographics, and even to year-specific versions of these trends. Furthermore, it was relatively robust to the sole use of demographic group-level variation in tax rate changes for identification. We interpret these results as evidence of instrument validity. We also found a reduced-form estimate of the policy elasticity of 0.46. For broad income, we found an ETI of 0.21 and a policy elasticity of 0.13.

Reconciling estimates in the literature based on different methods was an important goal of our empirical analysis. We found an IV estimate of 0.26 using the base-year net-of-tax change instrument. To demonstrate the empirical importance of elasticity heterogeneity, we decomposed this instrument into two components: (1) a part subject to elasticity heterogeneity bias that varies purely by base-year income and year and (2) a remainder that is stripped off income-by-year variation and free from such bias. We obtained estimates between 1 and 1.4 when only utilizing income-by-year variation and 0.2 when only exploiting the remaining variation. The discrepancy between the estimates reveals a positive elasticity heterogeneity bias. On the other hand, the difference between the bias-corrected local base-year ETI estimate of 0.2 and our global ETI estimate of 0.7 demonstrates that localness has an attenuating impact.

Our analysis offers alternative explanations of the widespread divergence in ETI estimates in the previous literature. The grouping estimates (1 to 3 in, e.g., Feldstein, 1995) were larger than the subsequent ungrouped estimates (0.2 to 1.5 in, e.g., Gruber and Saez, 2002; Weber, 2014) mainly because income-grouping methods discard substantial variation in tax structure changes, and therefore, suffer from a larger elasticity heterogeneity bias. The discrepancies between the ungrouped estimates, on the other hand, are primarily due to the estimated local elasticities being representative for different sets of taxpayers.

Appendix

Proof of Proposition 1. By the law of total covariance:

$$\beta^{IV} = \frac{Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] + E_{\Delta\boldsymbol{\tau}}[Cov(\beta_i\Delta\tau, z|\Delta\boldsymbol{\tau})] + E_{\Delta\boldsymbol{\tau}}[Cov(\Delta\alpha, z|\Delta\boldsymbol{\tau})]}{Cov[E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] + E_{\Delta\boldsymbol{\tau}}[Cov(\Delta\tau, z|\Delta\boldsymbol{\tau})]}. \quad \text{(A1)}$$

Define:

$$\beta^{LATE} = \frac{Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})]}{Cov[E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})]} = \frac{Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})]}{Cov(\Delta\tau, z) - E_{\Delta\boldsymbol{\tau}}[Cov(\Delta\tau, z|\Delta\boldsymbol{\tau})]}. \quad \text{(A2)}$$

Collecting terms gives Eq. (13). By the independence of $\Delta\boldsymbol{\tau}$:

$$\begin{aligned}
&Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] \\
&= E[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau})E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] - E[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau})]E[E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] \\
&= E\big[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau})\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big] \\
&= E_{\Delta\boldsymbol{\tau}}\big[E_{\boldsymbol{e}}(\beta_i\Delta\tau|\Delta\boldsymbol{\tau})\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big] \\
&= E_{\Delta\boldsymbol{\tau}}\Big[E_{\beta_i}\big[\beta_i E_{\boldsymbol{e}|\beta_i}(\Delta\tau|\Delta\boldsymbol{\tau}, \beta_i)\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big]\Big] \\
&= E_{\beta_i}\Big[\beta_i E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big]\Big],
\end{aligned} \quad \text{(A3)}$$

$$Cov[E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau})] = E_{\beta_i}\Big[E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big]\Big]. \quad \text{(A4)}$$

Let $p(.)$ denote the probability density function. Eqs. (A3) and (A4) yield:

$$w_{\beta_i}^{LATE} = \frac{E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big]p(\beta_i)}{\int E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(z|\Delta\boldsymbol{\tau}) - E(z)\big)\big]p(\beta_i)d\beta_i}, \quad \text{(A5)}$$

which proves Eq. (14) and the a-part of the proposition:

By plugging in $z = E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})$ into Eq. (13), it follows that:

$$\beta^{IV} = \frac{Cov[\Delta y, E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})]}{Cov[\Delta\tau, E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})]} = \frac{Cov[E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}), E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})]}{Var[E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau})]}. \quad \text{(A6)}$$

Going through the steps in Eq. (A3) to (A5) yields:

$$w_{\beta_i}^{ATT} = \frac{E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}) - E(\Delta\tau)\big)\big]p(\beta_i)}{\int E\big[(\Delta\tau|\beta_i)\big(E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}) - E(\Delta\tau)\big)\big]p(\beta_i)d\beta_i}, \quad \text{(A7)}$$

which proves the b-part of the proposition. ■

Proof of Proposition 2. This proposition is a straightforward implication of Proposition 1 by which a sufficient condition for $bias^a = 0$ in Eq. (16) is $Cov(\Delta\alpha, \Delta\tau_0|\Delta\boldsymbol{\tau}) = 0$. By Eq. (12), $\Delta\alpha$ is a function of $\boldsymbol{e}$, and by Eq. (20) $\Delta\tau_0$ is a function of y_{it} which in turn is a function of $\boldsymbol{e}$. Thus, $\Delta\alpha$ and $\Delta\tau_0$ are correlated. However, conditional on y_{it}, $\Delta\tau_0$ is uncorrelated with $\boldsymbol{e}$ and:

$$Cov(\Delta\alpha, \Delta\tau_0|\Delta\boldsymbol{\tau}, y_{it}) = 0, \quad \text{(A8)}$$

which proves the a-part of the proposition.

A sufficient set of conditions for $bias^b = 0$ in Eq. (15) is $Cov(\beta_i\Delta\tau, \Delta\tau_0|\Delta\boldsymbol{\tau}) = 0$ and $Cov(\Delta\tau, \Delta\tau_0|\Delta\boldsymbol{\tau}) = 0$. Now, as $(\beta_i, \Delta\tau)$ and $\Delta\tau_0$ are correlated with each other by both being functions of (y_{it}, μ_t), conditional on (y_{it}, μ_t):

$$Cov(\beta_i \Delta\tau, \Delta\tau_0 | \Delta\boldsymbol{\tau}, y_{it}, \mu_t) = 0, \tag{A9}$$

which proves the b-part of the proposition. ■

Proof of Proposition 3. Clearly, $\Delta\tau = \Delta\tau_0$ for non-switchers but not for switchers. Thus, the correlation between $\Delta\tau$ and the instrument is one for non-switchers and less than one for switchers. Therefore, compared to switchers, non-switchers comply more with the instrument and receive a greater weight by the base-year ETI. Now, non-switchers also have smaller Δy. By Eq. (11), $\Delta y = \beta_i \Delta\tau + \Delta\alpha$, and ceteris paribus, $d|\Delta y|/d\beta_i \geq 0$. It follows that non-switchers have lower elasticities. We also note that unless elasticities are strongly correlated with separable income trends or pre- and post-reform tax structures, non-switchers have a lower average elasticity than switchers in the sample. Furthermore, under some additional assumptions on separable income trends and tax structures, it is possible to show that among instruments using variation in tax rate changes at some income level, the base-year net-of-tax change minimizes the local ETI. ■

Proof of Proposition 4. Weighted averages of net-of-tax changes can be expressed as:

$$\int \mu_j \Delta\tau_{jt} dy_j, \tag{A10}$$

with an expectation over tax structure changes:

$$E_{\Delta\boldsymbol{\tau}}\left(\int \mu_j \Delta\tau_{jt} dy_j\right) = \int \mu_j E_{\Delta\boldsymbol{\tau}}(\Delta\tau_{jt}) dy_j. \tag{A11}$$

To create an instrument that approximates z^{ATT} in Eq. (17) the best, note that:

$$\begin{aligned} z^{ATT} = E_{\boldsymbol{e}}(\Delta\tau|\Delta\boldsymbol{\tau}) &= \int \left[(p_{jT}|\Delta\boldsymbol{\tau})\tau_{jT} - (p_{jt}|\Delta\boldsymbol{\tau})\tau_{jt}\right] dy_j \\ &= \int (p_{jt}|\Delta\boldsymbol{\tau})\Delta\tau_{jt} dy_j + \int \tau_{jT}(\Delta p_{jt}|\Delta\boldsymbol{\tau}) dy_j. \end{aligned} \tag{A12}$$

We cannot observe distribution functions conditional on tax structure change. Thus, none of the terms in the two integrals are separable in fixed-income net-of-tax changes, unlike our candidates in Eq. (A10). We can at best hope to approximate the expectation over tax structure changes. By the independence assumption on $\Delta\boldsymbol{\tau}$:

$$E_{\Delta\boldsymbol{\tau}}(z^{ATT}) = E(\Delta\tau) = \int p_{jt} E_{\Delta\boldsymbol{\tau}}(\Delta\tau_{jt}) dy_j + \int E_{\Delta\boldsymbol{\tau}}\left[\tau_{jT}(\Delta p_{jt}|\Delta\boldsymbol{\tau})\right] dy_j. \tag{A13}$$

The first term is the mechanical effect of tax structure change on observed net-of-tax change and separable. Choosing $\mu_j = p_{jt}$ reproduces this term. The second term is the behavioral effect which is still is not separable. The choice of μ_j cannot generally minimize expected error further, which proves the a-part of the proposition, up to the discretized approximation of the integral.

By the second fundamental theorem of calculus, let the weighted average marginal effect of $\Delta\tau_j$ on $E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau})$ over possible nonlinearities in $\Delta\tau_j$ be:

$$\gamma_j p_j = \int_{1(\Delta\tau_a \leq \Delta\tau_j)} \frac{dE_{\boldsymbol{e}}[\Delta y|\Delta\boldsymbol{\tau}]}{d\Delta\tau_a} d\Delta\tau_a \bigg/ \int_{1(\Delta\tau_a \leq \Delta\tau_j)} d\Delta\tau_a. \tag{A14}$$

The parameter γ_j is a per-taxpayer marginal effect of $\Delta\tau_j$ for nonlinear tax structure changes. Integrating over y_j yields:

$$E_{\boldsymbol{e}}(\Delta y|\Delta\boldsymbol{\tau}) = \int \gamma_j p_j \Delta\tau_j dy_j. \tag{A15}$$

Thus, regressing Δy on $\int p_j \Delta\tau_j \, dy_j$ gives:

$$\begin{aligned} &\frac{Cov\left[\int \gamma_j p_j \Delta\tau_j dy_j \, , \int p_j \Delta\tau_j dy_j\right]}{Var\left[\int p_j \Delta\tau_j dy_j\right]} \\ &= \frac{\int \gamma_j E\left[p_j \Delta\tau_j [\int p_k \Delta\tau_k dy_k - E\left(\int p_k \Delta\tau_k dy_k\right)]\right] dy_j}{\int E\left[p_j \Delta\tau_j [\int p_k \Delta\tau_k dy_k - E\left(\int p_k \Delta\tau_k dy_k\right)]\right] dy_j}, \end{aligned} \tag{A16}$$

which is a weighted average of γ_j. ■

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