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Earnings Responses to Even Higher Taxes[a]

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Abstract

We exploit a recent Swedish tax reform, implying higher marginal tax rates for the top 5% of the earnings distribution, to learn about earnings responses in an economy where taxes already are high. Using a simple and graphical cross sectional method, we estimate earnings elasticities in the range 0.13-0.16. We interpret the response using a simulation model in which people face uncertain marginal tax rates due to earnings dynamics. The tax response is surprisingly sharp given the earnings variability at the top of the earnings distribution.

Keywords: Earnings supply, Income taxation.

JEL Classification: H24; J22.

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1 Introduction

The idea that tax cuts sometimes or often pay for themselves was popularized by Arthur B. Laffer in the 1970's. The logic behind the "Laffer curve" is straightforward and uncontroversial: At some point, higher taxes will no longer generate larger revenues, because the negative behavorial effect on the tax base outweighs the mechanical revenue gain from the tax increase. Hence, if the economy is on the wrong side of the peak of the Laffer curve, everyone is better off from tax cuts, because the greater revenues can be used to help the worst off. The more controversial empirical question is of course: How sensitive are top incomes to tax changes? And to what extent is it possible for us to clearly detect such responses given the earnings variability for top income earners?

In 2015, Swedish high income earners faced an effective marginal tax rate of around 72.5 % after accounting for both direct and indirect taxes, a figure that is very close to the revenue maximizing tax rate computed by Diamond and Saez (2011, p.171) for the United States.[1] But in 2016, marginal tax rates reached even higher levels when Sweden introduced a phase-out of the earned income tax credit (EITC) at high earnings levels. As a consequence, taxpayers in the top 5 percentile groups of the earnings distribution experienced a 7 % reduction in their net-of-tax rates (3 pecentage points increase in their marginal tax rates). Is such a reform sufficiently large for a clear response to occur in an environment where people plausibly have imperfect knowledge about the tax code and face income uncertainty?

We evaluate the 2016 EITC phase-out reform by comparing treated and non-treated percentile groups of the earnings distribution. There is a significant relative earnings reduction in the treatment group immediately appearing in 2016, growing in 2017, and stabilizing in 2018. The earnings elasticities implied by the 2018 response are in the range 0.13-0.16. The magnitude of the response indicates that marginal tax rates facing high earners exceeded the peak of the

[1] In fact, among economists analyzing Swedish labor income taxation before 2016 a mainstream view has been that tax rates applying to high incomes were either very close to or above the peak of the Laffer curve, see e.g. Holmlund and Söderström (2011), Pirttilä and Selin (2011b), and Sørensen (2010).

Laffer curve already before the 2016 EITC phase-out reform.[2]

We contribute to the large and important literature on income responses to taxes primarily in two ways. Our first contribution is to exploit a clean source of identification – an introduction of a new marginal tax braket – together with an extremely simple and transparent empirical strategy. By deviating from the standard approach in the literature, i.e. longitudinal individual level comparisons of income growth, we are able to provide graphical expositions that are standard in the policy evaluation literature, but rare in the literature on taxable income and earnings responses.[3]

The transparent cross sectional technique we are using highlights features of tax responses that are otherwise hidden to researchers. We document e.g. that estimated responses are smaller when including percentile groups that are close to the new EITC phase-out kink. This makes perfect sense, because in the presence of earnings dynamics, we do not expect *perceived* marginal tax rates to change sharply at kink points. Our second contribution is to analyze the sharpness of the observed tax repsonse in a simulation model in which individuals face earnings uncertainty and thereby uncertainty about their marginal tax rates.

Let us elaborate more on the first contribution. Why is the Swedish EITC phase-out reform a promising source of quasi-experimental variation for the purpose of estimating earnings responses to taxes? First, the Swedish earnings distribution has been surprisingly stable since the turn of the millennium. More specifically, we will demonstrate that earnings growth evolved remarkably similar in the treatment and control groups in the pre-reform years. Second, the reform brought about an isolated policy change – an introduction of a new tax bracket, while essentially leaving other relevant aspects of the tax system in-

[2]Our elasticity estimates are well aligned with the preferred estimates in related studies on Nordic data, see e.g. Matikka (2018) (Finland), Kleven and Schultz (2014) (Denmark), Blomquist and Selin (2010) (Sweden), and Thoresen and Vattø (2015) (Norway), who found elasticities of around 0.2 or lower. Some studies, however, like Gelber (2014) and Hansson (2007), have found larger elasticities for Sweden. A salient distinguishing feature of our study is that the identifiying variation is to be found locally at the top of the earnings distribution.

[3]A similar point was made by Kleven and Schultz (2014). We believe, however, that our graphs are even more basic, because they can be reproduced directly from the population register files with no further restrictions.

tact. Historically, many tax reforms combined tax rate changes with changes to the tax base, and it has often been challenging for researchers to separate between the two (Kopczuk, 2005). Third, the richness of our population-wide administrative data allows us to control for a large number of factors, and we are also able to examine potential shifting between the labor and capital income tax bases.

Since gross earnings (before deductions) is the base for the EITC, we are estimating an earnings rather than a taxable income elasticity.[4] Still, from a methodological perspective our study is part of the taxable income literature. There is no consensus on how to estimate the elasticity of taxable income (ETI). While the pioneering study by Lindsey (1987) was conducted on cross-sectional tax return data, the main approach in the ETI literature since Feldstein (1995) has been to estimate difference-in-difference models using individual level panel data. Identification comes from tax reforms treating different income groups differentially.[5] However, since taxpayers typically are assigned to treatment and control groups based on pre-reform income – and individual incomes vary stochastically from one year to another for non-tax reasons – standard difference-in-difference graphs are often absent in these studies.[6]

According to the terminology of the ETI literature, panel data methods must account for the "mean reversion problem". This challenge has led to interesting econometric proposals, see e.g. Blomquist and Selin (2010), Holmlund and Söderström (2011), Kawano et al. (2016), Kumar and Liang (2020), and Weber (2014). But the methodological advances have come with the cost of lost transparency. Actually, when Saez et al. (2012) surveyed the ETI literature some

[4]The earnings elasticity is arguably more interesting from the perspective of optimal taxation since it has a stronger connection to real behavior (labor supply and effort responses). The taxable income elasticity also captures tax planning responses, which tend to be more informative on the loopholes of the tax system than preferences (Slemrod and Kopczuk, 2002). A recent meta-study by Neisser (2021) confirms that the underlying context is important for taxable income estimates.

[5]Matikka (2018), who used municipal variation in Finland, and Burns and Ziliak (2017), who exploited state level variation in the United States, are notable exceptions.

[6]Very recently, Jakobsen and Søgaard (2020) proposed a new graphical panel data method that imposes different identifying assumptions than our approach does. The authors compare individual log income differences at treated and untreated parts of the income distribution over time (triple differences).

years ago, the superiority of the panel data approach was questioned with reference to the mean reversion problem.[7] A simulation study by Aronsson et al. (2017) confirmed that panel data estimators of the ETI are highly sensitive to the modeling of the stochastic income process. In this study, we instead compare earnings growth in different *percentile groups* over time. Using our rich population wide data, we discuss and adress well-known disadvantages of the cross sectional approach. We also consider alternatives like conventional panel data methods and instruments based on predicted earnings.

Our second contribution is to interpret the response using a simple model of noisy tax perceptions. Remember that our identifying tax variation is relatively small. Are individuals' perceptions of the tax system sufficiently precise for a clear response to appear? To begin with, we highlight some interesting features of the observed response: We find that earnings growth slows down also in the control group, and we find that the effect increases when omitting percentile groups around the treatment cut-off. Hence, it seems like perceived incentives change gradually rather than sharply around the new kink. We therefore built a simulation model that allows us to interpret these aspects numerically.

Up to now, earnings dynamics has been discussed in terms of a statistical problem in the ETI literature. But the stochastic element of earnings may also impact the underlying behavioral economic model. When people facing non-linear tax schedules cannot perfectly control their incomes, perceptions of marginal incentives will be noisy as well, becase they do not know the tax they will pay on their realized income, even if they would be fully aware of the tax code. Following Saez (1999) we hypothesize that taxpayers maximize expected utilty under *ex ante* uncertainty about *ex post* realized earnings. This model framework has earlier been used to explain why people do not bunch at kinks, but it has not been used to simulate broader responses to tax changes.

We simulate data from this model for different values of earnings variability and underlying elasticity parameter, and we examine under which parameter

[7] Saez et al. (2012, p.29) remarked that "the advantage of longitudinal analysis relative to repeated cross-section analysis has been somewhat exaggerated in the empirical literature following Feldstein (1995), especially when one wants to analyze tax changes happening primarily at the top of the income distribution. In some contexts, repeated cross-section analysis or share-based time-series analysis may be a more robust and transparent approach."

values we obtain the empirically estimated elasticities. In the empirical analysis we obtained an earnings elasticity estimate of 0.130 when including all percentile groups, and we obtained a point estimate of 0.164 when excluding two percentile groups at each side of the new bracket cut-off. The simulation exercise suggests that a *smaller* earnings variance than the actual one better reproduces the empirically observed response. This underlines that the observed response is surprisingly sharp, especially since we do not take other sources of optimization errors into account in the analysis (like imperfect knowledge of the tax system).

The rest of the paper is organized as follows. In Section 2 we describe the Swedish tax system, and we discuss various aspects of the EITC phase-out reform. Section 3 provides a brief account of the data source. We report the empirical analysis in Section 4. In Section 5 we move on to the simulation model. Section 6, finally, concludes the paper.

2 Institutional setting

2.1 The Swedish system

The Swedish income tax system is a dual income tax system in which labor earnings and capital incomes are taxed separately. Additionally, the income tax is individual based rather than family based, i.e. spouses are taxed separately. All individuals aged up to 65 essentially face the same tax schedule, with some variations in the local tax rate. The basic structure of Swedish labor income taxation is fairly simple. A proportional local tax rate applies to the sum of all earned income and taxable transfers (net of some deductions).[8] The average local income tax rate (unweighted) in 2016 was 32.1 percent. For total labor incomes exceeding a certain threshold (SEK 443,200 in 2016) a central government income tax is due (1 USD $\approx$ 10 SEK). 17 percent of the population aged 20-65 paid the central government income tax in 2016. The central government

[8]The legal term in Swedish is ”fastställd förvärvsinkomst”. The income tax is assessed on the basis of yearly income, and the tax year coincides with the calendar year. A basic allowance affects marginal tax rates at lower and medium incomes. The basic allowance does not affect marginal tax rates for the income groups we study in this paper.

income tax schedule consisted of two brackets until 2020; the marginal tax rates in each bracket were 20 percent (for incomes between SEK 443,200 and 638,800 in 2016) and 25 percent (for incomes above SEK 638,800 in 2016) respectively. Throughout the paper, we deflate and inflate nominal incomes using the consumer price index, with 2016 as the reference year.

The Swedish Earned Income Tax Credit (EITC) (”jobbskatteavdraget”) was first introduced in 2007 by a center-right wing government coalition, see Edmark et al. (2016) for more details. The base for the EITC is not identical to the base for the local and central government tax, because the EITC is solely a function of earned income. The tax reduction is not granted for social transfers (like unemployment insurance and sickness insurance). In a stepwise fashion, the EITC has become more generous since 2007, and in 2016 the maximum tax credit was around SEK 26,500. The EITC slightly varies with the local tax rate. The Swedish EITC is very general: all individuals aged below 66 face the same tax credit scheme, regardless of marital status or number of children in the household, and indiviuals do not need to apply for the credit.

2.2 The 2016 reform

It is a standard feature of in-work tax credit policies that the tax credit tapers off when earnings rise. This was not the case, however, in Sweden until 2016, when a left wing-green government reformed the EITC schedule. While the tax credit in other countries, e.g. the US and the U.K., is phased out at relatively low earnings levels, the Swedish phase-out impacts taxpayers at the upper end of the earnings distribution. In the 2016 reform, the reduction rate was set to 3 percent. Accordingly, if the individual increases her income by SEK 100 she forgoes SEK 3 in tax credit.

Figure 1a visualizes the EITC schedule in 2016 with and without the phase-out. We have plotted the compressed Swedish earnings distribution in the background. Evidently, the phase-out impacts work incentives for a significant number of high-income earners. In Figure 1b we visualize the effect of the EITC phase-out on the marginal tax schedule, again with the earnings distribution in the background. The new EITC kink was placed just below the second central government kink. In a sense, the 3 percentage point increase in the marginal tax rate appears small. But then one should keep in mind that the marginal tax rate in this region was at a very high level already before the reform, 0.57. Hence, the percentage change in the net-of-tax rate is $\frac{0.03}{1-0.57} \approx 7\%$.[9] The reform also implied that a kink point was created at the income level at which the entire tax credit had been phased out. At this (non-convex) kink, located at around SEK 1.5 million in annual earnings, the marginal tax rate decreased by 3 percentage points. Figure 1 illustrates that very few taxpayers – not more than 0.2 percent of the population – earned incomes above this point.

We wish to emphasize that Figure 1b plots the marginal tax schedule under the assumption that the individual does not receive any taxable transfers, e.g. sickness insurance benefits or parental insurance benefits. Remember that earned income is the base for the EITC, whereas the sum of earned income and taxable transfers is the base for local and central government taxes. Consequently, EITC varies more with sickness absence and parental leave spells. This should be kept in mind when we in Section 5 model uncertainty in earnings realizations.

The EITC phase-out reform came into effect on January 1, 2016. Did other reforms that are relevant to our study occur at the same time? The EITC phase-out was part of a government bill, in which several taxes were adjusted upwards. This was the first budget proposal from the new social democratic-green party coalition that gained parliamantary support.[10] The new budget also contained stricter rules for the household tax reduction, higher payroll taxes for people aged over 65 (who are excluded from our analysis), and higher energy taxes. Moreover, the kink points of the central government tax scheudule were not fully adjusted for wage growth in 2016 and 2017. Finally, deductability of contributions to tax-favored pension savings accounts were abolished in two steps

[9] If one also takes payroll and consumption taxes into account, the level of marginal tax rates was even higher after the reform, around 75%, which could be the highest in the world (Lundberg, 2017). However, these indirect taxes do not affect the percentage change in the net-of-tax rate. Accordingly, we do not include them in the further analysis.

[10] A similar budget proposal from the same government coalition was sensationally rejected by the parlament in December 2014. Center-right wing budgets shaped tax policy in Sweden 2007-2015.

2015-16.[11] We will in Section 4.6 examine several possible explanations for our findings, including consequences of the new deduction rules.

3 Data

We utilize full-population individual register data from Statistics Sweden. The most important register we use ("LISA") contains data from the labor market, educational and social sector and is updated each year (Statistics Sweden, 2020c). The key variable of interest is earned income, i.e. the base for the EITC. We construct this variable by taking the sum of wage and self-employment income. We also have detailed information on other demographic characteristics. These include age, gender, level and field of education, country of origin, county of residence, marital status, number of children and industry codes. The only sample restricition we make is to limit the sample to individuals aged 20-65. No other restrictions are imposed, which implies that our empirical analysis is easily reproducable.

We also want to study income shifting between the labor and capital income tax bases. Therefore, we use separate data from Statistics Sweden, which include full population tax registers linked with tax return data from owners of closely held corporations (Statistics Sweden, 2020a,b). These data are analyzed in Section 4.5 below.[12].

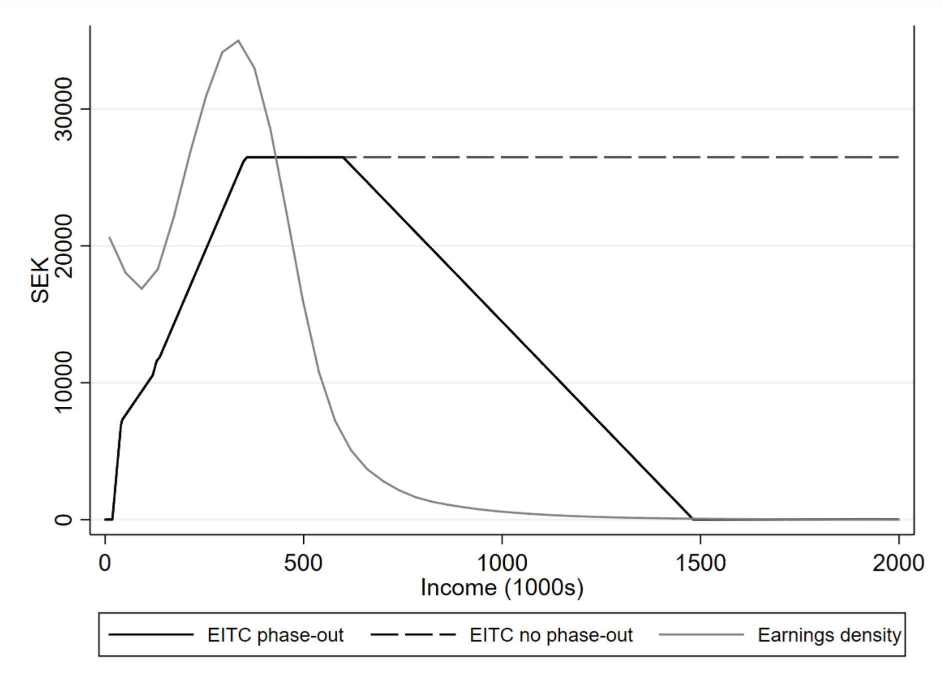
4 Empirical analysis

4.1 Empirical model

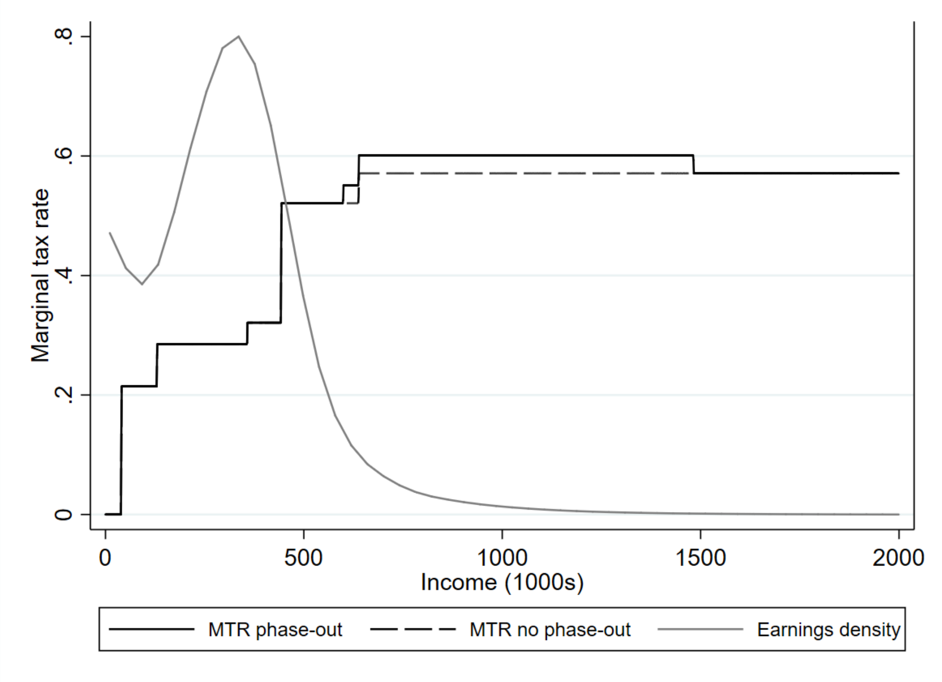
The basic idea behind the empirical model is to compare earnings growth at treated and untreated parts of the earnings distribution. For each year t we

[11] In 2015 the maximum deduction was lowered from SEK 12,000 to SEK 1,200. In 2016 the deduction was completely abolished. Hence, the biggest change occured the year before the 2016 EITC reform.

[12] In this subanalysis we use the same data source as Selin (2021), who was supported by SNS (Center for Business and Policy Studies). Unfortunately, we are not able to link these data directly to our main analysis data.



(a) EITC in SEK as function of earned income.



(b) Marginal tax rate, with and without EITC, as function of earned income. Social transfers are assumed to be zero.

Figure 1: Taxes as function of earned income in 2016

rank all individuals aged 20 to 65 by their earnings, z, and we partition the population into 100 equally sized percentile groups, which we follow over time.[13] One may think of a percentile group j as a synthetic unit who a given year face marginal tax rate $\overline{\tau}_{jt}$. In the main analysis we focus on the upper part of the earnings distribution, where the central government tax applies, during the time period 2012-2017. We start the analysis in 2012, when the Swedish economy had recovered from the 2008 financial crisis. In 2016 synthetic units belonging to perecentile groups 96-100 faced a marginal tax increase due to the EITC phase-out.[14] We will refer to percentile groups 96-100 as the treatment group, while percentile groups 88-95 constitute the control group. We want the individuals in the control group to be well above the first central government kink point, and therefore percentile group 88 is the lower limit of the control group. All percentile groups we study (88-100) are unexposed to marginal tax changes 2012-2015.

We first estimate reduced form regressions of the following type on percentile groups 88-100:

$$\log z_{ijt} = \sum_{t \geq 2016} \gamma_t^{post} D_{jt} + \sum_{t<2015} \gamma_t^{pre} D_{jt} + \kappa_t + \mu_j + \delta X_{ijt} + \epsilon_{ijt} \tag{1}$$

, where D_{jt} is an indicator that takes the value of 1 if percentile group j falls in the interval 96-100 and the year is t, and it is 0 otherwise. κ_t is a shorthand for the vector of time dummies, and μ_j represents a fixed effect at the percentile group level. In some specifications, we will also control for a vector of individual characteristics, which we denote by X. These include age, gender, education level, field of education, immigrant status, marital status, number of children, county, and 3-digit industry-codes. The year immediately preceding the reform, 2015, is the reference year. We cluster the standard errors at the percentile group

[13] We include people with zero earnings to reduce the influence of unemployment and non-participation.

[14] The extreme high-income earners in the top 0.2 percent group did not experience increasing *marginal* tax rates and were unaffected by the policy (their entire tax credit was phased-out on infra-marginal earnings). For simplicity, we include these taxpayers in the treatment group in the main analysis. The results are robust to excluding the same group.

level.[15]

The analysis requires two central identifying assumptions. *The parallel trends assumption* implies that the treatment and control groups would evolve in the same way in the absence of the 2016 reform. If the pre-reform trends are parallel in the treatment and control groups we expect γ_t^{pre} to be zero for 2012-14. By contrast, γ_t^{post} for 2016, 2017 and 2018 should be negative if the EITC phase-out has a negative impact on earnings in the post-reform period. *The constant group composition assumption* fails if the reform brings about non-random compositional changes to the treatment and control groups. A concern could e.g. be that responsive people in the treatment group respond to the reform by transitioning into the control group (or migrating from the analysis sample). In the next two subsections, these two critical assumptions will be tested (without being rejected).

We will estimate an empirical earnings elasticy, defined as the percentage change in earnings in response to a percentage change in the *statutory* net-of-tax rate, i.e. the tax rate that individuals belonging to a certain percentile group face as a function of their realized earnings. Note that the statutory tax rate is not necessarily equal to the perceived marginal tax rate. A simple Wald estimator for the empirical elasticity, η_t, evaluated in post-reform year t, is

$$\eta_t = \frac{\Delta_t E(\log z|X, D=1) - \Delta_t E(\log z|X, D=0)}{\Delta_t E(\log(1-\tau)|X, D=1) - \Delta_t E(\log(1-\tau)|X, D=0)} \tag{2}$$

, and the base year is always 2015. D is an indicator that is 1 if the percentile group is 96-100, and zero otherwise. Hence, Δ_t denotes the change in mean quantities in the treatment and control groups between t and 2015. If the earnings distribution is transformed by the EITC reform, it is reasonable to think that the effect materializes gradually rather than immediately. Therefore, the treatment estimates for 2016, 2017 and 2018 are likely to imply different earnings elasticities, and we will focus on the elasticity for 2018, which is more informative on the longer run elasticity.

In some specifications, we exclude four percentile groups in a symmetric

[15]We report analytical standard errors, but we have also computed standard errors using wild bootstrap. The results are similar.

window around the treatment cut-off, i.e. percentile groups 94-97. We hypothesize that the marginal tax increase is more salient to people who earn incomes at some distance from the kink. In Section 5 below we show that the "structural" elasticity differs from the empirically observed elasticity in (2) when people have noisy perceptions of the tax code.

4.2 Group composition over time

Before turning to the central graphical analysis we briefly comment on the distribution of observable characteristics. Table A1 in the Appendix shows descriptive statistics, 2012-2017, for the treatment group and the control group. The control group is larger than the treatment group, since it contains more percentile groups. On average, the treatment group contains almost 300,000 individuals per year, and the control group more than 450,000 individuals. Individuals in the treatment group are slightly older (48 years of age compared to 46), fewer are females, and they have higher education. This was to be expected since earnings is higher in the treatment group. It is maybe a little bit more surprising that the immigrant shares in the two groups are quite similar.

Remember that a disadvantage of the cross sectional approach – as opposed to the longitudinal approach – is that the composition of treatment and control groups may change endogenosly due to the reform. From this perspective it is interesting to look at how *observable* characteristics develop over time in the two groups. If there is a discontinous change in the distribution of covariates in the reform year, compositional changes is likely to be an issue. Therefore, we graphed the evolution of four central covariates in the treatment and control groups over time in Figure 2. Individuals have higher education over time, the immigrant share increases, and the average age increases in both groups. This is in line with the evolution of the composition of the labor force in general. The share of females among high-income earners (both in the treatment and control group) increases over time, which is consistent with a shrinking gender wage-gap in Sweden in recent years (Medlingsinstitutet, 2021). Note that the trends in the two groups are not always parallel. E.g., the share with university degree grows substantially faster in the control group. But it is of central im-

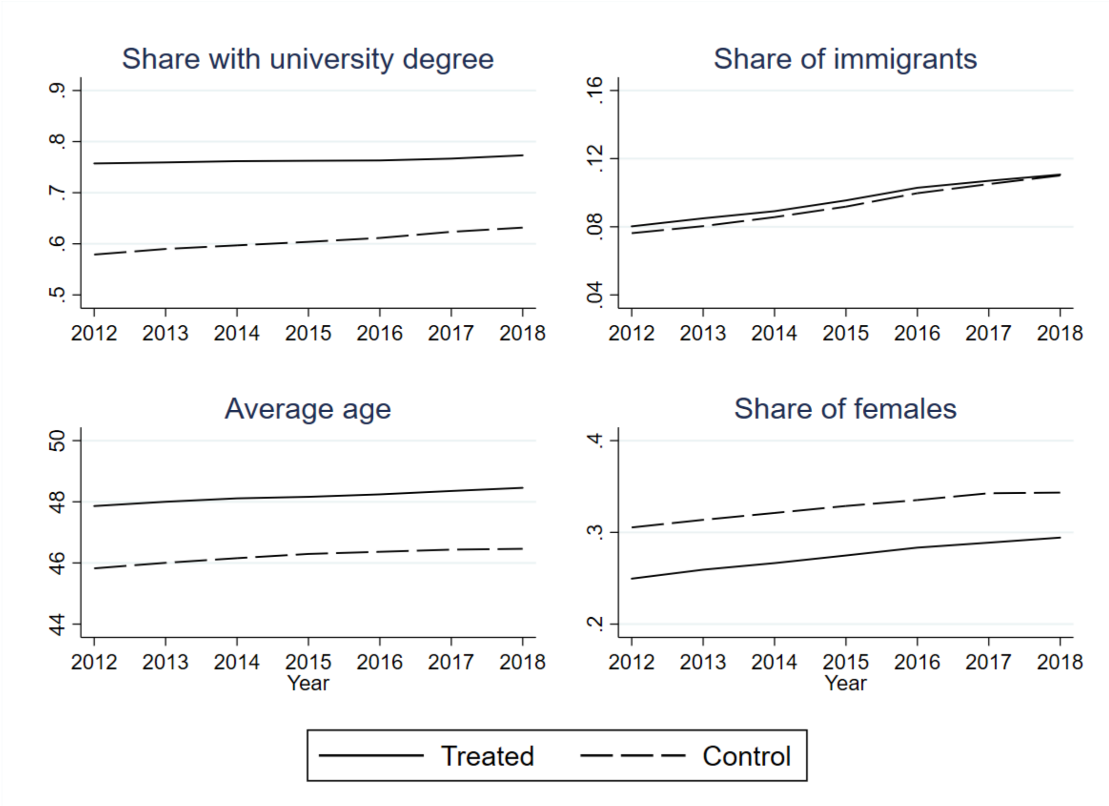


Figure 2: Demographics in treatment and control groups over time.

portance that there are no discontinous changes in 2016.[16] More information on covariates is reported in Table A1 in Appendix.

Another possibility is to exploit the panel element of the data, and to study transitions between the groups over time. The probability that an individual, who in year t belonged to the treatment group, is part of the treatment group also in $t+1$ is surprisingly stable over time. The fraction of stayers is around 83 % in the whole time period. Taken together, these specification tests did not detect significant compositional changes of the treatment and control groups.

[16]To test for this, we have run regressions with the covariate in question on the left hand side. The right hand side featured time dummies, a treatment group dummy, a treatment group dummy interacted with a time dummy and a linear treatment group specific trend. The interaction between the post-reform period and the treatment group dummy was always close to zero and insignificant.

4.3 Graphical evidence and main results

In Figure 3a we graph average log earnings in the treatment and control groups 2012-18. The graph entirely reflects raw data, where we have normalized the levels to be zero in 2015. There is a small tendency that log earnings grow faster in the control group between 2012 and 2013. However, the trends are extremely parallel 2013-15, suggesting that the parallel trends assumption holds. In the reform year of 2016 earnings growth begins to divert, and in 2017 there is a salient gap between the two lines. The gap widens slightly also 2017-18, but it is fair to say that the response stabilizes, and we are likely to capture a longer run response. Interestingly, earnings growth goes down also in the control group, and we will return to this phenomenon in Section 5 below. When we in Figure 3b exlude two percentile groups at each side of the cut-off, the pre-reform trends are still parallel. However, the post-refom gap is larger now. When evaluating elasticities for 2018 using the Wald estimtor of equation (2) we obtain an elasticity of 0.13 when we include all percentile groups 88-100, and we obtain an elasticity of 0.16 when excluding 2 percentile groups at each side of the cuf-off. This aspect of the observed response will also be further discussed in the simulation section 5 below.

Figure 3 provides a standard graphical difference-in-difference comparison. Still, clear graphical evidence of responses to income taxes is rare in the ETI literature. The closest examples we know of are Kleven and Schultz (2014, Figure 4) and Bergolo et al. (2022, Figure 3). Our graphical anlaysis is in fact even more basic, becasue we simply partition the raw data into percentile groups and compare them, and our policy experiment is a clean introduction of a new bracket.[17]

The regression results of Table 1 complements the visual analysis. In the absence of controls (columns 1 and 3), the coefficients for the interaction between the treatment group and the dummy for year t, γ_t^{pre} and γ_t^{post} in equation (1), correspond to the vertical distance between the solid line and the dashed line in year t in Figure 3. Interestingly, pre-reform interactions are insignificant across

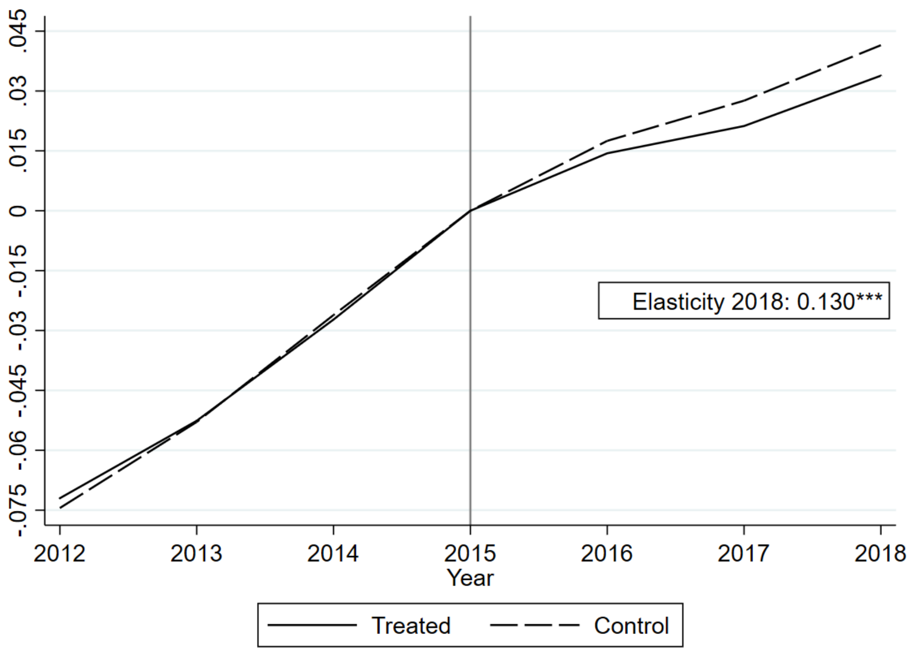
[17]The Danish 1987 reform was more complex than the reform we consider here, and it included both tax rate and tax base changes. Kleven and Schultz (2014) restrict their sample to a balanced panel of individuals who are observed throughout the period.

specifications, while the opposite holds for post-reform interactions. This indicates a causal effect of the EITC reform. There is a significant relative earnings reduction in the treatment group immediately appearing in 2016, growing in 2017, and stabilizing in 2018. What about the magnitudes? The logic behind the Wald estimate is highly transparent: When no percentile groups are excluded, the 2018 effect amounts to -0.76 log points without controls. When exluding percentile groups 94-97, we estimate a larger effect: -1.07 without controls in column 3, implying an elasticity of 0.164.

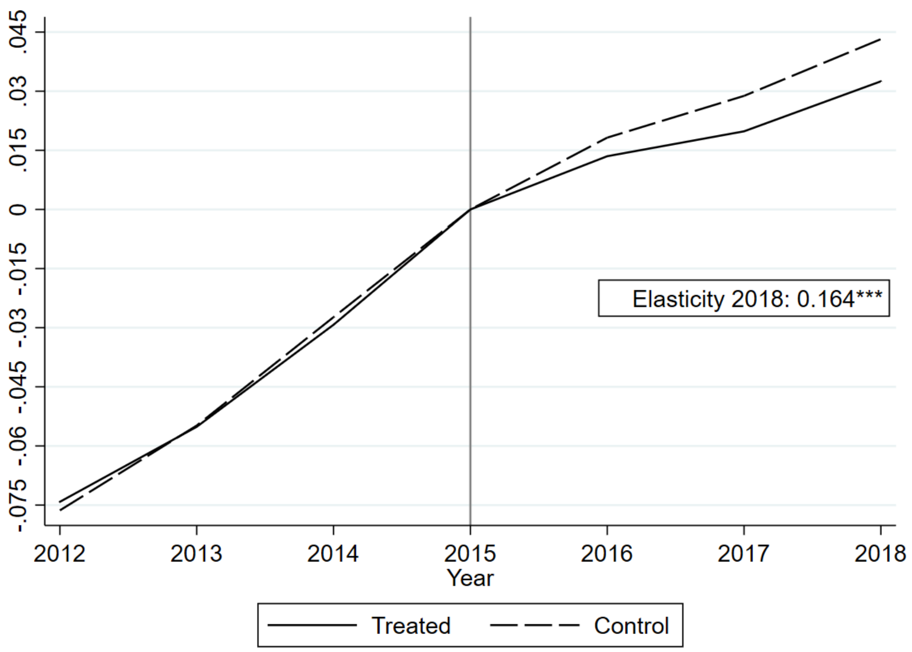
The results of columns 2 and 4, where we control for a rich sets of potentially confounding factors, deserve special attention. If the interactions of interest would be correlated with the error term in (1), we expect control variables to have a large impact on the estimated reform effects. Since we are working on a large adminstrative data set, we are able to include a rich set of controls in a very flexible way. We use dummies for age, gender, education level, education field, immigrant status, marital status, number of children, county, and a 3-digit industry-codes. Moreover, we interact all these dummies with the full set of time dummies. Still, it turns out that the controls only have a negligible effect in the specification including all percentile groups (column 2). Similarly, the same holds for the results when excluding percentile groups 94-97. Given that endogenous compositional changes is one of the main concerns with the cross sectional approach, we find these reults reassuring. They are also consistent with the analysis of Section 4.2, which demonstrated that there are no discontinuous changes in central observable characteristics in the reform year.When exluding percentile groups 94-97, we estimate a larger effect: -0.90 without controls in column 3, implying an elasticity of 0.14. The 2017 effect is always estimated to be larger than the 2016 effect.

4.4 Alternative specifications

In this section we summarize what we get from some alternative approaches.



(a) All percentile groups 88-100



(b) Excluding percentile groups 94-97 around the new bracket cut-off

Figure 3: Average log earnings in treatment and control groups. Average log earnings are normalized to be zero in both groups in 2015. Raw data. Incomes are expressed in the price level of 2016.

Table 1: DiD-regressions

	(1)	(2)	(3)	(4)
	All percentiles		Percentiles 94-97 excluded	
	No controls	Controls	No controls	Controls
Treated * 2012	0.246	0.255	0.217	0.260
	(0.270)	(0.243)	(0.349)	(0.304)
Treated * 2013	0.029	0.043	-0.031	0.017
	(0.296)	(0.267)	(0.384)	(0.330)
Treated * 2014	-0.113	-0.095	-0.193	-0.154
	(0.211)	(0.187)	(0.270)	(0.230)
Treated * 2016	-0.312***	-0.306***	-0.472***	-0.498***
	(0.091)	(0.104)	(0.101)	(0.139)
Treated * 2017	-0.635***	-0.633***	-0.899***	-0.944***
	(0.128)	(0.140)	(0.089)	(0.118)
Treated * 2018	-0.763***	-0.757***	-1.070***	-1.114***
	(0.177)	(0.177)	(0.177)	(0.167)
Observations	5,261,287	5,261,287	3,642,428	3,642,428

Notes: All regressions (columns 1–4) include controls for year and percentile group. Regressions in cloumns (2) and (4) include controls for age, gender, education level, education field, immigrant status, marital status, number of children, county, and 3-digit industry-codes. All control variables are also interacted with year dummies. Standard errors are clustered at percentile groups. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.4.1 Predicted income

The main analysis is simple and transparent, and it has a close connection to basic descriptive analysis. However, a potential criticism is that we assign individuals into treatment and control groups based on the outcome variable (log earnings). As already discussed, this is controversial from an econometric perspective, because the composition of these groups may change if people respond to the reform in such a way the ordering of taxpayers is altered. Therefore, we also estimated a model, in which we use percentile groups based on *predicted* rather than *actual* income. In a first step, we predict income as functions of pre-determined characteristics, which are plausibly orthogonal to the reform. Actual earnings now differ less in different percentile groups, because someone who is in the top percentile group may report very low actual earnings, and vice versa. Unfortunately, when predicting income based on genuinely pre-determined characteristics we do not obtain a sufficiently strong first stage, see Appendix B.1 for more details. Intuitively, observable characteristics simply explains too little of the variation in earnings at high income levels.

4.4.2 Panel data

We have run Gruber and Saez (2002) like regressions, which means that we regress individual level changes in log earnings on indiviudal level changes in log net-of-tax rates 2015-18. We construct a tax instrument as a function of base year earnings, and we control for log base year earnings. The results are not that informative: estimates varies a lot depending on specification. As expected, transitory incomes play an important role. More information is provided in Appendix B.2.

4.5 Is the response driven by income shifting?

An important feature of the Swedish income tax system is that labor incomes are taxed progressively, whereas capital incomes are taxed at a low proportional rate. In the Swedish dual income tax system high-income earners therefore face substantial incentives to shift income between the tax bases. Usually, regular wage earners cannot transform earnings into capital income, because their wage income is third-party reported. The situation is different, however, for active owners of closely held corporations (CHC:s), who are working in their own firms, and are able to distribute themselves dividends instead of wages. Alstadsæter and Jacob (2016) have documented that such activities are important in Sweden.[18] Already before the 2016 reform there was a large gap of almost 30 percentage points between the top effective labor marginal tax rate and the effective tax rate on dividends from CHC:s after accounting for payroll taxes and corporate taxes.[19] When the EITC phase-out was introduced in 2016, the gap widened to 32 percentage points.

Against this background it is natural to ask whether the response we observe in the main analysis is driven by income shifting of active CHC owners. The most simple way to examine this is to exclude the potential group of "shifters", namely the active owners of the CHC:s. In Table 2, we do this in two steps. In column 2 we first exclude CHC owners who receive dividends from their own corporation. They correspond to 7 % of the baseline sample. However, all active CHC owners do not receive dividends a specific year. In column 3 we exclude all CHC owners, i.e. 13 % of our original study population in percentile groups 88-100. When re-estimating the model we use the same percentile limits as in the main analysis.

We infer from Table 2 that there are no dramatic changes to the results when excluding CHC owners.[20] If anything, there is a tendency that the reform effects

[18]Swedish CHC owners cannot distribute lightly taxed dividends freely. Each year there is a cap on dividends that can be taxed at the low rate, commonly referred to as the *dividend allowance*. The dividend allowance has become more genereous over time.

[19]The effective marginal tax rate on labor income can be written $1 - \frac{1-\tau}{1+\tau^p}$, where τ is the personal marginal tax rate and τ^p is the payroll tax rate. The effective dividend tax rate can be written $1-(1-\tau^d)(1-\tau^{\Pi})$, where τ^d is the dividend tax rate and τ^{Π} is the corporate tax rate. (Here we do not account for consumption taxes, because they do not change the relation between the two tax rates.) In 2015 we had $\tau = 0.57$, $\tau^p = 0.3142$, $\tau^d = 0.2$, and $\tau^{\Pi} = 0.22$. Hence, the effective labor tax rate was around 0.67, wheras the effective dividend tax rate amounted to around 0.38. In the 2016 phase-out reform τ increased from 0.57 to 0.6, and the effective labor tax rate rose to around 0.7.

[20]As discussed in Section 3, in these estimations we used a separate data source that contains information on the corporate owners' income tax returns. To identify the groups of "shifters" we used information from the so-called K10 form. The K10 form must be filled in by all active CHC owners who want to accumulate or use dividend allowances.Column 2 excludes everyone

amplify in columns 2 and 3. Therefore, we do not think that the estimated response is driven by income shifting between the labor and capital income tax bases.

4.6 Other potential explanations

Two other potential explanations are reported in Table C1 of Appendix C, with our baseline results reported in column 1. In column 2, we exclude individuals who made deductible contributions to tax-favored pension savings accounts in 2014. The reason is that the pension deduction of employees was abolished in two steps 2015-16, and we want to examine if the overall response could be driven by employees who after 2014 lowered their wages in exhange of higher occupational pension benefits (”löneväxling”). However, when excluding individuals who made pension deductions in 2014, it turns out that the estimated treatment effects actually become slightly larger. Hence, we do not believe that the observed response is driven by a substitution from deductable pension contributions (not subtracted from the wage bill) to occupational pension contributions (subtracted from the wage bill).

One could argue that the observed response could be driven by people being less inclined to switch jobs, or that people to a lesser extent take on second jobs. In column 3, we examine these margins by only studying people who have had income from only one employer each calender year, 2012-2017. Since the results do not change if we restrict the analysis to this group, we infer that the results are not driven by job-movers or those having second jobs.

Finally, we have examined response heterogeneity along several other dimensions, and the general picture is that the response is quite stable across different subgroups, like industries and gender (not reported).

who reports dividends on a K10 form, and column 3 excludes all those filing a K10 form. Note also that there are slight differences in parameter estimates and in the number of observations in column 1 of Table 2 and column 1 of Table 1, reflecting that we use a different data source.

Table 2: DiD-regressions excluding "shifters"

	(1) All	(2) No CHC dividends	(3) No CHC owners
Treated * 2012	0.267	0.315	0.422*
	(0.261)	(0.260)	(0.227)
Treated * 2013	0.068	0.200	0.334
	(0.283)	(0.272)	(0.273)
Treated * 2014	-0.078	0.025	0.136
	(0.203)	(0.205)	(0.186)
Treated * 2016	-0.281***	-0.333***	-0.310***
	(0.075)	(0.107)	(0.098)
Treated * 2017	-0.598***	-0.701***	-0.704***
	(0.118)	(0.143)	(0.146)
Treated * 2018	-0.727***	-0.887***	-0.985***
	(0.172)	(0.147)	(0.161)
Observations	5,270,161	4,898,638	4,576,718

Notes: All regressions (columns 1–3) include controls for year and percentile group. Standard errors, clustered at percentile groups, in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Percentile limits in columns 2 and 3 are the same as in the main analysis reported in column 1.

5 How sharp is the response? A model interpretation

In this section we set up a simulation model in which taxpayers cannot perfectly control their incomes and, hence, face uncertain marginal tax rates. Our objective is to examine if a realistically calibrated model can explain the sharpness of the response we observe.

5.1 Model

Consider a model economy in which agents differ with respect to potential incomes (skills) z_0. Individuals derive utility from consumption, c, and disutility from earnings supply, z. The budget constraint is $c = z - T(z)$, where $T(z)$ is a piece-wise linear tax function, perfectly perceived by the individuals. Under certainty, an individual maximizes the utility function

$$U = z - T(z) - \frac{z_0}{1+\frac{1}{e}}\left(\frac{z}{z_0}\right)^{1+\frac{1}{e}} \tag{3}$$

with respect to z. If the individual has an optimum at a linear segment of the budget constraint, the optimal earnings supply function is $z = (1-\tau)^e z_0$, where τ is the marginal tax rate. Under these assumptions, the earnings supply only depends on the marginal tax rate, τ, and the potential income z_0. If $\tau = 0$ we have $z = z_0$. The (compensated) earnings elasticity is e. Note that there are no income effects on earnings supply. This is quite an innocuous assumption in this context. Most treated taxpayers only experienced small changes in disposable incomes. We choose to refer to the "earnings elasticity" rather than the "taxable income elasticity", since labor earnings is the base for the EITC (the tax rate variation we are using).

Following Saez (1999) we now extend the standard earnings supply model to a choice environment featuring uncertainty. Historically, this framework has been used to rationalize that taxpayers do not bunch at kinks, see e.g. Blomquist and Simula (2019). We instead use it to interpret broader transformations of the earnings distributions when taxes change. Suppose that individuals are able to control expected, but not realized, earnings. More specifically, suppose that realized earnings is given by

$$\tilde{z} = z + \varepsilon \tag{4}$$

, where z is chosen by the individual, and ε is a stochastic term, which the individual cannot control. An example of a positive shock could be an end-of-year bonus. A negative shock could be an unexpected sickness absence spell. The agent does, however, know the distibution of ε. For simplicity, we assume that the stochastic element of realized earnings is normally distributed with mean zero, i.e. $\varepsilon \sim N(0, \sigma^2)$. The agent maximizes expected utility

$$EU = \int \{z + \varepsilon - T(z + \varepsilon) - \frac{z_0}{1 + \frac{1}{e}} \left(\frac{z}{z_0}\right)^{1+\frac{1}{e}}\} f(\varepsilon) d\varepsilon \tag{5}$$

, where $T(\tilde{z})$ is the piecewise linear tax function and $f(\varepsilon)$ is the pdf of the normal distribution. Note that the disutility of earnings supply is known with certainty, while consumption differs depending on the realization of ε. When utitlity is quasi-linear in consumption (5) can be rewritten as

$$EU = z - \hat{T}(z) - \frac{z_0}{1 + \frac{1}{e}} \left(\frac{z}{z_0}\right)^{1+\frac{1}{e}}. \tag{6}$$

$\hat{T}(z) = \int T(z + \varepsilon) f(\varepsilon) d\varepsilon$ can be thought of as the *effective* tax schedule facing the individual. In a way, the choice problem under uncertainty has been transformed into a choice problem under certainty. Maximizing (6) with respect to z is akin to the certainty problem given by (3). The only diffference is that the actual *piece-wise linear* tax function $T(z)$ is replaced by a *smooth* tax function $\hat{T}(z)$.

Intuitively, when people do not know their incomes by the end of the year with certainty, the marginal incentives to work will be given by a weighted average of actual marginal tax rates around their expected earnings levels. As most other tax systems, the actual Swedish income tax system features sharp kink points, where marginal tax rates change. These kinks are absent in the effective tax schedule, $\hat{T}(z)$, where marginal tax rates instead changes gradually around the actual kink.[21] When a new tax bracket is introduced, like the Swedish EITC phase-out in 2016, effective tax rates of those with realized incomes below the new statutory kink will also be affected. The standard deviation of ε, σ, is a key paramater determining the shape of the effective marginal tax schedule. If σ is small, effective and actual marginal tax rates will differ only locally around kinks. In the special case in which $\sigma = 0$ the two schedules will be identical. If σ is large, the introduction of a new kink to the actual tax system will impact effective marginal tax rates over wide ranges of income that would be unaffected in the certainty model.

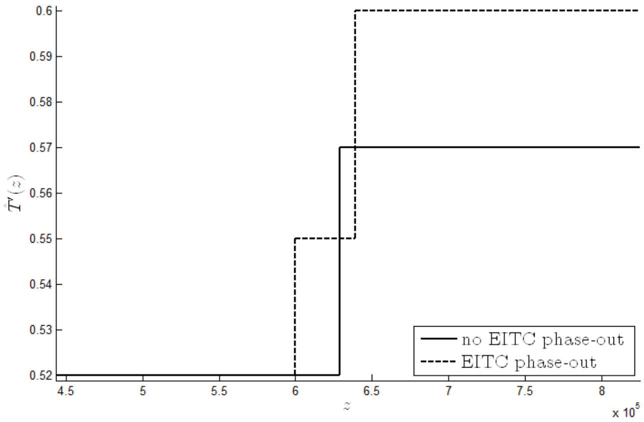
5.2 Earnings dynamics

What is a reasonable value of σ from the perspective of earnings dynamics? Suppose that taxes are constant over time and that individuals have skill level, z_0, both in year $t-1$ and t. Since $\tilde{z}_t \sim N(z_t, \sigma^2)$ we have that the difference is also normally distributed with twice the variance, i.e. $\tilde{z}_t - \tilde{z}_{t-1} \sim N(0, 2\sigma^2)$. Accordingly, if σ^{diff} is the standard deviation of the distribution of indivdidual realized earnings differences, it holds true that $\sigma = \frac{\sigma^{diff}}{\sqrt{2}}$. Following Saez (1999) we use this relationship to quantify σ on panel data data from 2012-15, i.e. the pre-reform years with no major tax changes and a stable earnings distribution. In the 95th percentile group the standard deviation amounts to $\sigma = 70,000$, and we think that this is an upper bound of a relevant value of σ. We elaborate more on this in Appendix D.1.[22] We pick σ for the 95th percentile group, because the kink that we want to smooth is approximately located there.

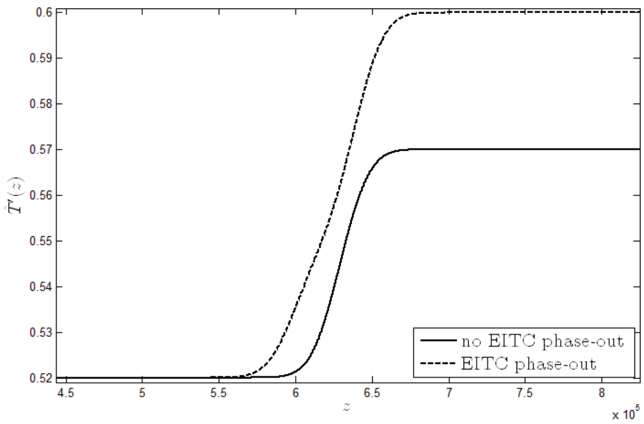
In Figure 4 we illustrate the EITC reform in the effective marginal tax schedule for 2016, with and without an EITC phase-out for different values of σ. When $\sigma = 70,000$ many taxpayers both to the left and to the right of the EITC

[21] In Appendix D we describe how we smooth the actual tax schedule.

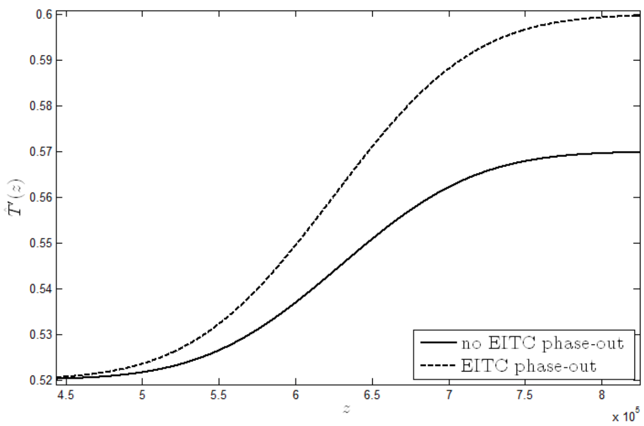
[22] When quantifying σ it is important to distinguish between ε and the error term in the empirical equation, which e.g. is denoted by ϵ in the reduced form equation (1) above. While the latter represents factors unobserved by the econometrician, the former refers to factors that are random from the individual's perspective. Of course, if σ is large, the variance of the transitory empirical error term will probably be large as well. However, the individual may non-randomly choose different earnings levels from year to year for reasons that are known by the individual but unobserved by the econometrician.



(a) $\sigma = 0$



(b) $\sigma =$ SEK $15,000$



(c) $\sigma =$ SEK $70,000$

Figure 4: Reform in effective marginal tax schedules for different values of σ.

kink are affected by the reform.

5.3 The simulation model

Can the empirically motivated value of σ reproduce the empirically observed response for a plausible value of the elasticity paramater e? To obtain a view on this issue, we now build a simulation model. For a given skill distribution, we will simulate the 2015 and 2018 earnings distributions, and we will estimate our empirical model on the simulated data.

The skill distribution is a key input to the simulation model. In the spririt of Saez (2001), we recover the distribution of z_0 from the empirically observed earnings distribution and the elasticity parameter e, see Appendix D for details. When changing e we always recalibrate the skill distribution.

A second input is the statutory tax schedules for 2015 and 2018. For tractability, we simplify the tax schedules by merging the EITC kink and the second central government kink in 2018, and we remove the non-convex kink where the entire EITC has been phased out. We therefore end up with two-bracket schedules for 2015 and 2018. In 2015, the marginal tax increases by 5 percentage points at the kink, and in 2018 it increases by 8 percentage points. In the simulations, the kink point is located at the same nominal value both years.[23].The piece-wise linear schedule is smoothened using different values of σ.

Given the skill distribution z_0 and the effective tax schedules $\hat{T}(z)$ we obtain two distributions of deterministic incomes z. When $\sigma > 0$ there is no sharp bunching of taxpayers' z at the kink point of the statutory schedule, and when σ becomes larger the response in z around the kink will smoothen out. There is, however, another source of noise in the model, because the individual's realized income is given by $\tilde{z} = z + \varepsilon$. We will estimate difference-in-difference models by partitioning agents into treatment and control groups based on the realized income distribution and the statutory tax schedules $T(\hat{z})$. Agents with realized incomes larger than the statutory kink belong to the control group, and those below belong to the control group.

[23] There is a one-to-one mapping between skill, z_0, and deterministic earnings z. However, the distribution of *realized* earnings $\tilde{z} = z + \varepsilon$, from which we infer the elasticities, will be slightly different.

In the empirical analysis we obtained an earnings elasticity estimate of 0.130 when including all percentile groups, and we obtained a point estimate of 0.164 when excluding two percentile groups at each side of the cut-off. In our simulation model, such a pattern is also likely to occur, because the tax change is smaller near the kink relative to higher earnings levels. Let η^1 refer to the estimated elasticity on empirical data when all percentile groups are included, and let η^2 refer to the estimated elasticy when two percentile groups at each side of the bracket cut-off are excluded. The corresponding elasticities obtained on simulated data are denoted by $\alpha^1(e, \sigma)$ and $\alpha^2(e, \sigma)$. Conditional on $\sigma =$ SEK 70,000 we want to find the value of e that are closest to reproduce η^1 and η^2. Formally, we wish to minimize the following loss function:

$$\mathcal{L}(e|\sigma) = \sum_{i=1,2} [\eta^i - \alpha^i(e|\sigma)]^2, \tag{7}$$

with respect to e. In practise, we compute $\mathcal{L}(e|\sigma)$ for different values of e.

5.4 Simulation results

In Table 3 we vary the underlying elastiticy parameter e, while conditioning on $\sigma =$ SEK 70,000, the empirically motivated value. Both $\alpha^1(e|\sigma)$ and $\alpha^2(e|\sigma)$ are monotonically increasing in e. We also see that there is always a substantial gap between $\alpha^1(e|\sigma)$ and $\alpha^2(e|\sigma)$. We are closest to reproduce the empirial elasticities when $e = 0.22$ when constraining σ to be SEK 70,000. Still, the gap is somewhat larger than the empirical gap. This suggests that, if anything, the empirical response is sharper than the simulated one. Of course, the simulated elasticities for $e = 0.22$ are still close to the empirical point estimates (and contained in the confidence intervals).

In Table 4 we illustrate the "global" minimum of the loss function $\mathcal{L}(e, \sigma)$. For different values of σ we report results for $e = e^*$, where e^* minimizes $\mathcal{L}(e|\sigma)$. For all values of σ there is a gap between $\alpha^1(e|\sigma)$ and $\alpha^2(e|\sigma)$. This also holds true for the "frictionless setting" when $\sigma = 0$, because a fraction of agents in the treatment group then bunch at the kink, causing the estimated elasticity to be smaller than e when all percentile groups are included. We see that the loss

Table 3: Simulated responses for σ = SEK 70,000

e	0.19	0.2	0.21	0.22	0.23
Exclude +/- 2 percentiles	0.145	0.155	0.160	0.168	0.175
All percentiles	0.108	0.115	0.119	0.125	0.130
Loss function	$8.16e-4$	$2.87e-4$	$1.30e-4$	$4.39e-5$	$1.32e-4$

Table 4: Simulated responses for different noisy perceptions

σ	0	30,000	40,000	50,000	60,000	70,000
e^*	0.15	0.16	0.19	0.19	0.21	0.22
Exclude +/- 2 percentiles	0.150	0.162	0.165	0.171	0.167	0.168
All percentiles	0.147	0.135	0.133	0.131	0.126	0.125
Loss function	$4.84e-4$	$3.16e-5$	$1.24e-5$	$4.62e-5$	$2.10e-5$	$4.39e-5$

function is minimized when σ = SEK 40,000.[24]

Note that our model assumes that agents have full knowledge about the tax system, and all noise to perceptions is driven by earnings dynamics. Taxpayers' knowledge of the tax code has been questioned in recent years. Liebman and Zeckhauser (2004) and **?** claim that people rely on simple mental heuristics rather than perfect information when responding to complex non-linear tax schedules. In particular, people tend to confuse marginal and average tax rates. Taking such noise in perceptions into account in the simulation would lead to an even more diffuse response. Hence, from this exercise we learn that the empirically observed response is surprisingly sharp given the earnings variability at the top of the earnings distribution, and taxpayers' imperfect knowledge of the tax code.

Another lesson is that the estimated elasticity is consistent with an underlying elasticity parameter of 0.22 given σ = SEK 70,000. One should keep in mind that we abstracted from the class of optimization frictions analyzed by Chetty (2012). Chetty claims that researchers tend to obtain downward biased estimates of the underlying structural elasticity when using small tax reforms for identification. We do indeed exploit a relatively small tax reform, and such

[24]We have also run simulations for larger values of σ (80,000, 90,000, and 100,000). e^* is monotonically increasing in σ, and the loss function is always larger than $1.24e-5$.

a bias might be present. On the other hand, our estimates are well in line with those for married males in Blomquist and Selin (2010), who exploited a much larger Swedish tax variation.

Finally, we also acknowledge that there are other potential mechanisms than earnings uncertainty that could generate the difference between the two empirically estimated elasticities. One would expect to estimate a larger elasticity when omitting four percentiles around the kink if the underlying elasticity were increasing in the income level, as suggested by Gruber and Saez (2002). A similar pattern may also arise if taxpayers choose between a number of discrete earnings levels, see Kosonen and Matikka (2020) for a recent discussion. In such a model, average tax rates become relevant, and these increase more for agents who have pre-reform earnings far to the right of the kink.

6 Concluding discussion

We evaluate earnings responses to a 7 % reduction in the net-of-tax rate, affecting Swedish high-income earners already facing high taxes. We exploit full-population administrative data, and we graphically compare earnings growth at different parts of the income distribution. With three years of post-reform data, we estimate earnings elasticities in the range 0.13-0.16. The response is not driven by income shifting of active owners of closely held corporations.

When people facing non-linear tax schedules cannot perfectly control their incomes, perceptions of marginal incentives will be noisy as well, especially for high-skilled workers. We interpret essential features of the response using a simulation model, in which people have noisy perceptions of the piece-wise linear tax code. When simulating the model, we find that the empirically estimated response is surprisingly sharp given the earnings variability at the top of the earnings distribution. Actually, we get closer to the actual empirical estimates if we impose a standard deviation in earnings shocks that is lower than the empirically observed standard deviation.

What are the implications of our results for the revenue maximixing tax rate? When the right tail of the skill distribution is approximately Pareto distributed, and there are no income effects, there is a well-known relationship between the Pareto coefficient, a, and the earnings elasticity e, namely $\tau^* = \frac{1}{1+a \times e}$. This asymptotic top tax formula, originally presented by Diamond (1998) only requires two quantities, and hence offers a simple tool to detect the peak of the Laffer curve.[25] The effective marginal tax rate was 72.5% before the 2016 EITC phase-out reform. Moreover, the Pareto parameter of the earnings distribution was around 3.[26] For the pre-reform tax level to be below the peak of the Laffer curve, the earnings elasticity must not exceed 0.13. Our analysis indicates that the relevant elasticy is close to, or somewhat larger, than this cut-off value: the empirically observed elasticity is in the range 0.13 to 0.16. In the simulations the observed response was consistent with an underlying elasticity of 0.21. These elasticity estimates indicate that Sweden operated at the wrong side of the Laffer curve already before the reform we evaluate in this paper.

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[25]When the social welfare weight applying to extreme high income earners asymptotically approaches zero, the expression also reflects the socially optimal asymptotic marginal tax rate. Saez (2001) shows how to include income effects in this formula.

[26]See e.g. Bastani and Lundberg (2017, Figure 5a), who report that the Pareto coefficient is around 3. However, the Pareto coefficient of the distribution of factor incomes is substantially smaller and around 2 (the right tail is thicker) (Pirttilä and Selin, 2011a).

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Appendix

A Summary statistics

Summary statistics are reported in Table A1.

B IV estimates using predicted earnings and panel data models

B.1 Predicted earnings

To avoid potential problems with endogeneity, we have estimated models where we group individuals into percentile groups based on predicted earnings rather than actual earnings. We regress earnings on a set of pre-determined characteristics that are arguably exogenous to reform, and we use the predicted values from these regressions to group individuals to percentiles. The treatment group is defined as individuals belonging to percentile groups 96 and above, as when we grouped on actual earnings.

After classifying individuals to percentile groups based on predicted earnings, we estimate the following model on data from 2015 and 2018:

$$log(z)_{ijt} = \alpha + \beta * log(ntr)_{ijt} + \mu_t + \mu_j + \eta_{ijt}. \tag{B1}$$

Log earnings (log(z)) for individual i in percentile j in year t is regressed on log net-of-tax rates (log(ntr)), time fixed effects (μ_t), and percentile fixed effects (μ_j). Log(ntr) is instrumented by belonging to the treatment group in 2018 (i.e. the interaction of treatment status and a dummy for 2018). The model is estimated by 2sls, the control group are individuals in percentile groups 88-95, and the standard errors are clustered at the percentile group level. As a sensitivity check, we include controls for age, gender, education (level and field), marital status, immigrant status, municipality, industry, and occupation.

It is difficult to find pre-determined characteristics that are exogenous to reform, *and* are able to predict individuals correctly to treatment and control

Table A1: Descriptives, 2012–2018

	(1) 2012	(2) 2013	(3) 2014	(4) 2015	(5) 2016	(6) 2017	(7) 2018
				Panel A: Treated			
Age	47.86	48.00	48.11	48.16	48.24	48.35	48.46
	(8.725)	(8.642)	(8.582)	(8.540)	(8.518)	(8.509)	(8.501)
Female	0.250	0.259	0.267	0.275	0.283	0.289	0.294
	(0.433)	(0.438)	(0.442)	(0.446)	(0.451)	(0.453)	(0.456)
Immigrant	0.080	0.085	0.089	0.096	0.103	0.107	0.111
	(0.272)	(0.279)	(0.285)	(0.294)	(0.304)	(0.309)	(0.314)
Secondary educ	0.211	0.209	0.208	0.207	0.206	0.203	0.199
	(0.408)	(0.407)	(0.406)	(0.405)	(0.404)	(0.402)	(0.399)
University	0.757	0.759	0.762	0.762	0.763	0.767	0.773
	(0.429)	(0.428)	(0.426)	(0.426)	(0.425)	(0.423)	(0.419)
Married	0.651	0.650	0.649	0.648	0.646	0.645	0.644
	(0.477)	(0.477)	(0.477)	(0.478)	(0.478)	(0.478)	(0.479)
Observations	283,717	285,234	287,147	288,889	291,815	294,203	292,564
				Panel B: Control			
Age	45.82	46.00	46.16	46.30	46.37	46.44	46.46
	(9.892)	(9.829)	(9.754)	(9.698)	(9.693)	(9.696)	(9.722)
Female	0.305	0.314	0.321	0.329	0.335	0.343	0.343
	(0.461)	(0.464)	(0.467)	(0.470)	(0.472)	(0.475)	(0.475)
Immigrant	0.076	0.080	0.086	0.092	0.100	0.105	0.110
	(0.265)	(0.272)	(0.280)	(0.289)	(0.300)	(0.307)	(0.313)
Secondary educ	0.360	0.352	0.347	0.342	0.336	0.327	0.322
	(0.480)	(0.478)	(0.476)	(0.474)	(0.472)	(0.469)	(0.467)
University	0.579	0.590	0.597	0.604	0.612	0.624	0.632
	(0.494)	(0.492)	(0.491)	(0.489)	(0.487)	(0.484)	(0.482)
Married	0.538	0.538	0.539	0.539	0.540	0.539	0.536
	(0.499)	(0.499)	(0.498)	(0.498)	(0.498)	(0.498)	(0.499)
Observations	453,948	456,376	459,436	462,223	466,906	470,725	468.104

Notes: Means, with standard deviations in parenthesis.

groups, i.e. are able to provide a significant first stage. Since the reform affects individuals with very high earnings, we need to find observable pre-determined characteristics that can differentiate between individuals at the very top of the earnings distribution. Typically, when good earnings predictors are observable, they are related to labor market characteristics (e.g. occupation), which in turn could be argued to be endogenous to reform.

In Table B.1, we show results for two types of predictions. In the first case (columns 3 and 4), we aim to use strict pre-determined characteristics. First, we use information on age, gender, immigrant status, and education. Second, we use information that potentially could be endogenous (occupation, industry, municipality of residence and marital status), but we lag those characteristics two years in order to circumvent endogeneity issues. In the second case of predictions (column 5 and 6), we use all the above mentioned information contemporary. The idea is to get best possible predictions. In these specifications, we should get a stronger first stage, but potentially have a bigger issue with endogeneity. As a comparison, we replicate the results where we have grouped individuals based on actual earnings (columns 1 and 2).

In the case with strictly pre-determined characteristics, we get an insigificant estimate, when we do not include control variables (Table B.1, column 3). However, when we include control variables, we obtain a very high elastiicty estimate of 1.9 (Table B.1, column 4). If we allow ourselves to view all characteristics as being pre-determined, our first stage gets somewhat stronger when adding controls (column 6). The elasticity estimate reported in column 6 is substantially lower than in columnn 4 – it now amounts to 0.62. We conclude that the elasticity esitmates are highly sensitive to the choice of specification when using instruments that are functions of predicted earnings. Maybe this should not be that surprising, it is genuinly hard to predict earnings in the very top of the earnings distribution where individuals' earnings vary substantially between years.

Table B1: IV-estimates using predicted earnings

	Outcome variable: ln(earnings)					
	Actual earnings		Predicted earnings (Lagged charac.)		Predicted earnings (Contemporary charac.)	
	No controls	With controls	No controls	With controls	No controls	With controls
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(ntr)	0.130***	0.137***	0.285	1.884***	0.667	0.624***
	(0.028)	(0.032)	(0.332)	(0.540)	(0.487)	(0.199)
Controls	No	Yes	No	Yes	No	Yes
F-values	30	30	25	18	12	23
Clusters	13	13	13	13	13	13
Observations	1,511,780	1,511,780	1,511,780	1,511,780	1,511,780	1,511,780

Notes: The outcome variable is ln(earnings). All specifications include controles for percentiles, treatment status, and year effects. The other control variables included in columns 2, 4, and 6 are: age, gender, education (level and field), marital status, immigrant status, municipality, industry (3-digit), and occupation (3-digit). Standard errors are clustered at percentile level and shown in parentheses. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

B.2 Panel data models

First, we have estimated panel data models, using data from 2015 and 2018, to study the effects of changes in log net-of-tax-rates on changes in log earnings. More specific, we have estimated the following model using 2sls:

$$\Delta log(z)_{ijt} = \alpha + \beta * \Delta log(ntr)_{ijt} + \eta_{ijt} \tag{B2}$$

where the we instrument the changes in log net-of-tax rates. We create tax instruments by using previous earnings, i.e. we calculate net-of-tax rates for 2017 using previous earnings (Gruber and Saez, 2002). To correct for mean reversion, we include measures of previous earnings in the model. We estimate the model on individuals aged 20–50 in 2015, earning more than 500 000 SEK, and cluster standard errors at the individual level.

We have tried three different measures of previous earnings, creating three different tax instruments: earnings from 2015 (base-year earnings, as standard in the literature), as well as average earnings 2015–2017. For all three instruments, we present results with and without including controls for the previous earnings measure (Table B.2).

The overall picture from Table B.2 is that the estimates vary considerably, both between instruments, and with/without controlling for previous earnings.[27] Our conclusion is that this type of model appear not to be suited for analyzing this reform.

Table B2: IV estimates

	Dependent variable: $\Delta ln(earnings)$			
	Instrument: Earnings 2015		Instrument: Avg. earnings 2015–2018	
	(1)	(2)	(5)	(6)
Δln(ntr)	0.996***	-0.790***	-2.464***	-0.090
	(0.138)	(0.151)	(0.031)	(.067)
Controlling for ln(e)	No	Yes	No	Yes
Observations	364,292		364,292	

Notes: The sample consists of individuals aged 20–50 in 2015, earning more than 500 000 SEK. Differences corresponding to changes between 2015 and 2018. Standard errors clustered at individual level. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

Next, in order to gain precision/stability, we have utilized more pre-reform data, i.e. data back to 2012. We calculate 1-, 2-, and 3-year differences in log earnings, and instrument the changes in log net-of-tax-rates using base year incomes. We stack differences as done by Gruber and Saez (2002), and we estimate the same model as before. The results, presented in Table B.2, however, still vary considerably, and we infer that these panel data approaches do not work very well in our setting.

C Heterogeneity

A subgroup analysis is provided in Table C1.

[27] We have controlled for previous earnings in different ways, and the results vary substantially between specifications.

Table B3: Stacked IV estimates (Gruber/Saez)

	Dependent variable: Δln(earnings)					
	Instrument: Earnings t-1		Instrument: Earnings t-2		Instrument: Earnings t-3	
	(1)	(2)	(3)	(4)	(5)	(6)
Δln(ntr)	0.187***	-0.153***	0.373***	-0.404***	1.080***	-0.775***
	(0.038)	(0.039)	(0.057)	(0.057)	(0.097)	(0.097)
Controlling for ln(e)	No	Yes	No	Yes	No	Yes
Base years	2012-2017		2012-2016		2012-2015	
Observations	2,182,792		1,750,476		1,338,658	

Notes: The sample consists of individuals aged 20–50, earning more than 500 000 SEK, in base year. Standard errors clustered at individual level. Asterisks indicate that the estimates are significantly different from zero at the * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ level.

D Details of the simulation model

To simulate tax policy effects on earnings we essentially need, (i), a model for individual behavior, (ii), data on the relevant tax function, and, (iii), data on the skill distribution. In Section 5.1 we elaborated on (i). In this Appendix we describe in greater detail how we deal with (ii) and (iii), and how we solve the models. First, we show how to calibrate a value of σ using earnings dynamics. Second, we outline how we go about to smoothen the income tax schedule given asssumptions about σ. Third, we show how to calibrate a potential income (skill) distribution with desirable properties. Fourth, we solve the model for the piece-wise linear case, and finally, fifth, we solve the model for the uncertainty (smooth) case.

D.1 Calibrating σ using earnings dynamics

A key parameter when smoothening the tax schedule is the standard deviation of the earnings noise term, σ. Ideally, the noise term should capture variation in earnings that the individual cannot control when choosing optimal expected

Table C1: Subgroups

	(1) All	(2) No pension deduction 2014	(3) Exclude job switchers, people with extra jobs
Treated * 2012	0.246	0.048	0.242
	(0.270)	(0.426)	(0.275)
Treated * 2013	0.029	-0.061	-0.010
	(0.296)	(0.366)	(0.330)
Treated * 2014	-0.113	-0.162	-0.127
	(0.211)	(0.232)	(0.227)
Treated * 2016	-0.312***	-0.363**	-0.274***
	(0.091)	(0.136)	(0.074)
Treated * 2017	-0.635***	-0.764***	-0.610***
	(0.128)	(0.214)	(0.124)
Treated * 2018	-0.763***	-0.932***	-0.862***
	(0.177)	(0.206)	(0.186)
Observations	5,261,287	2,240,424	3,375,553

Notes: All regressions (columns 1–3) include controls for year and percentile group. Standard errors, clustered at percentile groups, in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1. Percentile limits in columns 2 and 3 are the same as in the main analysis reported in column 1.

earnings. Following Saez (1999), we back out a value of σ by using empirical year-to-year differences in earnings. We assume that empirical earnings realizations are generated by $\tilde{z} = z + \varepsilon$. Crucially, we only use pre-reform data 2012-15, which was a period without any substantial changes to tax rates. Hence, we do not expect z to change because of tax changes. When $\varepsilon \sim N(0, \sigma^2)$, it follows that $\tilde{z}_t - \tilde{z}_{t-1} \sim \mathcal{N}(0, {\sigma^{diff}}^2)$ where ${\sigma^{diff}}^2 \equiv 2\sigma^2$. We pool $\tilde{z}_t - \tilde{z}_{t-1}$ for $t = 2013, 2014, 2015$, and we study the distribution of first differences in realized earnings. Since the variability in earnings differs a lot across the earnings distribution, we compute different σ for different percentile groups.

Our ambition is that σ should capture earnings dynamics at the new EITC kink, which we wish to smoothen. Figure D1 illustrates the distribution of $\tilde{z}_{t-1} - \tilde{z}_t$ in the 95th percentile group, which is located just at the earnings level of the kink. When imposing the normality assumption we obtain $\sigma \approx$ SEK $70,000$ in this group. Having said that, the validity of the normality assumption can be questioned. The histogram displays the empirical distribution of earnings differences. Note that the mode value is larger than zero due to real wage growth. Obviously, when fitting the normal density to the raw distribution (solid line) it turns out that the fit is poor. In particular, the empirical distribution contains a substantially larger mass at small earnings differences. Moreover, the distribution is skewed to the left – it is more common with large earnings reductions than with large increases in earnings. It is important to keep in mind that year-to-year differences in earnings actually may reflect active choices that the individual is able to control, but the econometrician cannot observe. When considering all these aspects, we believe that our approach overestimates the earnings shocks facing the individual.

As already mentioned, year-to-year variation in earnings differs depending on the earnings level. Figure D2 illustrates that the calibrated value of σ tends to increase in earnings. In the 95:th percentile group we have $\sigma \approx$ SEK$70,000$, which will be the baseline choice of σ in the simulation exercise.

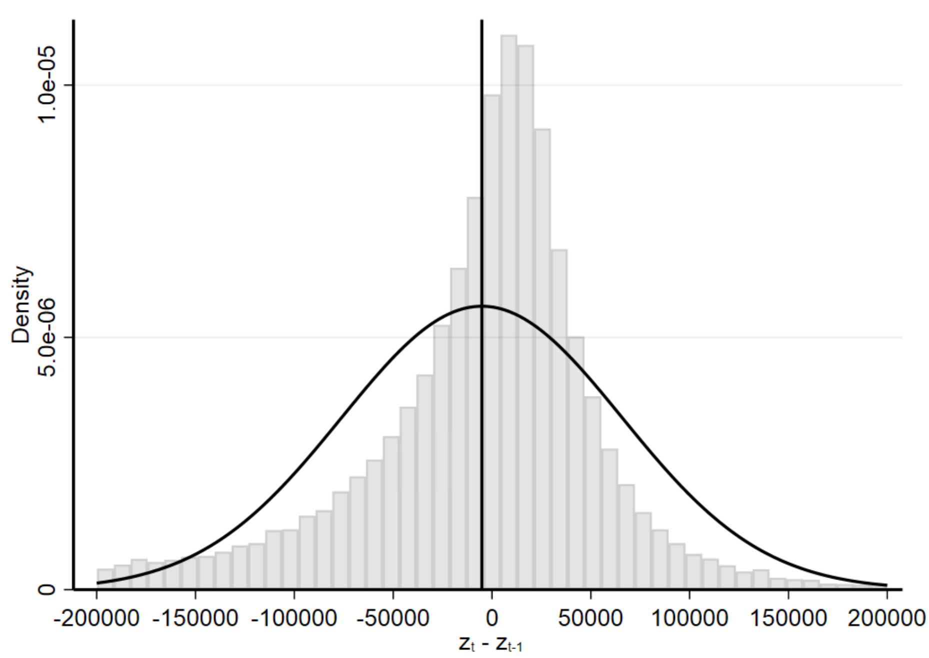

Figure D1: Distribution of $\tilde{z}_t - \tilde{z}_{t-1}$ in the 95th percentile group

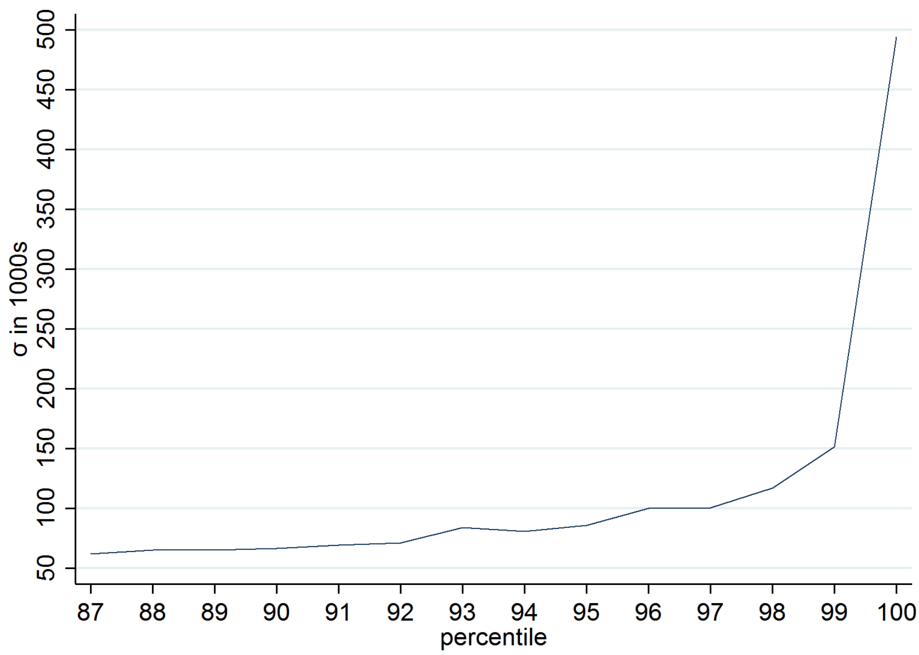

Figure D2: σ by percentile group

D.2 Piece-wise linear marginal tax rates

In Section 2 we outlined the main features of the Swedish tax system. A salient feature of the Swedish system is that the marginal tax rate jumps dramatically at the income level where the central government tax kicks in ("the first central government kink point"), see Bastani and Selin (2014). In this simulation exercise we will consider individuals with earnings exceeding the 88th percentile in 2015 and 2018, i.e. they earn well above the first central government kink, which is located at the 84th percentile. As can be seen from Figure 1b the pre-reform system contains only one kink above the first central government kink point. This is the "second central government kink", where the central government tax increases by 5 percentage points. The EITC phase-out reform of 2016 introduced a new convex kink point just below the second central government kink. In the baseline simulation model we will merge the EITC kink and the second central government kink point. Moreover, we abstract from the non-convex kink at the extreme top of the distribution. The tax schedule will then exhibit the following two-bracket structure both 2015 and 2018:

$$T'(\tilde{z}) = \begin{cases} \tau_1 \text{ if } \tilde{z} < z_1^* \\ \tau_2 \text{ if } \tilde{z} \geq z_1^* \end{cases} \text{(D3)}$$

, while the pre-reform system has a similar two-bracket structure.

D.3 Smoothened marginal tax rates

Following Saez (1999) we showed in Section 5.1 that the optimization problem under uncertainty is similar to an optimiziation problem under certainty, with the piecewise linear tax fuction $T(z)$ replaced by the new "effective" tax function

$$\hat{T}(z) = \int T(z+\varepsilon)f(\varepsilon)d\varepsilon = \int T(\tilde{z})f(\tilde{z}-z)d\tilde{z}$$

The effective marginal tax rate can be expressed as $\hat{T}'(z) = \int T(\tilde{z})\frac{\partial f(\tilde{z}-z)}{\partial z}d\tilde{z}$. Since $\frac{\partial f(\tilde{z}-z)}{\partial z} = -\frac{\partial f(\tilde{z}-z)}{\partial \tilde{z}}$ we can write $\hat{T}'(z) = -\int T(\tilde{z})\frac{\partial f(\tilde{z}-z)}{\partial \tilde{z}}d\tilde{z}$. Using integration by parts, we obtain

$$\hat{T}'(z) = \int T'(\tilde{z}) f(\tilde{z} - z) d\tilde{z} \tag{D4}$$

Intuitively, at a given level of earnings, z, the effective marginal tax rate is a weighted sum of the true marginal tax rates of the piece-wise linear tax function, $T'(\tilde{z})$. Combining (D3) and (D4) we obtain

$$\begin{aligned} \hat{T}'(z) = &\ \tau_1 \int_0^{z_1^*} f(\tilde{z} - z) d\tilde{z} + \tau_2 \int_{z_1^*}^{\infty} f(\tilde{z} - z) d\tilde{z}, \\ = &\ \tau_1 F(z_1^* - z) + \tau_2 (1 - F(z_1^* - z)) \end{aligned} \tag{D5}$$

, where $F(\varepsilon)$ is the cumulative density function. To compute the effective marginal tax rate at z, $\hat{T}'(z)$, it is hence sufficient to consider the kink points of the piece-wise linear schedule and the cdf of the normal distribution with standard deviation σ. We use (D5) to compute the effective marginal tax schedule 2015 and 2018. τ_2 is 3 percentage points larger in 2018. The kink point kicks in at the same percentile of the skill distribution in each year. In Figure 4 we used a three bracket schedule for the post-reform period.

D.4 The skill distribution

Our approach to calibrate the skill (potential income) distribution roughly follows Saez (2001), who simulated optimal tax schedules and recalibrated the skill distribtution for different values of the elasticity parameter. To make progress, we assume that the empirical earnings distribution of 2015 reflects the distribution of deterministic incomes, z. As we show below in Section D.6, when agents optimize subject to a smooth tax schedule, the following endogenous relationship between optimal (deterministic) z and skill z_0 holds in the individual's optimum

$$z_0(z) = \frac{z}{[1 - \hat{T}'(z)]^e}. \tag{D6}$$

We recover the frequency distribution of z_0 by plugging in the smoothened 2015 marginal tax rate $\hat{T}'(z)$ (computed for $\sigma =$ SEK $70,000$), and the relevant value of e and z into (D6). We have verified that there is a one-to-one mapping between z_0 and z.

D.5 Solving the model – piece-wise linear tax function

When $\sigma = 0$ agents solve

$$U = z - T(z) - \frac{z_0}{1+\frac{1}{e}}(\frac{z}{z_0})^{1+\frac{1}{e}}$$

, where $T(z)$ is a piece-wise linear tax function. In this environment agents may have optima at interior points of segments, or they may have optima at convex kink points, where marginal tax rates increase. We solve for the individuals' optima numerically, and our own code builds on MATLAB scripts originally constructed by Spencer Bastani for the simulations in Bastani and Selin (2014). The optimization routine finds the value of z that maximizes indirect utility. The set of optimal solutions contains two parts: the interior solution of realized income is $z = (1-\tau)^e z_0$, and the bunching solution for individuals with skill level $z_0 \in [\frac{z^*}{(1-\tau_1)^e}, \frac{z^*}{(1-\tau_2)^e}]$ is $z = z^*$ where z^* is the kink point, and τ_1 and τ_2 are the marginal tax rates before and after the kink respectively. In the baseline simulations we use this two-bracket structure, but it is straightforward to use a similar algorithm when there are multiple kinks.

D.6 Solving the model – smooth tax function

In Section 5.1 we demonstrated that the uncertainty model is equivalent to a setting in which agents choose expected earnings, z, subject to a smooth tax schedule $\hat{T}(z)$:

$$EU = z - \hat{T}(z) - \frac{z_0}{1+\frac{1}{e}}(\frac{z}{z_0})^{1+\frac{1}{e}}.$$

Individuals now choose optimal deterministic income z, and realized income $\tilde{z}$ is unknown. The first order condition can be written

$$1 - \hat{T}'(z) = (\frac{z}{z_0})^{\frac{1}{e}}. \tag{D7}$$

As the left hand side is endogenous, it is not possible to obtain an analytical solution for optimal z. We instead use a simulation approach to solve this problem. For each observation with skill z_0, we loop over all values z to find the unique value that satisfies (D7). As mentioned in Appendix Section D.3 above, we have verified that there is a one-to-one mapping between z_0 and z for chosen values of e and σ.