American Economic Journal: Economic Policy 2014, 6(4): 271–301
http://dx.doi.org/10.1257/pol.6.4.271

Estimating Taxable Income Responses Using Danish Tax Reforms[†]

By Henrik Jacobsen Kleven and Esben Anton Schultz*

This paper estimates taxable income responses using a series of Danish tax reforms and population-wide administrative data since 1980. The tax variation and data in Denmark makes it possible to overcome the biases from nontax changes in inequality and mean reversion that plague the existing literature. We provide compelling graphical evidence of taxable income responses, arguably representing the first nonparametrically identified evidence of taxable income elasticities using tax reforms. We also present panel regression evidence that is extremely robust to specification, unlike previous results which have been very sensitive. (*JEL* D31, H24, H31, J22)

The modern literature on behavioral responses to taxes has shifted much of its focus from the elasticity of hours worked to the elasticity of taxable income. Effects on taxable income capture the full range of behavioral responses, including hours worked, unobserved effort, career choices, and tax avoidance and evasion, and therefore provide a more complete picture of the distortionary effects of taxation. Under certain conditions, as argued by Feldstein (1995, 1999), the elasticity of taxable income provides a sufficient statistic for efficiency and optimal taxation, which places this parameter at the center stage of all the major normative questions in public finance.[1]

A large and growing literature estimates the elasticity of taxable income using tax return data, as recently surveyed by Saez, Slemrod, and Giertz (2012). Much of this work is based on the United States and uses as its source of identification a series of tax reforms in the 1980s and 1990s that were associated with substantial tax changes at the top of the income distribution (e.g., Feldstein 1995; Auten and Carroll 1999; Moffitt and Wilhelm 2000; Goolsbee 2000; Gruber and Saez 2002; Kopczuk 2005;

*Kleven: London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom (e-mail: h.j.kleven@lse.ac.uk); Schultz: Kraka, Kompagnistræde 20A, DK-1208 Copenhagen K (e-mail: ess@kraka.org). We thank Raj Chetty, John Friedman, Hilary Hoynes, Niels Johannesen, Wojciech Kopczuk, Claus Kreiner, Tore Olsen, Emmanuel Saez, Monica Singhal, Joel Slemrod, and anonymous referees for helpful comments and suggestions. We are grateful to the Centre for Applied Microeconometrics (CAM) at the University of Copenhagen for data access, and to the Tuborg Foundation and the Economic Policy Research Network (EPRN) for financial support.

[†]Go to http://dx.doi.org/10.1257/pol.6.4.271 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1]The general condition for the elasticity of taxable income to be the (only) sufficient statistic for welfare analysis is that there exists a *single wedge* between the social marginal costs and benefits of *any* change in taxable income, independently of its source. This is unlikely to be fully satisfied as different types of taxable income responses (such as hours worked, wage bargaining, income shifting, and tax evasion) are associated with different wedges due to different fiscal and other externalities in which case some degree of decomposition is required.

and Giertz 2007). In addition to the US literature, a number of recent studies estimate taxable income responses in other countries that have lowered marginal tax rates at the top of the income distribution, including the United Kingdom (Brewer, Saez, and Shephard 2010), Canada (Sillamaa and Veall 2001; Saez and Veall 2005), Norway (Aarbu and Thoresen 2001), Sweden (e.g., Hansson 2007; Blomquist and Selin 2010; Gelber 2012), and Poland (Kopczuk 2012).[2]

Reforms that target strongly the top of the income distribution provide interesting variation, but are also associated with some important empirical difficulties. Because the allocation of tax treatments is determined by pre-reform income level, we have to consider the possibility that different income groups differ in a number of nontax dimensions that impact on taxable income and are correlated with the tax law changes. This problem is reinforced by the fact that tax return data contain typically very little information about taxpayers besides income variables and tax rates, making it difficult to control for any nontax differences across different taxpayers.

Two key problems have been discussed extensively in the literature (e.g., Slemrod 1998; Saez 2004; Saez, Slemrod, and Giertz 2012). First, it is very hard to disentangle tax-driven changes in top incomes from changes that are driven by nontax factors such as skill-biased technical progress and globalization. This problem is particularly important in countries that have experienced strong secular increases in top income shares. When considering tax cuts at the top of the distribution, this may result in a substantial upward bias in the elasticity estimates. Second, defining treatments and controls according to pre-reform income level creates a mean-reversion problem, because a taxpayer with a positive income shock in the pre-reform year will tend to have a lower income in the following years, independently of the reform. For tax cuts at the top, this biases elasticity estimates downward. In order to correct for the two biases mentioned above, the literature has attempted to control in a number of ways for pre-reform income levels. However, the richness of such income controls is constrained by the fact that the identification comes from different tax changes across pre-reform income levels, and in general the results turn out to be very sensitive to specification.

This paper estimates taxable income responses using a series of Danish tax reforms and rich administrative data covering the full Danish population since 1980. Unlike US studies, the dataset combines tax return information with detailed labor market, education, and sociodemographic information. Besides the quality of the data, the Danish setting offers two important advantages allowing us to overcome the biases discussed above.

First, the Danish income distribution has been much more stable than in most other countries, including English-speaking countries and other Nordic countries

[2] Alongside the literature using tax reforms to estimate taxable income responses, a recent literature estimates taxable income responses using bunching at kinks or notches (Saez 2010; Chetty et al. 2011; Kleven and Waseem 2013). While bunching provides a compelling source of identification in theory, a key limitation in practice is that there tends to be very little bunching in empirical distributions and so the estimated elasticities are often tiny. The likely explanation is the presence of optimization frictions such as switching and attention costs combined with the fact that the utility gain of bunching in response to kinks is typically not very large (Chetty et al. 2011; Chetty 2012). Attenuation bias from frictions can be controlled for using notches (Kleven and Waseem 2013), but this source of variation is not always available. We come back to the question of frictions below.

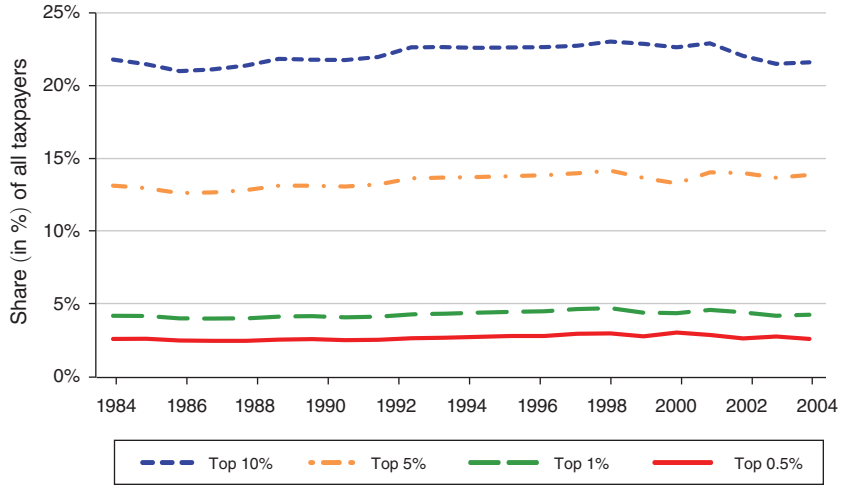

FIGURE 1. THE EVOLUTION OF TOP INCOME SHARES IN DENMARK

Notes: The income shares are based on tax return information and a broad income measure including labor income, other personal income, and capital income (as defined in detail in Table 1). The sample includes all personal income tax filers aged 25–55.

that have provided testing grounds for previous taxable income studies.[3] To provide evidence on this, Figure 1 shows the evolution of Danish top income shares since the early 1980s based on a broad income measure including all labor income and capital income. We see that top income shares in Denmark have been remarkably constant over time. The stable income distribution in Denmark eliminates the threat to identification coming from nontax changes in inequality and therefore isolates mean-reversion as the key source of bias that must be controlled for.

Second, we exploit a series of tax reforms that create large and compelling identifying variation. In some years, the variation created by the Danish tax reforms is larger than the variation created by the major US tax reforms of the 1980s, and importantly the Danish variation does not feature the same strong correlation with income level as the US variation. The Danish reforms are associated with three main changes: (i) differential changes in marginal tax rates across different tax brackets, (ii) changes in bracket cutoffs that move large groups of taxpayers to different brackets, and (iii) a change from symmetric to asymmetric treatment of different income components (labor income, capital income, and deductions). The combination of points (i) and (ii) create large and nonlinear tax variation through the income distribution in a way that is not systematically correlated with income level. Point (iii) implies that income composition, besides income level, plays a key role for the tax bill, thereby creating variation across individuals at the same income level. All three changes together therefore create very rich identifying variation.

Because the tax system imposes differential treatment of different income types (labor income, capital income, and deductions), we estimate separately the

[3] An international overview of the long-run evolution of top income shares in more than 20 countries (but not including Denmark) is provided by Atkinson, Piketty, and Saez (2011).

elasticities of taxable labor income and taxable capital income with respect to the marginal tax rate on each. In the presence of multiple tax bases with different tax rates, the *overall* taxable income elasticity is no longer a sufficient statistic for welfare analysis; one must in general estimate both own-tax and cross-tax elasticities for each base as we do here.[4] Nevertheless, for comparability with the existing literature, we also present estimates of the overall taxable income elasticity with respect to a joint increase in the marginal tax rate on the underlying components.

Our main findings are the following. First, considering a large and salient tax reform in the 1980s, we present compelling graphical evidence of behavioral responses for both labor and capital income. The evolution of labor and capital income in a treatment group (facing large tax cuts) and a control group (facing tax increases) are completely parallel in the pre-reform period and then diverge sharply just after the reform. A difference-in-differences approach based on the graphical analysis produces elasticities in the range of 0.2–0.3. We view these findings as a key contribution of the paper, especially considering that the previous taxable income literature has been unable to produce compelling nonparametric evidence of this kind.

Second, turning to panel regressions using all tax reform variation over a long time period, we find that elasticities are in general quite modest. Labor income elasticities are around 0.05 for wage earners and 0.10 for self-employed individuals, while capital income elasticities are around two to three times larger than labor income elasticities. Third, behavioral elasticities are larger when estimated from large tax reform episodes than from small tax reform episodes, consistent with the general argument by Chetty et al. (2011) and Chetty (2012) that large tax changes are more likely to overcome optimization frictions and therefore reveal the structural long-run elasticity. We find that the large tax reform variation in the 1980s is associated with a population-wide labor income elasticity of 0.12—and about twice as high when zooming in on the very largest tax variation in the data—whereas the smaller tax reform variation of the 1990s and 2000s is associated with a labor income elasticity of only 0.02. Interestingly, our labor income elasticity obtained from a large Danish tax reform is an order of magnitude larger than the labor income elasticity that can be obtained from the largest and most salient Danish tax kink (Chetty et al. 2011), suggesting that reform-based estimates are potentially more revealing of long-run (frictionless) behavior than bunching-based estimates. Finally, using the existence of sharp differential tax variation across income types, we are able to estimate separately own-tax and cross-tax elasticities of labor income and capital income. These estimates indicate that the two income types are substitutes, consistent with the presence of income-shifting behavior. Although the importance of shifting for estimating and interpreting taxable income responses has been discussed extensively (e.g., Saez, Slemrod, and Giertz 2012), our study appears to be the first in the taxable income literature that tackles such cross-effects directly.

[4] This general insight is relevant for most countries in the world, including the United States. As a specific example, consider the argument by Saez, Slemrod, and Giertz (2012) that the US elasticity of taxable income is not a sufficient statistic due to the presence of both personal and corporate tax bases with potential shifting (i.e., cross-tax effects) between the two.

Importantly, we show that the above findings are extremely robust to empirical specification, including socioeconomic controls and the specification of pre-reform income controls (which have been so crucial in previous work). The robustness of our findings is a result of the stable income distribution and the rich tax variation in Denmark. We therefore conclude that the Danish context offers a useful laboratory for a credible identification of taxable income responses.

The paper proceeds as follows. Section I describes the Danish tax system and tax reforms. Section II describes the data and Section III sets out the empirical strategy. Section IV presents empirical results and Section V concludes.

I. The Danish Tax System and Tax Reforms

The Danish personal income tax treats different income forms in a partially separate fashion, as opposed to standard tax systems that apply a progressive rate structure to a single measure of taxable income. As shown in Table 1, the income concepts of the Danish income tax are labor income (LI), personal income (PI = LI + other PI), capital income exclusive of stock income (CI), stock income (SI), deductions (D), and taxable income (TI = PI + CI + SI − D). These income concepts are aggregated into several different tax bases that are taxed at different rates. The definition of those bases as well as the associated tax rates have undergone substantial changes over time due to a series of tax reforms, and this is the variation that we exploit to estimate behavioral elasticities.

Taxes are divided into national and regional taxes, which are enforced and administered in an integrated system. At the national level, a series of important tax acts have been implemented in recent decades. The tax acts analyzed here are the 1987 reform, the 1994 reform, the 1999 reform (called the *Pentecost Package*), and the 2004 reform (called the *Spring Package*). Most of these reforms were phased in over several years, which generates considerable tax variation in most years of the period we consider. We also exploit changes in tax schedules at the regional level, but those changes have been much smaller and are more uniform across taxpayers than the national changes.

The national income tax is divided into three main brackets: a bottom bracket, a middle bracket, and a top bracket. The past 25 years of tax reform have been associated with three main changes. First, a lowering of marginal tax rates in each bracket, with larger cuts in the middle and top brackets than in the bottom bracket. Second, a substantial broadening of the tax base as negative capital income and deductions were prevented from offsetting positive income on a one-to-one basis. This change was implemented by changing the tax schedule from a function of total taxable income (TI) to a function of each of the underlying income components (LI, PI, CI, SI, D), with a higher marginal tax rate on labor income than on the other income components as well as a higher marginal tax on positive income than on negative income (such as mortgage interest and deductions). With the exception of stock income, the taxation of the different income components is not fully separate and cross-effects in the tax function are therefore non-zero. Third, adjustments of bracket cutoffs that did not correspond to the base broadening, thereby pushing taxpayers into higher brackets. This bracket push combined with the fact that tax rates were

Table 1—Income Concepts in the Danish Individual Income Tax

Income concept	Acronym	Main items included
1. Labor income	LI	Salary, wages, honoraria, fees, bonuses, fringe benefits, business earnings
2. Personal income	PI	LI + transfers, grants, awards, gifts, received alimony − Labor Market Contribution, certain pension contributions
3. Capital income	CI	Interest income, rental income, business capital income − interest on debt (mortgage, bank loans, credit cards, student loans)
4. Stock income	SI	Dividends and realized capital gains from shares
5. Deductions	D	Commuting, union fees, UI contributions, other work expenditures, charity, paid alimony
6. Taxable income[a]	TI	PI + CI + SI − D

[a]The definition of taxable income in this table does not correspond to what is currently labeled taxable income in the Danish tax code, which excludes stock income as it is taxed on a separate schedule (see Table 2).

reduced within each bracket imply substantial and very heterogeneous tax rate variation through the income distribution. All of the changes together create strong variation across taxpayers at different income levels, across taxpayers at similar income levels (but different income compositions), and across different income types.

Table 2 shows tax rates and tax bases in four specific years: 1986 (before the 1987 reform), 1993 (before 1994 reform), 1998 (before the 1999 reform), and 2005 (after the 1999 and 2004 reforms). The tax system consists of a flat regional tax (shown for the average municipality) along with progressive national taxes levied on varying tax bases. Besides the national bottom, middle, and top taxes that are present throughout the period, there are social security contributions, labor market contributions, and an EITC (Earned Income Tax Credit) featured during different parts of the period. The tax rates shown in the table are cumulative such that a taxpayer in the top bracket is subject to the sum of the bottom, middle, and top taxes (along with the other flat taxes).[5] The table shows the tax base changes mentioned above. In the mid-1980s, all tax rates applied to overall taxable income, whereas in the 1990s and 2000s no tax rate applies to this net income measure. In 2005, for example, tax liability is calculated from four different tax bases: taxable income exclusive of stock income (PI + CI – D), personal income plus positive net capital income (PI + [CI > 0]), labor income (LI), and stock income (SI).

Two points are worth making regarding those tax base changes. First, when taxable income consists of subcomponents that are treated differently, the elasticity of overall taxable income is no longer a sufficient statistic for welfare analysis. In this case, one must estimate elasticities of each underlying tax base, in principle including both own-tax and cross-tax elasticities. We therefore consider separately the elasticities of

[5]For example, in 1986, a taxpayer in the top bracket would face a marginal tax rate equal to $28.1 + 14.4 + 14.4 + 10.8 + 5.5 = 73.2$ percent. However, a marginal tax ceiling is in place in all years, and this ceiling equals 73 percent in 1986 and is therefore binding for a taxpayer living in an average municipality. In 2005, the marginal tax ceiling has dropped to 59.0 percent and was indeed also binding for a taxpayer in the top bracket living in the average municipality. For labor income, there is a labor market contribution of 8 percent on top of the tax ceiling, but at the same time labor income enters all the other tax bases net of the labor market contribution. The effective tax ceiling on labor income in 2005 is therefore equal to $8.0 + (1 - 0.08) \times 59.0 = 62.3$ percent.

Table 2—Tax Bases and Tax Rates over Time in the Danish Individual Income Tax System

	1986		1993	
Tax type[a]	Base	Rate (%)	Base	Rate (%)
Panel A				
Regional tax[b]	TI	28.1	PI + CI − D	30.2
National taxes				
Bottom tax	TI	14.4	PI + CI − D	22.0
Middle tax	TI	14.4	PI + [CI > 0]	6.0
Top tax	TI	10.8	PI	12.0
Social security contribution	TI	5.5	—	—
Labor market contribution[c]	—	—	—	—
EITC	—	—	—	—
Tax on stock income[d]	—	—	SI	30.0; 40.0
Marginal tax ceiling[e]	TI	73.0	PI/CI/TI	68.0

	1998		2005	
Tax type[a]	Base	Rate (%)	Base	Rate (%)
Panel B				
Regional tax[b]	PI + CI − D	32.4	PI + CI − D	33.3
National taxes				
Bottom tax	PI + CI − D	8.0	PI + [CI > 0]	5.5
Middle tax	PI + CI	6.0	PI + [CI > 0]	6.0
Top tax	PI + [CI > 21k]	15.0	PI + [CI > 0]	15.0
Social security contribution	—	—	—	—
Labor market contribution[c]	LI	8.0	LI	8.0
EITC	—	—	LI	2.5
Tax on stock income[d]	SI	25.0; 40.0	SI	28.0; 43.0
Marginal tax ceiling[e]	PI/CI/TI	58.0	PI/CI/TI	59.0

[a] Tax rates are cumulative. For example, the marginal tax rate in the top bracket in 1986 is equal to 28.1 + 14.4 + 14.4 + 10.8 + 5.5 = 73.2 percent (but see footnote 4 regarding marginal tax ceiling adjustment).
[b] The regional tax includes municipal, county, and church taxes. The regional tax rate in the table is an average across all municipalities in Denmark in each year.
[c] After the introduction of the labor market contribution, labor income enters the other tax bases net of the labor market contribution. Hence, in those years, the effective tax rate on labor income equals the statutory tax rate times (1 − labor market contribution).
[d] After the 1987 reform, the taxation of stock income is completely separate from the rest of the income tax and follows a two-bracket progressive schedule with the marginal tax rates shown in the table.
[e] If the sum of all regional and national tax rates (excluding the stock income tax after the 1987 reform) exceeds the specified ceiling, the top tax is adjusted downward until the the marginal tax rate equals the ceiling.

taxable labor income and taxable capital income.[6] Second, by estimating elasticities of the underlying income components, we avoid the identification problems posed by base broadening that have been discussed extensively in the literature on taxable income responses (Slemrod 1998; Kopczuk 2005). The usual problem is that broadening of the dependent variable (taxable income) forces researchers to consider a constant-definition measure of taxable income in order not to confound behavioral and mechanical changes, but in so doing they are relating the tax rate to an artificial

[6] Consistent with the income definitions in Table 1, we consider capital income (CI) exclusive of stock income (SI). The latter is taxed on a completely separate schedule, which has remained relatively constant through most of the period and therefore offers less reform-based variation than the rest of the income tax code. The most useful quasiexperimental variation in stock income taxation is created, not by tax reforms, but by a sharp kink at the cutoff between two brackets in the stock income tax. Kleven et al. (2011) use bunching around this kink to estimate the elasticity of stock income and find evidence of strong behavioral responses driven by tax avoidance.

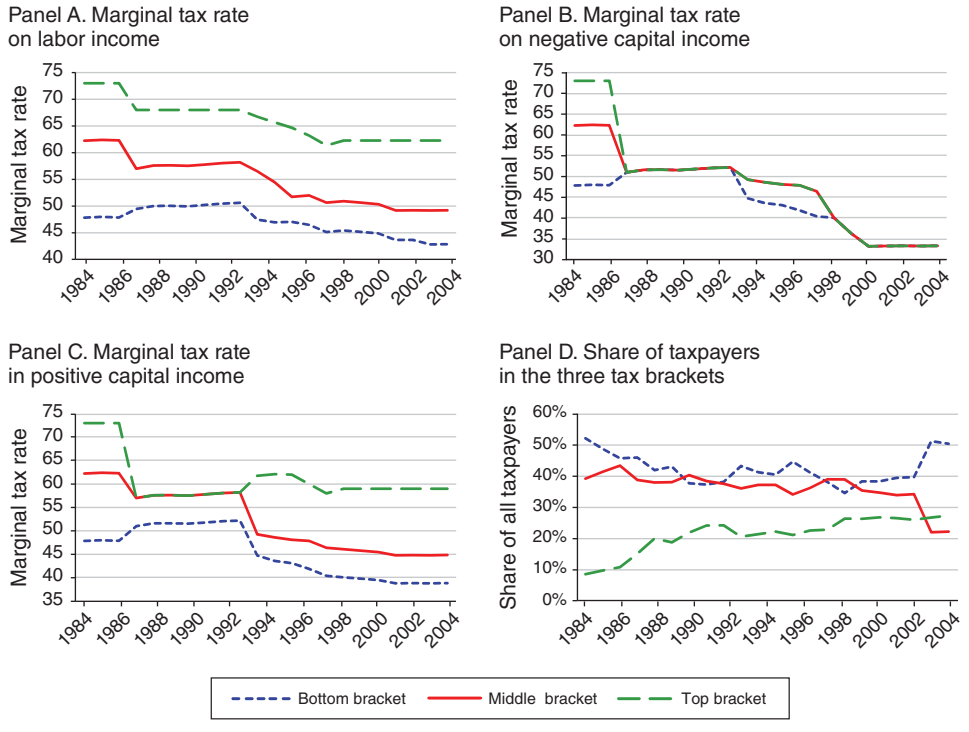

FIGURE 2. TWO DECADES OF DANISH TAX REFORM

Notes: The figure shows the evolution of marginal tax rates on labor income (panel A), negative capital income (panel B), and positive capital income (panel C) in each tax bracket (bottom, middle, and top) over time, along with the evolution in the share of taxpayers located in each bracket over time (panel D).

tax base different from the one in the tax code in a given year. This is not an issue here as we consider the actual income subcomponents in the tax code, the definition of which has been (almost) constant and which are observed throughout the period. To put it differently, the empirical advantage of the Danish base broadening is that it does not consist in including previously untaxed (and therefore unobserved) components in the tax system, but consists instead in reducing the tax rate associated with negative income and deductions that are in the tax code throughout the period.

Figure 2 illustrates the implications of the tax rate and tax base changes described above for the effective marginal tax rates on labor and capital income in each bracket (bottom, middle, and top) over time. For labor income (panel A), the marginal tax rate in the top bracket has been declining from 73 percent to 62 percent, while the tax rate in the middle bracket has been declining from 62 percent to 49 percent. On the other hand, the bottom tax rate is increasing over the early part of the period and then declining over the later part of the period. Overall, the difference between the bottom tax and the middle/top taxes has been shrinking over this period, although the relative changes have not been dramatic. However, these graphs do not reveal the important implications of bracket push as we come back to below.

For capital income, we distinguish between negative capital income (panel B) and positive capital income (panel C) as the two are taxed very differently. For negative capital income, the three brackets have collapsed into one bracket, subject to the

bottom tax rate (as negative capital income was excluded from the middle and top tax bases). For taxpayers in the top bracket, the marginal tax rate associated with negative capital income has dropped from about 73 percent to 33 percent over the period, while for taxpayers at the bottom the drop has been much smaller. These dramatic tax changes affect a very large number of taxpayers, because capital income is in fact negative for the majority of Danish taxpayers as a result of interest payments on loans (mortgage and other loans). For positive capital income, we also see very large changes as the band between the top and the bottom first narrows substantially (since all capital income is excluded from the top tax base) and then widens substantially (since positive capital income is reintroduced in the top tax base).

Finally, to see the importance of bracket push due to underadjustment of bracket cutoffs as bases were broadened, panel D shows the evolution over time in the share of taxpayers located in each bracket. We see that the share of taxpayers liable to pay the top tax has increased dramatically from less than 10 percent of the population in the mid-1980s to almost 30 percent of the population in the mid-2000s. The share of individuals in the middle bracket has fallen from about 40 percent to slightly above 20 percent over the whole period, while the share of taxpayers in the bottom bracket falls from about 50 percent to 40 percent in the early part of the period and then rises back to 50 percent in the latter part of the period.[7] These movements across brackets create substantial tax variation, especially for labor income. The combination of the tax rate changes for labor income in panel A and the bracket push in panel D create very strong and nonlinear tax variation through the income distribution.

Overall, the reforms described in this section imply substantial tax variation over time and across individuals. Indeed, as we show in Section III when discussing the identification strategy, the variation in some years is comparable to the major tax acts in the United States in the 1980s.

II. Data

The dataset includes the full Danish population since 1980. It has been constructed by Statistics Denmark based on several administrative registers, including the Income Tax Register and the Integrated Database for Labor Market Research (IDA). For each individual, the dataset contains detailed tax return information along with a large set of socioeconomic variables such as address, gender, age, marital status, children, immigration status, ethnicity, employment status, job experience, education, occupation, and industry.

Marginal tax rates are not directly observed in tax return data, and we therefore have to simulate marginal tax rates for each taxpayer based on tax return information and a model of the Danish tax system. As there exists no publicly available tax simulation model for Denmark (such as the NBER TAXSIM model for the United States), we have constructed our own tax simulator accounting for all details of the Danish

[7] The bottom, middle, and top bracket shares do not quite add up to 1, because a small amount of taxpayers below a basic exemption level are not liable to pay the bottom tax.

tax system between 1984 and 2005.[8] Based on this model and tax return data, we compute the marginal tax rate on a given income component by increasing income by DKK 100 ($\simeq$ US$18 as of August 2013). In particular, if tax liability $T(\cdot)$ is a function of n different income components $z^1, \ldots, z^n$, we compute the marginal tax on z^j as $\tau^j = [T(z^1, \ldots, z^j + 100, \ldots, z^n) - (Tz^1, \ldots, z^j, \ldots, z^n)]/100$.[9]

Following Gruber and Saez (2002), the empirical strategy is to relate changes in taxable income over time to changes in marginal tax rates over time for individual taxpayers. We consider three-year intervals (1984–1987, ..., 2002–2005), which correspond to the differencing in most US studies and more importantly fit the data in our context. In particular, we show graphically that three-year intervals are *just enough* to account for sluggishness in behavioral adjustments—long enough to capture long-term effects, but not longer than that to avoid unnecessarily losing variation and power. We denote the first year in any given three-year interval by s and the last year by $s + 3$. We include only taxpayers that are also observed in year $s - 1$, because this year is used to construct pre-reform income controls. The three-year differences are stacked to obtain a dataset with about 49 million observations.

We impose the following restrictions on the estimation sample. First, we restrict attention to individuals aged 15–70 years. Second, individuals whose income in base year s comes primarily from welfare benefits are excluded, because including them would require us to account for the important incentive effects of the welfare system and model extensive responses. Third, we limit the sample to people who are fully tax liable in Denmark. These restrictions leave us with a sample of about 37 million observations, with summary statistics shown in Table A1 in the Appendix.

III. Empirical Strategy

A. *Conceptual Framework*

The economic model underlying the taxable income literature is a simple extension of the traditional labor supply model. It is assumed that each taxpayer maximizes a utility function $u(c, z, \mathbf{x})$, where c is consumption, z is reported taxable income, and $\mathbf{x}$ is a vector of individual characteristics. We may think of taxable income z as being generated by a number of underlying choices such as hours worked, unobserved effort, training, occupational choice, tax sheltering activities, etc. The implicit assumption in the literature is therefore that all those underlying activities are weakly separable from consumption in the utility function. Utility is maximized subject to a budget constraint $c = z - T(z) = (1 - \tau) \cdot z + y$, where $T(\cdot)$ is tax liability, $\tau \equiv T'(\cdot)$ is the marginal tax rate, and $y \equiv \tau \cdot z - T(z)$ is virtual income. We may then write the optimal choice of taxable income as $z = z(1 - \tau, y, \mathbf{x})$.

[8] We restrict the tax simulator to the period 1984–2005 (even though the dataset goes back to 1980) due mainly to difficulties of precisely measuring all the subcomponents of taxable income before 1984. But since we control for pre-reform income levels in the panel regressions, we will be using data from before 1984.

[9] While the Danish income tax system is based on individual filing for married couples, it involves certain elements of jointness due to the fact that some exemptions can be transferred across spouses. This implies that, for a married person, income tax liability depends on both individual incomes and on spousal incomes. Our TAXSIM model accounts fully for this jointness.

Consistent with the Danish setting, we extend the above model to account for the presence of multiple income types that are taxed differently. Consider therefore a consumer choosing incomes $z^1, \ldots, z^n$ under a tax schedule $T(z^1, \ldots, z^n)$. This consumer maximizes utility

$$u = u(c, z^1, \ldots, z^n, \mathbf{x}), \tag{1}$$

subject to a budget constraint

$$c = \sum_{j=1}^{n} z^j - T(z^1, \ldots, z^n) = \sum_{j=1}^{n} (1 - \tau^j) z^j + y, \tag{2}$$

where $\tau^j \equiv \partial T / \partial z^j$ is the marginal tax rate on income type j and $y \equiv \sum_{j=1}^{n} \tau^j z^j - T(z^1, \ldots, z^n)$ is virtual income. Our measure of virtual income is a generalization of standard virtual income to a situation with multidimensional income.[10] As all z-variables in equation (2) are defined as income, if a given component z^j reflects a deduction in taxable income, then this component is defined as minus deductions.

In this model, the optimal choice of income type j depends on all the net-of-tax rates and virtual income, i.e.,

$$z^j = z^j(1 - \tau^1, \ldots, 1 - \tau^n, y, \mathbf{x}) \quad \forall j. \tag{3}$$

In general, an empirical specification for income type j should account for both own-price effects of the marginal net-of-tax rate on income type j as well as cross-price effects of the net-of-tax rates on all the other income types. In the empirical analysis, we first consider baseline specifications without cross-tax effects, and then turn to specifications that allow for cross-tax effects by exploiting the sharp tax variation across different income types in Denmark. The analysis of cross-tax effects enables us to evaluate the potential importance of income shifting between labor and capital income, an issue that has been much discussed in the literature.

In the baseline model without cross-tax effects, expression (3) implies $z_{is}^j = z^j(1 - \tau_{is}^j, y_{is}, \mathbf{x}_{is})$ for taxpayer i at time s. Adopting a log-linear specification, we have

$$\log(z_{is}^j) = \alpha + \varepsilon \cdot \log(1 - \tau_{is}^j) + \eta \cdot \log(y_{is}) + \gamma_s^c \cdot \mathbf{x}_i^c + \gamma^v \cdot \mathbf{x}_{is}^v + \mu_i + \nu_{is}. \tag{4}$$

In this specification, we distinguish between time-invariant individual characteristics $\mathbf{x}_i^c$ whose effect may change over time and time-variant individual characteristics $\mathbf{x}_{is}^v$ whose effect is constant over time. The effect of time-invariant individual characteristics whose effect is constant over time is subsumed in the individual fixed effect μ_i. The key variables of interest are the uncompensated elasticity with respect

[10] By modeling the income effect in terms of virtual income, we deviate from previous taxable income studies (such as Gruber and Saez 2002) where the income effect is specified simply in terms of after-tax income $z - T(z)$. Our virtual income specification keeps a clear link between the estimated coefficients and the compensated and uncompensated elasticities that represent sufficient statistics for welfare analysis.

to the marginal net-of-tax rate (ε) and the income elasticity (η), the combination of which gives the compensated elasticity using the Slutsky decomposition.[11]

In first-differenced form, the model can be written as

$$(5)\Delta \log(z_{is}^{j}) = \varepsilon \cdot \Delta \log(1 - \tau_{is}^{j}) + \eta \cdot \Delta \log(y_{is}) + \Delta \gamma_{s}^{c} \cdot \mathbf{x}_{i}^{c} + \gamma^{v} \cdot \Delta \mathbf{x}_{is}^{v} + \Delta \nu_{is}.$$

In the baseline specification, differences at time s are three-year differences from s to $s + 3$.

B. *Identification and Relationship to Previous Literature*

Because of the nonlinearity of the tax system, the marginal tax rate and virtual income are endogenous to the choice of taxable income, which creates a correlation between $\Delta \log(1 - \tau_{s}^{j})$, $\Delta \log(y_{s})$, and the error term. The usual way to construct instruments for these variables is to use mechanical tax changes driven by changes in tax laws. Hence, using the Danish tax simulation model described above, we simulate post-reform marginal tax rates under pre-reform behavior, $\tau_{s+3}^{j}(z_{s}^{1}, \ldots, z_{s}^{n})$, where we account for the fact that the marginal tax rate on income j may depend not just on the level of income j but also on the levels of the other incomes. From the simulated marginal tax rates, we obtain mechanical net-of-tax rate changes, $\log\big(1 - \tau_{s+3}^{j}(z_{s}^{1}, \ldots, z_{s}^{n})\big) - \log\big(1 - \tau_{s}^{j}(z_{s}^{1}, \ldots, z_{s}^{n})\big)$, which are used as instruments for the observed changes $\Delta \log(1 - \tau_{s}^{j})$. Analogously, we simulate post-reform virtual incomes under pre-reform behavior, $y_{s+3}(z_{s}^{1}, \ldots, z_{s}^{n}) = \sum_{j=1}^{N} \tau_{s+3}^{j}(z_{s}^{1}, \ldots, z_{s}^{n})\, z_{s}^{j} - T_{s+3}(z_{s}^{1}, \ldots, z_{s}^{n})$, and associated mechanical changes in virtual income, $\log\big(y_{s+3}(z_{s}^{1}, \ldots, z_{s}^{n})\big) - \log\big(y_{s}(z_{s}^{1}, \ldots, z_{s}^{n})\big)$, which are used as instruments for the observed changes $\Delta \log(y_{s})$.

While the mechanical tax changes used as instruments are exogenous to post-reform incomes, they do depend on pre-reform incomes. Hence, the instruments may be correlated with the error term if the pre-reform income level is correlated with the error term. The literature has discussed two channels through which this may occur. First, taxpayers at different pre-reform income levels may experience different income trends for nontax reasons. Indeed, many countries have experienced sharply increasing top income shares over the past few decades, and several studies have argued that these changes are driven by skill-biased demand shocks resulting from innovation and globalization. Unless skill can be directly controlled for, it would be captured by pre-reform income levels and skill-biased changes would then be absorbed in the estimated elasticity. Second, the pre-reform income level reflects both permanent and transitory income components, which creates a mean-reversion problem: a taxpayer with a very high income in the pre-reform year will tend to have a lower income in the post-reform year, other things being equal. In the absence of

[11] The estimate ε is an *uncompensated* elasticity due to budget set linearization implied by the virtual income formulation. Under this formulation, the coefficient ε captures the effect of a proportional tax rate change on *all* units of earnings, holding constant virtual income (the linearized budget intercept with the consumption axis) and therefore *not* compensating for any income effects of the tax change. This is conceptually similar to a wage rate change in a standard labor supply function, which produces both substitution and income effects.

controls for transitory income components, they would be captured by pre-reform income levels and hence be absorbed by the estimated tax effect.

The problems just described are particularly acute when considering tax reforms that are strongly targeted to certain income groups such as high-income earners (as in the case of the US tax reforms in the 1980s). In that case, the mechanical tax changes will be strongly correlated with income level and therefore with skill-dependent demand shocks and transitory income components. To deal with this problem, Auten and Carroll (1999), Moffitt and Wilhelm (2000), Gruber and Saez (2002), and Kopczuk (2005) propose to control in different ways for pre-reform income. For example, Kopczuk (2005) proposes a specification that includes the change in income in the year prior to the reform, $z_s - z_{s-1}$, as a proxy for transitory income components, along with the lagged income level z_{s-1} as a proxy for the permanent income level. He allows for nonlinearity by experimenting with ten-piece splines in the logarithms of either of the two controls. He also explores a number of other specifications, including those adopted by Auten and Carroll (1999) and Gruber and Saez (2002). The results show that the elasticity estimates are extremely sensitive to the specification of pre-reform income controls.

We consider the main pre-reform income controls that have been proposed in the literature. Unlike previous studies, we find that our results are extremely robust to the specification of income controls, which suggests that unobserved nontax factors impacting on taxable income do not pose a threat to identification here. There are two main reasons for the robustness of our findings. First, as discussed in the introduction and shown in Figure 1, the income distribution in Denmark has remained very stable over the period that we study, implying that bias from nontax changes in the income distribution is not a concern. This isolates mean-reversion as the only potential bias that the income controls have to correct for. Second, the biases discussed above rely on the presence of a correlation between tax changes and pre-reform income level, which is not an important feature of the Danish reforms. As described earlier, the Danish reforms were not systematically targeted to certain income groups and created are lot of up-and-down movements in tax rates throughout the income distribution. In fact, the increasing asymmetry in the tax treatment of different income components creates variation even for taxpayers at the same income level (but with different income compositions). In the next section, we demonstrate the exact nature of the Danish variation around specific reform episodes.

C. *Mechanical Variation in Marginal Net-of-Tax Rates*

To give a clear sense of the identifying variation, Figure 3 shows the mechanical variation in marginal net-of-tax rates (i.e., the variation in the instrument) for different income types in panels A–C around the two largest reform episodes in our data, the 1987 reform (left side) and the 1994 reform (right side). Each figure shows three-year differences in percent, where we have split the sample into seven groups using base-year income variables: (i) individuals who are in the bottom bracket both before and after; (ii) individuals who are pushed from the middle to the bottom bracket; (iii) individuals who are pushed from the bottom to the middle bracket; (iv) individuals who are in the middle bracket both before and after;

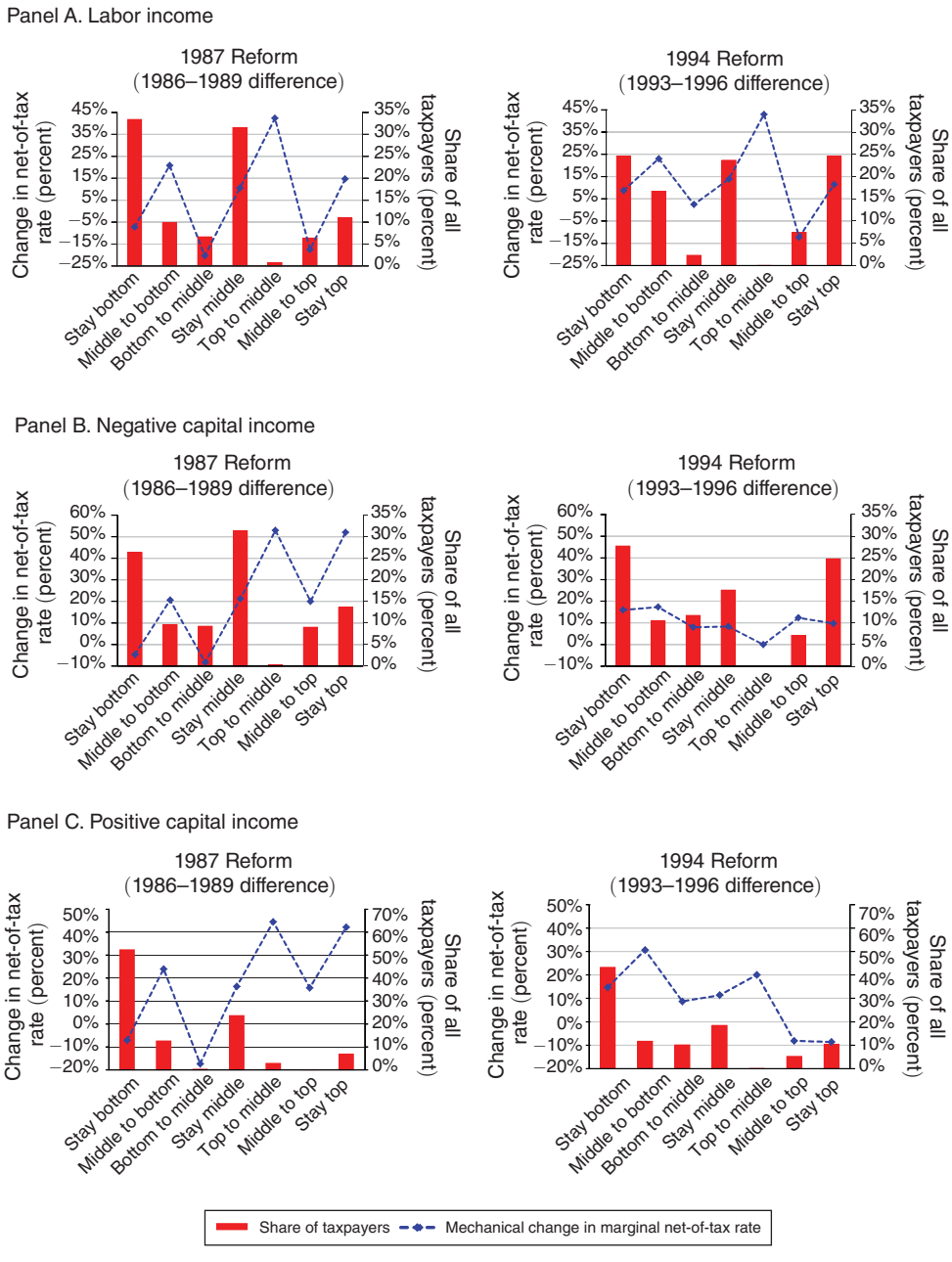

FIGURE 3. MECHANICAL VARIATION IN THE MARGINAL NET-OF-TAX RATE

Notes: The figure shows the mechanical variation in marginal net-of-tax rates (dashed lines) due to the 1987 reform and 1994 reform, respectively, on labor income (panel A), negative capital income (panel B), and positive capital income (panel C). Each panel shows three-year differences in percent, where we have split the sample into seven groups using base-year income variables: (i) individuals who are in the bottom bracket both before and after, (ii) individuals who are pushed from the middle to the bottom bracket, (iii) individuals who are pushed from the bottom to the middle bracket, (iv) individuals who are in the middle bracket both before and after, (v) individuals who are pushed from the top to the middle bracket, (vi) individuals who are pushed from the middle to the top bracket, and (vii) individuals who are in the top bracket both before and after. The figure also shows the size of each group as a share of all taxpayers (bars).

(v) individuals who are pushed from the top to the middle bracket; (vi) individuals who are pushed from the middle to the top bracket; and (vii) individuals who are in the top bracket before and after.

Two aspects of the figure are worth noting. First, it is the combination of changes in tax bases and bracket cutoffs that makes it possible for a tax reform to push some taxpayers from a lower to a higher bracket (e.g., bottom to middle) and simultaneously push other taxpayers in the opposite direction (e.g., middle to bottom). Second, the grouping of taxpayers in the figure is useful to make the identifying tax changes stand out. The grouping is different from one based on quantiles of the income distribution. Such a grouping would show much less *average* tax variation in each quantile group as it lumps together tax reductions for those who stay in a given bracket or move to a lower bracket with tax increases for those who are pushed into a higher bracket. Hence, an income quantile representation of tax changes would hide a lot of the identifying variation in the data.

Each panel shows the mechanical change in the marginal net-of-tax rate in different groups (dashed line, left *y*-axis) and the size of each treatment group (bars, right *y*-axis).[12] Panel A shows the change in labor income taxation around the 1987 reform (1986–1989 difference) and around the 1994 reform (1993–1996 difference). For the 1987 reform, there are very large *and* strongly heterogeneous tax changes across taxpayers, with the percentage change in the net-of-tax rate varying between −20 percent and +42 percent. These differences in tax treatments across groups are larger than the tax treatment differences created by the Tax Reform Act of 1986 in the United States and the Tax Reform of the Century in Sweden in 1991, two reforms that have been extensively analyzed in the literature. For the 1994 reform, tax changes are also very large and heterogeneous, but not quite to the same degree as for the 1987 reform.

Panels B and C show the variation in the taxation of negative and positive capital income around the same two reform episodes. For the 1987 reform, the tax variation on capital income, especially negative capital income, is even stronger than for labor income. The marginal net-of-tax rate for those in the top bracket increased by more than 50 percent (40 percent) in the case of negative (positive) capital income, while other groups of taxpayers experienced much smaller increases or reductions in the net-of-tax rate. The 1994 reform has much smaller effects than the 1987 reform and, importantly, the tax variation created by the 1994 reform is qualitatively very different. For positive capital income, for example, the net-of-tax rate is reduced at the top and increased at the bottom directly opposite the 1987 reform.

Although the tax changes around 1987 and 1994 constitute the strongest variation in the data, there is in fact a lot of variation throughout the period we consider. Importantly, the tax variation in other years is often qualitatively different in terms of who experience tax increases and who experiences tax cuts.

[12] The population shares of the seven groups do not quite sum to 100 percent due to a small number of taxpayers below the exemption level for the bottom bracket.

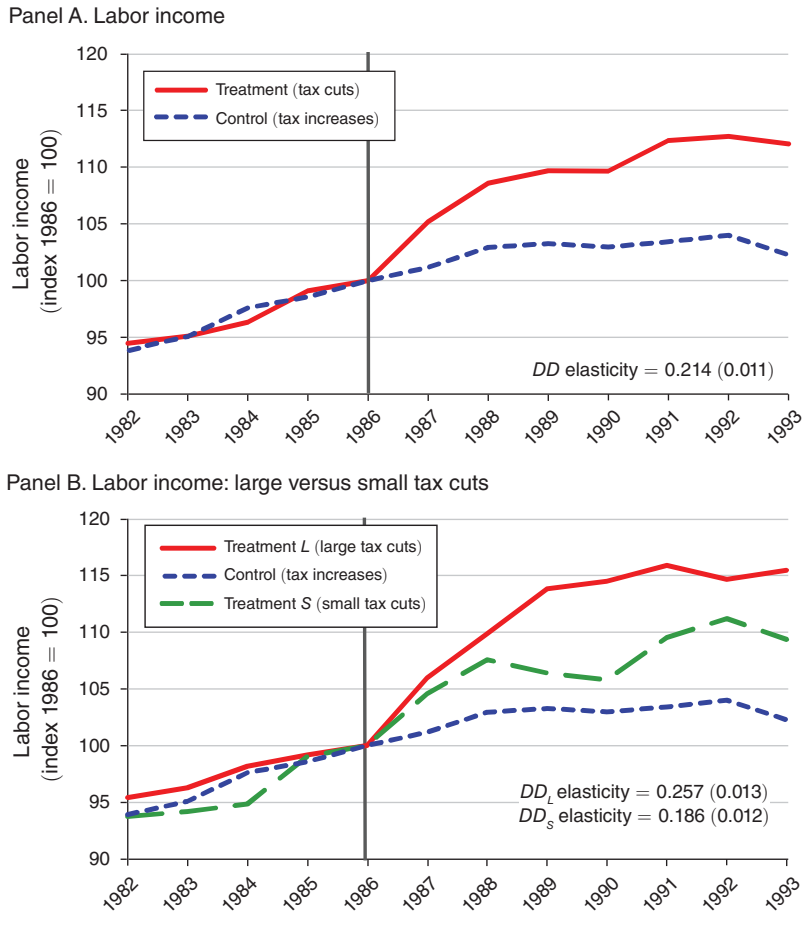

FIGURE 4. GRAPHICAL EVIDENCE ON TAXABLE INCOME RESPONSES TO THE DANISH 1987 REFORM

(*Continued*)

IV. Empirical Results

A. *Graphical Evidence*

This section presents graphical evidence on taxable income responses to the large 1987 reform. Figure 4 shows the evolution of labor income (panels A–B) and capital income (panel C) between 1982–1993 for groups that were affected differently by the 1987 reform, demarcated by a vertical line.[13] The figure is based on a balanced panel of individuals who are observed throughout the period. Panel A shows the effect on labor income using a simple treatment-control assignment based on the reform-induced tax variation shown in Figure 3: the treatment group includes those who experience an increase in the marginal net-of-tax rate on labor income due to

[13] The vertical line demarcates 1986, which is the last pre-reform year (as the reform was passed in parliament during 1986 and changed tax rates starting from 1987). Income levels in 1986 are normalized to 100 for all groups.

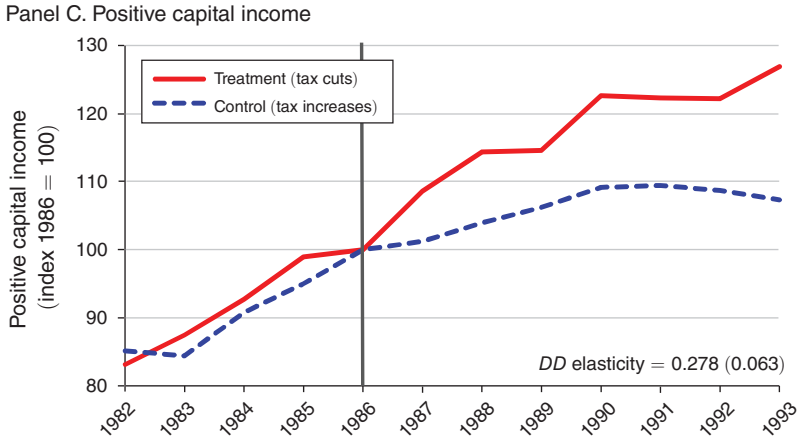

FIGURE 4. GRAPHICAL EVIDENCE ON TAXABLE INCOME RESPONSES TO THE DANISH 1987 REFORM (*Continued*)

Notes: The figure shows the evolution of labor income (panels A–B) and capital income (panel C) between 1982–1993 for groups that were affected differently by the 1987 reform. The figure is based on a balanced panel of individuals who are observed throughout the period. The vertical line at 1986 denotes the last pre-reform year (as the reform was passed in parliament during 1986 and changed tax rates starting from 1987), and income levels in 1986 are normalized to 100 in all groups. The treatment-control definition is based on the reform-induced tax variation for the different groups shown in Figure 3 (1986–1989 change for labor income and positive capital income), with treatments (controls) being an aggregation of groups who experience an increase (decrease) in the marginal net-of-tax rate due to the reform. Panel B splits the treatment group for labor income into those who experience the largest net-of-tax rate increases (Treatment *L* excludes the "stay middle" group in Figure 3) and those who experience smaller net-of-tax rate increases (Treatment *S* is the "stay middle" group in Figure 3). All panels show that income trends are very parallel in the years prior to the reform and then start to diverge precisely in 1987, the first year of tax cuts on the treatment groups. Most of the effect of the tax reform materializes within three years. The figure reports difference-in-differences estimates of the elasticities of taxable labor and capital income, comparing treatment and control groups over the three-year interval 1986–1989. The estimates DD_L and DD_S in Panel B refer to treatment L and treatment S, respectively. The DD estimates in all panels are based on 2SLS regressions of log income on an after-reform time dummy, a treatment-group dummy, and the log marginal net-of-tax rate, the latter variable being instrumented by the interaction between the after-reform and treatment-group dummies.

the reform (1986–1989 difference), while the control group includes those who experience a reduction in the marginal net-of-tax rate on labor income due to the reform. Panel B also shows effects on labor income, but splitting the treatment sample into those experiencing the largest net-of-tax rate increases (at least 15 percent) and those experiencing smaller net-of-tax rate increases. Panel C shows the effect on positive capital income, with the treatment (control) group defined as those who experience an increase (decrease) in the marginal net-of-tax rate on positive capital income due to the reform. The figure also reports difference-in-differences estimates of the elasticities of taxable income with respect to the marginal net-of-tax rate, comparing treatment and control groups over the three-year interval from 1986 to 1989.[14]

The following main findings emerge from the figure. First, the income trends of treatments and controls are completely parallel in the years prior to the reform and then start to diverge precisely in 1987, the first post-reform year. The tax reform effect builds up gradually, with most of the effect materializing within about three

[14]The difference-in-differences estimates are based on 2SLS (two-stage least-squares) regressions of log income on an after-reform time dummy, a treatment-group dummy, and the log marginal net-of-tax rate, the latter variable being instrumented by the interaction of the after-reform and treatment-group dummies.

years.[15] This provides compelling evidence of taxable income responses to the reform, and in particular the remarkable similarity of pre-trends shows that differential nontax income changes is not a threat to identification here. As discussed earlier, this is a result of the stability of the Danish income distribution and the rich identifying variation. Second, the effect on labor income is larger for those experiencing the largest tax cuts (treatment L) than for those experiencing smaller tax cuts (treatment S). Importantly, the effect of large treatments is larger both in absolute terms and in elasticity terms as shown by the difference-in-differences estimates DD_L and DD_S. The large-treatment elasticity of 0.26 is about 40 percent larger than the small-treatment elasticity. This is consistent with the idea that larger incentive changes are better able to overcome optimization frictions (such as switching and attention costs) and are therefore more revealing of structural long-run elasticities (Chetty 2012; Kleven and Waseem 2013), a point we come back to below. Third, capital income responses are stronger than labor income responses. The capital income elasticity is close to 0.3, roughly 30 percent larger than the average labor income elasticity shown in panel A. The finding of larger capital income elasticities will come out more strongly when we turn to the full tax reform variation over time in the next section.

To conclude, the graphical analysis in Figure 4 provides compelling evidence of taxable responses, arguably representing the first nonparametrically identified evidence of taxable income elasticities using tax reforms.

B. *Panel Regression Evidence*

This section presents panel regression evidence using all the tax reform variation between 1984 and 2005. The results are based on 2SLS estimations of equation (5) using mechanical tax changes as instruments. We present separate estimations for labor income, capital income, broad income (labor plus capital income), and taxable income as defined in Table 1. The first-stage regressions (not shown) are always very strong. The full details of the different regressions are provided in the notes to each table.

Labor Income Elasticities.—The first set of results is presented in Table 3, which shows estimates of labor income elasticities based on specifications that assume no income effects and no cross-tax effects. The table splits the sample by wage earners (panel A) and self-employed individuals (panel B), and shows results for a number of different specifications and sample restrictions considered in the previous literature.

The table rows consider alternative ways of controlling for pre-reform income (base-year s income in equation (5)): no income controls (as in Feldstein 1995), log base-year income (as in Auten and Carroll 1999), ten-piece spline in log base-year income (as in Gruber and Saez 2002), and the combination of a ten-piece spline in log base-year income and the log-deviation between $s-1$ and s income (as in Kopczuk 2005). Results in the previous literature have been extremely sensitive to the specification of these income controls. The table columns consider alternative

[15] As discussed earlier, this finding provides a justification for using three-year intervals when we come to the panel difference-in-differences regressions below.

Table 3—The Elasticity of Labor Income

	Panel A. Wage earners				*Panel B. Self-employed*			
Socioeconomic controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Broad income restriction	> 0K	> 0K	> 100K	> 0K	> 0K	> 0K	> 100K	> 0K
Taxpayers around kinks	Include	Include	Include	Exclude	Include	Include	Include	Exclude
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-reform income controls								
No pre-reform income controls	−0.203***	−0.191***	−0.102***	−0.132***	−0.171***	−0.155***	−0.126***	−0.162***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.015)	(0.013)	(0.015)	(0.015)
log base-year (period s) income	0.065***	0.060***	0.065***	0.062***	0.102***	0.095***	0.085***	0.098***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.016)	(0.015)	(0.016)	(0.016)
Splines of log base-year (period s) income	0.044***	0.042***	0.044***	0.043***	0.112***	0.100***	0.093***	0.101***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.014)	(0.013)	(0.014)	(0.015)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.049***	0.046***	0.052***	0.047***	0.094***	0.090***	0.081***	0.094***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.015)	(0.014)	(0.015)	(0.016)
Observations	29,568,870	29,568,870	27,121,055	28,060,857	1,646,270	1,646,270	1,381,560	1,405,915

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable in all specifications is the three-year growth rate in real wage earnings. The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate, instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior (i.e., mechanical tax variation from tax reforms). All elasticities in the table are based on specifications without income effects. Socioeconomic controls include labor market experience, experience squared, age, gender, marital status, number of kids aged 0–18 years, educational degree, industry, municipality, and local unemployment rate. All specifications also include base-year fixed effects. Regressions are weighted by labor income and restricts the sample to individuals with positive labor income (in addition to the sample restrictions described in Section III). "Splines" refer to a flexible piecewise linear functional form with ten components. Taxpayers close to kink points are defined as those who have an income within 5,000 DKK of the top kink, 3,000 DKK of the middle kink, or 2,000 DKK of the bottom kink.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

specifications of socioeconomic controls and sample: with and without socioeconomic controls, different income restrictions at the bottom (observations with broad income above zero or above 100,000 kroner), and whether or not taxpayers located close to kink points are included or excluded in the estimation. The last sensitivity check is done because the Gruber-Saez style specification considered here assumes that taxpayers behave as if they are located in the interior of brackets and do not bunch at kink points. If there is significant bunching at kink points, this may create bias in the estimates. As shown by Chetty et al. (2011), there is indeed bunching at the top kink in Denmark (but not at the bottom and middle kinks) and we therefore investigate if our results are sensitive to this.

The table shows that results are extremely robust to specification, with an elasticity of labor income consistently estimated to about 0.05 for wage earners and 0.10 for self-employed individuals. To be precise, while it does matter whether *any* income controls are included due to mean-reversion (first row versus subsequent rows), the exact specification of pre-reform income controls has no impact on the results. This robustness derives from the stable Danish income distribution and would survive even richer pre-reform income controls than those shown in the table (see Kleven and Schultz 2012). Furthermore, the table also shows that results are essentially unaffected by socioeconomic controls, excluding taxpayers at the bottom (to avoid mean reversion at the bottom), and excluding taxpayers around kink points

Table 4—The Elasticity of Labor Income: Heterogeneity

	Full sample (1)	Top 20 percent (2)	Top 10 percent (3)	College degree or more (4)	Women (5)	With kids below 18 years old (6)	With kids below 6 years old (7)
Panel A. All workers							
Elasticity wrt. $1-\tau$	0.049***	0.076***	0.085***	0.062***	0.054***	0.054***	0.083***
	(0.002)	(0.008)	(0.012)	(0.009)	(0.004)	(0.005)	(0.010)
Observations	31,215,140	6,243,028	3,121,514	5,056,852	15,295,419	14,325,926	4,751,852
Panel B. Wage earners							
Elasticity w.r.t. $1-\tau$	0.046***	0.073***	0.081***	0.061***	0.052***	0.052***	0.080***
	(0.002)	(0.009)	(0.012)	(0.010)	(0.005)	(0.006)	(0.010)
Observations	29,568,870	5,913,774	2,956,887	4,844,483	14,785,075	13,631,249	4,593,606
Panel C. Self-employed							
Elasticity w.r.t. $1-\tau$	0.090***	0.135***	0.147***	0.113***	0.116***	0.119***	0.171***
	(0.014)	(0.037)	(0.044)	(0.039)	(0.026)	(0.022)	(0.046)
Observations	1,646,270	329,254	164,627	212,369	510,344	694,677	158,246

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable in all specifications is the three-year growth rate in real wage earnings. The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate, instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior (i.e., mechanical tax variation from tax reforms). All elasticities in the table are based on specifications without income effects. Regressions are weighted by labor income and restricts the sample to individuals with positive labor income (in addition to the sample restrictions described in Section III). All regressions control for pre-reform income using splines of log $s-1$ income and the log-deviation between $s-1$ and s incomes, where "splines" refer to a flexible piecewise linear functional form with ten components. The specifications also control for a rich set of socioeconomic variables and base-year fixed effects as described in Table 3.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

(to avoid results being attenuated by bunching).[16] Because the robustness shown in Table 3 holds for all the results in the paper, the following tables restrict attention to a smaller subset of the specifications considered above. Unless otherwise stated, we consider specifications that controls for socioeconomic variables, includes all observations with positive broad income, and do not drop observations around kink points. In tables that do not consider different pre-reform income controls, we have used the richest specification from Table 3 (bottom row).

Table 4 investigates heterogeneity in the labor income elasticity. The first column repeats results for the full sample (as in the previous table) while the following columns show results for different subgroups (top 20 percent earners, top 10 percent earners, highly-educated workers, women, those with kids younger than 18 years,

[16] It is not surprising that bunching around kink points has no significant impact on our results. Even though there is visually clear bunching at the top kink in Denmark, it affects a small part of the population (see Table A1) and is small in magnitude, especially for wage earners where the elasticity implied by bunching is only 0.01 (Chetty et al. 2011). Bunching is stronger for self-employed individuals, which is consistent with our finding that the impact of excluding taxpayers around kink points is slightly larger for the self-employed than for wage earners. Notice also that, as one would expect, elasticities become larger when excluding observations close to kinks, because those taxpayers are constrained in their response to the reform-driven tax variation that we use for identification.

TABLE 5—THE ELASTICITY OF LABOR INCOME: UNCOMPENSATED AND INCOME ELASTICITIES

	Panel A. Wage earners			*Panel B. Self-employed*		
	Without income effects	With income effects		Without income effects	With income effects	
Pre-reform income controls	Elasticity w.r.t. $1-\tau$ (1)	Uncompensated elasticity w.r.t. $1-\tau$ (2)	Elasticity w.r.t. virtual income (3)	Elasticity w.r.t. $1-\tau$ (4)	Uncompensated elasticity w.r.t. $1-\tau$ (5)	Elasticity w.r.t. virtual income (6)
No pre-reform income controls	−0.191*** (0.002)	−0.140*** (0.003)	−0.007*** (0.002)	−0.155*** (0.013)	−0.159*** (0.030)	−0.003 (0.021)
log base-year (period s) income	0.060*** (0.002)	0.042*** (0.003)	−0.015*** (0.003)	0.095*** (0.015)	0.114*** (0.033)	−0.015 (0.021)
Splines of log base-year (periods) income	0.042*** (0.002)	0.034*** (0.003)	−0.007*** (0.003)	0.100*** (0.013)	0.105*** (0.032)	−0.004 (0.021)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.046*** (0.002)	0.030*** (0.003)	−0.013*** (0.002)	0.090*** (0.014)	0.096*** (0.032)	−0.005 (0.021)
Observations		29,568,870			1,646,270	

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. Columns 1 and 4 repeat results from Table 3 based on a specification without income effects, while the other columns consider a specification with income effects. The dependent variable in all specifications is the three-year growth rate in real wage earnings. The independent variables of interest are three-year growth rates in the marginal net-of-tax rate and virtual income, instrumented using mechanical variation in those variables created by tax reforms. All specifications include controls for labor market experience, experience squared, age, gender, marital status, number of kids aged 0–18 years, educational degree, industry, municipality, local unemployment rate, and base-year fixed effects. Regressions are weighted by labor income and restricts the sample to individuals with positive labor income (in addition to the sample restrictions described in Section III). "Splines" refer to a flexible piecewise linear functional form with ten components.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

and those with kids younger than six years). The different rows consider all workers, wage earners alone, and the self-employed alone. The direction of the heterogeneity corresponds to what one would expect, with larger elasticities for top earners, women and those with kids (especially young kids), and with larger elasticities for the self-employed within each group. But the amount of heterogeneity is not huge and elasticities are consistently modest (below 0.2).

Table 5 considers the importance of income effects by including virtual income in the specification, again splitting the sample by wage earners and self-employed individuals. As a benchmark, columns 1 and 4 repeat results from specifications without income effect. Columns 2–3 and 5–6 consider specifications with income effects, showing estimates of both the uncompensated elasticity with respect to the marginal net-of-tax rate and the elasticity with respect to virtual income. The compensated elasticity can be inferred from those estimates using the Slutsky equation. The general finding in Table 5 is that income elasticities are negative, implying that leisure is a normal good, but very small.[17] Furthermore, the uncompensated elasticities obtained from the specification with income effects (corresponding roughly to compensated elasticities due to the smallness of income effects) are very similar

[17] The point estimates of income elasticities are roughly the same for wage earners and the self-employed, but they are statistically significant only for wage earners where we have much more power.

to the elasticities obtained from the baseline specification without income effects. Hence, accounting for income effects is not very important in our setting (and so we ignore them from now on), a finding that is consistent with many previous labor supply and taxable income studies (e.g., Gruber and Saez 2002).

Capital Income Elasticities.—We now turn to the analysis of capital income responses. Capital income is a net income concept that may be either positive or negative, and is in fact negative for the vast majority of taxpayers in Denmark due to interest payments on mortgages and other loans. As described in Section I, the tax treatment of capital income is very different depending on whether the net value is positive or negative, with much higher tax rates on positive than on negative capital income. Since we consider log-linear regression specifications that do not allow for nonpositive income values, we consider capital income in absolute value and run separate regressions for negative and positive capital income.[18]

The results are shown in Table 6, which compares elasticities of labor income (panel A) to elasticities of negative and positive capital income (panels B–C). The table is based on the full sample (wage earners and self-employed individuals together) and shows results for specifications with different pre-reform income controls and socioeconomic controls (none versus a rich set). Notice that we would expect the elasticity of negative capital income (in absolute value) to be negative and the elasticity of positive capital income to be positive, and this is indeed what we find for all specifications. Overall, capital income elasticities are 2–3 times larger in absolute value than labor income elasticities, and again the results are very robust to the specification of both pre-reform income controls and socioeconomic controls. Elasticities of negative capital income vary between -0.10 and -0.13 across all specifications, while elasticities of positive capital income vary between 0.10 and 0.14 (ignoring the specification without *any* pre-reform income controls).

Small-Reform versus Large-Reforms Elasticities.—The average elasticities that we estimate when using the full reform variation in Denmark are fairly small, consistent with the findings of many other micro-studies of intensive labor supply responses (e.g., Blundell and MaCurdy 1999). An important question is whether observed micro-elasticities are small because they are attenuated by optimization frictions (such as inattention and adjustment costs) or because the true structural elasticity that overcomes frictions and matters for long-run behavior is small (Chetty et al. 2011; Chetty 2012; Kleven and Waseem 2013). As argued by Chetty (2012), the estimation of structural long-run elasticities requires tax variation that is large

[18] This strategy requires us to drop individuals with zero capital income as well as those whose capital income switch sign between base and post year. An additional argument for dropping observations around zero capital income is that the imposition of much higher marginal tax rates on positive than on negative capital income (after the 1987 reform) creates a large kink in the capital income tax schedule at zero. This is associated with strong incentives for bunching at zero capital income, a type of response that is not captured by the Gruber-Saez estimation strategy and may create bias as discussed earlier. Indeed, we find strong bunching in the data around zero capital income. While this is interesting by itself and might offer a different way of uncovering capital income elasticities, a key problem of exploiting bunching at zero capital income is that it is likely to partly reflect nontax factors. Even without the tax kink, there would have been some excess clustering at zero as many taxpayers have not accumulated any saving or debt because of their stage in the life cycle (e.g., young taxpayers) or because of credit constraints.

Table 6—Elasticities of Labor Income versus Capital Income

	Panel A. Labor income		*Panel B. Negative capital income*		*Panel C. Positive capital income*	
Socioeconomic controls	No (1)	Yes (2)	No (3)	Yes (4)	No (5)	Yes (6)
Pre-reform income controls						
No pre-reform income controls	−0.201***	−0.189***	−0.089***	−0.084***	0.087***	0.081***
	(0.003)	(0.002)	(0.009)	(0.009)	(0.025)	(0.024)
log base-year (period s) income	0.065***	0.060***	−0.107***	−0.103***	0.112***	0.106***
	(0.002)	(0.002)	(0.007)	(0.007)	(0.024)	(0.024)
Splines of log base-year (period s) income	0.047***	0.044***	−0.129***	−0.127***	0.138***	0.135***
	(0.002)	(0.002)	(0.006)	(0.005)	(0.025)	(0.025)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.052***	0.049***	−0.123***	−0.120***	0.119***	0.113***
	(0.002)	(0.002)	(0.005)	(0.005)	(0.024)	(0.024)
Observations	31,215,140	31,215,140	27,125,664	27,125,664	4,837,538	4,837,538

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable is the three-year growth rate in real labor income (panel A), negative capital income in absolute value (panel B), and positive capital income (panel C). The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate on labor income (panel A), negative capital income (panel B), and positive capital income (panel C), each instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior. All elasticities are based on specifications without income effects and without cross-tax effects between labor and capital income. Socioeconomic controls include labor market experience, experience squared, age, gender, marital status, number of kids aged 0–18 years, educational degree, industry, municipality, and local unemployment rate. All specifications also include base-year fixed effects. Regressions are weighted by income (labor income in columns 1–2, capital income in columns 3–6). Labor income regressions restrict the sample to individuals with positive labor income, while capital income regressions drop individuals with zero capital income and individuals whose capital income changes sign between base-year and post-year (in addition to the basic sample restrictions described in Section III). "Splines" refer to a flexible piecewise linear functional form with ten components.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

enough to overcome frictions. The Danish setting allows us to explore this question, because the time period we consider includes one very large tax reform episode (1987 reform) along with several smaller tax reform episodes. The graphical difference-in-differences analysis of the 1987 reform produced much larger elasticities than the estimations using all the reforms together, consistent with the notion that larger tax changes generate larger elasticities due to optimization frictions. To investigate this point further, Table 7 compares panel-regression elasticities for the large 1987 reform (1984–1990 period) and the smaller post-1987 reforms (1991–2005 period) for labor income and positive capital income in the full sample (wage earners and self-employed individuals together).[19]

The results in the table support the hypothesis that micro-elasticities are larger when estimated using large tax variation. The labor income elasticity estimated from the 1987 reform alone is about 0.11, which is three to five times larger than elasticities based on the post-1987 reforms alone. Results for capital income are qualitatively

[19] We do not consider the 1987 reform versus post-1987 reform split for negative capital income, because it is associated with very little identifying variation after the 1987 reform as there was just one bracket for negative capital income through most of this period (see panel B of Figure 2).

Table 7—Labor and Capital Income Elasticities: Small versus Large Reforms

	Panel A. Labor income			*Panel B. Positive capital income*		
Pre-reform income controls	All reforms (1984–2005) (1)	1987 reform (1984–1990) (2)	Post-1987 reforms (1991–2005) (3)	All reforms (1984–2005) (4)	1987 reform (1984–1990) (5)	Post-1987 reforms (1991–2005) (6)
No pre-reform income controls	−0.189*** (0.002)	−0.182*** (0.002)	−0.192*** (0.004)	0.081*** (0.024)	0.131*** (0.021)	0.124*** (0.032)
log base-year (period s) income	0.060*** (0.002)	0.112*** (0.002)	0.043*** (0.004)	0.106*** (0.024)	0.137*** (0.023)	0.076** (0.032)
Splines of log base-year (period s) income	0.044*** (0.002)	0.104*** (0.002)	0.023*** (0.004)	0.135*** (0.025)	0.151*** (0.024)	0.109*** (0.035)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.049*** (0.002)	0.111*** (0.002)	0.025*** (0.004)	0.113*** (0.024)	0.155*** (0.023)	0.094*** (0.032)
Observations	31,215,140	11,799,628	19,415,512	4,837,538	1,756,743	3,080,795

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable is the three-year growth rate in real labor income (panel A) and in positive capital income (panel B). The independent variable of interest is the three-year growth rate in the marginal net-of-tax rate on labor income (panel A) and on positive capital income (panel B), each instrumented using the three-year growth rate in the simulated marginal net-of-tax rate under base-year behavior. Columns 1 and 4 are based on the full data period (1984–2005) and repeat results shown in Table 6. Columns 2–3 and 5–6 split the data into a period with large tax reform variation (1984–1990) and a period with smaller tax reform variation (1991–2005). All specifications are otherwise identical to those described in Table 6.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

similar, but the difference between large-reform elasticities (0.14–0.16) and small-reform elasticities (0.08–0.11) is not as strong as for labor income. It is intuitive that the size of the tax change matters more for labor income than for capital income, because labor income responses are more likely to be affected by real adjustment costs (e.g., search costs) than capital income responses. On the other hand, frictions due to (for example) inattention would matter for both labor and capital income.

It is possible to generate even larger elasticities by zooming in on those parts of the 1987 reform that were associated with the very largest tax changes, corresponding to the graphical difference-in-differences analysis above. The graphical analysis produced elasticities of 0.2–0.3 (or about ten times larger than the small-reform elasticities in Table 7) by using only the three-year interval (1986–1989) featuring the largest tax changes and by focusing on a treatment group experiencing the largest net-of-tax rate increases over that three-year interval.

Finally, while our finding of a positive correlation between the size of the elasticity and the size of the identifying variation is qualitatively consistent with Chetty et al. (2011), our difference-in-differences estimates using large tax reforms are quantitatively much stronger than their bunching estimates using large kinks (as even the large-kink elasticity in Denmark is tiny, about 0.01). A likely explanation is that bunching around kink points, even large kink points, are much more affected by frictions due to the fact that bunching requires precise knowledge of bracket thresholds along with a very precise behavioral response, both of which may be costly to achieve due to adjustment costs, attention costs, etc.

Broad Income versus Taxable Income Elasticities.—The previous literature has focused much attention on the distinction between broad income (labor plus capital income) and taxable income (broad income minus deductions), finding that the elasticities of broad income tend to be much smaller. By estimating labor income and capital income elasticities, not including deductions, our estimates are closest in spirit to broad income elasticities. In fact, assuming no cross-effects between labor and capital income, the broad income elasticity with respect to a *joint* change in the net-of-tax rates on labor and capital income is a weighted average of the underlying labor and capital income elasticities, with weights equal to the broad income shares of each component.[20] This gives a broad income elasticity close to the labor income elasticities estimated here, because labor income represent a very large share of broad income for most taxpayers.

To investigate the difference between broad and taxable income elasticities, we run regressions of broad income and taxable income, respectively, on the marginal net-of-tax rates on labor and capital income simultaneously. Based on the conceptual framework in Section III, total income (either broad income or taxable income) can be written as a function of all of the underlying net-of-tax rates $1 - \tau^1, \ldots, 1 - \tau^n$. Such a specification does not assume the absence of cross-effects and is therefore more general than what we have considered above. While in principle one should include *all* the underlying tax rates in such a regression (the tax rates associated with each income concept in Table 1), separately identifying the effect of each is not feasible and so we focus on the effects of the tax rates on labor and capital income. The results are shown in Table 8 for broad income (panel A) and taxable income (panel B) based on either the full reform variation (1984–2005) or only the large reform variation (1984–1990). The table shows elasticities with respect to the net-of-tax rate on labor income alone, capital income alone, and a joint increase in both of them.

The following findings emerge from the table. First, taxable income elasticities are larger than broad income elasticities as one would expect, but the difference is much smaller than in the US literature. This suggests that the additional avoidance or evasion opportunities associated with the deduction component of taxable income are fairly small in Denmark. Second, the elasticities of taxable and broad income with respect to a *joint* increase in the net-of-tax rates on labor and capital income are only slightly larger than the elasticities with respect to an *isolated* increase in the net-of-tax rate on labor income, which reflects the point above that labor income represent most of total income for most taxpayers. Third, elasticities estimated using

[20] Absent cross-effects, total income can be written as $z = \sum_{j=1}^{n} z^j(1 - \tau^j, y)$, where the underlying income components $1, \ldots, n$ depends on the total income measure (e.g., broad or taxable income). Considering a common percentage change δ in all net-of-tax rates, we have $d(1 - \tau^j) = \delta(1 - \tau^j) \quad \forall j$. Denoting the elasticity for income component j by $\varepsilon^j = \frac{\partial z^j}{\partial(1-\tau^j)} \frac{1-\tau^j}{z^j}$, we obtain

$$\frac{dz/z}{\delta} = \sum_{j=1}^{n} \left(\frac{z^j}{z} \right) \varepsilon^j,$$

where $\frac{dz/z}{\delta}$ is the elasticity of total income z with respect to a joint percentage change δ in the net-of-tax rates on each underlying income component.

Table 8—Elasticities of Broad Income versus Taxable Income

	Panel A. Broad income			*Panel B. Taxable income*		
	All individuals (1)	Wage earners (2)	Self-employed (3)	All individuals (4)	Wage earners (5)	Self-employed (6)
	All reforms (1984–2005)					
$1-\tau$ on labor income	0.054***	0.052***	0.084***	0.066***	0.064***	0.104***
	(0.003)	(0.003)	(0.039)	(0.003)	(0.003)	(0.040)
$1-\tau$ on capital income	0.014***	0.014***	0.003	0.015***	0.016***	−0.006
	(0.002)	(0.002)	(0.029)	(0.002)	(0.002)	(0.031)
Joint increase in $1-\tau$ on labor and capital income	0.068***	0.067***	0.084*	0.082***	0.080***	0.099*
	(0.005)	(0.005)	(0.048)	(0.006)	(0.005)	(0.050)
Number of observations	31,103,309	29,540,762	1,562,547	30,893,781	29,398,652	1,495,129
	1987 reform (1984–1990)					
$1-\tau$ on labor income	0.136***	0.134***	0.191***	0.142***	0.137***	0.203***
	(0.003)	(0.004)	(0.043)	(0.004)	(0.004)	(0.044)
$1-\tau$ on capital income	0.045***	0.044***	0.055	0.046***	0.043***	0.064*
	(0.004)	(0.003)	(0.033)	(0.004)	(0.004)	(0.035)
Joint increase in $1-\tau$ on labor and capital income	0.181***	0.177***	0.246***	0.189***	0.181***	0.267***
	(0.005)	(0.005)	(0.056)	(0.006)	(0.006)	(0.058)
Number of observations	11,713,264	11,106,010	607,254	11,608,130	11,044,629	563,501

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable is the three-year growth rate in broad income (labor income + capital income) in panel A and in taxable income (labor income + other personal income + capital income − deductions) in panel B. The independent variables of interest are the three-year growth rates in the marginal net-of-tax rates on labor income and capital income, instrumented using mechanical variation in those variables created by tax reforms. The elasticities are based on specifications without income effects. Regressions are weighted by broad income in panel A and by taxable income in panel B. In all specifications the sample is restricted to individuals with positive income (in addition to the sample restrictions described in Section III). All regressions control for pre-reform income using splines of log $s-1$ income and the log-deviation between $s-1$ and s incomes, where "splines" refer to a flexible piecewise linear functional form with ten components. The specifications also control for a rich set of socioeconomic variables and base-year fixed effects as described in Table 3.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

only the large-reform variation are again an order of magnitude larger than elasticities estimated using the full reform variation. Fourth, the largest estimates are obtained by considering the elasticity of total *taxable* income with respect to a *joint* change in both tax rates, using only the large 1987 reform variation, which yields an elasticity of about 0.2 for the full sample. As above, if we zoom in on the very largest tax changes created by the 1987 reform (1986–1989 difference for specific groups), it is possible to generate even larger taxable income elasticities that come fairly close to the elasticity levels of 0.3–0.4 often found in the US literature (but which were sensitive to specification as discussed above).

Cross-Tax Effects between Labor and Capital Income.—In a setting with multiple tax bases, the sufficient statistics for welfare analysis consist of own-tax and cross-tax elasticities for each tax base in the system. In such settings, the elasticity of *total* taxable income with respect to its *own* tax rate is quite far from being

Table 9—Labor and Capital Income Elasticities: Own-Tax Effect and Cross-Tax Effect

Pre-reform income controls	*Panel A. Labor income with cross-tax effect of capital income*		*Panel B. Negative capital income with cross-tax effect of labor income*	
	Own-tax elasticity (1)	Cross-tax elasticity (2)	Own-tax elasticity (3)	Cross-tax elasticity (4)
	All reforms (1984–2005)			
No pre-reform income controls	−0.123*** (0.004)	−0.104*** (0.003)	−0.054*** (0.007)	−0.012*** (0.003)
log base-year (period s) income	0.067*** (0.003)	−0.001 (0.003)	−0.112*** (0.006)	0.216*** (0.004)
Splines of log base-year (period s) income	0.053*** (0.004)	0.001 (0.003)	−0.129*** (0.006)	0.199*** (0.004)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.059*** (0.004)	−0.000 (0.003)	−0.126*** (0.005)	0.204*** (0.004)
Number of observations	26,394,236		26,394,236	
	1987 reform (1984–1990)			
No pre-reform income controls	−0.118*** (0.005)	−0.131*** (0.004)	−0.026*** (0.008)	0.024*** (0.005)
log base-year (period s) income	0.122*** (0.004)	−0.023*** (0.004)	−0.137*** (0.007)	0.223*** (0.005)
Splines of log base-year (period s) income	0.111*** (0.004)	−0.020*** (0.005)	−0.147*** (0.007)	0.203*** (0.006)
Splines of log $s-1$ income and log deviation between $s-1$ and s incomes	0.124*** (0.004)	−0.028*** (0.004)	−0.153*** (0.007)	0.217*** (0.006)
Number of observations	8,398,725		8,398,725	

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. The dependent variable is the three-year growth rate in real labor income (panel A) and in negative capital income in absolute value (panel B). The independent variables of interest are the three-year growth rates in the marginal net-of-tax rates on labor income (own-tax effect in panel A, cross-tax effect in panel B) and on capital income (cross-tax effect in panel A, own-tax effect in panel B). Both of these marginal net-of-tax rates are instrumented using three-year growth rates in simulated marginal net-of-tax rates under base-year behavior. All elasticities in the table are based on specifications without income effects. Regressions include taxpayers with positive labor income and negative capital income, and are otherwise based on the same sample restrictions and include the same controls as the specifications described in Tables 3, 6, and 7.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

sufficient by itself. This general insight is not just relevant to the Danish tax system with its multiple personal income tax bases, but applies to most, if not all, tax systems in the world as they always include more than one base (e.g., personal versus corporate income tax bases).

Our specifications have so far ignored potential cross-tax effects, although the taxable income and broad income elasticities estimated in the previous subsection at least did not have to rule them out. Here we take a first step toward a direct analysis of cross-tax effects between capital and labor income, exploiting the sharp differential tax variation across those income types created by the Danish reforms. We focus on the cross-tax effects between *negative* capital income and labor income both

because the tax variation between those two is less correlated than for positive capital income (see Figure 2) and because most of the population have negative capital income (giving us more power). Table 9 shows results of specifications with either labor income as the outcome (panel A) or negative capital income as the outcome (panel B), with each panel showing both the own-tax elasticity (labor income tax in panel A; capital income tax in panel B) and the cross-tax elasticity (capital income tax in panel A; labor income tax in panel B).

The following results emerge from the table. First, the own-tax elasticities estimated from specifications allowing for cross-tax effects are almost unaffected compared to the more parsimonious specifications considered earlier. This provides an additional robustness check on the previous results. Second, cross-elasticities of capital income with respect to the tax rate on labor income are much larger than the reverse cross-elasticity. This is not very surprising and is, at least in part, a mechanical implication of the fact that labor income is a much larger base than capital income. Third, since we consider positive income in panel A and negative income in panel B, the signs of the estimated cross-effects in the two panels are consistent and suggest that the two income forms are substitutes. The presence of substitutability is consistent with income shifting for tax avoidance purposes as discussed extensively in the previous literature (e.g., Saez, Slemrod, and Giertz 2012), but may also reflect real responses due to preferences.[21] Fourth, consistent with the estimation of own-tax elasticities, we find that estimated cross-tax elasticities are larger and more significant when using only the large 1987 reform.

V. Conclusion

This paper has estimated taxable income responses using a series of Danish tax reforms and full-population administrative data since 1980. Two key advantages allow us to overcome the identification problems that plague the previous taxable income literature: the Danish income distribution has been very stable over time (eliminating bias from nontax changes in inequality) and the Danish tax reforms create large and compelling variation that is not strongly correlated with income level (eliminating or alleviating bias from mean reversion). We have provided compelling graphical evidence of taxable income responses, arguably representing the first nonparametrically identified evidence of taxable income elasticities using tax reforms.[22] We have also presented panel regression evidence that is extremely robust to specification (such as pre-reform income controls), unlike previous results based on the United States and other countries that have been very fragile to specification.

Despite the clear advantages of the Danish setting, there is of course a concern about external validity of any single-country study and especially a small-country study. It would be interesting to know if the modest elasticity estimates for Denmark

[21] Previous evidence on income shifting includes Slemrod (1996) and Gordon and Slemrod (2000) on shifting between personal and corporate income in the United States; Pirttilä and Selin (2011) on shifting between labor and capital income in Finland; and Kleven and Waseem (2013) on shifting between wage income and self-employment income in Pakistan.

[22] There is also clear graphical evidence of taxable income responses based on bunching approaches, but those approaches are likely to be (more) affected by optimization frictions and therefore more difficult to interpret.

compared to the United States can be explained solely by better identification or if they are partly explained by a difference in the true elasticities due to different preferences, tax system, etc. This question is particularly important because taxable income elasticities are not structural parameters that depend only on individual preferences, but depend in important ways on the opportunities for tax avoidance and tax evasion that are a reflection of policy choices (Slemrod 1998; Slemrod and Kopczuk 2002). The fairly low taxable income elasticities that we find for Denmark, despite the presence of very high marginal tax rates, suggests that the Danish system offers small opportunities for avoidance and evasion. There are two main reasons for this. First, tax bases are very broad and offer limited opportunities for deductions and negative capital income to count against the income tax base. Second, as shown by Kleven et al. (2011), tax enforcement is very effective and overall tax compliance is high due to the widespread use of double-reporting by third parties such as employers and financial institutions. The overall conclusion that emerges from the two studies together is that a tax system with the broadest possible bases and extensive use of information reporting can impose high marginal tax rates with fairly modest behavioral responses.

Appendix

Table A1—Summary Statistics for the Estimation Sample (*1984–2005*)

	Full sample (1)	Wage earners (2)	Self-employed (3)
Demographics			
Age	40.5	40.0	48.3
Number of children (0–17 years)	0.7	0.7	0.7
Labor market experience (years)	13.2	13.6	7.2
Male (percent)	52.5	51.3	72.2
Married (percent)	55.1	54.1	71.4
Primary and secondary education (percent)	41.8	41.8	42.7
Vocational education (percent)	41.8	41.6	44.1
Tertiary education (percent)	16.4	16.6	13.2
Taxable income			
Labor income	247,935	249,328	226,275
Other personal income	3,204	737	41,554
Capital income	−27,585	−27,760	−24,853
Deductions	16,056	16,490	9,299
Fraction of taxpayers at kink points			
Top kink (percent)	2.4	2.0	8.4
Middle kink (percent)	2.4	2.4	1.7
Bottom kink (percent)	1.2	1.0	3.8
Number of observations	37,599,492	35,326,867	2,272,625

Notes: The table shows sample means. Monetary values are shown in real 2005 Danish kroner (DKK), where US$ 1 = 5.6 DKK as of August 2013. Taxpayers at kink points are defined as those who have an income within a range of 5,000 DKK of the top kink, 3,000 DKK of the middle kink, and 2,000 DKK of the bottom kink, respectively.

REFERENCES

Aarbu, Karl O., and Thor O. Thoresen. 2001. "Income Responses to Tax Changes—Evidence from the Norwegian Tax Reform." *National Tax Journal* 54 (2): 319–38.

Atkinson, Anthony B., Thomas Piketty, and Emmanuel Saez. 2011. "Top Incomes in the Long Run of History." *Journal of Economic Literature* 49 (1): 3–71.

Auten, Gerald, and Robert Carroll. 1999. "The Effect of Income Taxes on Household Behavior." *Review of Economics and Statistics* 81 (4): 681–93.

Blomquist, Sören, and Håkan Selin. 2010. "Hourly Wage Rate and Taxable Labor Responsiveness to Changes in Marginal Tax Rates." *Journal of Public Economics* 94 (11–12): 878–89.

Blundell, Richard W., and Thomas MaCurdy. 1999. "Labor Supply: A Review of Alternative Approaches." In *Handbook of Labor Economics*, Vol. 3A, edited by Orley Ashenfelter and David Card, 1559–1695. Amsterdam: Elsevier Science.

Brewer, Mike, Emmanuel Saez, and Andrew Shephard 2010. "Means Testing and Tax Rates on Earnings." In *Dimensions of Tax Design: the Mirrlees Review*, edited by James Mirrlees et al., 90–173. Oxford: Oxford University Press.

Chetty, Raj. 2012. "Bounds on Elasticities with Optimization Frictions: A Synthesis of Micro and Macro Evidence on Labor Supply." *Econometrica* 80 (3): 969–1018.

Chetty, Raj, John N. Friedman, Tore Olsen, and Luigi Pistaferri. 2011. "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records." *Quarterly Journal of Economics* 126 (2): 749–804.

Feldstein, Martin. 1995. "The Effect of Marginal Tax Rates on Taxable Income: A Panel Study of the 1986 Tax Reform Act." *Journal of Political Economy* 103 (3): 551–72.

Feldstein, Martin. 1999. "Tax Avoidance and the Deadweight Loss of the Income Tax." *Review of Economics and Statistics* 81 (4): 674–80.

Gelber, Alexander M. 2012. "Taxation and the Earnings of Husbands and Wives: Evidence from Sweden." *Review of Economics and Statistics* 96 (2): 287–305.

Giertz, Seth H. 2007. "The Elasticity of Taxable Income over the 1980s and 1990s." *National Tax Journal* 60 (4): 743–68.

Goolsbee, Austan. 2000. "What Happens When You Tax the Rich? Evidence from Executive Compensation." *Journal of Political Economy* 108 (2): 352–78.

Gordon, Roger H., and Joel Slemrod. 2000. "Are 'Real' Responses to Taxes Simply Income Shifting between Corporate and Personal Tax Bases?" In *Does Atlas Shrug? The Economic Consequences of Taxing the Rich*, edited by Joel B. Slemrod, 240–80. Cambridge: Harvard University Press.

Gruber, Jon, and Emmanuel Saez. 2002. "The Elasticity of Taxable Income: Evidence and Implications." *Journal of Public Economics* 84 (1): 1–32.

Hansson, Åsa. 2007. "Taxpayers' Responsiveness to Tax Rate Changes and Implications for the Cost of Taxation in Sweden." *International Tax and Public Finance* 14: 563–82.

Kleven, Henrik Jacobsen, Martin B. Knudsen, Claus Thustrup Kreiner, Søren Pedersen, and Emmanuel Saez. 2011. "Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark." *Econometrica* 79 (3): 651–92.

Kleven, Henrik Jacobsen, and Esben Anton Schultz. 2012. "Estimating Taxable Income Responses Using Danish Tax Reforms." London School of Economics (LSE) Working Paper PEP 13.

Kleven, Henrik Jacobsen, and Esben Anton Schultz. 2014. "Estimating Taxable Income Responses Using Danish Tax Reforms: Dataset." *American Economic Journal: Economic Policy*. http://dx.doi.org/10.1257/pol.6.4.271.

Kleven, Henrik Jacobsen, and Mazhar Waseem. 2013. "Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan." *Quarterly Journal of Economics* 128 (2): 669–723.

Kopczuk, Wojciech. 2005. "Tax Bases, Tax Rates, and the Elasticity of Reported Income." *Journal of Public Economics* 89 (11–12): 2093–2119.

Kopczuk, Wojciech. 2012. "The Polish Business 'Flat' Tax and Its Effect on Reported Incomes: A Pareto Improving Tax Reform?" http://www.columbia.edu/~wk2110/bin/PolishFlatTax.pdf.

Moffitt, Robert, and Mark O. Wilhelm. 2000. "Taxes and Labor Supply Decisions of the Affluent." In *Does Atlas Shrug? The Economic Consequences of Taxing the Rich*, edited by Joel B. Slemrod, 193–234. Russell Sage Foundation.

Pirttilä, Jukka, and Håkan Selin. 2011. "Income Shifting within a Dual Income Tax System: Evidence from the Finnish Tax Reform of 1993." *Scandinavian Journal of Economics* 113 (1): 120–44.

Saez, Emmanuel. 2004. "Reported Incomes and Marginal Tax Rates, 1960–2000: Evidence and Policy Implications." In *Tax Policy and the Economy*, Vol. 18, edited by James M. Poterba, 117–74. Cambridge: MIT Press.

Saez, Emmanuel. 2010. "Do Taxpayers Bunch at Kink Points?" *American Economic Journal: Economic Policy* 2 (3): 180–212.

Saez, Emmanuel, Joel Slemrod, and Seth H. Giertz. 2012. "The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review." *Journal of Economic Literature* 50 (1): 3–50.

Saez, Emmanuel, and Michael R. Veall. 2005. "The Evolution of High Incomes in Northern America: Lessons from Canadian Evidence." *American Economic Review* 95 (3): 831–49.

Sillamaa, Mary-Anne, and Michael R. Veall. 2001. "The Effect of Marginal Tax Rates on Taxable Income: A Panel Study of the 1988 Tax Flattening in Canada." *Journal of Public Economics* 80 (3): 341–56.

Slemrod, Joel. 1996. "High Income Families and the Tax Changes of the 1980s: The Anatomy of Behavioral Response." In *Empirical Foundations of Household Taxation*, edited by Martin Feldstein and James M. Poterba, 169–92. Chicago: University of Chicago Press.

Slemrod, Joel. 1998. "Methodological Issues in Measuring and Interpreting Taxable Income Elasticities." *National Tax Journal* 51 (4): 773–88.

Slemrod, Joel, and Wojciech Kopczuk. 2002. "The Optimal Elasticity of Taxable Income." *Journal of Public Economics* 84 (1): 91–112.