The B.E. Journal of Economic Analysis & Policy

Contributions

Volume 11, *Issue* 1 | 2011 | *Article* 71

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Recommended Citation

Bertil Holmlund and Martin Söderström (2011) "Estimating Dynamic Income Responses to Tax Reform," *The B.E. Journal of Economic Analysis & Policy*: Vol. 11: Iss. 1 (Contributions), Article 71.

Available at: http://www.bepress.com/bejeap/vol11/iss1/art71

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Estimating Dynamic Income Responses to Tax Reform*

Bertil Holmlund and Martin Söderström

Abstract

We study income responses to income tax changes by using a large panel of Swedish tax payers over the period 1991–2002. Changes in statutory tax rates as well as changes in tax bracket thresholds provide exogenous variations in tax rates that can be used to identify income responses. We estimate dynamic income models which allow us to distinguish between short-run and long-run effects in a straightforward fashion. For men, the estimates of the long-run elasticity of income with respect to the net-of-tax rate hover in a range between 0.10 and 0.30. The estimates for women are statistically insignificant. We simulate the fiscal consequences of a tax reform that reduces the top marginal tax rate by five percentage points. Such a reform may have negligible effects on tax revenues when the interactions between income taxes and other taxes are taken into account.

KEYWORDS: marginal tax rates, progressive taxes, earned income, tax reform

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We are grateful for valuable comments from the editor, two referees, Per-Anders Edin, Per Engström, Alexander Gelber, Anders Kristoffersson, Chuan-Zhong Li, Håkan Selin, seminar participants at IZA (Bonn) and participants at the Nordic Summer Institute in Empirical Labor Economics (Helsinki). Financial support from FAS (Swedish Council for Working Life and Social Research) is gratefully acknowledged.

1. Introduction

Research on the behavioral effects of labor taxation has largely focused on the impact on hours of work and labor force participation. More recently, a literature has emerged that focuses on the impact on taxable income and other measures of income. One reason for this new direction is that taxes may affect individual behavior along a number of margins in addition to the effects on hours of work and participation, such as work effort and job mobility. By examining the impact on income, one could arguably capture a wider set of responses than those studied in traditional labor supply studies. The new literature is mainly based on U.S. data and has so far not converged to a consensus view regarding the quantitative magnitudes of the behavioral responses.

The present paper highlights the distinction between short-run and long-run responses by estimating dynamic models using panel data. Previous research has in general been vague as to whether the estimated elasticities of interest have pertained to the short run or the long run. In fact, the modeling approaches commonly used have not been adequate for distinguishing between short-run and long-run responses. Our study provides the first comprehensive empirical analysis of the impact of Swedish tax changes subsequent to the major reforms in 1990–91. These changes have been targeted at high-income earners and involved tax hikes as well as tax cuts.

We make use of a large panel data set that comprises information for 12 years, namely 1991–2002. We identify the effects of income tax changes on income by exploiting two main sources of exogenous variations in the tax rules. First, changes in the statutory tax rates have occurred in 1995 as well as in 1999. Second, the “kinks” of the progressive tax schedule have been subject to changes which induce exogenous changes in marginal tax rates for a given level of income.[1]

The broad features of the Swedish tax system have remained largely intact since the early 1990s when the “tax reform of the century” was introduced. This major reform involved substantial simplifications and base broadening of the tax system. One key element was the introduction of a two-tiered marginal tax schedule for earned income. The idea was that most income earners should pay only the proportional municipality tax (hovering around 30 percent). For incomes above a certain threshold, a state tax kicked in at the marginal rate of 20 percent. The state tax was raised to 25 percent in 1995. From 1999 and onwards, the state tax has comprised two levels, viz. 20 and 25 percent.

[1] Consider a person in the top tax bracket who is very close to the bracket below the top. An increase in the kink point at the top may move the person to the bracket below the top, thus reducing the marginal tax rate.

A noteworthy element of the Swedish tax system is its treatment of the individual, rather than the family, as the basic tax unit. The tax schedule for earned income pertains to individual income and is independent of spousal income. Another notable element is the so-called dual income tax system, i.e., the fact that earned income and capital income are subject to different tax treatments: earned income is subject to progressive taxes whereas income from capital is taxed at the flat rate of 30 percent.

The conventional approach to the estimation of income responses to tax changes has involved regressing changes in log income on changes in the log of the net-of-tax rate, where the net-of-tax rate is defined as one minus the marginal tax rate. This regression framework in first differences is typically, implicitly or explicitly, derived from a relationship between the *level* of income and the *level* of the marginal tax rate. However, the "difference specification" in the regression models implicitly assumes immediate adjustment to changes in tax policy. If the adjustment is gradual rather than immediate, the model is misspecified.

Our approach also posits a relationship between income and tax rate levels but allows for lagged adjustment. This leads to an "error correction" specification which encompasses the conventional regression model as a special case and yields estimates of short-run as well as long-run responses. A dynamic specification can be rationalized from several considerations. First, there may be lags in the diffusion of information about changes in tax policy. Second, individual behavior may exhibit "habit persistence," an idea that has a long tradition in studies of consumer demand. The hypothesis is that the individual gauges his consumption (or labor supply) against some benchmark that in part depends on her own behavior in the past.[2] Third, there may be costs associated with changes in hours worked.[3] For example, a change in working time may require a switch to a new employer. Fourth, to the extent that tax changes affect human capital investment, the earnings response will not be immediate.[4]

For men, our estimates of the long-run income elasticity with respect to the net-of-tax rate range between 0.10 and 0.30. For women, the estimates are statistically insignificant. We simulate the implications for government revenues of a tax reform that reduces the top marginal tax rate by five percentage points. Such a reform may have negligible effects on tax revenues. An important reason for this outcome is the presence of interactions between income taxes and other taxes, such as payroll taxes. A cut in the top marginal tax rate brings about higher

[2] See Johnson and Pencavel (1984) for an empirical implementation of this idea in a study of labor supply.

[3] Formal models of dynamic labor supply with explicit costs of adjustment are rare in the literature. The paper by Nadiri and Roach (1974) is one of the exceptions.

[4] There is a somewhat related literature on the effects of transitory and permanent capital-gains tax changes; see Burman and Randolph (1994).

DOI: 10.2202/1935-1682.2891

incomes which in turn are associated with higher tax revenues from income as well as payroll taxes. Another reason is that the income distribution is thin at the top, implying that tax cuts are associated with modest direct revenue losses.

2. Previous Research

2.1 Theoretical Issues

The research on income responses to tax changes has several forerunners. One strand consists of the large literature on labor supply which focuses on how taxes affect hours of work and labor force participation. This literature builds on the canonical model of labor supply, where wage rates are taken as given and individual choices are dictated by preferences and budget restrictions (see Blundell and MaCurdy, 1999, for a survey of this research).[5] A smaller and more recent literature has examined how wage rates respond to changes in the tax system in imperfectly competitive labor markets. In this literature, hours of work are determined by the worker (as in the canonical model) or through some bargaining mechanism, whereas wages may be set by firms (as in efficiency wage models) or through individual or collective bargaining (as in search models or trade union models).

The focus on how taxes affect income is motivated by the view that taxes may have a multitude of effects on individual behavior in addition to the traditional labor supply responses. This view emphasizes that traditional estimates of the labor supply responsiveness to tax rates may substantially underestimate the overall effect, the reason being that individual adjustments along other margins are ignored. Taxes may affect work effort, the type of jobs that are accepted, and incentives for geographical and occupational mobility. This line of argument has been articulated by Martin Feldstein in a series of papers; see Feldstein (1995a, 1995b, 1999). A normative conclusion that follows from this perspective is that tax distortions are more serious, the higher the elasticity of taxable income is with respect to changes in tax rates. Indeed, Feldstein (1999) shows that the elasticity (ETI for short) can be used to compute the deadweight loss from income taxes.[6]

The literature on how wages are affected by the tax system in imperfect labor markets has paid particular attention to the role of higher tax progressivity, i.e., the impact of a rise in the marginal tax rate relative to the average tax rate. A common result in a variety of models is that higher tax progressivity leads to wage moderation, which in turn will bring about a fall in equilibrium unemployment. The intuition for this result is perhaps most easily seen in a

[5] Chetty (2009) examines the implications of optimization errors for behavioral responses.
[6] Slemrod and Kopczuk (2002) argue that ETI is not a structural parameter.

bargaining model, where a higher marginal tax rate raises the cost of wage increases and leads the parties in the bargain to opt for lower wages. Although the result is reasonably robust across a variety of models and specific assumptions, it is not completely robust.[7] So in the end and as usual, empirical work is needed to establish how taxes affect wages, hours and labor income. By and large, the empirical literature tends to confirm that increased tax progressivity is conducive to wage moderation, but there are also some conflicting results.[8]

The normative implications of estimated elasticities of income with respect to tax rates will generally depend on what is assumed about labor market imperfections. High elasticities reflect potentially large tax distortions from the perspective of the traditional model. In a bargaining model, by contrast, a rise in the marginal tax rate that leads to wage moderation and to a decline in overall income may conceivably be welfare improving via the associated increase in employment. Indeed, there is typically an optimal degree of tax progressivity in models of imperfect labor markets; see Holmlund and Kolm (1995) and Sorensen (1999) for elaborations of this idea.

The recent literature on income responses to tax rates has typically been framed within the context of the canonical model, where income responses are viewed as the outcome of individual labor supply choices (broadly interpreted) rather than individual or collective bargaining. However, in reality incomes are presumably influenced partly by individual choice and partly by bargaining mechanisms.

2.2 Empirical Research on Income Responses to Tax Changes

The empirical literature on the responses of taxable income to changes in tax rates was pioneered by Lindsey (1987) and Feldstein (1995a, 1995b). The key parameter of interest in their studies as well as in the subsequent literature was ETI, i.e., the elasticity of taxable income with respect to the net-of-tax rate, i.e., one minus the marginal tax rate. ETI was found to be remarkably high in these early studies which exploited data from U.S. tax reforms in the 1980s. Lindsey reported ETIs in a range between 1.6 and 1.8, whereas Feldstein's estimates ranged from 1.0 to 3.

The subsequent literature has addressed a number of pitfalls that plagued the seminal papers and also highlighted the sensitivity of the estimates to

[7] Contributions to the theoretical literature on taxes in imperfectly competitive labor markets include Hoel (1990), Koskela and Vilmunen (1996), Lockwood and Manning (1993), Pissarides (1998), Hansen (1999), Sorensen (1999), Fuest and Huber (2000) and Sandemann Rasmussen (2002). Bovenberg (2003) provides a comprehensive treatment.

[8] Contributions to the empirical literature include Lockwood and Manning (1993), Holmlund and Kolm (1995), Wulfsberg (1997) and Lockwood et al. (2000).

DOI: 10.2202/1935-1682.2891

alternative specifications and data. A crucial issue has been to separate the impact of tax policy from other factors that may have affected the evolution of incomes. In the U.S., cuts in marginal tax rates among high-income earners occurred concomitantly with rising income inequality, a development that easily can produce large ETI estimates even absent much impact from tax policy. The contributors following Lindsey's and Feldstein's papers have typically arrived at much smaller ETIs than their predecessors. There is, however, substantial variation in the reported estimates.

The post-Feldstein contributions to the U.S. literature include papers by Auten and Carroll (1999), Sammartino and Weiner (1997), Goolsbee (2000), Hall and Liebman (2000), Gruber and Saez (2002), Kopczuk (2005), Saez (2003), Moffitt and Wilhelm (2000), Heim (2009) and Giertz (2010). Giertz (2004) and Saez et al. (2009) provide surveys of this literature. These studies typically adopt instrumental variables estimation of "difference specifications," i.e., they regress changes in income on changes in the net-of-tax share while treating the tax variable as endogenous. A very brief summary of the results reads as follows: (i) ETI is typically positive, although not always so; (ii) ETI is typically well below unity, although not always so; (iii) the "preferred estimates" are typically located in a range between 0.2 and 0.5.

Three studies have examined the effects of the Swedish tax reform 1990-91, viz. Hansson (2007), Ljunge and Ragan (2005) and Selén (2002). Ljunge and Ragan are most explicit about theoretical interpretations and consider alternative behavioral assumptions, including a static model as well as versions of life-cycle models. Empirically, all studies boil down to conventional difference specifications and report estimates of ETI in a range between 0.2 and 0.4. Selén also briefly looks at the tax reforms during the 1990s and reports estimates of ETI close to zero. A study on Norwegian data by Aarbu and Thoresen (2001) present ETI estimates close to zero, hovering between -0.6 and 0.2.[9]

Despite a good deal of research, there is thus considerable dispersion of the estimates. The reasons for the differences are not well understood. The definition of income seems to matter, but it is not clear exactly how and why. The results are sensitive to sample selection rules, but exactly how remains unclear. And the strategies employed to control for exogenous income trends seem to affect the results. See Saez et al. (2009) for a comprehensive discussion of the issues involved.

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[9] Roine and Waldenström (2008) also examine income responsiveness to marginal tax rates using Swedish time series on marginal tax rates and income shares for top earners for the period 1943–1990. They find substantial responsiveness at the very top, but their aggregate approach makes direct comparisons with the micro studies problematic.

3. The Swedish Tax System

A major tax reform took place in Sweden in 1990 and 1991 (see Agell et al., 1996, for details). The reform involved broadening of the tax bases and cuts of the statutory marginal income tax rates. An important ingredient was the introduction of a dual income tax, where earned income and income from capital were taxed according to different schedules. The new proportional capital income tax, levied on dividends, interest income and capital gains, was set to 30 percent and has remained at this level. The tax on earned income consists of two parts, one “local” and one national. The local income tax – determined by the local and regional governments – is proportional to income (above a basic deduction) but varies across localities. In 1991, the national average of the local income tax rates stood at 31 percent. On top of the local income tax, a national (state) tax kicked in at a rate of 20 percent for earned taxable incomes above a level that corresponded to the 75th percentile of the distribution of taxable incomes. By 1991, the income tax schedule pertaining to earned income thus involved two brackets with (average) marginal rates of 31 and 51 percent.

Although the main features of the new tax system have remained intact over the following years, several changes have taken place. One important change took place in 1995 when the national tax rate was raised to 25 percent. This system was modified 1999 when two brackets – 20 and 25 percent – for the national tax were introduced. This basic structure remains in place since then. By 1999, the top rate kicked in at around the 95th percentile of the distribution of taxable incomes.

We use mainly two sources of exogenous variations in tax policies that should enable identification of behavioral responses. A first source is the changes in the statutory national tax rate: the increase from 20 to 25 percent in 1995 and the introduction in 1999 of a two-bracket system. The 1999 reform implied that some individuals actually experienced cuts in the marginal tax rate since the threshold for the 25 percent rate was increased. A second source is the changes in the income thresholds which determine when the national tax kicks in; tax payers close to the threshold may be exposed to changes in the marginal tax rate through these bracket adjustments. The local tax rates have changed very little over the studied period; the average rates have hovered between 30 and 31 percent. Actual individual local tax rates are affected by mobility decisions and should be treated as endogenous.

Taxable income is obtained after various deductions from gross income. First, there are work-related deductions that may be requested by the tax payers and will be scrutinized by the tax authorities. Expenses associated with travel to work are the typical examples; such expenses are deductible if they exceed a certain amount. To arrive at taxable income, two further deductions are

DOI: 10.2202/1935-1682.2891

undertaken without any involvement by the tax payer. There is a "basic deduction" (grundavdrag) and there are social security fees that have been partly or wholly deductible. The basic deduction varies with the level of assessed income[10] up to a ceiling and takes a hump-shaped pattern, whereby the effective marginal tax rate is reduced at low incomes and increased over a range of incomes higher up in the distribution. Employee social security fees became increasingly important during the 1990s and had reached 7 percent of assessed income by the end of the decade. These fees are proportional up to a ceiling beyond which the marginal rate is zero. They involve some actuarial elements, however, and should not be treated as analogous to ordinary taxes.

In addition to the abovementioned ingredients of the tax system, there are some progressive features in the systems for housing allowances and child care fees. We have no information at our disposal regarding how these characteristics vary across individuals in our data set.

4. The Data

The data come from a longitudinal data base, LINDA, which is a 3.35 percent random sample of the Swedish population. LINDA is based on a combination of several registers, for example income tax registers and population censuses.[11] From this data base, all individuals aged 20 to 59 are extracted for 12 consecutive years, viz. from 1991 to 2002. We focus on the years following the major tax reform of 1990–91, thus avoiding the complications associated with the changes in capital income taxation that were part of that reform. The unbalanced panel includes about 1.9 million observations whereas the balanced panel comprises 113 904 individuals observed during 12 years (in total 1 366 848 observations). The regression models will be estimated on the balanced panel.[12]

In addition to rich information on incomes, the data include information on human capital attributes as well as some demographic characteristics (such as age, gender, education and marital status). We have added information on local labor market conditions, measured as the ratio between the number of vacancies and the number of unemployed.[13] Labor market conditions matter for bargaining

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[10] Assessed income is gross income minus work-related deductions. More information on income definitions is given below.

[11] For a description of LINDA, see Edin and Fredriksson (2000).

[12] The sampling procedures do not depend on incomes or other endogenous variables. See Edin and Fredriksson (2000) for details.

[13] We are grateful to Kerstin Johansson for giving us access to these data. The local labor market is based on the commuting patterns of individuals in 284 municipalities of Sweden, resulting in 100 local labor markets. The tightness measure is defined as the number of vacant jobs divided by the number of job seekers registered at the public employment offices. The number of job seekers

outcomes and influence the risk of being quantity-constrained in labor supply decisions.

The fraction of tax payers affected by the national tax has hovered between 19 and 26 percent over the period 1991–2002. The top national rate of 25 percent has affected around 5 percent of the tax payers since its introduction in 1999. Although there have been no drastic changes in the statutory income tax rates, the changes that have occurred as well as "bracket switching" entail a non-trivial number of tax changes that can be taken as exogenous to individual behavior. To illustrate, consider a measure of the change in the individual marginal tax rate, $\Delta\hat{\tau}_{it}$, of the form

$$\Delta\hat{\tau}_{it} = \tau\left[Y_{it-1}(1+g);z_t\right] - \tau\left(Y_{it-1};z_{t-1}\right) \quad (1)$$

where subscript *i* refers to individual *i*, Y_{it-1} refers to taxable income as of year *t*-1, *g* is the general growth in incomes and z_t (as well as z_{t-1}) captures the tax code. The first term on the right-hand side is the predicted marginal tax rate pertaining to year *t* and the second term is the actual marginal tax rate in year *t*-1. Eq. (1) gives a measure of the change in the marginal tax rate that is independent of fluctuations in the individual income level. Over the period 1992–2002, there are around 74 000 observations where $|\Delta\hat{\tau}_{it}| \geq 0.05$ holds, i.e., where the marginal tax rate increases or decreases by at least 5 percentage points. All these changes take place in the upper half of the income distribution and three quarters of them occur in 1995 and 1999, i.e., the years when changes in the statutory tax rates kick in. Over 70 percent of these changes pertain to men.[14]

There are a number of income variables in the data. We focus on labor income and entrepreneurial income, with or without associated taxable transfer payments, and exclude income from capital and capital gains.[15] Most social transfers are taxed as labor income; this includes for example unemployment benefits and sickness benefits. We consider mainly three income concepts: *earned income* (*YE*), which includes labor income but excludes social transfers; *broad income* (*YB*), which includes taxable transfers in addition to earned income; and *assessed income* (*YA*), which equals broad income minus work-related

is the sum of the stock of openly unemployed and the stock of participants in active labor market programs.

[14] The local tax rate is set at the national average in these calculations (which pertain to the balanced panel). The measure is thus not affected by changes in tax rates that are due to residential mobility.

[15] Entrepreneurial income captures self-employment income associated with unincorporated businesses. Owners of incorporated businesses receive their compensation in the form of wages and salaries.

DOI: 10.2202/1935-1682.2891

deductions. We do not explicitly study movements in *taxable income* (*YT*), the reason being that taxable income is essentially a nonlinear function of the other income variables. As mentioned, taxable income equals assessed income minus deductions that are determined by the tax authorities.[16]

Table 1 shows some descriptive statistics concerning the various income variables. Earned income exhibits much more variation than the other income variables, a fact that reflects that transfers are excluded from earned income but included in the other income measures. Transfers produce a sizeable level difference between broad income and earned income; the average difference between the two measures amounts to almost 10 percent (not shown in the table). The correlation matrix reveals a high degree of correlation between changes in broad income and assessed income but more modest correlations between earned income and the other income variables.

The fraction of people with positive earned income has hovered between 85 and 87 percent over the period 1993–2002. The fraction fell sharply (from 91 to 86 percent) over the period 1991–1993, when a deep recession hit the Swedish economy. The fraction with positive broad income has been around 95 percent over the period 1993–2002. The fraction of entrepreneurial income in earned income has been 6 percent on average. Women account for 49 percent of the sample.

The income distribution has widened over the period. Table 2 shows the evolution of incomes by various percentiles. For earned income, the growth in real income has been 5 percent for the 10th percentile and 40 percent for the 99th percentile. Aside from these divergent trends at the bottom and the top, there are modest increases in income inequality in the major part of the distribution. These broad patterns hold for both men and women as well as for all four measures of income.

[16] In LINDA, these three measures correspond to (i) “primärinkomst” (CPRIM), (ii) “sammanräknad förvärvsinkomst” (CSFVI) and (iii) “taxerad förvärvsinkomst.” Taxable income corresponds to “beskattningsbar förvärvsinkomst” in LINDA.

Table 1. Income correlations and other statistics, 1991–2002.

	ln *YE*	ln *YB*	ln *YA*	ln *YT*
ln *YE*	1.000			
ln *YB*	0.727 [0.560]	1.000		
ln *YA*	0.725 [0.558]	0.998 [0.995]	1.000	
ln *YT*	0.711 [0.521]	0.980 [0.940]	0.982 [0.944]	1.000
Means	11.695 [0.048]	11.870 [0.065]	11.860 [0.065]	11.758 [0.067]
St dev	1.147 [0.821]	0.864 [0.525]	0.859 [0.523]	0.861 [0.512]
# observations	1 660 041	1 828 182	1 827 065	1 794 674

Notes: *YE*, *YB*, *YA* and *YT* stand for earned, broad, assessed and taxable income. The total number of observations is 1 913 038. The last row reports the number of observations with positive *YE*, *YB*, *YA* and *YT*, respectively. Numbers in brackets refer to first differences of log incomes. The age range is 20–59. The table is based on the unbalanced panel.

Table 2. Real income increases (%) by percentiles, 1991–2002.

	p10	**p25**	**p50**	**p75**	**p90**	**p95**	**p99**
Taxable income	3.4	35.3	35.3	30.7	35.2	35.3	44.9
Assessed income	5.9	27.7	29.2	30.7	33.7	35.3	44.9
Broad income	4.6	27.7	29.2	30.7	32.2	35.3	41.6
Earned income	4.6	23.3	32.2	32.2	32.2	35.3	40.0

Note: The table is based on the unbalanced panel and compares income levels by percentiles in 1991 and 2002.

DOI: 10.2202/1935-1682.2891

Previous research, largely based on U.S. data, has often tried to identify income responses to cuts in marginal tax rates by using data showing sharply widening income differentials. A difficulty that arises in this setting is to separate the effects of tax cuts from other determinants of increasing income dispersion. If the tax cuts are concentrated at the top, there is an obvious risk that the income responses will be overstated.[17] The risk of overestimating the income responses should be smaller in the Swedish setting. The main statutory tax change has involved a tax hike rather than a tax cut for top incomes, something that would tend to underestimate rather than overestimate the income responses absent suitable controls for rising income differentials. We will, however, also control for (smooth) trend changes in the income distribution.

5. Empirical Analysis

5.1 Preliminary Evidence

We first take a look at income changes among people "affected" and "not affected" by the 1995 tax hike. Two periods are considered, namely 1992–94 (the pre-reform period) and 1996–98 (the post-reform period). Those affected – the treatment group – are defined as those who pay state taxes in the pre-reform period. Those not affected – the control group – are those who don't pay state taxes in the pre-reform period. We use the balanced panel and compare the growth in mean income in the treatment group with growth in mean income in the control group. The results are displayed in Table 3.

The raw comparisons in column (1) reveal that income growth is substantially lower in the group affected by the 1995 tax hike. Looking separately at income outcomes 1996–97 and 1997–98, we find that the impact is bigger in the latter period, a pattern consistent with gradual adjustment.

[17] Mean reversion at the top of the income distribution may however work in the opposite direction. See Saez et al. (2009).

Table 3. Change in mean income for the treatment group relative to the control group, percent.

	All 1992–94 vs. 1996–98	**All** 1992–94 vs. 1996–97	**All** 1992–94 vs. 1997–98	**Age 35–49** 1992–94 vs. 1996–98	**Age 35–49** income restr. 1992–94 vs. 1996–98
	(1)	(2)	(3)	(4)	(5)
Assessed income	-11.1	-9.2	-12.9	-6.1	-0.8
Broad income	-11.3	-9.4	-13.1	-6.2	-1.1
Earned income	-18.0	-15.8	-20.2	-7.8	-1.9
# individuals	113 904	113 904	113 904	59 227	45 522

Note: The income restriction in column (5) pertains to the 1992–94 period and is 100 000 < Income (SEK) < 2 000 000. The average SEK/USD exchange rate in 1993 was 7.8.

There are several reasons why the numbers in the three first columns will overstate the causal effect of the tax hike. First, the treatment group consists of generally older people and it is a well established empirical fact that earnings growth declines over the life cycle. Indeed, the estimates in column (4) based on prime-aged individuals aged 35-49 are about 50 percent lower. Second, it is plausible that people who happen to have exceptionally low incomes in one year will catch up during subsequent years. Analogous arguments hold for people who happen to have very high incomes in a year. The implications of this "regression to the mean" argument are illuminated in the fifth column which excludes very low incomes as well as very high incomes. The differences in income growth between treatment and control groups have been substantially reduced in column (5). The regression models specified and estimated in what follows will control for age (along with other covariates) as well as the tendency of income to exhibit regression to the mean.

5.2 Regression Models

Lagged adjustment to tax changes can be rationalized by several arguments including habit persistence, adjustment costs and human capital accumulation. We make no attempt to identify the precise mechanisms; indeed this is difficult since different mechanisms may have observationally equivalent implications. Appendix A outlines a stylized model where market work is associated with learning by doing. An increase in work hours results in higher immediate income but also entitles to higher future income via accumulation of human capital.

DOI: 10.2202/1935-1682.2891

Market work and the existing stock of human capital are combined to invest in additional human capital. A tax reform affects earnings by changing hours of work as well as wages (via human capital) and there is gradual adjustment towards a new steady state.

We consider a relatively flexible specification of the dynamics so as to let the data speak. Although there is often a presumption that the response is stronger in the long run than in the short run, the specification is not restrictive in that respect. The response may be stronger in the short run than in the long run for a variety of reasons. For example, if a tax hike increases tax avoidance, this may well be a "one-time effect" rather than a gradual adjustment. Responses driven by adjustment costs or human capital accumulation may entail more gradualness (as in the model in the appendix).

The previous empirical literature has typically regressed changes in log income on changes in the log of the net-of-tax rate and a few other covariates. Our approach is a generalization of this benchmark formulation where the benchmark obtains as a special case. The basic model is written as an equation of the form

$$y_{it} = \alpha + \beta_1 n_{it} + \beta_2 n_{it-1} + X_{it}\gamma_1 + X_{it-1}\gamma_2 + \rho y_{it-1} + \theta_i + \varepsilon_{it} \tag{2}$$

where y_{it} is log income pertaining to individual i in year t and $n_{it} \equiv \ln(1-\tau_{it})$ is the log of the net-of-tax rate, where τ_{it} is the marginal tax rate. X_{it} is a vector of other regressors, θ_i is an individual-specific fixed effect and ε_{it} is a mean zero random error term.[18] The short-run effect of interest is captured by β_1 and the long-run effect is given by $(\beta_1+\beta_2)/(1-\rho)$, where the assumption is that $\rho \in (0,1)$ holds. The specification incorporates "mean reversion" in the sense that income changes in a period are related to the previous period's distance from long-run equilibrium. In other words, the model has an error correction specification which can be written as follows:

[18] The model given by eq. (2) is under some conditions isomorphic to a static model with AR(1) errors. Consider a static model with two explanatory variables of the form $y_{it} = \lambda_0 + \lambda_1 n_{it} + \lambda_2 x_{it} + \theta_i + \varepsilon_{it}$ where ε_{it} is an AR(1) process $\varepsilon_{it} = \kappa\varepsilon_{it-1} + u_{it}$ with $\kappa \in (-1,1)$. This model can be written as a restricted version of eq. (2):

$$y_{it} = \lambda_0(1-\kappa) + \lambda_1 n_{it} - \kappa\lambda_1 n_{it-1} + \lambda_2 x_{it} - \kappa\lambda_2 x_{it-1} + \kappa y_{it-1} + \theta_i(1-\kappa) + u_{it}$$

The two nonlinear restrictions are $\beta_2 = -\rho\beta_1$ and $\gamma_2 = -\rho\gamma_1$. With k explanatory variables there will be k restrictions. Remarks concerning these common factor restrictions will be offered below.

(3) $$\Delta y_{it} = \beta_1 \Delta n_{it} + \Delta X_{it}\gamma_1 + (\rho - 1)\left[y_{it-1} - \left(\frac{1}{1-\rho}\right)\left(\alpha + \overline{\beta} n_{it-1} + X_{it-1}\overline{\gamma} + \theta_i\right)\right] + \varepsilon_{it}$$

where $\Delta n_{it} = \ln(n_{it} / n_{it-1})$, $\overline{\beta} \equiv \beta_1 + \beta_2$ and $\overline{\gamma} \equiv \gamma_1 + \gamma_2$. The squared brackets include the error correction term which is zero in long-run equilibrium.

The individual-specific fixed effects can be removed by first differencing. We obtain

(4) $$\Delta y_{it} = \beta_1 \Delta n_{it} + \beta_2 \Delta n_{it-1} + \Delta X_{it}\gamma_1 + \Delta X_{it-1}\gamma_2 + \rho \Delta y_{it-1} + \Delta \varepsilon_{it}$$

which is equivalent to

(4′) $$\Delta y_{it} = \beta_1 \Delta^2 n_{it} + (\beta_1 + \beta_2)\Delta n_{it-1} + \Delta X_{it}\gamma_1 + \Delta X_{it-1}\gamma_2 + \rho \Delta y_{it-1} + \Delta \varepsilon_{it}$$

The remainder error term and the differenced lagged dependent variable are correlated (since the former contains ε_{it-1} and the latter y_{it-1}). Anderson and Hsiao (1981) suggested the use of either y_{it-2} or $\Delta y_{it-2} = y_{it-2} - y_{it-3}$ as instrument for Δy_{it-1}; such instruments are valid as long as ε_{it} is not serially correlated. The use of y_{it-2} as instrument is known as the "level" form of the Anderson and Hsiao estimator, whereas the use of Δy_{it-2} is referred to as the "difference" estimator. An advantage of the level estimator is that we can exploit data available from year t=3 and onwards (since y_{it-2} is available from t=3), whereas the difference estimator is not available until t=4 (since Δy_{it-2} is first available at year t=4). Arellano (1989) reports that the estimator that uses instruments in levels has much smaller variances than the difference estimator. Since the validity of y_{it-2} as instrument is conditional on absence of serial correlation in the idiosyncratic error term, ε_{it}, it becomes important to test for serial correlation. We use the Arellano-Bond tests of first- and second-order correlation in the differenced residuals (see Arellano and Bond, 1991). Absence

DOI: 10.2202/1935-1682.2891

of serial correlation in ε_{it} would imply negative first-order autocorrelation as well as zero second-order correlation in $\Delta\varepsilon_{it}$.

An endogeneity problem specific to the problem at hand concerns the net-of-tax rate. This variable is clearly endogenous when the tax system is progressive. In general, we can write the net-of-tax rate as a function $n_{it} = n(y_{it}; z_t)$, where z_t captures the tax code.[19] Analogously, $n_{it-1} = n(y_{it-1}; z_{t-1})$. It is obvious that n_{it} is endogenous because it depends on income in the current period. However, it is clear that n_{it-1} is also correlated with the error term in (4) since it contains lagged income. To deal with these issues we construct two instruments (in addition to y_{it-2}). These instruments are as follows:

$$n_{it}^{p} = n(y_{it-2}^{P,t}; z_t) \tag{5a}$$

$$n_{it-1}^{p} = n(y_{it-2}^{P,t-1}; z_{t-1}) \tag{5b}$$

where $y_{it-2}^{P,t}$ and $y_{it-2}^{P,t-1}$ are predicted taxable incomes pertaining to period *t* and *t*-1, respectively. These predictions are based on observed taxable incomes in year *t*-2. We inflate y_{it-2} by the growth of median income between year *t*-2 and *t* (as well as between *t*-2 and *t*-1).[20] The model is thus exactly identified.

Comparisons with Conventional Specifications

How is our approach related to the specifications used in previous work? A conventional specification, e.g., Auten and Caroll (1999) and Gruber and Saez (2002), involves regressing the change in income against the change in the net-of-tax rate and lagged *levels* of other covariates, i.e.,

$$\Delta y_{it} = \beta \Delta n_{it} + X_{it-1}\lambda + v_{it} \tag{6}$$

It is also commonly argued that it is important to control for first-period income and thus include the lagged income level as an additional regressor, i.e.,

[19] In the regressions, y_{it} stands for log income. With some abuse of notation, we let y_{it} represent the level of income when discussing instruments.
[20] The national averages of the 284 local tax rates are applied when constructing the instruments since the actual local tax rate is endogenous to mobility decisions. For 2000–2002 we add the national average of the "church fee" (kyrkoavgiften) to the national average of the local tax rate so as to get a consistently defined instrument over the whole period.

(7) $$\Delta y_{it} = \beta \Delta n_{it} + \delta y_{it-1} + X_{it-1}\lambda + v_{it}$$

The lagged income variable may appear in a linear fashion as in (7) and in Auten and Carroll (1999), or as a flexible spline function as in Gruber and Saez (2002). The inclusion of lagged income among the right-hand side variables is often motivated by mean reversion arguments, i.e., the need to control for the possibility that large positive (negative) shocks to income in a particular year are offset by slow (fast) income growth in subsequent years.[21] Another argument for the inclusion of lagged (or "initial") income is the desire to control for changes in the income distribution. The endogeneity of Δn_{it} is recognized and the typical procedure is to instrument by means of lagged income and the current tax system, i.e., an instrument of the form $n_{it}^{p} = n(y_{it-1}^{P,t}; z_t)$ is used.

There may be a couple of problems with this conventional approach. First, it fails to make a clear distinction between short-run and long-run effects. A common view seems to be that the parameter on Δn_{it} should capture long-run effects, at least if the time differencing encompasses several years. This interpretation is problematic if lagged income is included since lagged income may imply a feedback effect that should be accounted for when computing the long-run response. A second issue concerns the econometric methodology. The validity of y_{it-1} as an instrument requires that there is no first-order autocorrelation in v_{it}. This condition seems to have been largely ignored in the previous literature; in fact, we are unaware of any previous study in this area that presents autocorrelation tests.[22] For reasons discussed above, lagged income is plausibly correlated with the error term and thus needs to be instrumented.

To what extent can the conventional difference model be nested as a special case of our dynamic specification? Specification (7) is not nested within (4) since it includes y_{it-1} rather than Δy_{it-1}. Specification (6) can however be nested within (4) after suitable modifications. By imposing $\beta_2 = \rho = 0$ in (4), we obtain a specification that is identical to (6) except for the treatment of the non-tax covariates included in the *X*-vector. A slight generalization of the treatment of the *X*-variables makes it possible to get reasonably congruent specifications. Note that $\Psi \equiv \Delta X_{it}\gamma_1 + \Delta X_{it-1}\gamma_2$ in (4) obtains as a special case of

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[21] Blomquist and Selin (2010) motivate the inclusion of lagged income by arguing that it serves as a proxy for unobserved heterogeneity that interacts with time.

[22] Moffitt and Wilhelm (2000) do however discuss the likelihood of correlation between the error term and lagged income. Some of the previous studies have used two-wave panels which have precluded tests of autocorrelation of the differenced error term.

DOI: 10.2202/1935-1682.2891

$\Psi^* \equiv X_{it}\gamma_1 + X_{it-1}\lambda - X_{it-2}\gamma_2$ when $\lambda = \gamma_2 - \gamma_1$. The specification in (6) is then obtained if $\gamma_1 = \gamma_2 = 0$. In order to avoid possible endogeneity of some covariates, we exclude potentially endogenous *t*-dated covariates. In particular, variables representing education and marital status appear only as lagged levels, i.e., as X_{it-1} and X_{it-2}.

In summary, the conventional specification can be nested as a special case of our general dynamic model. The key restrictions are $\beta_2 = \rho = 0$. Moreover, it is important to test for serial correlation when estimating the conventional model as well as our general dynamic model.

5.3 Empirical Results

We focus on statutory marginal tax rates. These are the tax rates that apply to taxable income and consist of the local tax rate plus the national tax. A broader concept, which arguably may be thought of as representing the effective marginal tax rate, recognizes income-dependent basic deductions and social security fees. A well-informed agent, behaving according to the canonical labor supply model, would care about effective tax rates rather than the statutory ones. But the rules concerning deductions and fees are complex and it is not clear that they are well understood by the tax payers. The statutory rules, however, are simple since they involve only two or three brackets. It may well be the case that the statutory rules come closer than the effective rates to what agents perceive as relevant for their decisions. Ultimately, this is an empirical issue. The major changes in the tax system over the studied period are associated with changes in statutory rates and bracket adjustments. By Occam's razor we opt for the simpler alternative and focus on results based on statutory tax rates.

The tax reforms we study have been relevant for individuals well above median earnings. Recall that only 20–25 percent of the tax payers have been affected by the national tax and only around 5 percent by the top bracket in place since 1999. There is no obvious criterion, however, for choosing the appropriate limit for sample inclusion. Although the changes in the marginal tax rates have been most pronounced among people with relatively high incomes, an exclusive focus on these groups would weaken identification. And inclusion of all income levels would imply an implausible degree of homogeneity in behavior across the distribution.[23] We have settled for a benchmark rule that includes individuals with above-median income in the beginning of the studied period. A cutoff rule that depends on income in the first available year (1991) minimizes the risk of

[23] There is some evidence from U.S. studies that high-income earners exhibit stronger responsiveness to tax changes; see Gruber and Saez (2002).

choosing a sample based on endogenous outcomes. However, income is volatile and sample inclusion based on only one year's income will miss individuals who have experienced temporary income cuts in that year. We have therefore computed average taxable income for each individual over the years 1991–94 and include in our sample only those with above-median average incomes. Recall that there is no major tax change before 1995. The robustness checks that we have undertaken, and which will be presented below, do not indicate that the results are very sensitive to the choice of cutoff point.

Our data comprise 12 years, i.e., 1991 through 2002. We employ the Anderson-Hsiao level estimator in order to maximize the number of time periods and thus end up with 10 years covering the period 1993–2002. The results are set out in Table 4. The estimates correspond to eq. (2), estimated as first differences as given by eq. $(4')$. In addition to the tax variables, we include as covariates age squared, local labor market tightness (vacancies/unemployment), and dummies for marital status, education and year.[24] We also include controls for smooth changes in the income distribution that are unrelated to tax policy by interacting log average 1991–92 income with a linear trend.

The instruments are strong as judged by very high *F*-values for inclusion of the instruments in the first stage; see Appendix B. The IV estimates are very different from the OLS results (not reported): OLS yields negative and large coefficients on the net-of-tax rates. These differences are as should be expected given a progressive tax system where the net-of-tax rate is decreasing in income. The lagged dependent variables enter with coefficients in the range between 0.25 and 0.35, significantly different from zero as well as significantly different from unity.

For men, the point estimates of the long-run elasticities vary – depending on the definition of income – in a range between 0.1 and 0.3 and are significantly different from zero.[25] The short-run elasticities are located in a range between 0.2 and 0.3 and are also significantly different from zero. The tests for autocorrelation reveal negative first-order but no second-order autocorrelation in the differenced residuals. These patterns are consistent with zero autocorrelation in the residuals in levels. For women, we are unable to detect any significant short-run or long-run effects.

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[24] The linear age effect is not identified. Education levels (three dummies) as well as marital status are included as lagged levels (one and two years). The data contain information on the number of children in the household but the definitions are not entirely consistent over the years. We have therefore excluded this variable from the regression models but the results are almost identical if the variable is included.

[25] The results are broadly robust to more flexible trend specifications. If log average 1991–92 income is interacted with squared and cubic trends – in addition to linear trends – the estimated long-run elasticities are all significant for men and insignificant for women. The point estimates for men are 0.085, 0.122 and 0.252, respectively.

DOI: 10.2202/1935-1682.2891

As noted above (footnote 18), the estimated models can be given a serial-correlation interpretation provided that some restrictions are satisfied. A key restriction of interest is $\beta_2 = -\rho\beta_1$. This restriction is rejected in four of the six cases given in Table 4; the restriction is not rejected in the regressions given in columns (3) and (6).[26]

Robustness Checks

How sensitive are the results to the chosen cutoff rules for sample inclusion? We consider two alternative cutoff rules based on average taxable income over the period 1991–94. The lower cutoff corresponds to 90 percent of benchmark taxable income level whereas the higher cutoff kicks in at 110 percent of the benchmark. The results for men are shown in Table 5 and do not reveal much sensitivity to the chosen cutoff rules. The results for women are also broadly in line with those displayed in Table 4.

[26] Our estimated models do not perfectly nest the serial correlation interpretation as special cases which make it problematic to test restrictions pertaining to all explanatory variables. As noted above, we exclude *t*-dated education variables. We have considered specifications which do allow us to examine restrictions on the *X*-variables. In particular, we have included the education variables as differences and lagged differences, i.e., Δx_{it} and Δx_{it-1}, noting that the serial correlation interpretation implies that the estimated coefficients pertaining to Δx_{it} and Δx_{it-1} should have opposite signs (as long as there is positive autocorrelation). In all cases corresponding to Table 4, the coefficients have the same signs, thus being inconsistent with a serial correlation interpretation. Formal testing of the restrictions pertaining to Δx_{it} and Δx_{it-1} resoundingly rejects them.

Table 4. Estimation results by gender.

	Men			**Women**		
	(1)	(2)	(3)	(4)	(5)	(6)
	Assessed income	Broad income	Earned income	Assessed income	Broad income	Earned income
$\Delta^2 n_{it}$	0.218 (3.46)	0.257 (4.00)	0.270 (2.22)	-0.086 (0.88)	-0.103 (1.08)	-0.275 (1.07)
Δn_{it-1}	0.079 (2.23)	0.106 (2.96)	0.201 (2.98)	-0.011 (0.21)	-0.021 (0.41)	-0.098 (0.75)
Δy_{it-1}	0.263 (16.74)	0.270 (16.50)	0.304 (26.54)	0.354 (15.53)	0.359 (15.37)	0.271 (22.44)
Long-run elasticity	0.107 (2.22)	0.146 (2.94)	0.289 (2.96)	-0.017 (0.21)	-0.032 (0.41)	-0.134 (0.75)
AR(1) AR(2)	-27.97 0.97	-25.73 0.62	-32.82 -1.59	-19.94 -1.58	-19.16 -1.32	-34.31 -2.08
# obs.	381 823	381 949	363 308	183 543	183 592	174 520

Notes: The model is estimated in first differences (Anderson-Hsiao using lagged levels as instruments). The *t*-statistics in parentheses are robust to heteroskedasticity and arbitrary intra-individual correlation. Other variables are three education categories, marital status, local labor market tightness and year dummies. Lagged income and the two tax variables are treated as endogenous and instrumented as described in the text. Controls for trends in the income distribution are included by means of interactions between income 1991–92 and trend as described in the text. The Arellano-Bond statistics, AR(1) and AR(2), are asymptotically N(0,1) and test for first- and second-order serial correlation in the first-differenced residuals. The sample inclusion rule is based on average taxable income during 1991–94; observations above the median are included.

DOI: 10.2202/1935-1682.2891

Table 5. Estimation results for alternative cutoff rules, men.

	Income 1991–94 at least 90 % of the benchmark			Income 1991–94 at least 110 % of the benchmark		
	(1)	(2)	(3)	(4)	(5)	(6)
	Assessed income	Broad income	Earned income	Assessed income	Broad income	Earned income
$\Delta^2 n_{it}$	0.223 (3.47)	0.250 (3.84)	0.263 (2.12)	0.202 (3.32)	0.241 (3.87)	0.298 (2.52)
Δn_{it-1}	0.086 (2.38)	0.112 (3.08)	0.208 (3.04)	0.061 (1.79)	0.090 (2.59)	0.194 (2.96)
Δy_{it-1}	0.269 (19.02)	0.281 (18.59)	0.300 (29.29)	0.250 (14.16)	0.260 (13.90)	0.297 (22.55)
Long-run elasticity	0.118 (2.36)	0.156 (3.06)	0.297 (3.02)	0.081 (1.78)	0.121 (2.57)	0.276 (2.94)
AR(1) AR(2)	-30.01 1.41	-27.79 0.87	-35.75 -0.92	-26.56 0.93	-23.93 0.62	-29.12 -0.76
# obs.	417 931	418 087	393 724	335 760	335 871	322 223

Note: See notes to Table 4.

We have undertaken a number of other sensitivity tests. As shown in Table 6, the results are reasonably robust to the inclusion or exclusion of various non-tax covariates. Exclusion of the trends leads to higher estimated short-run elasticities but has little impact on the estimates of the long-run elasticities. The estimates are not much affected by exclusion of other variables, such as education, marital status and local labor market conditions. The estimated long-run elasticities tend to be somewhat smaller when we restrict the sample to the prime-aged groups and also when entrepreneurial income is excluded.

Table 6. Further robustness checks, elasticity estimates for men.

	Assessed income			Broad income			Earned income		
	Short-run elasticity	Long-run elasticity	AR(2)	Short-run elasticity	Long-run elasticity	AR(2)	Short-run elasticity	Long-run elasticity	AR(2)
Benchmark specification	0.218 (3.46)	0.107 (2.22)	0.97	0.257 (4.00)	0.146 (2.94)	0.62	0.270 (2.22)	0.289 (2.96)	-1.59
No trend	0.350 (4.88)	0.119 (2.12)	2.83	0.388 (5.29)	0.160 (2.77)	2.39	0.390 (3.05)	0.300 (2.92)	-0.73
Few covariates	0.173 (2.85)	0.113 (2.39)	0.62	0.215 (3.46)	0.151 (3.12)	0.29	0.164 (1.39)	0.294 (3.09)	-1.86
29<age<50	0.170 (2.50)	0.085 (1.59)	0.97	0.206 (3.01)	0.118 (2.17)	0.92	0.274 (2.07)	0.274 (2.46)	-0.74
No entrepreneurial income	0.154 (2.69)	0.068 (1.62)	-0.30	0.202 (3.39)	0.108 (2.46)	-0.57	0.233 (1.96)	0.269 (2.93)	-2.37

Notes: Few covariates mean inclusion of the tax variables, the trend, age squared and the year dummies. See also notes to Table 4.

DOI: 10.2202/1935-1682.2891

Comparisons with Conventional Specifications

We now turn to a comparison with results that are based on the traditional difference specification as given by eqs. (6) and (7) above. To implement this specification, we follow the common practice of instrumenting the net-of-tax rate in year *t* by means of income in year *t*-1 – inflated to year *t* – and the tax code as of year *t*, i.e., $n_{it}^{p} = n(y_{it-1}^{P,t}; z_t)$, where $y_{it-1}^{P,t}$ is predicted income for year *t*. The relevant instrument in the difference specification thus becomes $\Delta n_{it}^{P} = n(y_{it-1}^{P,t}; z_t) - n(y_{it-1}; z_{t-1})$. The use of y_{it-1} as an instrument is valid as long as there is no first-order autocorrelation in v_{it}. We include the same controls for income trends as in the previous regressions.

The results are displayed in Table 7. The three first columns correspond to eq. (6), whereas the remaining columns include lagged income and thus correspond to model (7). For men, the estimated coefficients on the net-of-tax variables range between 0.06 and 0.11 in the first three columns. This is clearly lower than the corresponding long-run elasticities estimated in the dynamic model; cf. Table 3. Note however that there is severe first-order autocorrelation in the residuals, a fact that casts doubts on the validity of this model. For women, the estimated parameters are tiny.

The estimates of the net-of-tax effects for men are markedly reduced when lagged income is included among the regressors. The coefficients on the lagged income variables are negative, which is the common finding in these kinds of specifications. For women, the positive point estimates turn negative. Again, there is evidence of severe first-order autocorrelation in the residuals.

As noted, the dynamic specification nests the conventional formulation (for one year differences) as a special case; the relevant restrictions on eq. (4) are $\beta_2 = \rho = 0$. The restriction $\rho = 0$ is always rejected as is clear from Table 3. The restriction $\beta_2 = 0$, which implies equal coefficients on $\Delta^2 n_{it}$ and Δn_{it-1}, is rejected for male assessed and broad income but not for male earned income and female incomes.

Table 7. Estimation results by gender, conventional difference specifications.

	(1)	(2)	(3)	(4)	(5)	(6)
	Assessed income	Broad income	Earned income	Assessed income	Broad income	Earned income
Men						
Δn_{it}	0.058 (6.92)	0.060 (7.09)	0.105 (6.93)	-0.003 (0.33)	-0.001 (0.07)	0.0003 (0.02)
y_{it-1}				-0.255 (43.30)	-0.252 (39.21)	-0.306 (59.86)
AR(1)	-24.13	-21.16	-30.29	-15.60	-14.30	-12.78
AR(2)	-9.90	-9.64	-16.21	7.89	7.19	8.54
# obs.	382 344	382 453	366 236	382 344	382 453	366 236
Women						
Δn_{it}	0.036 (2.93)	0.034 (2.94)	0.016 (0.59)	-0.022 (1.74)	-0.021 (1.79)	-0.101 (3.75)
y_{it-1}				-0.245 (29.66)	-0.241 (26.58)	-0.344 (53.08)
AR(1)	-16.42	-16.20	-30.00	-9.93	-7.85	-12.00
AR(2)	-10.15	-10.05	-13.97	2.84	2.85	8.78
# obs.	183 672	183 717	175 756	183 672	183 717	175 756

Notes: Other variables are those included in previous regressions (except Δn_{it-1} and Δy_{it-1}).

5.4 Fiscal Consequences of a Tax Cut

A frequently advocated Swedish tax reform would entail abolishment of the top bracket in the state income tax system.[27] Such a reform, involving a 5 percentage point cut in marginal income tax rates for top-income earners, would restore the Swedish tax system to the basic principles set out in the major tax reform of the early 1990s.

What are the fiscal consequences of such a reform?[28] Consider a stylized representation of the tax system pertaining to high incomes, i.e., incomes above

[27] Two of the three major Swedish union federations, organizing white collar workers, have repeatedly argued in favor of such a reform.

[28] See Giertz (2009) for an analysis of tax revenue implications of U.S. tax reforms.

DOI: 10.2202/1935-1682.2891

the threshold where the top rate kicks in. Let τ represent the statutory marginal income tax rate, YA_i assessed income for individual *i*, *D* deductions and Y^* the threshold above which the top rate applies. Taxable income is given as $YT_i = YA_i - D$, where deductions are constant for top-income earners. Tax revenues for individual *i* in the top bracket can thus be written as:

(8) $$R_i = \tau\left(YA_i - D - Y^*\right)$$

Differentiation with respect to the marginal tax rate yields:

(9) $$\frac{dR_i}{d\tau} = \left(YA_i - D - Y^*\right) + \tau\frac{dYA_i}{d\tau}$$

which can be written as

(10) $$\frac{dR_i}{d\tau} = \left(YA_i - D - Y^*\right) - \eta\left(\frac{\tau}{1-\tau}\right)YA_i$$

where $\eta \equiv d\ln YA_i / d\ln(1-\tau)$ is the elasticity of assessed income with respect to the net-of-tax rate. The expression in the first parenthesis captures the direct (or mechanical) effect of a tax change, whereas the remaining term represents the indirect effect associated with the income response. Note that the direct effect is negligible for incomes close to the threshold so $dR_i / d\tau < 0$ holds for incomes in the neighborhood of the threshold; the thinner the income distribution at the top, the smaller the direct effect.

Expression (10) captures only the impact on tax revenues that operates through the income tax system whereas revenue effects via payroll and value added taxes are ignored. By slightly modifying (8) so as to incorporate proportional payroll taxes levied on employers, τ^e, we obtain an expression that captures the total change in labor taxes associated with a change in the top marginal income tax rate:

(11) $$\frac{dR_i}{d\tau} = \left(YA_i - D - Y^*\right) - \eta\left(\frac{\tau + \tau^e}{1-\tau}\right)YA_i$$

The total effect on tax revenues is thus obtained as:

(12) $$\frac{dR}{d\tau} = \sum_i \left[\left(YA_i - D - Y^* \right) - \eta \left(\frac{\tau + \tau^e}{1-\tau} \right) YA_i \right]$$

As noted by Saez (2004), one can also derive the impact on tax revenues in a more direct fashion if the top tail of the income distribution is Pareto distributed. In that case, the ratio $a \equiv YA / \left(YA - D - Y^* \right)$ is constant and equal to the Pareto parameter; *YA* is mean income in the top bracket. The ratio between actual and notional tax revenues can be written as

(13) $$\frac{dR}{dM} = 1 - \eta \left(\frac{\tau + \tau^e}{1-\tau} \right) a$$

where *dM* is the notional (or mechanical) effect.

Table 8 shows the results of experiments where the top tax bracket is abolished in 2002, the last year in our data. We set $\tau = 0.525$ and $\tau^e = 0.33$ (or $\tau^e = 0$) in these experiments.[29] Three alternatives for η are considered, viz. 0.10, 0.20 and 0.30. The mechanical fall in tax revenues is more than offset by the increase in tax revenues associated with the positive income response for $\eta = 0.20$ or higher and with account taken of revenues from the payroll tax. It should be noted that these calculations provide lower bound estimates since we have ignored increased revenues from value added taxes. The results are very similar if eq. (13) is applied to compute *dR/dM*, using the estimated value of the Pareto parameter in the top bracket, *a*=3.15. The top tail of the Swedish income distribution is thin; a fact that implies that tax cuts for top income earners may be fiscally inexpensive.[30]

Finally, we note by inspection of (13) that the tax cut has no effect on tax revenues provided that the following equality holds:

[29] This experiment involves calculation of

$$\Delta R = \Sigma_i \left[YA_i - D - Y^* - \eta \left((\tau + \tau^e)/(1-\tau) \right) YA_i \right] \times (-0.05).$$

The summation pertains to all income earners above the relevant income threshold. The numbers are scaled to the macro level by dividing by the sample probability in LINDA, i.e., 0.0335. The calculations ignore the fact that τ varies across individuals (because of variations in municipality tax rates), but this has negligible effects on the results. The proportional statutory payroll tax rate was 32.8 percent. We place no restrictions on age in the calculations.

[30] Saez et al. (2009) report that the Pareto parameter is 1.6 in the top one percent of the U.S. income distribution.

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(14) $$\eta = \left(\frac{1-\tau}{\tau + \tau^e} \right) \frac{1}{a}$$

For a=3.15, $\tau = 0.525$ and $\tau^e = 0$, it follows that $\eta > 0.29$ must hold if the tax reform should be revenue neutral. For $\tau^e = 0.33$, it is sufficient that $\eta > 0.16$ holds.

The uncertainty regarding the female elasticities cautions against strong conclusions about the fiscal outcomes in the Swedish case. Note also that the estimated elasticities may not hold for other tax reforms, such as cuts in marginal tax rates at lower income levels which may have non-negligible effects on average tax rates for high income earners.

Table 8. Fiscal implications of elimination of the top tax bracket ($\Delta\tau = -0.05$) in 2002. Millions SEK.

	$\eta = 0.10$		$\eta = 0.20$		$\eta = 0.30$	
	(*i*)	(*ii*)	(*i*)	(*ii*)	(*i*)	(*ii*)
Mechanical effect: ΔM	-2 910	-2 910	-2 910	-2 910	-2 910	-2 910
Indirect effect (income response)	1 100	1 790	2 200	3 580	3 300	5 370
Net revenue effect: ΔR	-1 810	-1 120	-710	670	390	2 460
$\Delta R / \Delta M$	0.62	0.38	0.24	-0.23	-0.15	-0.84

Notes: (*i*) ignores the payroll tax effect whereas (*ii*) incorporates the payroll tax effect. No age restrictions are imposed. The SEK/USD exchange rate in 2007 is around 7 SEK per USD.

6. Concluding Remarks

We have proposed a new approach to the estimation of income responses to changes in marginal tax rates. The conventional approach, where changes in income are regressed on changes in tax rates, can only capture long-run responses under restrictive and implausible conditions. Our more general approach, which involves application of a standard lagged adjustment framework, nests the conventional specification as a special case and delivers estimates of both short-run and long-run effects.

Our estimates of long-run responses among males are much larger than the estimates we obtain by employing the conventional difference specifications. At the same time, these long-run estimates are considerably smaller than some of the previous estimates in the literature. However, our simulations of a tax reform that abolishes the top marginal tax rate suggest that even modest elasticities are sufficient to make such a reform fiscally inexpensive when we account for induced increases in revenues from payroll taxes and value added taxes.

The somewhat puzzling results for women may be related to the fact that we have ignored the household context, i.e., the possibility that labor supply decisions in a two-earner family are jointly determined by the spouses. It should be a prioritized research agenda to incorporate a family perspective in this area.[31] It would also be desirable to account for income effects, although data on exogenous changes in wealth are generally hard to come by.

Finally, there is a need for better understanding of how and why the estimates differ across alternative definitions of income. Much has been said in favor of the view that one should focus on taxable income since this is what matters for the effects on the government's tax revenues. However, it may well be the case that separate structural approaches to the modeling of the components of taxable income will prove to be the route forward.

[31] See Gelber (2008) for a recent study along these lines (using the standard difference specification).

DOI: 10.2202/1935-1682.2891

Appendix A. Labor supply with learning by doing

Consider an infinitely-lived individual with an instantaneous utility function of the quasi-linear form $u = c - h^{1+\gamma}/(1+\gamma)$, where c is consumption, h is work hours and $\gamma > 0$. Labor earnings is determined by wage rates, *w*, and work hours, i.e., *Y=wh*. Wage rates are proportional to the stock of human capital, *w=RK*. Human capital is produced by use of work hours and human capital:

(A1) $$dK/dt = \alpha_0 K^{\alpha_1} h^{\alpha_2} - \delta K, \qquad \alpha_1 \in (0,1),\ \alpha_2 \in (0,1)$$

where $\delta > 0$ captures depreciation of human capital. Financial assets, denoted *A*, evolves according to $dA/dt = rA + RKh(1-\tau) - c$, where *r* is the rate of interest and τ is the tax rate. The individual maximizes the discounted present value of utilities. With quasi-linear utility, this involves maximization of the present value of income. The Hamiltonian for this problem is

$$H = e^{-rt}\left[RKh(1-\tau) - \frac{h^{1+\gamma}}{1+\gamma} + \lambda\left(\alpha_0 K^{\alpha_1} h^{\alpha_2} - \delta K\right)\right]$$

where λ is the multiplier associated with the human capital constraint. The necessary conditions for maximum include:

(A2) $$-h^{\gamma} + RK(1-\tau) + \lambda\alpha_0\alpha_2 K^{\alpha_1} h^{\alpha_2 - 1} = 0$$

(A3) $$d\lambda/dt = \lambda(r+\delta) - Rh(1-\tau) - \lambda\alpha_0\alpha_1 K^{\alpha_1 - 1} h^{\alpha_2}$$

The dynamics of the system can be illustrated in the *h*,*K*-space (Figure A1). Let $G(K,h)$ denote the *dK/dt*=0 line and note that it is positively sloped. Consider also the *dh/dt*=0 line which is obtained by differentiating (A2) with respect to time and using (A1), (A2) and (A3) to eliminate *dK/dt*, $d\lambda/dt$ and λ. We obtain

(A4) $$dh/dt = N(K,h)/D(K,h)$$

where

$$N(K,h) \equiv \alpha_0 R(1-\tau)K^{\alpha_1}h^{\alpha_2}(1-\alpha_2) + \left(h^{\gamma} - R(1-\tau)K\right)\left(\delta + r - \delta\alpha_1\right) - \delta R(1-\tau)K$$

and $D(K,h) \equiv \gamma h^{\gamma-1} + (1-\alpha_2)\left(h^{\gamma} - R(1-\tau)K\right)h^{-1} > 0$. The *dh/dt*=0 line, denoted *H*(*K,h*), is positively sloped since $N_K < 0$ and $N_h > 0$. Figure A1 illustrates the phase diagram in the *h*,*K*-space under the assumption that the slope of $G(K,h)$ is greater than the slope of $H(K,h)$, an assumption that holds for sufficiently low values of α_2. The optimal trajectory – the bold-faced arrows – is a saddle path and involves gradual adjustment towards the steady state where $G(K,h)$ and $H(K,h)$ intersects. If the slope of $G(K,h)$ is smaller than the slope of $H(K,h)$, the outcome would be an unstable focus. A rise in the net-of-tax rate shifts the $H(K,h)$ line to the left. There is a positive steady state impact on hours of work as well as on wage rates (via the accumulation of human capital). It is also straightforward to establish that the short-run impact of a tax cut will always entail an immediate increase in work hours irrespective of the level of human capital. The long-run impact on earnings is bigger than the short-run impact.[32]

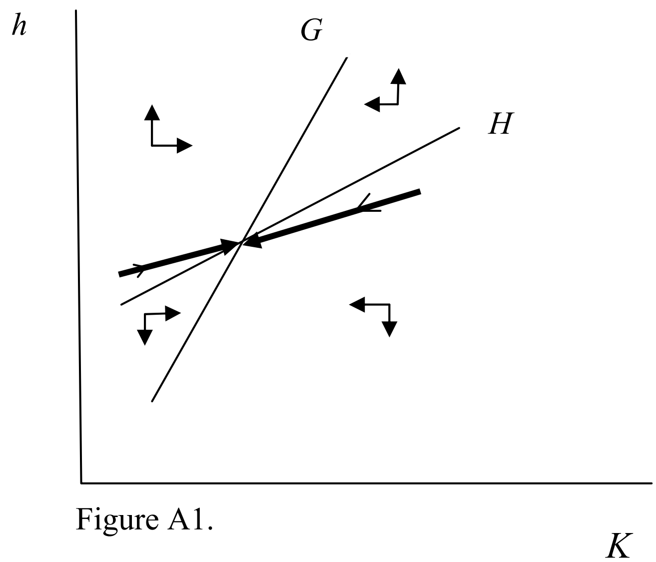

Figure A1.

Appendix B. First-stage results

The two endogenous right-hand tax variables $\Delta^2 n_{it}$ and Δn_{it-1} are instrumented (along with Δy_{it-1}) by y_{it-2} as well as by n_{it}^p and n_{it-1}^p; see eqs. (5a) and (5b) in the main text. Tables B1 and B2 show some first-stage estimates. We also show estimates where the *levels* of the tax variables, n_{it} and n_{it-1} are regressed on the levels of the instruments

[32] To verify this claim, consider how the slope of the optimal path changes as a response to an increase in the net-of-tax rate. If the instantaneous impact is unambiguously positive, it must be the case the new path is always above the old one. The slope of the new path must be steeper than the slope of the old one in a hypothetical point of intersection (for $dK/dt>0$). Differentiation of the slope of the optimal path reveals that a tax cut reduces the slope for $dK/dt>0$ and increases the slope for $dK/dt<0$. Thus no intersection between new and old paths occurs.

DOI: 10.2202/1935-1682.2891

Table B1. First-stage results, men.

	Assessed income				Broad income				Earned income			
	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}
n_{it}^p	079 (11.3)	.022 (5.3)	.336 (81.9)	.235 (63.8)	.079 (11.2)	.022 (5.3)	.336 (81.8)	.235 (63.8)	.072 (10.1)	.031 (7.2)	.354 (86.7)	.252 (68.9)
n_{it-1}^p	.053 (7.4)	-.238 (53.6)	.215 (55.9)	.410 (107.9)	.054 (7.4)	-.239 (53.6)	.225 (55.9)	.410 (107.1)	.050 (6.8)	-.235 (52.8)	.237 (59.0)	.421 (111.8)
y_{it-2}	.007 (5.9)	-.007 (7.2)	-.036 (26.9)	-.037 (31.9)	.008 (5.9)	-.007 (7.6)	-.037 (27.0)	-.037 (31.4)	.008 (12.3)	-.008 (17.1)	-.019 (28.4)	-.019 (35.4)
F-value	886	3 955	20 855	36 741	879	3 881	20 069	35 618	799	3 874	21 978	40 008
# obs.	381 823	381 823	381 823	381 823	381 949	381 949	381 949	381 949	363 308	363 308	363 308	363 308

Notes: Other included covariates are those listed in notes to Table 4. The F-values are F tests for exclusion of the instruments in the first-stage equations. Absolute *t*-values in parentheses.

Table B2. First-stage results, women.

	Assessed income				**Broad income**				**Earned income**			
	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}	$\Delta^2 n_{it}$	Δn_{it-1}	n_{it}	n_{it-1}
n_{it}^p	062 (5.6)	.029 (4.4)	.322 (50.5)	.230 (39.3)	.062 (5.7)	.027 (4.2)	.320 (50.4)	.229 (39.3)	.048 (4.3)	.050 (7.4)	.370 (59.3)	.272 (47.2)
n_{it-1}^p	.062 (5.3)	-.246 (34.5)	.200 (32.2)	.384 (63.2)	.062 (5.3)	-.247 (34.6)	.199 (32.0)	.384 (63.1)	.051 (4.4)	-.231 (32.8)	.238 (39.1)	.419 (70.1)
y_{it-2}	.005 (3.0)	-.008 (6.7)	-.049 (26.9)	-.037 (27.0)	.006 (3.2)	-.009 (7.3)	-.053 (24.4)	-.050 (26.7)	.004 (6.2)	-.004 (7.9)	-.012 (19.0)	-.013 (24.1)
F-value	349	1 658	7 659	13 818	347	1 664	7 623	13 673	285	1 578	10 610	19 816
# obs.	183 543	183 543	183 543	183 543	183 592	183 592	183 592	183 592	174 520	174 520	174 520	174 520

Notes: Other included covariates are those listed in notes to Table 4. The F-values are F tests for exclusion of the instruments in the first-stage equations. Absolute *t*-values in parentheses.

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