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IZA DP No. 15502

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I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

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ISSN: 2365-9793

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ABSTRACT

Income Taxes, Gross Hourly Wages, and the Anatomy of Behavioral Responses: Evidence from a Danish Tax Reform*

This paper provides quasi-experimental evidence on the effects of income taxes on gross hourly wages by utilizing administrative data and a tax reform in Denmark. The reform introduced joint taxation to a middle tax bracket, bringing large changes to the tax system facing married couples. Using variation in spousal income for identification, we present non-parametric graphical evidence based on a difference-in-differences design among working married males. First, we find hetero- geneous effects across income levels. For low-income workers, taxes have negative and dynamic effects on wages. Their elasticity of wages (with respect to net-of-marginal-tax rates) is close to one. For higher-income workers, the effects are small and static, with an elasticity of approximately 0.2. Second, wages respond to taxes through human capital accumulation and job changes. Finally, with smaller magnitudes than wages, daily hours worked also respond negatively to taxes, which contrasts with the prediction from a standard labor supply-and-demand model.

JEL Classification: H22, H24, J22, J24, J30, J62

Keywords: income taxation, administrative data, tax reforms, difference-in-differences, gross hourly wages, labor supply, human capital accumulation, job changes

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* We would like to thank Mike Brewer, Pierre Cahuc, Manasi Deshpande, Peter Fredriksson, Chishio Furukawa, Nikolaj A. Harmon, Bo Honoré, Niels Johannesen, Daiji Kawaguchi, Francis Kramarz, Claus T. Kreiner, Jakob R. Munch, Erik Plug, Fabien Postel-Vinay, Andrew Shephard, Sebastian Siegloch, Oskar N. Skans, Linh T. Tô, Shintaro Yamaguchi, and seminar/conference participants at Aarhus University, CREST, EALE, IIPF, Osaka University, RIETI, Royal Holloway, and ZEW for their comments. Financial support from Aarhus University, RIETI, and Royal Holloway is gratefully acknowledged. Kazuhiko Sumiya would also like to thank AUFF and, in particular, the Associate Professor Starting Grant awarded to Rune Vejlin.

1 Introduction

Economists have long studied the distortionary and distributional effects of income taxes on labor market outcomes. Empirical literature in the field has mainly focused on labor supply responses, especially by females along the extensive margin (Kleven, 2021). However, when it comes to gross hourly wages (i.e., the other component of earnings), the effects of taxes are scarcely investigated and theoretically ambiguous. Public economists predict that taxes have positive effects on wages because higher tax rates shift a labor supply curve leftward, inducing higher equilibrium wages in a standard labor supply-and-demand model (Fullerton and Metcalf, 2002). By contrast, labor economists predict negative and dynamic effects because taxes depress wage growth by disincentivizing, e.g., human capital accumulation and job changes, the two main drivers of wage growth (Rubinstein and Weiss, 2006). Therefore, to better understand policy implications, it is crucial to empirically identify both the effects of taxes on wages and the channels through which wages respond to taxes.

Given these motivations, this paper asks the following research questions: Do income taxes have positive or negative effects on gross hourly wages? Are the effects static or dynamic? Through what channels do wages respond to taxes? How do wage responses compare to labor supply responses?

To answer these questions compellingly, we provide quasi-experimental evidence by utilizing detailed Danish administrative data and a large tax reform that came into effect in 1987. First, the dataset is a population-wide annual panel that contains a wide range of information on individual income and worker/job characteristics. We select a sample of married males who are strongly attached to the labor market. Next, we use the 1987 tax reform for an empirical strategy because it introduced joint taxation to a middle tax bracket, bringing large changes to the tax system facing married couples. Thanks to this unique institutional change, we can use variation in spousal income as a source of identification and find two similar groups located in a bottom tax bracket before the reform. The two groups are similar in pre-reform covariates and differ exclusively in spousal income. After the reform, one group with higher spousal income is pushed upward to the middle bracket, whereas the other group with lower spousal income stays in the bottom bracket. We compare the outcome dynamics of these two groups in a difference-in-differences (DID) design. Although Gruber et al. (2021) and Kleven and Schultz (2014) also use this reform as a natural experiment, we use it in a novel way focusing on the introduction of joint taxation.

We present clear non-parametric graphical evidence and regression results regarding the effects of taxes on various outcomes; e.g., see Figure 2. Table 4 summarizes the main elasticities (with respect to net-of-marginal-tax rates) estimated in this paper. Our findings are as follows.

First, taxes have heterogeneous effects on wages across income levels. Low-income workers respond to taxes negatively and dynamically. Higher marginal tax rates gradually depress wage growth over time; that is, distortion dynamically accumulates on wages, which we refer to as accumulating effects in this paper. Their elasticity of wages with respect to net-of-marginal-tax rates is close to one. We obtain a relatively large elasticity because our DID design uncovers the accumulating effects rather than short-run effects attenuated by optimization frictions: workers will change behavior sluggishly in response to taxes and gradually overcome optimization frictions, which leads to the accumulating effects with a large elasticity. By contrast, for medium- and high-income workers, the effects of taxes on wages are small and static, with an elasticity of approximately 0.2. Given the large elasticity and novel accumulating effects, we move on to the details of low-income workers.

Next, we find that wages respond to taxes through human capital accumulation and job changes. First, workers facing higher tax rates are more likely to be categorized as unskilled (as opposed to skilled) in occupation ranks. Second, taxes have negative effects on the cumulative number of job-to-job transitions over time. Therefore, higher marginal tax rates arguably disincentivize human capital accumulation and job changes, two dynamic channels that can explain the negative and accumulating effects of taxes on wages. To the best of our knowledge, this paper provides the first quasi-experimental evidence on these channels.

Next, we compare wage responses to labor supply responses measured by daily hours worked. We highlight two findings. First, wage responses are larger than labor supply responses in magnitude. Second, especially for part-time workers, both wages and hours respond negatively to taxes. This finding contrasts with the prediction from a standard labor supply-and-demand model because it predicts negative labor supply responses and positive wage responses.

Finally, the estimation results survive all threats to identification and robustness checks. First, since spousal income works as an instrumental variable in our DID design, we verify its exclusion restriction, which requires that spousal income affects outcome dynamics only through the treatment (i.e., tax rates). Specifically, in addition to confirming parallel pre-reform outcome dynamics, we conduct a placebo test and show parallel pre- and post-reform outcome dynamics between two placebo groups that differ exclusively in spousal income but face almost the same tax rates (i.e., the absence of the treatment). Second, although workers can become non-employed with missing wages, we show that the compositional changes of employed workers do not create the spurious negative effects of taxes on wages. Third, we show that individuals do not bunch at a tax bracket cutoff; otherwise, bunching might bias estimates. Fourth, the estimation results are robust to modest changes to the definition of low-income workers.

This paper broadly contributes to the literature on public and labor economics. First, we offer novel implications regarding the effects of taxes on various labor market outcomes. Although Blomquist and Selin (2010) and Martínez et al. (2021) also provide credible quasi-experimental evidence on negative wage responses, we found substantial heterogeneous responses across income levels. Our finding on large wage responses by low-income workers contrasts with existing findings on large taxable income responses by high-income earners (Saez et al., 2012), which has implications for progressive tax policies, such as the Earned Income Tax Credit in the US. Furthermore, we found that wage responses occur through human capital accumulation and job changes, rather than through the interaction between labor supply and labor demand. Martínez et al. (2021) also do not find the labor demand channel, whereas Azmat (2019), Leigh (2010), and Rothstein (2010) provide recent evidence on tax incidence from tax credits in the UK and US; thus, we add new evidence to these mixed results.

Next, this paper fills the gap between the micro and macro literature by estimating the accumulating effects. Broadly speaking, to study the effects of income taxes on labor market outcomes (e.g., wages), the micro literature estimates short-run elasticities by using quasi-experimental methods, mostly DID designs (Saez et al., 2012). By contrast, the macro literature estimates long-run (steady-state) elasticities by calibrating dynamic models such as learning-by-doing models (Keane and Rogerson, 2012, 2015), Ben-Porath models (Heckman et al., 1998, 1999), and job search models (Kreiner et al., 2015; Shephard, 2017). Often, the micro literature reports smaller elasticities than the macro literature. We shed new light on the discrepancy between these short- and long-run elasticities as follows: by estimating the accumulating effects over the post-reform period of seven years in the credible DID design, we overcome the issue of optimization frictions, which attenuate short-run elasticities estimated in the micro DID literature, and thus uncover a structural elasticity relevant for long-run welfare, which is close to long-run elasticities estimated in the macro calibration literature (Chetty, 2012; Chetty et al., 2011; Kleven and Waseem, 2013). Therefore, the accumulating effects bridge short- and long-run elasticities.

Finally, this paper provides an empirical basis for welfare analysis and optimal taxation. Our findings motivate recent research on optimal policies with human capital accumulation (Stantcheva, 2020) or job search (Chetty, 2008; Kroft et al., 2020). Moreover, given that we found larger wage responses than labor supply responses, it will also be fruitful to further extend the canonical labor supply model (Piketty and Saez, 2013) by considering wage responses through, e.g., wage bargaining (Piketty et al., 2014) or employer learning (Craig, 2021).

The paper proceeds as follows: Section 2 describes the Danish income tax system and the 1987 tax reform. Section 3 describes our empirical strategy. Section 4 describes the Danish administrative data. Section 5 presents the estimation results. Section 6 presents the conclusion. All tables and figures are placed after the text.

2 The Danish income tax system and the 1987 tax reform

This section describes the Danish income tax system and the 1987 tax reform in terms of our empirical strategy. The key features of the tax system are that it has a progressive structure with three tax brackets and is based on individual tax filing for married couples. The reform then introduced joint taxation to a middle tax bracket, bringing large changes to the tax system facing married couples.

2.1 Income concepts

In Denmark, income taxes are levied at the source with individual tax filing and based mainly on three income concepts: labor income (LI), capital income (CI), and itemized deductions (D). For ease of exposition, our description omits a small number of other income concepts of minor importance for our sample, such as stock income. However, we stress that we take full account of all the income concepts when simulating tax liabilities for the empirical analysis in Section 3. The three income concepts are presented in Table 1. Labor income is the main source of income for most individuals in our sample. Furthermore, their capital income is negative because of interest payments on debt, mostly made up of mortgage loans.[1]

2.2 The pre-reform tax system

The left panel of Table 2 shows the key features of the Danish income tax system in the final pre-reform year, 1986. Danish income taxes are divided into regional and national taxes. Regional taxes were paid on taxable income LI + CI – D exceeding the cutoff of DKK 20,700 (DKK 1 in 1986 $\approx$ USD 0.3 in 2016). The regional tax rate is a flat rate that varies slightly by municipality and county: in 1986, the average regional tax rate was 28.0 percent, and the 90–10 percentile range was approximately four percentage points.[2]

National taxes have a progressive structure with three tax brackets (bottom, middle, and top brackets). The tax base in each bracket was also LI + CI – D but taxed with different cutoffs at different rates. These national taxes are cumulative, making the tax system progressive: taxable income LI + CI – D exceeding the bottom bracket cutoff of DKK 23,200 was taxed at the bottom tax rate of 19.9 percent, LI

[1] Our description of the institutional settings is based on Kleven and Schultz (2014), the Danish Ministry of Finance (`https://fm.dk`), and the Danish Ministry of Taxation (`https://www.skm.dk`).

[2] For our sample period, 1983–1993, the administrative structure of Denmark involved 275 municipalities ("kommuner") and 14 counties ("amter", a county that spans a set of municipalities). Both municipalities and counties levy income taxes on the residents. The regional tax rate is the sum of the municipality, county, and Church tax rates. The Church taxes are minuscule and paid only by members of the Church of Denmark ("Folkekirken").

+ CI – D exceeding DKK 113,400 was taxed at the middle tax rate of 14.4 percent, and LI + CI – D exceeding DKK 186,100 was taxed at the top tax rate of 10.8 percent. Under the average regional tax rate of 28.0 percent, the marginal tax rate in 1986 was thus 47.9 percent in the bottom bracket, 62.3 percent in the middle bracket, and 73.1 percent in the top bracket (but adjusted downward to a marginal tax ceiling of 73.0 percent in place).

Crucially for our empirical strategy, taxation is based on individual tax filing for married couples. They file taxes individually and separately under equal tax treatment of spouses. Before the reform, even if married individuals were not liable for, e.g., the middle taxes and had unused allowances (the bracket cutoff DKK 113,400 minus their taxable income LI + CI – D), their unused allowances could not be transferred to spouses. The left panel of Table 2 emphasizes this point by "No" in the "Joint" column.

2.3 The 1987 tax reform

Before laying out its details, we emphasize that the 1987 reform is close to an ideal natural experiment for two reasons. First, it brought large changes to the tax system, especially for married couples. Second, at the same time, the tax system was stable for several years before and after the reform; therefore, the reform created not gradual changes phased in over an extended period but large one-shot changes in 1987. This feature of the reform allows us to identify the dynamic effects of taxes and verify the absence of differential pre-reform trends between treated and control individuals in a DID design. The reform was legislated on March 18, 1986, and came into immediate and full effect on January 1, 1987. We describe the background of the reform in Appendix A.

The right panel of Table 2 shows the key features of the Danish income tax system in 1987. There were only minor changes in regional taxes. In national taxes, the tax base in the middle bracket was changed to labor income plus positive capital income (LI + [CI>0]), and the tax base in the top bracket was changed to labor income plus capital income exceeding DKK 60,000 (LI + [CI>60k]). The three bracket cutoffs were all increased by more than a statutory inflation adjustment of 2.0 percent. Because of these changes to the tax bases and bracket cutoffs, some individuals were mechanically pushed to other brackets. Next, the bottom and top tax rates were increased, whereas the middle tax rate was decreased; as a result, under the average regional tax rate of 29.0 percent, the marginal tax rate in 1987 was 51.0 percent in the bottom bracket, 57.0 percent in the middle bracket, and 69.0 percent in the top bracket. Finally, the reform introduced joint taxation to the middle bracket: if married individuals are not liable for the middle taxes and have unused allowances (the bracket cutoff DKK 130,000 minus their taxable income LI + [CI>0]), their unused allowances can be transferred to spouses. This institutional change is crucial for our empirical strategy and thus clarified in the next paragraph.

To elaborate on the joint taxation, let us consider the following example: suppose, in 1987, a husband (our sample) has taxable income for the middle bracket equal to DKK 150,000 (i.e., LI + [CI>0] = 150,000), and his wife has taxable income equal to DKK 100,000 (i.e., LI^{w} + [CI^{w} >0] = 100,000 with the superscript w denoting "wife"). If the joint taxation is not in place (like the pre-reform tax system), he is liable for the middle taxes given the bracket cutoff of DKK 130,000, but she is not. Note that she has unused allowances equal to 130,000 – 100,000 = 30,000. Under the post-reform tax system, her unused allowances can be transferred to him; therefore, his taxable income is calculated as 150,000 – 30,000 = 120,000. Then, he is no longer liable for the middle taxes. Note that despite this transfer scheme, married couples continue to file taxes individually and separately; admittedly, using the words "joint taxation" might be a slight abuse of terminology.

We use this institutional change as a novel natural experiment. Specifically, we show in the next section that we can find two groups of individuals who had similar income (LI, CI, D) and thus were in the bottom bracket before the reform. Then, one group with higher wives' labor income (LI^{w}) is mechanically pushed upward to the middle bracket by the reform, whereas the other group with lower wives' labor income (LI^{w}) stays in the bottom bracket. These bracket movements provide the DID design of this paper.

3 Empirical strategy

This section explains how we select a sample, define treated and control individuals in our DID design, and address endogeneity caused by the correlation between treatment assignment and pre-reform income. Our empirical strategy uses the joint taxation introduced to the middle bracket, together with cross-sectional variation in wives' labor income (LI^{w}) as a source of identification.

3.1 Sample selection

We focus on the period 1983–1993 because the tax system was stable during this period, except for the 1987 reform. The year 1986 is the baseline pre-reform year of this paper. Although we describe our data in Section 4, here, it suffices to say that we have available annual panel data on individual income (LI, CI, D, LI^{w}, CI^{w}, D^{w}), demographic characteristics, and outcomes (e.g., wages) for our sample period 1983–1993.

We select prime-age working males whose wives had positive labor income in 1986. Specifically, we select males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. These two restrictions select males strongly attached to the labor market, facilitating the identification of the effects of taxes on wages among employed workers. We further impose the following two restrictions on the sample: (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. We impose the third restriction to exploit the joint taxation introduced by the reform. In addition, as shown in the following subsections, our empirical strategy also exploits cross-sectional variation in wives' labor income; thus, we impose the fourth restriction to make treated and control individuals similar by excluding non-working wives.

Hence, our sample consists of males who satisfy the four restrictions relating to the pre-reform years 1983–1986 and are followed until 1993 with small sample attrition due to, e.g., death or emigration (approximately two percent attrition in 1993). Based on this sample, we define treated and control individuals below.

3.2 Treated and control individuals

Since our DID design is based on movements between the bottom and middle brackets, we first identify which tax brackets individuals are located in using a tax simulator. The simulator was originally developed by Kleven and Schultz (2014) and Bagger et al. (2018), and encodes the details of the Danish income tax system in place each year. The simulator pins down a bracket location for each individual by taking as the main input his and spousal income (LI, CI, D, LI^{w}, CI^{w}, D^{w}) (see Appendix B for an overview of the simulator). Let $\mathrm{B}^{86}(z_{i86})$ denote that individual i with 1986 income $z_{i86} = \{\mathrm{LI}_{i86}, \mathrm{CI}_{i86}, \mathrm{D}_{i86}, \mathrm{LI}^{\mathrm{w}}_{i86}, \mathrm{CI}^{\mathrm{w}}_{i86}, \mathrm{D}^{\mathrm{w}}_{i86}\}$ is in the bottom bracket under the 1986 tax system. To be more precise, $\mathrm{B}^{86}(z_{i86})$ means that he is liable for the bottom taxes but neither the middle taxes nor the top taxes. $\mathrm{B}^{84}(z_{i84})$ and $\mathrm{B}^{85}(z_{i85})$ are analogously defined for 1984 and 1985, respectively.

To define treated and control individuals based on bracket movements, we next consider the following counterfactual bracket location: let $\widetilde{\mathrm{B}}^{87}(z_{i86})$ denote that individual i with 1986 income z_{i86} (rather than 1987 income z_{i87}) is in the bottom bracket if hypothetically facing the inflation-adjusted 1987 tax system. We adjust all monetary values regarding the 1987 tax system (e.g., bracket cutoffs) to the 1986 price level. Analogously, let $\widetilde{\mathrm{M}}^{87}(z_{i86})$ denote that individual i with z_{i86} is in the middle bracket under the inflation-adjusted 1987 tax system. To be more precise, $\widetilde{\mathrm{M}}^{87}(z_{i86})$ means that he is liable for both the bottom and middle taxes but not the top taxes. We identify individuals in $\widetilde{\mathrm{B}}^{87}(z_{i86})$ or $\widetilde{\mathrm{M}}^{87}(z_{i86})$ using the tax simulator.

By combining the actual and counterfactual bracket locations, we define treated and control individuals as follows:

$$\begin{aligned} \text{Treated: } & \mathrm{B}^{84}(z_{i84})\ \mathrm{B}^{85}(z_{i85})\ \mathrm{B}^{86}(z_{i86})\ \widetilde{\mathrm{M}}^{87}(z_{i86}) \\ \text{Control: } & \mathrm{B}^{84}(z_{i84})\ \mathrm{B}^{85}(z_{i85})\ \mathrm{B}^{86}(z_{i86})\ \widetilde{\mathrm{B}}^{87}(z_{i86}). \end{aligned} \tag{1}$$

The treated individuals were in the bottom bracket before the 1987 reform but are in the middle bracket under the 1987 tax system. $\widetilde{\mathrm{M}}^{87}(z_{i86})$ is independent of behavioral responses to the reform because their income is fixed at the pre-reform 1986 level; therefore, their movement from the bottom bracket to the middle bracket is mechanically created by the reform. By contrast, the control individuals stay in the bottom bracket in the absence of behavioral responses to the reform, i.e., $\widetilde{\mathrm{B}}^{87}(z_{i86})$. Hence, the treatment is being mechanically pushed upward to the middle bracket, as Saez (2003) similarly exploits bracket creep caused by high inflation. Requiring that these individuals were in the same tax bracket for several years before the reform is standard in the literature, e.g., Gruber et al. (2021) and Jakobsen et al. (2020), and makes it likely that they knew their bracket location before the reform.[3]

3.3 Endogeneity caused by pre-reform income

The treatment assignment determined by (1) correlates with pre-reform income z_{i86}, which will cause endogeneity: if the treated individuals had significantly higher z_{i86} than the control individuals, their outcomes (e.g., wages) would evolve differently even without any treatment. One thus needs to control for z_{i86}; however, controlling for every component of $z_{i86} = \{\mathrm{LI}_{i86}, \mathrm{CI}_{i86}, \mathrm{D}_{i86}, \mathrm{LI}^{\mathrm{w}}_{i86}, \mathrm{CI}^{\mathrm{w}}_{i86}, \mathrm{D}^{\mathrm{w}}_{i86}\}$ loses variation for identification, as recognized in the literature on the elasticity of taxable income (Saez et al., 2012). We show below that controlling for own labor income LI_{i86} non-parametrically also balances own and spousal capital income and deductions $(\mathrm{CI}_{i86}, \mathrm{D}_{i86}, \mathrm{CI}^{\mathrm{w}}_{i86}, \mathrm{D}^{\mathrm{w}}_{i86})$; thus, our empirical strategy uses variation in wives' labor income $\mathrm{LI}^{\mathrm{w}}_{i86}$ as a source of identification.

Figure 1 plots the kernel density estimates of pre-reform labor income LI_{i86} by treatment status. The two distributions are similar and sufficiently overlap with each other. The large overlap is thanks to the joint taxation and variation in wives' income. We elaborate on this point in Appendix C by showing that among single males, the distributions of LI_{i86} do not sufficiently overlap between treated and control individuals. Returning to married males (our sample) in Figure 1, we define three income groups: low-income $(100{,}000 \le \mathrm{LI}_{i86} < 150{,}000)$, medium-income $(150{,}000 \le \mathrm{LI}_{i86} < 200{,}000)$, and high-income $(200{,}000 \le \mathrm{LI}_{i86} < 250{,}000)$ groups. We analyze each group separately for two reasons.

[3] One could consider another bracket movement that exploits the joint taxation introduced to the middle bracket: treated individuals are pushed downward from the middle bracket to the bottom bracket, whereas control individuals stay in the middle bracket. In this case, however, we found that these individuals were not similar in pre-reform covariates. By contrast, we show in the next subsection that the treated and control individuals defined by (1) are similar, which makes our DID design credible.

First, the two distributions are parallel within each domain of LI_{i86}, which non-parametrically controls for not only the mean but also the distribution of LI_{i86}. Second, we expect that the effects of taxes will be heterogeneous across income levels; indeed, we find larger and more dynamic wage responses by the low-income group.

Next, we check whether the treated and control individuals are similar. Table 3 lists the mean values of covariates in 1986 by treatment status for each income group. The three groups show the same patterns. First, by construction, own labor income LI_{i86} is balanced between the treated and control individuals. Once LI_{i86} is controlled for, the following covariates are also balanced: worker/job characteristics, own and spousal capital income (CI_{i86}, $\mathrm{CI}^{\mathrm{w}}_{i86}$), and own and spousal deductions (D_{i86}, $\mathrm{D}^{\mathrm{w}}_{i86}$). These balanced covariates make our estimation results credible. Therefore, the main difference between the treated and control individuals lies in wives' labor income $\mathrm{LI}^{\mathrm{w}}_{i86}$; the treated individuals have higher $\mathrm{LI}^{\mathrm{w}}_{i86}$ and thus are pushed upward to the middle bracket. Although using variation in spousal income as a source of identification is not new in the literature, e.g., Eissa (1995, 1996), we leverage it with the joint taxation introduced by the reform.

Finally, we clarify our identification assumption. The DID design assumes that, in the absence of the treatment, the treated individuals have an average outcome evolving in parallel with that of the control individuals (Lechner, 2010). Since these individuals differ exclusively in wives' labor income $\mathrm{LI}^{\mathrm{w}}_{i86}$, this assumption is equivalent to stating that $\mathrm{LI}^{\mathrm{w}}_{i86}$ works as an instrumental variable: it affects the treatment (i.e., bracket locations and, thus, tax rates) and, only through the treatment, affects outcome dynamics. Although we confirm the first requirement (relevance) in Section 5, the second requirement (exclusion restriction) is not directly testable; however, parallel pre-reform outcome dynamics provide supporting evidence. Moreover, in Section 5, we conduct a placebo test and show parallel pre- and post-reform outcome dynamics between placebo-treated and placebo-control individuals who differ exclusively in $\mathrm{LI}^{\mathrm{w}}_{i86}$ but face almost the same tax rates (i.e., the absence of the treatment).

4 Danish administrative data

We use population-wide Danish administrative data constructed from three register-based sources: tax return data, the Integrated Database for Labor Market Research (IDA for "Integreret Database for Arbejdsmarkedsforskning"), and job spell data. These data are maintained by Statistics Denmark, cover all legal residents in Denmark aged 15–74 (on the 31st of December each year) since 1980, and have a common individual ID.

Tax return data. The tax return data are effectively the same annual panel data as those used by Kleven and Schultz (2014) and contain variables such as marital status and precisely measured individual income (LI, CI, D, LI^w, CI^w, D^w). We use the tax return data as, among other things, inputs to the tax simulator to simulate the bracket locations (used in Section 3 to define the treated and control individuals) and effective marginal tax rates (used in Section 5 to compute elasticities); see Appendix B for details on the tax return data in an overview of the tax simulator.

IDA. IDA is an annual panel constructed from several registers (e.g., social security and tax registers) and contains a wide range of information on workers and jobs, such as gross hourly wages and worker/job characteristics listed in Table 3. In the data, we observe employment status on the 28th of November each year. Employment is thus defined as holding a primary job on this particular day, referred to as a November job hereafter in this paper. IDA then contains gross hourly wages for a November job each year, our first key outcome variable; see Appendix D for the computation of gross hourly wages. Finally, note that *hourly* wages for a November job differ from *annual* labor income (LI).

Job spell data. The job spell data are constructed from employer-reported income tax reports, cover all primary job spells over the period 1985–2013, and contain (i) the start and end dates of each spell and (ii) hours worked in each spell in each year. The unit of observation is thus person-spell-year, as opposed to person-year in the tax return data and IDA. To link these three data sources, we construct an annual panel of November jobs from the job spell data by extracting job spells ongoing on the 28th of November each year. The unit of observation in this "November-job" annual panel is person-year. By using (i) and (ii), we then compute daily hours worked for a November job each year, our second key outcome variable; see Appendix D for the computation of daily hours worked.

Data construction. We link the tax return data, IDA, and the "November-job" data using the common individual ID each year over the sample period 1983–1993 (1985–1993 for the "November-job" data). Although wages and hours are missing among the non-employed observed in the tax return data and IDA, we retain all the observations regardless of employment status. The constructed dataset is an annual panel covering 1983–1993 and contains the following variables: wages, hours (for 1985–1993), worker/job characteristics, individual income, bracket locations, and effective marginal tax rates. The availability of gross hourly wages and daily hours worked together with the tax return data is novel and one of the advantages of our dataset.[4]

[4] In addition to these two key outcome variables, we use outcome variables regarding human capital accumulation and job changes, and describe them in the corresponding analyses to come.

5 Estimation results

This section presents non-parametric graphical evidence, regression results, and implied elasticities regarding the effects of taxes on various outcomes. We first study wage responses. Our results survive all threats to identification and robustness checks. To analyze the channels through which wages respond to taxes, we next study human capital accumulation and job changes. Finally, we compare wage responses to labor supply responses.

5.1 Wage responses by the low-income group

Graphical evidence. Figure 2 presents wage responses by the low-income group (100,000 $\leq$ LI$_{i86}$ $<$ 150,000). Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panel plots $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. Before the reform, the wage dynamics are parallel, which supports our identification assumption. After the reform, the treated individuals, who are pushed upward from the bottom bracket to the middle bracket, have lower wage growth than the control individuals, who stay in the bottom bracket. Furthermore, the wage dynamics diverge gradually: the wage growth rate of the treated individuals is lower than that of the control individuals by a small margin in 1987, but by approximately three percentage points in 1993, a sizable impact given that their wage growth rate is approximately 15 percent from 1986 to 1993. The left panel provides compelling non-parametric graphical evidence regarding the negative and accumulating effects of taxes on wages.

Regression analysis. To facilitate inference, we move on to regression analysis and estimate the following fixed-effect DID model for individual i and year $t = 83, ..., 93$:

$$Y_{it} = \alpha_i + \sum_{j \neq 86} \alpha_j \cdot Year_{t=j} + \sum_{j \neq 86} \beta_j \cdot Year_{t=j} \cdot Treated_i + u_{it}. \tag{2}$$

This specification is standard in the literature on the dynamic effects of taxes (Jakobsen et al., 2020). Outcome Y_{it} is the same as above, i.e., the log of real gross hourly wages for a November job that individual i holds in year t. α_i is an individual fixed effect. α_j is a year fixed effect, $Year_{t=j}$ is a dummy variable that equals one if year t equals j, and $t = 86$ is an excluded reference year. $Treated_i$ is a dummy variable that equals one if individual i is treated. β_t are the parameters of interest and measure differences in wage dynamics between the treated and control individuals before the reform (β_{83}, ..., β_{85}) and after the reform (β_{87}, ..., β_{93}). u_{it} is an error term. Standard errors are clustered at the individual level (Bertrand et al., 2004).

The right panel of Figure 2 plots the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals. The regression results align with the graphical evidence in the left panel. First, reassuringly, none of the pre-reform effects $(\widehat{\beta_{83}}, ..., \widehat{\beta_{85}})$ are statistically different from zero, which suggests that the parallel trends assumption is plausible. Next, all the post-reform effects $(\widehat{\beta_{87}}, ..., \widehat{\beta_{93}})$ are negative and statistically different from zero. Moreover, the point estimates tend to increase in magnitude over time; for example, we have $\widehat{\beta_{89}} = -0.025$ and $\widehat{\beta_{92}} = -0.039$, and reject a null hypothesis $\beta_{89} = \beta_{92}$ (with a p-value < 0.01). These results clearly show the dynamic and accumulating effects of taxes on wages.

Bracket locations. Next, we check bracket locations in Figure 3. The left (right) panel plots the fractions of individuals located in the bottom (middle, respectively) bracket by treatment status. Bracket locations in 1983 are missing due to data limitations. By definition (1), the treated and control individuals were in the bottom bracket before the reform. Although the treated individuals are pushed upward to the middle bracket by the reform, their bracket movement is only mechanical: in the absence of behavioral responses to the reform, they are in the middle bracket under the 1987 tax system, i.e., $\widetilde{\mathrm{M}}^{87}(z_{i86})$ in (1). Since their income will change in 1987, i.e., $z_{i86} \neq z_{i87}$, their actual brackets in 1987 can differ from the middle bracket. Despite this non-compliance with the treatment, the treated individuals are more likely to be in the middle bracket persistently from 1987 onward. Finally, Figure F.3 in Appendix F shows that small fractions of individuals are located in the top bracket (the left panel) or in none of the three brackets, i.e., not liable for national taxes (the right panel), without noticeable differences between the treated and control individuals.

Elasticity. Because of the non-compliance with the treatment, the DID coefficients β_t in (2) represent intention-to-treat (ITT) effects and thus provide the lower bounds of treatment-on-the-treated (TOT) effects in magnitude. To put the estimation results into perspective, we here convert the year-by-year ITT effects β_t into a TOT elasticity.

We follow Jakobsen et al. (2020) and compute an elasticity by averaging the year-by-year ITT effects and tax rates. First, we estimate the following fixed-effect DID model for individual i and year $t = 83, ..., 93$:

$$Y_{it} = \alpha_i + \alpha \cdot Year_{t \geq 87} + \beta \cdot Year_{t \geq 87} \cdot Treated_i + u_{it}.$$

This specification is analogous to (2) and collapses years into pre- and post-reform periods. α is a post-reform fixed effect, and $Year_{t \geq 87}$ is a dummy variable that equals one if year t exceeds 87. β measures the average of the year-by-year ITT effects.

Next, we define the elasticity of outcome Y (e.g., wages) with respect to net-of-tax rates as

$$\widehat{\epsilon} := \frac{\widehat{\beta}}{\text{avg}[\Delta \log(1-\tau_{it^*}) \mid \textit{Treated}] - \text{avg}[\Delta \log(1-\tau_{it^*}) \mid \textit{Control}]}, \tag{3}$$

where t^* is a post-reform year. The denominator is the difference-in-differences of net-of-tax rates. Its first term is the average of log differences in net-of-tax rates between t^* and 1986 among the treated individuals; specifically, $\text{avg}[\cdot \mid \textit{Treated}]$ denotes an average over i and t^* conditional on i being treated, and for $t^* = 87, ..., 93$,

$$\Delta \log(1-\tau_{it^*}) := \log(1-\tau_{it^*}) - \log(1-\tau_{i86}),$$

where τ_{it} is an effective marginal tax rate on labor income that individual i faces in year t. The second term of the denominator is analogously defined for the control individuals.

We clarify three points regarding $\widehat{\epsilon}$ in (3). First, since the treated and control individuals were in the bottom bracket before the reform, τ_{i86} is identical for all i; hence, only the post-reform tax rate τ_{it^*} ($t^* = 87, ..., 93$) matters for $\widehat{\epsilon}$ in (3). Second, due to the non-compliance with the treatment, τ_{it^*} among the treated (control) individuals can differ from the middle (bottom, respectively) tax rate; thus, $\widehat{\epsilon}$ in (3) scales up the estimated ITT effect $\widehat{\beta}$ and converts it into the TOT elasticity. Third, τ_{it} is not a statutory tax rate listed in Table 2 but the effective tax rate computed by the tax simulator taking account of the details of the Danish income tax system. Since τ_{it} is the marginal tax rate on labor income (LI_{it}), it is computed as

$$\tau_{it} = \frac{T^t(\text{LI}_{it}+100, \text{CI}_{it}, \text{D}_{it}, \text{LI}^{\text{w}}_{it}, \text{CI}^{\text{w}}_{it}, \text{D}^{\text{w}}_{it}) - T^t(\text{LI}_{it}, \text{CI}_{it}, \text{D}_{it}, \text{LI}^{\text{w}}_{it}, \text{CI}^{\text{w}}_{it}, \text{D}^{\text{w}}_{it})}{100},$$

where $T^t(\cdot)$ is tax liabilities under the year-t tax system and simulated by taking individual income as the main input (DKK 100 in 1986 $\approx$ USD 30 in 2016).

We obtain an elasticity $\widehat{\epsilon}$ of 0.97, with a standard error (SE) of 0.13 computed using the delta method. Table 4 summarizes the main elasticities estimated in this paper to compare them later across groups and outcomes.

5.2 Wage responses by the medium- and high-income groups

Medium-income group. We repeat the same analysis for the medium-income group ($150{,}000 \leq \text{LI}_{i86} < 200{,}000$) by providing graphical evidence, DID coefficients $\widehat{\beta}_t$, and an implied elasticity $\widehat{\epsilon}$. Figure 4 presents the graphical evidence and DID coefficients $\widehat{\beta}_t$. Note that the figures regarding wage responses, such as Figures 2, 4, and 5, have the same scales on the y-axis for ease of comparison. Reassuringly, none of the pre-reform effects ($\widehat{\beta_{83}}, ..., \widehat{\beta_{85}}$) are statistically different from zero. Next, the post-reform effects ($\widehat{\beta_{87}}$, ..., $\widehat{\beta_{93}}$) are statistically different from zero only for 89, 90, and 93. Furthermore, they are static: we fail to reject a null hypothesis on static responses among the significant coefficients $\beta_{89} = \beta_{90} = \beta_{93}$ (with a p-value = 0.61). Finally, the implied elasticity $\widehat{\epsilon}$ is 0.15 (with an SE = 0.06), which is smaller than that of the low-income group (see Table 4).

High-income group. The high-income group (200,000 $\leq \text{LI}_{i86} <$ 250,000) has a similar pattern to the medium-income group, except that their estimates are less precise due to the small sample size. Figure 5 shows that the wage dynamics are almost parallel before and after the reform, with insignificant differences between the treated and control individuals. The implied elasticity $\widehat{\epsilon}$ is also insignificant and 0.21 (with an SE = 0.18).

Summing up. We found heterogeneous wage responses across income levels. The low-income group responds to taxes negatively and dynamically, with an elasticity close to one. We highlight two points. First, the negative responses contrast with the prediction from a standard labor supply-and-demand model. We further explore this point when comparing wage responses to labor supply responses. Second, we obtained a relatively large elasticity because our DID design uncovers the dynamic and accumulating effects rather than short-run effects attenuated by optimization frictions: over seven years after the reform, workers will change behavior sluggishly in response to taxes and gradually overcome optimization frictions, which leads to the accumulating effects with a large elasticity. Therefore, our estimated elasticity will not be a frictional elasticity (Martínez et al., 2021) but can be interpreted as a structural elasticity that is more relevant for long-run welfare (Chetty, 2012; Chetty et al., 2011; Kleven and Waseem, 2013).

For the medium- and high-income groups, the effects of taxes on wages are small and static. Their elasticities are approximately 0.2, close to those estimated by Blomquist and Selin (2010) for a similar sample (Swedish married males of working age in the 1980s). The larger effects of taxes among lower-income groups are also found by Zidar (2019) using state-level data and variations in the US. These findings on heterogeneous responses across income levels have implications for tax policies toward low-income working individuals or families, such as the Earned Income Tax Credit in the US. Given the large elasticity and novel accumulating effects, the following analysis focuses on the low-income group and studies internal validity, the channels through which wages respond to taxes, and labor supply responses.

5.3 Threats to identification and robustness checks

This subsection shows that the estimation results for the low-income group survive all threats to identification (the exclusion restriction, compositional changes, and bunching) and robustness checks.

Exclusion restriction. Our empirical strategy relies on the variation in wives' labor income in 1986 ($\mathrm{LI}_{i86}^{\mathrm{w}}$) and uses it as an instrumental variable in the DID design. We here conduct a placebo test and verify the exclusion restriction, which requires that $\mathrm{LI}_{i86}^{\mathrm{w}}$ affects outcome dynamics only through the treatment (i.e., bracket locations and, thus, tax rates). Below, we show parallel pre- and post-reform outcome dynamics between placebo-treated and placebo-control individuals who differ exclusively in $\mathrm{LI}_{i86}^{\mathrm{w}}$ but face almost the same tax rates (i.e., the absence of the treatment).

We construct placebo-treated and placebo-control individuals from the control individuals in the low-income group using their wives' labor income in 1986 ($\mathrm{LI}_{i86}^{\mathrm{w}}$). Figure 6 plots the kernel density estimates of $\mathrm{LI}_{i86}^{\mathrm{w}}$ by treatment status for the low-income group. As expected, the treated individuals have higher $\mathrm{LI}_{i86}^{\mathrm{w}}$ than the control individuals on average. If we select two control individuals with low $\mathrm{LI}_{i86}^{\mathrm{w}}$, they will be away from the middle-bracket cutoff and thus face similar tax rates. Based on this idea, we define a placebo group (composed of placebo-treated and placebo-control individuals) as follows:

$$\begin{aligned} &\text{Placebo-treated: } Q_1 \leq \mathrm{LI}_{i86}^{\mathrm{wC}} < Q_2 \\ &\text{Placebo-control: } \mathrm{LI}_{i86}^{\mathrm{wC}} < Q_1, \end{aligned} \tag{4}$$

where $\mathrm{LI}_{i86}^{\mathrm{wC}}$ denotes $\mathrm{LI}_{i86}^{\mathrm{w}}$ of the control individuals in the low-income group, and Q_1 and Q_2 denote the first and second quartiles of $\mathrm{LI}_{i86}^{\mathrm{wC}}$. ($Q_1 \approx$ DKK 60,000 and $Q_2 \approx$ DKK 90,000.)

Let us check their bracket locations and pre-reform covariates. First, Figure 7 and Figure F.4 in Appendix F show that, unlike the low-income group, the placebo-treated and placebo-control individuals face almost the same tax rates; thus, their outcome dynamics represent those in the absence of the treatment. Next, Table 5 shows that, like the low-income group, the placebo-treated and placebo-control individuals are similar except in $\mathrm{LI}_{i86}^{\mathrm{w}}$. The table also shows that the placebo group is similar to the low-income group. Furthermore, the placebo group has a larger within-group difference in $\mathrm{LI}_{i86}^{\mathrm{w}}$ than the low-income group, and hence has a fair chance of having non-parallel outcome dynamics and thus rejecting the exclusion restriction. These points suggest the validity of our placebo test.

Figure 8 presents wage responses by the placebo-treated and placebo-control individuals. The wage dynamics are parallel and not statistically different from each other both before and after the reform; that is, $\mathrm{LI}_{i86}^{\mathrm{w}}$ does not affect the outcome dynamics in the absence of the treatment. Therefore, this placebo test provides evidence supporting the exclusion restriction for the low-income group.

Compositional changes. Recall that our sample consists of workers employed in all the pre-reform years 1983–1986. After the reform, they can become non-employed with missing wages or drop from the sample due to attrition, although the left panel of Figure 9 shows high (attrition-adjusted) employment rates. If the treated (control) individuals with higher (lower, respectively) wages exit from employment, these compositional changes of employed workers will create the spurious negative effects of taxes on wages.

To examine this concern, the right panel of Figure 9 plots mean log wages in 1986 among workers employed in year t, by treatment status. The 1986 level is normalized to zero. The treated and control individuals show almost the same pattern: compared to workers employed in 1986, workers employed after the reform had lower wages in 1986 by only approximately 0.4 percent. This result indicates that the compositional changes of employed workers (measured by the pre-reform wages) slightly occur in parallel between the treated and control individuals, and hence are not a confounding factor.

Finally, Table 6 compares covariates in 1986 between workers employed in 1986 and workers employed in 1993, by treatment status. The same pattern emerges as the pre-reform wages: the compositional changes of employed workers (measured by the pre-reform covariates) slightly occur in the same direction with similar magnitudes between the treated and control individuals. To sum up, seven years after the reform, the employed treated individuals are still similar to the employed control individuals in the pre-reform wages and covariates (except in wives' labor income).

Bunching. As Kleven and Schultz (2014) point out, quasi-experimental approaches that exploit tax reforms assume that individuals do not bunch at bracket cutoffs; otherwise, bunching might bias estimates. Figure 10 plots the frequencies of individuals by their taxable income relative to the middle-bracket cutoff in bins of DKK 1,000, for the post-reform years 1987–1993. We deflate the 1987–1993 taxable income and middle-bracket cutoffs at the 1986 price level. The figure shows no spikes around the cutoff; indeed, the lack of bunching by our sample (i.e., male wage-earners) at the middle-bracket cutoff is consistent with Chetty et al. (2011) and le Maire and Schjerning (2013), who find bunching by females, by self-employed workers, and at the top-bracket cutoff in Denmark.

Robustness checks. Recall that the low-income group is defined as $100{,}000 \leq \mathrm{LI}_{i86} < 150{,}000$. To check robustness to modest changes to this definition, we create four alternative low-income groups by adding $\pm 5{,}000$ to either the boundary of $100{,}000 \leq \mathrm{LI}_{i86} < 150{,}000$. These four groups show robust wage responses in Figure 11, which presents graphical evidence and DID coefficients $\widehat{\beta}_t$. Their implied elasticities $\widehat{\varepsilon}$ are, from the top to bottom panels, 1.01 (0.13), 0.90 (0.13), 1.21 (0.18), and 0.72 (0.09), where SEs are in parentheses. These results are similar to those shown in Figure 2 and $\widehat{\varepsilon}$ = 0.97 (0.13) of the original low-income group.

5.4 Two channels: human capital accumulation and job changes

This subsection studies two channels through which wages can respond to taxes: human capital accumulation and job changes. Over seven years after the reform, workers will gradually overcome optimization frictions and respond to taxes through these dynamic channels, which can explain the negative and accumulating effects of taxes on wages. In Appendix E, we clarify the meaning of the accumulating effects by constructing a simple theoretical model.

Human capital accumulation. We use information on occupation as a proxy for human capital. Statistics Denmark classifies employed workers into ranked categories such as unskilled, senior, and manager. We aggregate these categories into two by creating a dummy variable that equals one if workers are strictly higher ranked than unskilled; otherwise, it equals zero. The two categories are referred to as skilled and unskilled hereafter in this paper, and have similar sample sizes.

Although we believe that our dummy variable is a reasonable proxy for human capital, it will contain measurement errors due to misclassification. Therefore, only in this analysis, we analyze workers classified as unskilled between 1983 and 1985. This restriction mitigates measurement errors by selecting the core unskilled workers.

Like the wage responses, we provide graphical evidence, DID coefficients $\widehat{\beta}_t$, and an implied semi-elasticity $\widehat{\varepsilon}$. The left panel of Figure 12 plots the fractions of skilled workers in year t by treatment status. After the reform, the treated individuals are less likely to be skilled than the control individuals. Higher tax rates arguably create disincentives to accumulate human capital. Next, following Lechner (2010), we run a linear probability model with the same specification as (2), where outcome Y_{it} is the dummy variable indicating that worker i is skilled ($Y_{it} = 1$) or unskilled ($Y_{it} = 0$) in year t. We exclude $t = 83$ and 84 from the regression because the sample is restricted to unskilled workers ($Y_{it} = 0$) between 1983 and 1985. The right panel plots the DID coefficients $\widehat{\beta}_t$. Most post-reform effects are significant. The implied semi-elasticity $\widehat{\varepsilon}$ defined by (3) is 0.74 (with an SE = 0.25).

To the best of our knowledge, this paper provides the first quasi-experimental evidence on the negative effects of taxes on human capital accumulation. Our findings motivate recent research on optimal taxation with career effects or human capital investment (Stantcheva, 2020). Our findings also complement structural approaches that study the effects of income taxes on wages and hours by estimating learning-by-doing models (Keane and Rogerson, 2012, 2015) or Ben-Porath models (Heckman et al., 1998, 1999).

Job changes. We study job changes by defining a job-to-job transition (JJT) between year $t-1$ and year t as follows: (i) workplace IDs are different between $t-1$ and t, (ii) wages are higher in t than $t-1$, and (iii) a worker is not unemployed in $t-1$ and t. We clarify these three conditions. In the first condition, we use workplace IDs rather than firm IDs due to data limitations. Our definition of JJTs is thus somewhat broad because it includes internal transfers with workplace changes. Next, we impose the second condition to focus on workers climbing up job/wage ladders; in Denmark, 35 percent of job changes are associated with wage cuts (Jolivet et al., 2006). Finally, the third condition excludes involuntary job changes caused by layoffs. Unemployment is defined as receiving unemployment benefits.

Our outcome is the (cumulative) number of JJTs between 83 and t ($t = 84, ..., 93$). We clarify two points. First, recall that we observe only a November job for each worker and year; thus, we observe whether a worker makes zero or one JJT between $t-1$ and t. We calculate the number of JJTs between 83 and t by adding up the number of JJTs between 83 and 84, the number of JJTs between 84 and 85, ..., and the number of JJTs between $t-1$ and t. Second, as a standard on-the-job search model predicts, workers are less likely to make JJTs once settling down in high-paying jobs. Therefore, to capture worker dynamics along their job/wage ladders, we need to examine not the latest JJTs between $t-1$ and t but the total number of JJTs that workers have made since 1983 (the beginning of the sample period).

We provide graphical evidence, DID coefficients $\widehat{\beta}_t$, and an implied semi-elasticity $\widehat{\epsilon}$. The left panel of Figure 13 plots the mean number of JJTs between 83 and year t ($t = 84, ..., 93$) by treatment status. Over seven years after the reform, the treated individuals make fewer JJTs than the control individuals. Higher tax rates arguably create disincentives to search for better jobs. The right panel plots the DID coefficients $\widehat{\beta}_t$ from the same model as (2), where outcome Y_{it} is the number of JJTs between 83 and year t ($t = 84, ..., 93$). Most post-reform effects are significant (with slight non-parallel pre-reform trends). The implied semi-elasticity $\widehat{\epsilon}$ is defined by multiplying the denominator of (3) by 100 and computed as 0.016 (with an SE = 0.004).

Although Gentry and Hubbard (2004) also find the negative effects of taxes on job changes, we provide the first quasi-experimental evidence. Our findings motivate welfare analysis based on job search models using structural approaches (Kreiner et al., 2015; Shephard, 2017) or sufficient statistics approaches (Chetty, 2008; Kroft et al., 2020). Finally, job changes are deeply related to location choices, i.e., domestic or international migration. Given the recent interest in the effects of taxes on migration

(Kleven et al., 2020), the interaction between job changes and migration will be interesting for future research.

5.5 Labor supply responses versus wage responses

We explore labor supply along the intensive margin using daily hours worked for a November job. They are available only from 1985 and do not include overtime work. Although we argue in Appendix D that the lack of overtime work is not a serious concern for several reasons, it will have different impacts on full-time and part-time workers; thus, we analyze them separately by splitting the low-income group. Specifically, a worker is in the full-time subgroup if he worked more than 30 hours per week in 1986 (i.e., just before the reform); otherwise, he is in the part-time subgroup.

Hours. Figure 14 presents labor supply responses by the full-time and part-time subgroups. The left panels plot $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 85, ..., 93$ by treatment status, where outcome Y_{it} is the log of daily hours worked for a November job that individual i holds in year t. The right panels plot the DID coefficients $\widehat{\beta_t}$ from the same model as (2). For both subgroups, the pre-reform effects are not statistically different from zero. For the full-time subgroup, the post-reform effects are not statistically different from zero (except for $\widehat{\beta_{88}}$). Their implied elasticity $\widehat{\epsilon}$ defined by (3) is also insignificant and -0.08 (with an SE = 0.16), which is consistent with existing findings for married males (Meghir and Phillips, 2010). By contrast, the part-time subgroup responds to taxes negatively and significantly, and their implied elasticity $\widehat{\epsilon}$ is 0.99 (with an SE = 0.27). The large and significant responses are consistent that part-time workers have more room to change labor supply along the intensive margin.

Wages. Figure 15 presents wage responses by the two subgroups. Both show insignificant pre-reform effects and significant post-reform effects; thus, in contrast to the labor supply responses, the wage responses are relevant regardless of job types. Their implied elasticities $\widehat{\epsilon}$ are 0.74 (with an SE = 0.15) for the full-time subgroup and 1.21 (with an SE = 0.22) for the part-time subgroup.

Summing up. We highlight two findings regarding these elasticities (see Table 4 for a summary). First, regardless of job types, the elasticities of wages are larger than those of hours. This finding has implications for optimal taxation because the literature derives tax formulae focusing mostly on labor supply responses (Piketty and Saez, 2013). Second, especially for the part-time subgroup, both the elasticities of wages and hours (with respect to net-of-tax rates) are positive. This finding contrasts with the prediction from a standard labor supply-and-demand model because it predicts the positive elasticity of hours and the negative elasticity of wages (Azmat, 2019; Leigh, 2010; Rothstein, 2010). As we showed in the previous subsection, our positive elasticities of wages can be explained by human capital accumulation and job changes.

Related to the second point, Martínez et al. (2021) also provide credible quasi-experimental evidence on both wage and labor supply responses. They exploit a large and salient tax holiday in Switzerland, where income earned in some years was untaxed due to a reform. Since the tax holiday was a one-shot tax cut, their empirical settings are well suited for identifying intertemporal responses. Using bi-annual repeated cross-section data, they also find the positive (but insignificant) elasticities of hourly wages and monthly hours (with respect to net-of-tax rates). With some evidence on bonus shifting, they conclude that the responses are likely driven by tax avoidance rather than labor supply.

In contrast to Martínez et al. (2021), the current paper estimates the dynamic and accumulating effects of taxes (rather than the intertemporal effect of a one-shot tax cut). In addition to the positive (and significant) elasticities of wages and hours, we find that wages respond to taxes through human capital accumulation and job changes. Therefore, our anatomy of behavioral responses following Slemrod (1996) provides new insights that complement Martínez et al. (2021).

6 Conclusion

This paper provided quasi-experimental evidence on the effects of income taxes on gross hourly wages by exploiting administrative data and a tax reform in Denmark. Our findings are as follows. First, taxes have heterogeneous effects on wages across income levels. Low-income workers respond to taxes negatively and dynamically, with the elasticity of wages (with respect to net-of-tax rates) close to one. For medium- and high-income workers, the effects are small and static, with elasticities of approximately 0.2. Second, wages respond to taxes through human capital accumulation and job changes. Third, daily hours worked also respond negatively to taxes with smaller magnitudes than wages.

Finally, we briefly mention external validity. Although Denmark is different from the US in many dimensions, the two countries have similar patterns in the elasticity of taxable income (Gruber and Saez, 2002; Kleven and Schultz, 2014). Moreover, we found heterogeneity similar to that of Zidar (2019) for the US: the larger effects of taxes among lower-income groups. Therefore, we believe that our findings are also relevant to other countries such as the US.

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Tables

Table 1: Income concepts in the Danish income tax system

Income concept	Acronym	Main items included
Labor income	LI	Salary, wages, bonuses, fringe benefits
Capital income	CI	Interest income – interest on debt
Deductions	D	Commuting, union fees, UI contributions

Notes: The table is based on Kleven and Schultz (2014). For ease of exposition, our description omits a small number of other income concepts of minor importance for our sample, such as stock income. However, we stress that we take full account of all the income concepts when simulating tax liabilities for the empirical analysis in Section 3.

Table 2: The Danish income tax system before and after the 1987 tax reform

	1986				1987			
Tax type	Base	Cutoff	Joint	Rate	Base	Cutoff	Joint	Rate
Regional taxes	LI + CI – D	20,700	No	28.0	LI + CI – D	21,200	No	29.0
National taxes								
Bottom bracket	LI + CI – D	23,200	No	19.9	LI + CI – D	27,100	No	22.0
Middle bracket	LI + CI – D	113,400	No	14.4	LI + [CI>0]	130,000	Yes	6.0
Top bracket	LI + CI – D	186,100	No	10.8	LI + [CI>60k]	200,000	No	12.0

Notes: All monetary values are in Danish Krone (DKK), and DKK 1 in 1986 approximately equals USD 0.3 in 2016. The regional tax rate is the sum of the municipality, county, and Church tax rates. The Church taxes are minuscule and paid only by members of the Church of Denmark ("Folkekirken"). The regional tax rates in the table are averages across municipalities. The bottom tax rate in 1986 includes social security contributions levied at a tax rate of 5.5 percent. "Yes" in the "Joint" column means that if married individuals are not liable for the middle taxes and have unused allowances (the bracket cutoff DKK 130,000 minus their taxable income LI + [CI>0]), their unused allowances can be transferred to spouses.

Table 3: Pre-reform covariates in 1986 by income group

Variable	Low-income Treated	Low-income Control	Medium-income Treated	Medium-income Control	High-income Treated	High-income Control
Labor income	142,418	134,538	170,250	166,089	214,305	214,604
Age	37.1	36.1	37.0	36.4	37.9	37.8
Number of children	1.3	1.4	1.4	1.6	1.5	1.7
Low education (%)	40.1	42.1	27.4	27.6	18.6	18.7
Middle education (%)	55.5	54.1	63.0	64.7	62.0	65.4
High education (%)	4.4	3.8	9.6	7.7	19.5	15.9
Full-time job (%)	44.9	45.4	59.4	60.2	72.0	74.9
Private-sector job (%)	70.6	67.5	66.8	65.8	67.3	70.9
Capital income	-34,588	-39,042	-56,826	-64,188	-93,886	-100,556
Deductions	10,133	9,474	12,449	12,263	18,497	19,747
Capital income (wife)	-4,563	-6,443	-3,232	-5,088	-3,681	-5,196
Deductions (wife)	7,344	8,008	8,371	8,790	8,599	7,708
Labor income (wife)	117,748	84,135	123,186	84,369	134,354	82,199
Number of individuals	3,237	13,951	12,983	13,229	1,705	931
N. of observations (1983–1993)	35,372	152,367	141,912	144,613	18,597	10,141

Notes: The table lists the mean values of covariates in 1986 by treatment status for each income group. All monetary values are in Danish Krone (DKK), and DKK 1 in 1986 approximately equals USD 0.3 in 2016. Children are defined as being younger than 17 years of age. Low education is defined as completing primary education. Middle education is defined as completing high school or vocational education. High education is defined as holding a bachelor's, master's, or Ph.D. degree. Full-time jobs are defined as working more than 30 hours per week. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income ($100{,}000 \leq \mathrm{LI}_{i86} < 150{,}000$), medium-income ($150{,}000 \leq \mathrm{LI}_{i86} < 200{,}000$), and high-income ($200{,}000 \leq \mathrm{LI}_{i86} < 250{,}000$) groups.

Table 4: Elasticity of outcome Y with respect to net-of-tax rates

Outcome Y	Low-income		Medium-income	High-income
Gross hourly wages	0.97 (0.13)		0.15 (0.06)	0.21 (0.18)
	Part-time	Full-time		
	1.21 (0.22)	0.74 (0.15)		
Daily hours worked	0.99 (0.27)	-0.08 (0.16)		

Notes: The table summarizes the main elasticities estimated in this paper. The elasticity of outcome Y with respect to net-of-tax rates is defined by (3). Standard errors are in parentheses and computed using the delta method.

Table 5: Pre-reform covariates in 1986 for the low-income and placebo groups

	Low-income		Placebo	
Variable	Treated	Control	Treated	Control
Age	37.1	36.1	36.1	35.0
Number of children	1.3	1.4	1.5	1.5
Low education (%)	40.1	42.1	41.4	45.6
Middle education (%)	55.5	54.1	55.4	50.9
High education (%)	4.4	3.8	3.2	3.4
Full-time job (%)	44.9	45.4	45.2	37.9
Private-sector job (%)	70.6	67.5	68.8	72.0
Labor income	142,418	134,538	134,969	133,775
Capital income	-34,588	-39,042	-38,268	-37,603
Deductions	10,133	9,474	9,000	9,688
Capital income (wife)	-4,563	-6,443	-4,453	-4,815
Deductions (wife)	7,344	8,008	6,253	4,813
Labor income (wife)	117,748	84,135	75,657	32,828
Number of individuals	3,237	13,951	3,488	3,487
N. of observations (1983–1993)	35,372	152,367	38,115	38,060

Notes: The table lists the mean values of covariates in 1986 by treatment status for the low-income and placebo groups. All monetary values are in Danish Krone (DKK), and DKK 1 in 1986 approximately equals USD 0.3 in 2016. Children are defined as being younger than 17 years of age. Low education is defined as completing primary education. Middle education is defined as completing high school or vocational education. High education is defined as holding a bachelor's, master's, or Ph.D. degree. Full-time jobs are defined as working more than 30 hours per week. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income group ($100{,}000 \leq LI_{i86} < 150{,}000$). The placebo-treated and placebo-control individuals are defined by (4).

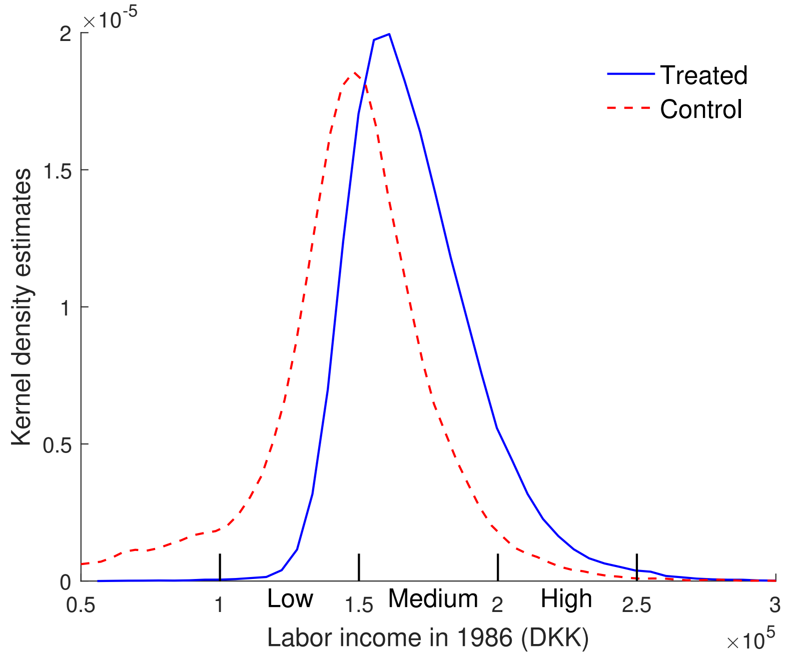
Table 6: Pre-reform covariates in 1986 for workers employed in 1986 or 1993

	Treated		Control	
Variable	Empl. in 86	Empl. in 93	Empl. in 86	Empl. in 93
Age	37.1	36.8	36.1	35.9
Number of children	1.3	1.4	1.4	1.5
Low education (%)	40.1	39.4	42.1	41.5
Middle education (%)	55.5	55.8	54.1	54.4
High education (%)	4.4	4.7	3.8	4.1
Full-time job (%)	44.9	46.0	45.4	46.7
Private-sector job (%)	70.6	68.7	67.5	65.1
Labor income	142,418	142,685	134,538	135,187
Capital income	-34,588	-34,469	-39,042	-39,279
Deductions	10,133	9,981	9,474	9,332
Capital income (wife)	-4,563	-4,690	-6,443	-6,474
Deductions (wife)	7,344	7,335	8,008	8,128
Labor income (wife)	117,748	118,233	84,135	84,613

Notes: The table lists the mean values of covariates in 1986 by treatment status for workers employed in 1986 or 1993. All monetary values are in Danish Krone (DKK), and DKK 1 in 1986 approximately equals USD 0.3 in 2016. Children are defined as being younger than 17 years of age. Low education is defined as completing primary education. Middle education is defined as completing high school or vocational education. High education is defined as holding a bachelor's, master's, or Ph.D. degree. Full-time jobs are defined as working more than 30 hours per week. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income group ($100{,}000 \leq LI_{i86} < 150{,}000$).

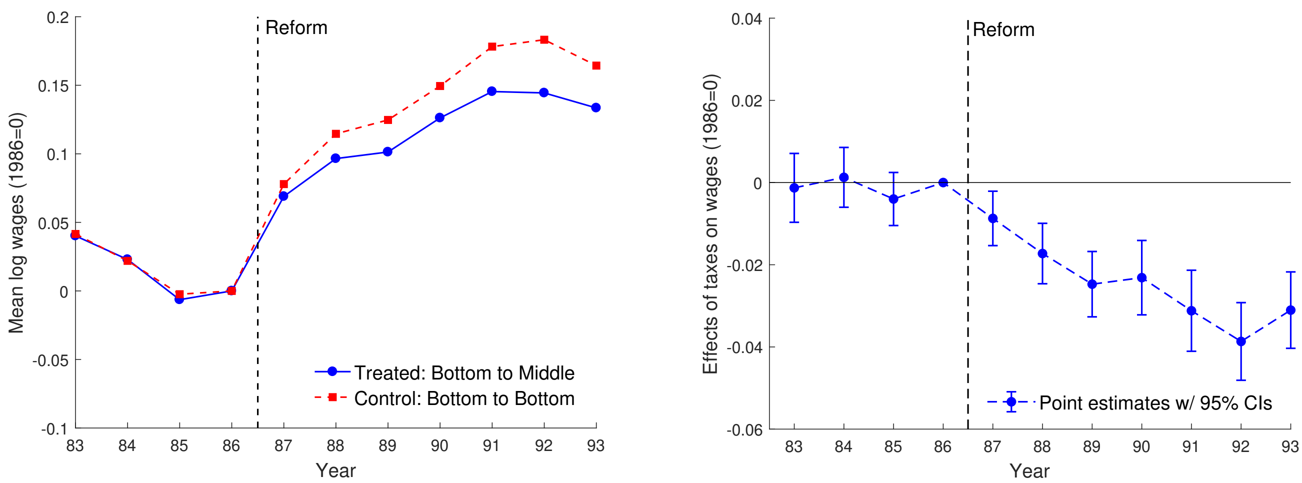
Figures

Figure 1: Distributions of pre-reform labor income LI_{i86}



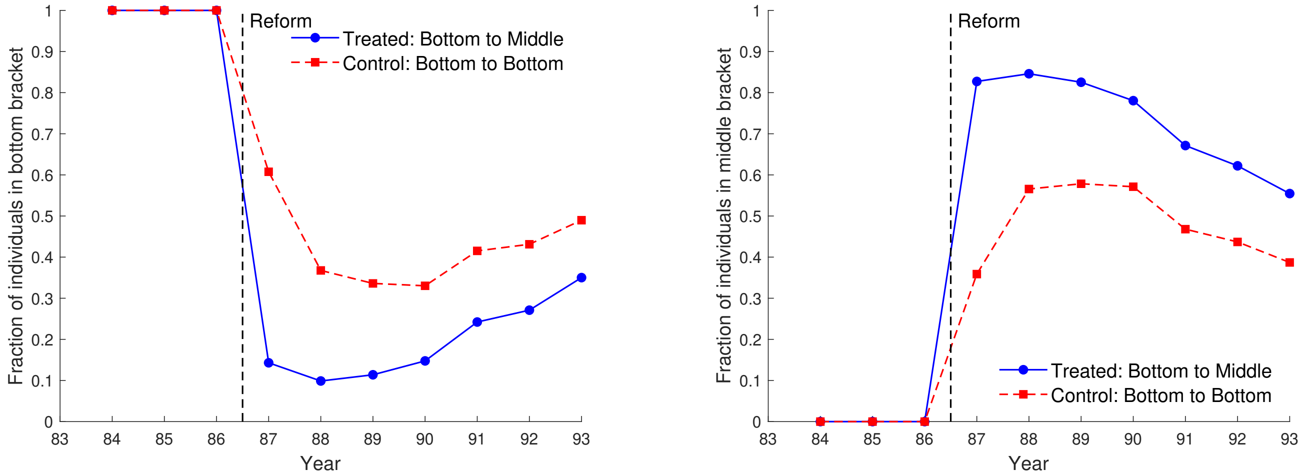
Notes: The figure plots the kernel density estimates of pre-reform labor income LI_{i86} by treatment status. The estimation is based on a `ksdensity` function in MATLAB with default settings. DKK 1 in 1986 approximately equals USD 0.3 in 2016. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1).

Figure 2: Wage responses by the low-income group



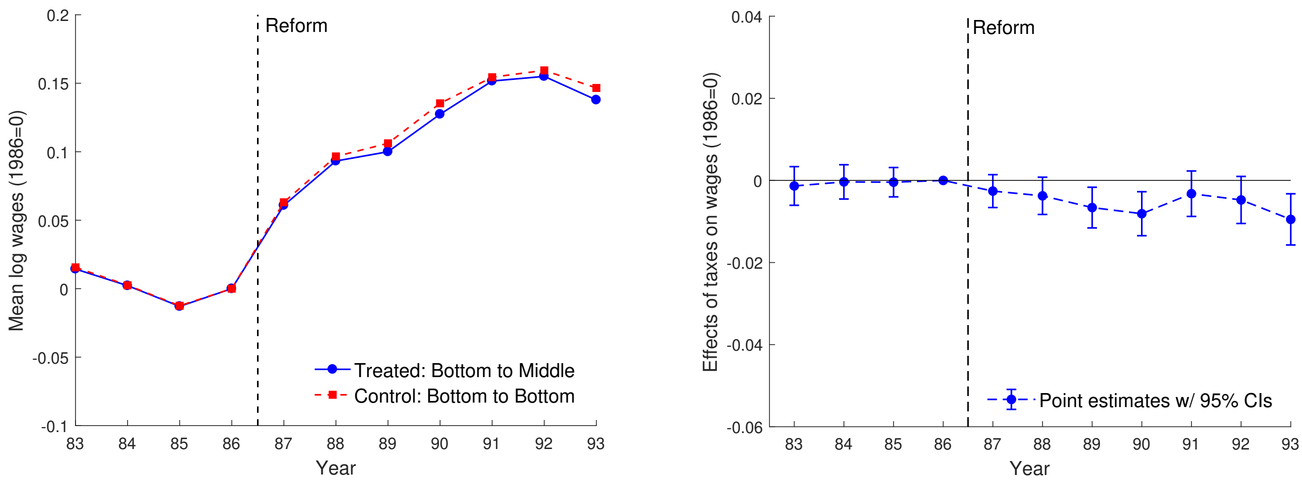
Notes: The figure presents wage responses by the low-income group ($100{,}000 \leq \text{LI}_{i86} < 150{,}000$). Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panel plots $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panel plots the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 3: Bracket locations among the low-income group



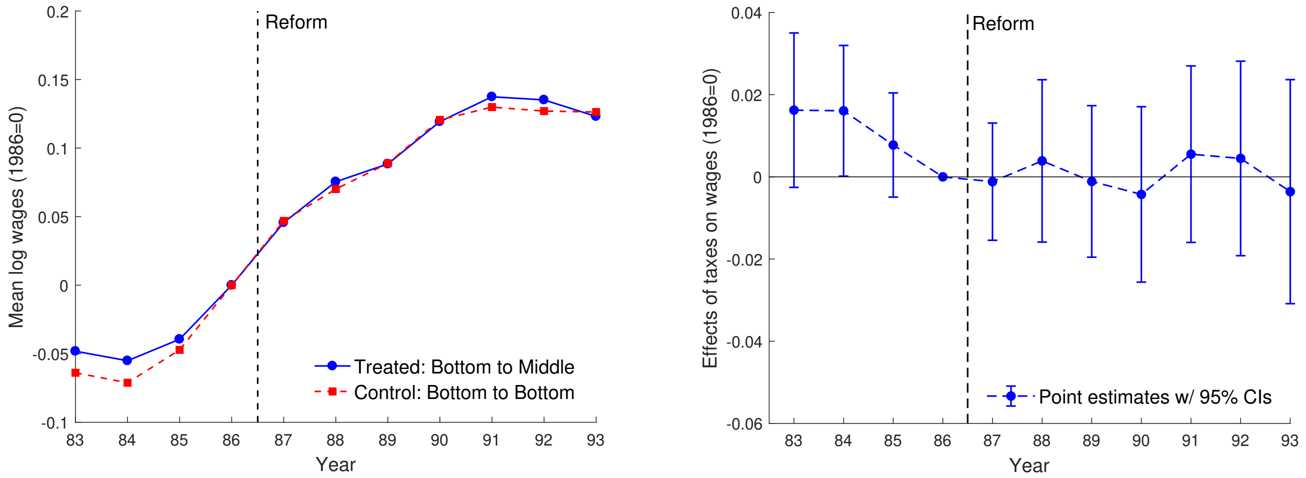
Notes: The figure presents bracket locations among the low-income group ($100{,}000 \leq \mathrm{LI}_{i86} < 150{,}000$). The left (right) panel plots the fractions of individuals located in the bottom (middle, respectively) bracket by treatment status. Bracket locations in 1983 are missing due to data limitations. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1).

Figure 4: Wage responses by the medium-income group



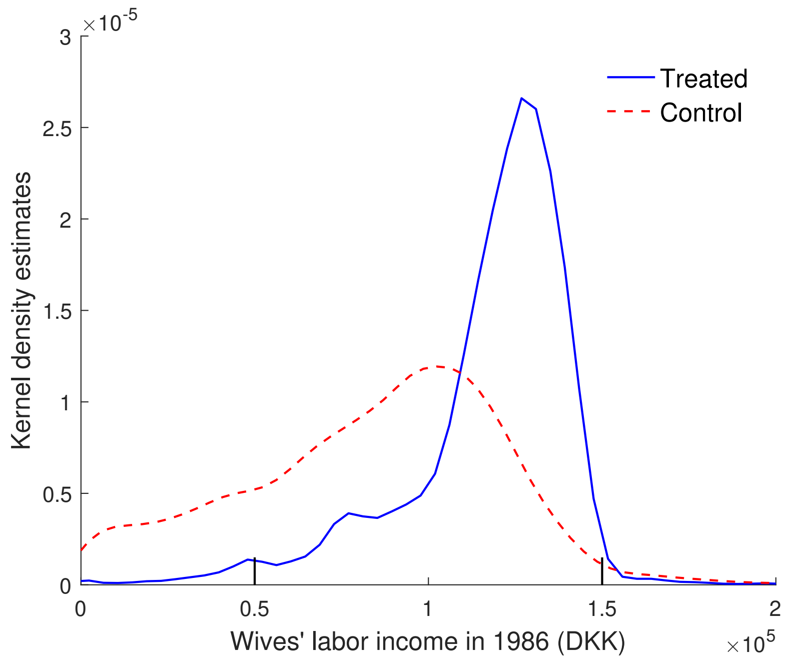
Notes: The figure presents wage responses by the medium-income group ($150{,}000 \leq \mathrm{LI}_{i86} < 200{,}000$). Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panel plots $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panel plots the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 5: Wage responses by the high-income group



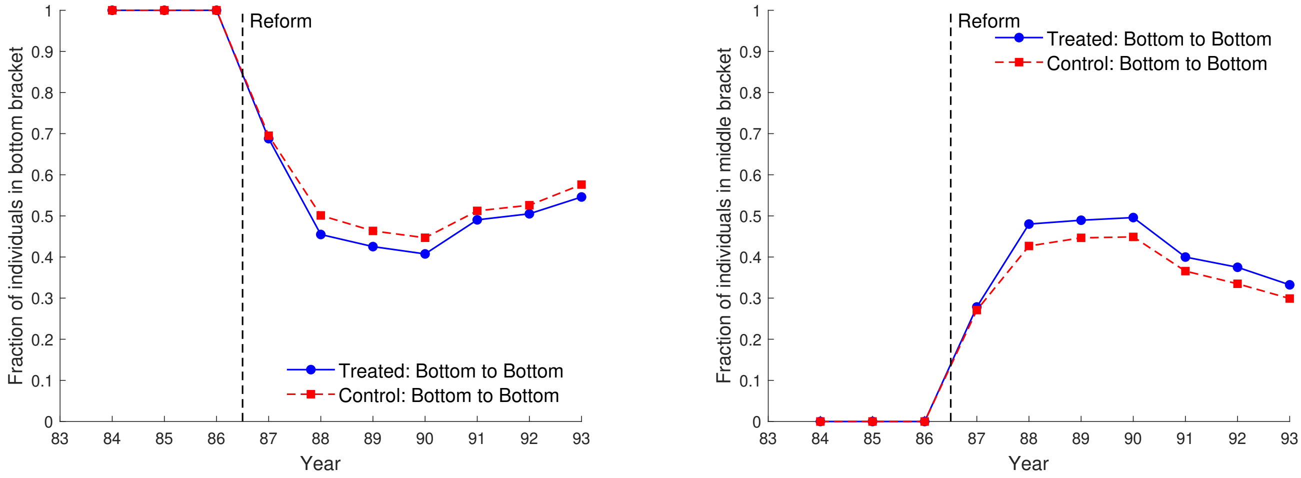
Notes: The figure presents wage responses by the high-income group (200,000 $\leq \mathrm{LI}_{i86} <$ 250,000). Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panel plots $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panel plots the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 6: Distributions of wives' pre-reform labor income LI^{w}_{i86}



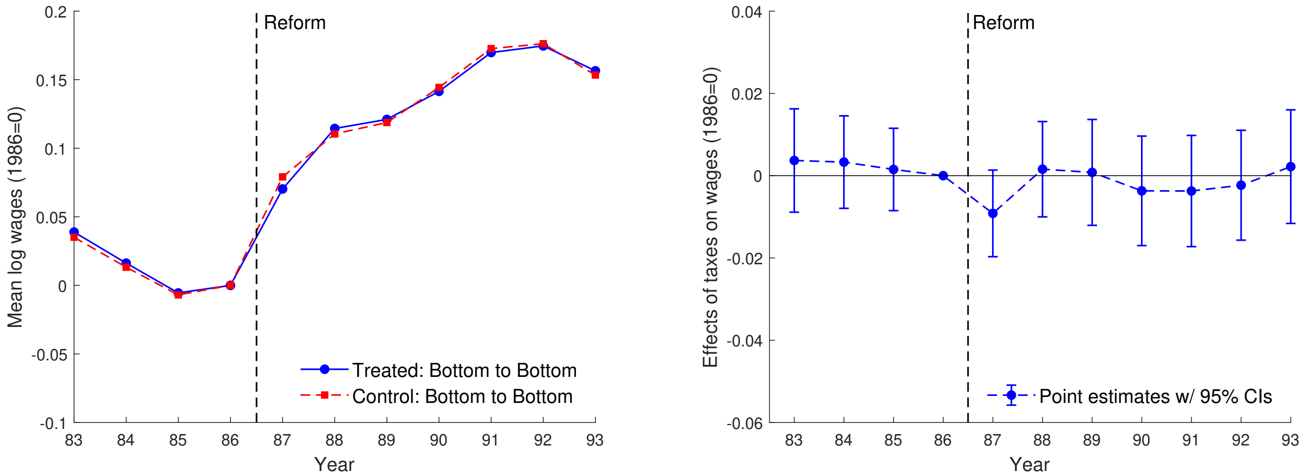
Notes: The figure plots the kernel density estimates of wives' pre-reform labor income LI^{w}_{i86} by treatment status. The estimation is based on a `ksdensity` function in MATLAB with default settings. DKK 1 in 1986 approximately equals USD 0.3 in 2016. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income group (100,000 $\leq \mathrm{LI}_{i86} <$ 150,000).

Figure 7: Bracket locations among the placebo group



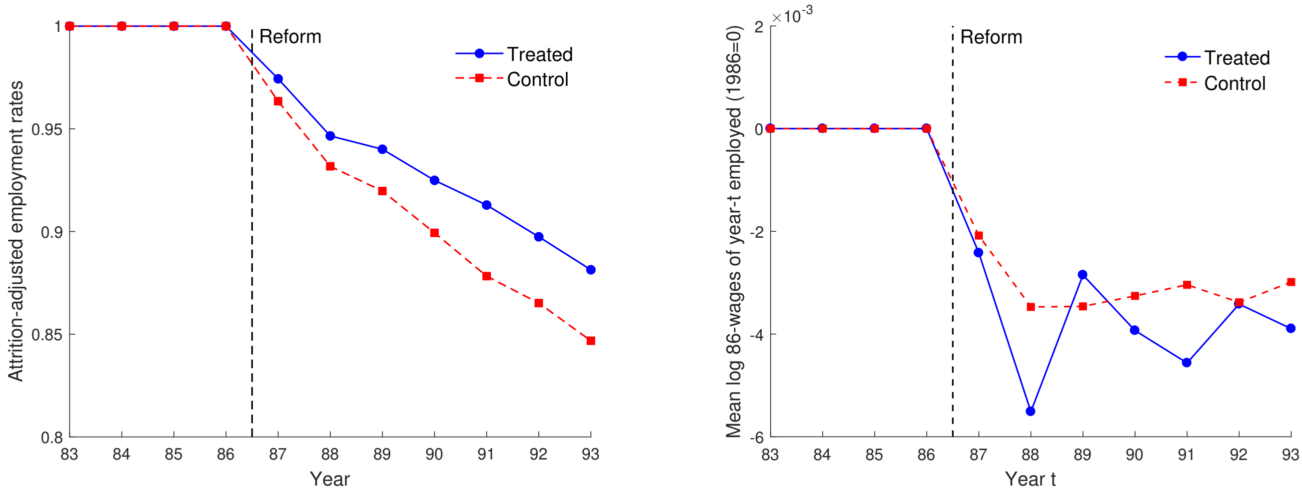
Notes: The figure presents bracket locations among the placebo group. The left (right) panel plots the fractions of individuals located in the bottom (middle, respectively) bracket by treatment status. Bracket locations in 1983 are missing due to data limitations. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. They are restricted to the low-income group (100,000 $\leq \text{LI}_{i86} <$ 150,000). The placebo-treated and placebo-control individuals are defined by (4).

Figure 8: Wage responses by the placebo group



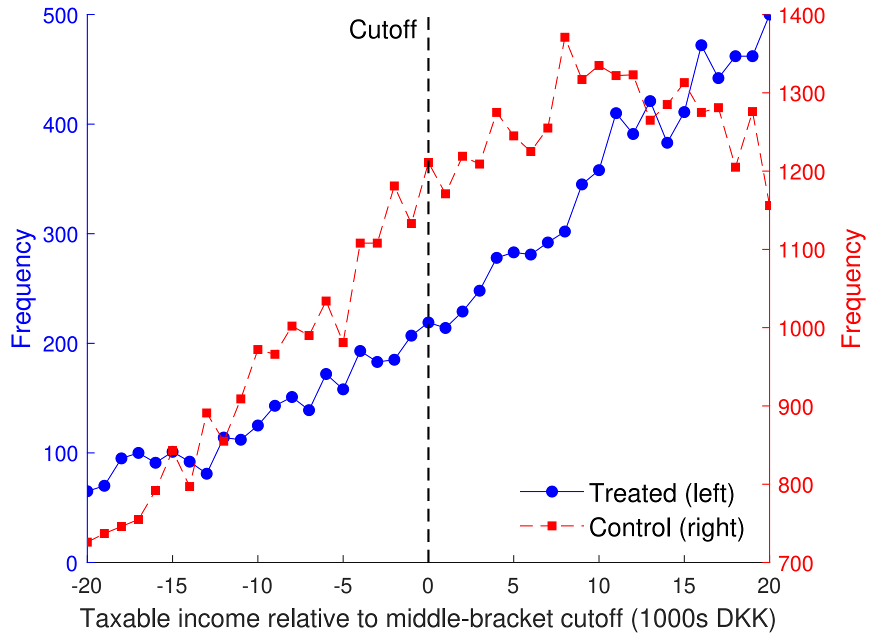
Notes: The figure presents wage responses by the placebo group. Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panel plots $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. They are restricted to the low-income group (100,000 $\leq \text{LI}_{i86} <$ 150,000). The placebo-treated and placebo-control individuals are defined by (4). The right panel plots the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 9: Compositional changes of employed workers



Notes: The figure presents the compositional changes of employed workers. The left panel plots an attrition-adjusted employment rate in year t by treatment status. It is computed as the number of employed workers in year t divided by the number of workers in 1986. Note that the denominator represents all the workers in the sample. The right panel plots mean log wages in 1986 among workers employed in year t, by treatment status. The 1986 level is normalized to zero. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income group (100,000 $\leq$ LI$_{i86}$ $<$ 150,000).

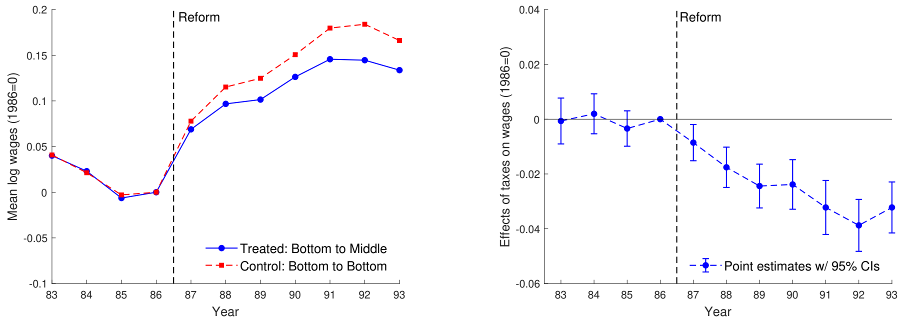
Figure 10: Density around the middle-bracket cutoff



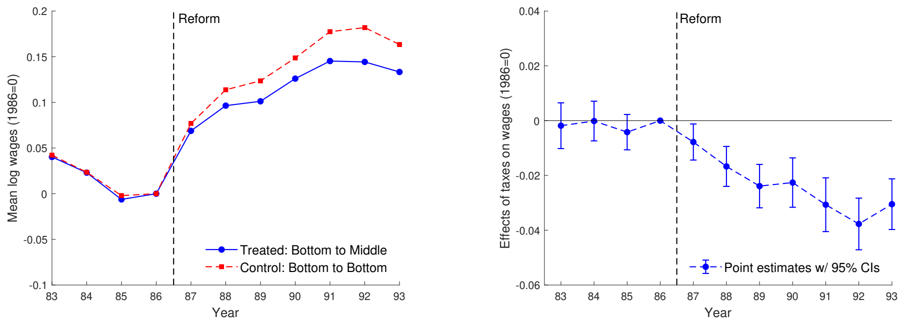
Notes: The figure plots the frequencies of individuals by their taxable income relative to the middle-bracket cutoff in bins of DKK 1,000, for the post-reform years 1987–1993. For example, in 1987, taxable income for the middle bracket was LI + [CI>0], and the middle-bracket cutoff was DKK 130,000, as listed in Table 2. We deflate the 1987–1993 taxable income and middle-bracket cutoffs at the 1986 price level. DKK 1 in 1986 approximately equals USD 0.3 in 2016. We have 41 bins in total: [-20.5k, -19.5k), ..., [-1.5k, -500), [-500, 500), [500, 1.5k), ..., [19.5k, 20.5k). The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to the low-income group (100,000 $\leq$ LI$_{i86}$ $<$ 150,000).

Figure 11: Wage responses by the four alternative low-income groups

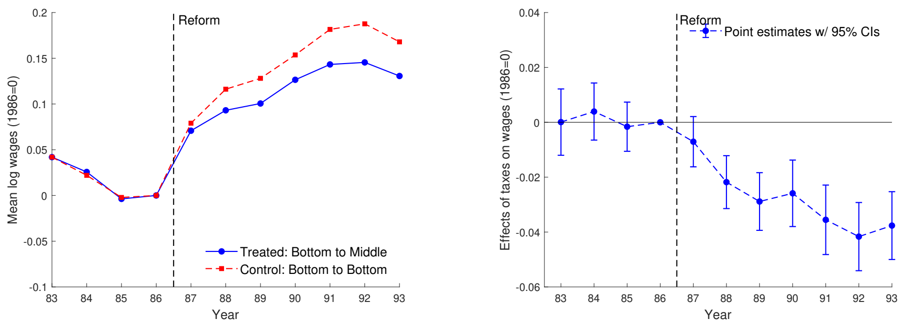
(a) $95{,}000 \leq \text{LI}_{i86} < 150{,}000$



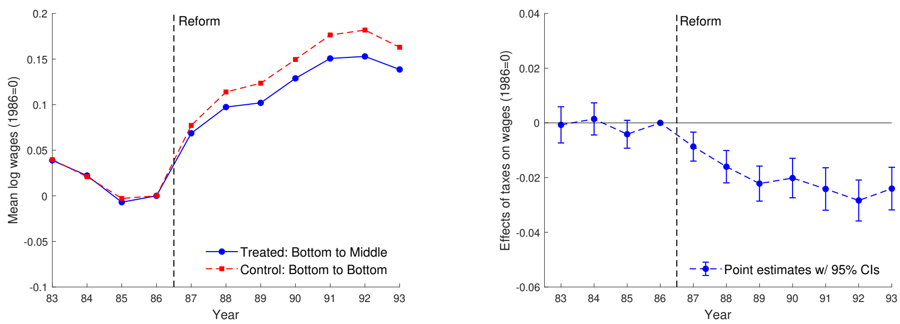
(b) $105{,}000 \leq \text{LI}_{i86} < 150{,}000$



(c) $100{,}000 \leq \text{LI}_{i86} < 145{,}000$

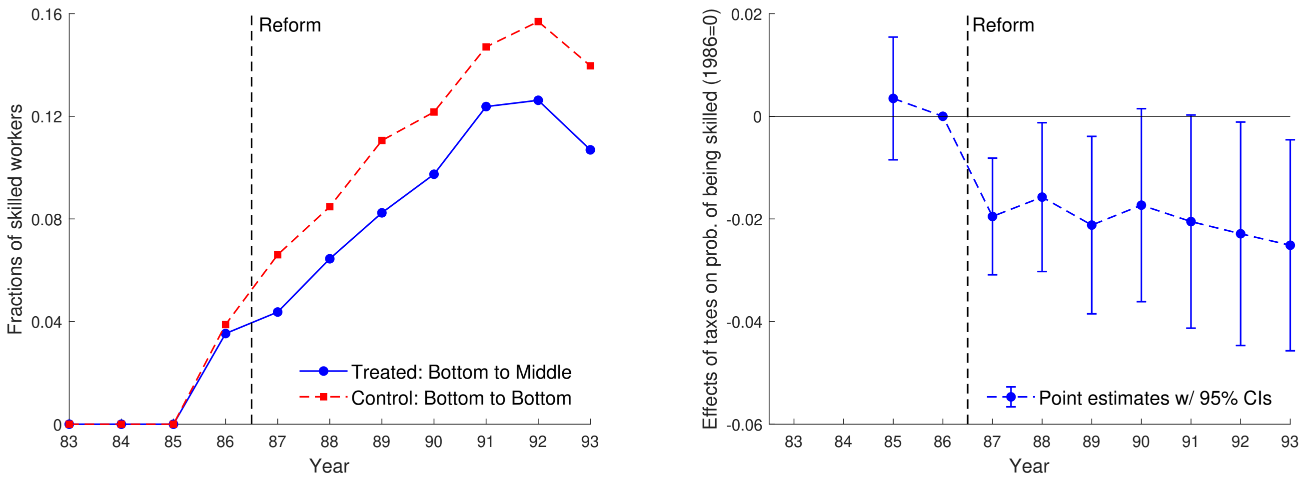


(d) $100{,}000 \leq \text{LI}_{i86} < 155{,}000$



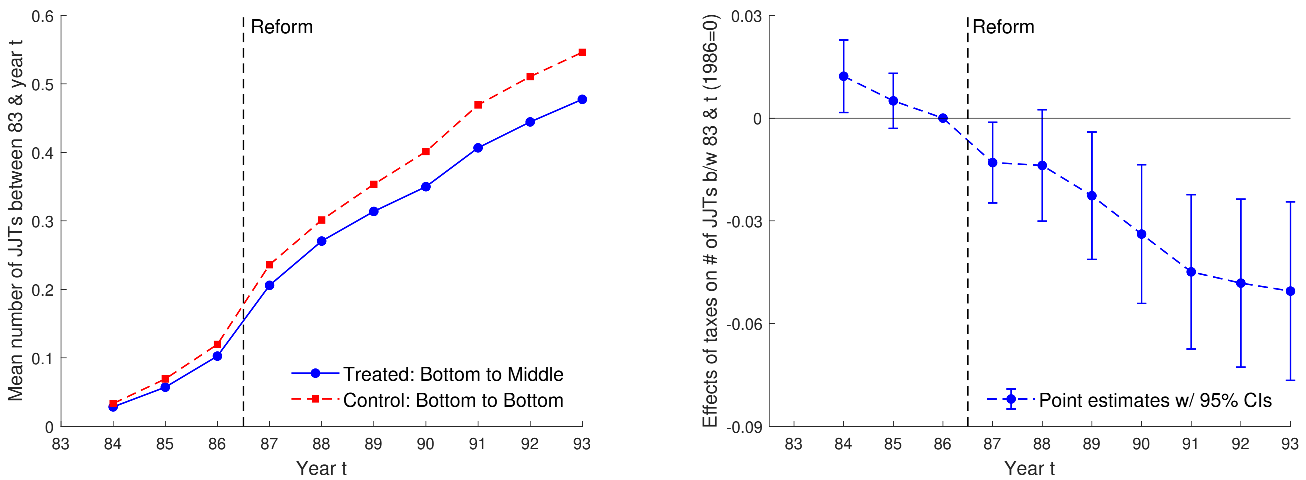
Notes: The figure presents wage responses by the four alternative low-income groups. Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panels plot $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panels plot the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 12: Human capital accumulation



Notes: The figure presents human capital accumulation by the low-income group (100,000 $\leq \text{LI}_{i86} < 150{,}000$). Outcome Y_{it} is the dummy variable indicating that worker i is skilled ($Y_{it} = 1$) or unskilled ($Y_{it} = 0$) in year t. The left panel plots $\overline{Y_{it}}$ for $t = 83, \ldots, 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). They are restricted to unskilled workers ($Y_{it} = 0$) between 1983 and 1985. The right panel plots the point estimates of β_t for $t = 85, \ldots, 93$ with their 95% confidence intervals from the fixed-effect (linear probability) DID model specified by (2). Standard errors are clustered at the individual level.

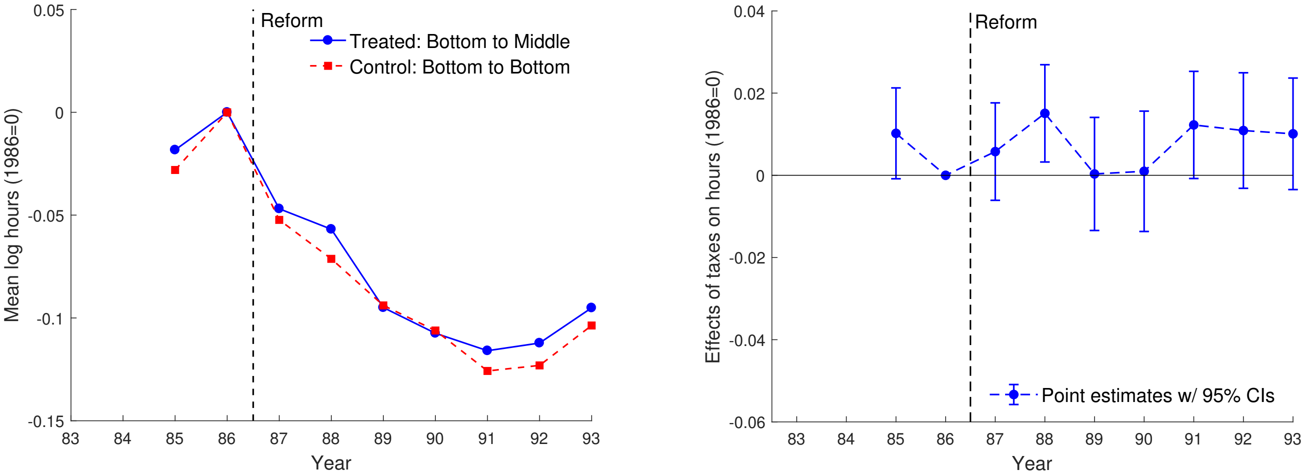
Figure 13: Job changes



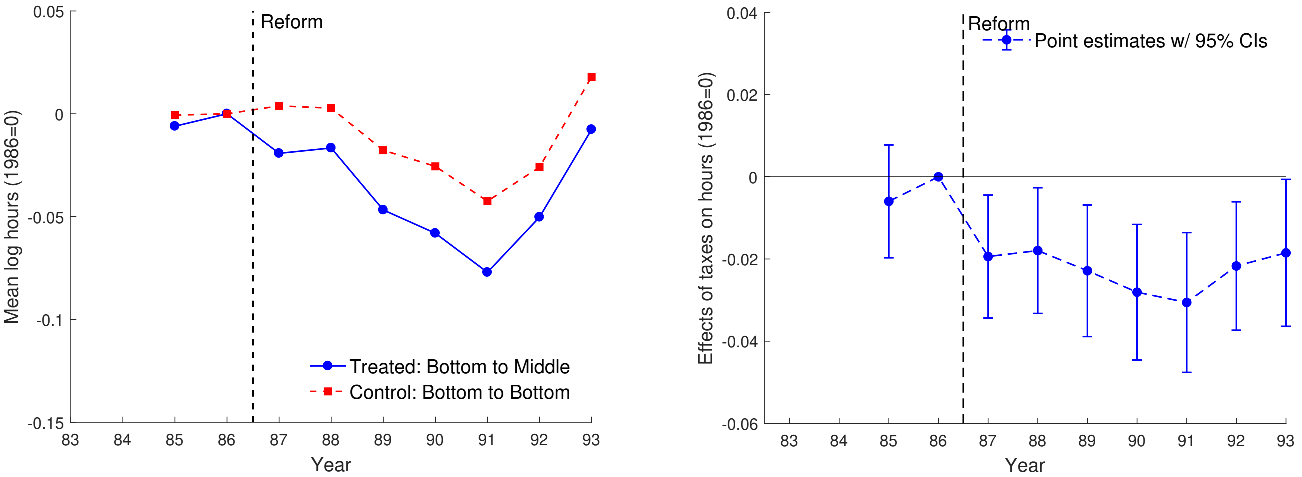
Notes: The figure presents job changes by the low-income group (100,000 $\leq \text{LI}_{i86} < 150{,}000$). Outcome Y_{it} is the number of job-to-job transitions (JJTs) between 83 and year t. The left panel plots $\overline{Y_{it}}$ for $t = 84, \ldots, 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right plots the point estimates of β_t for $t = 84, \ldots, 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 14: Labor supply responses by the full-time and part-time subgroups

(a) Full-time jobs in 1986



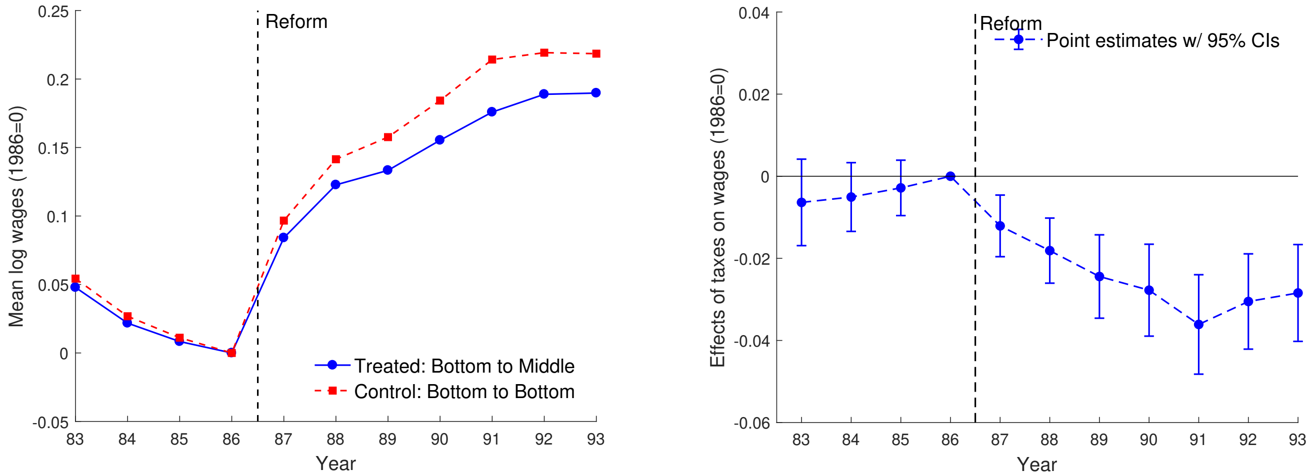
(b) Part-time jobs in 1986



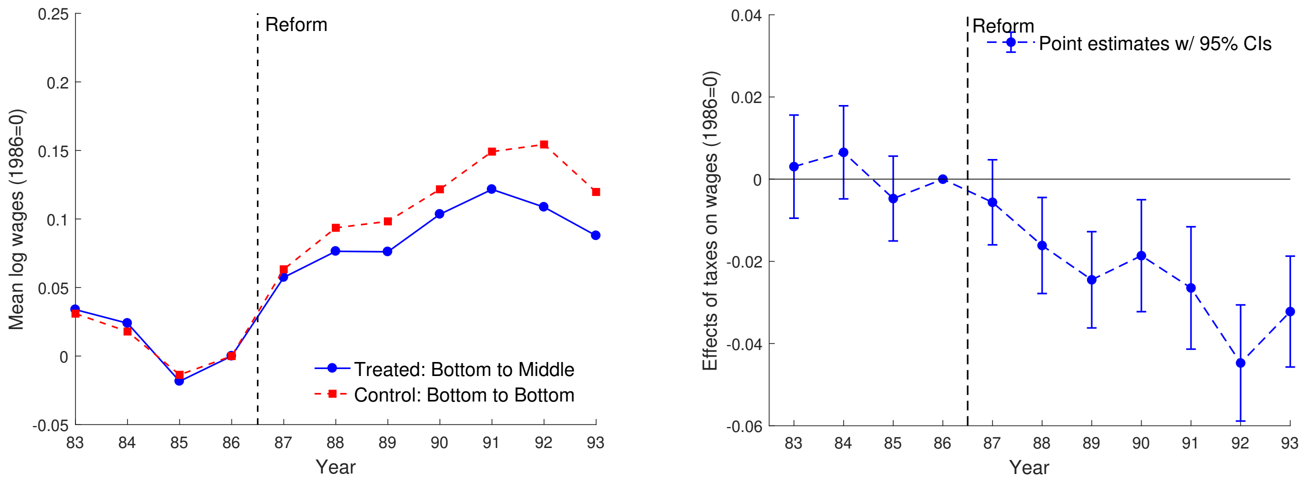
Notes: The figure presents labor supply responses by the full-time and part-time subgroups of the low-income group ($100{,}000 \leq \mathrm{LI}_{i86} < 150{,}000$). Full-time jobs are defined as working more than 30 hours per week. Outcome Y_{it} is the log of daily hours worked for a November job that individual i holds in year t. Daily hours worked are available only from 1985 due to data limitations. The left panels plot $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 85, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panels plot the point estimates of β_t for $t = 85, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Figure 15: Wage responses by the full-time and part-time subgroups

(a) Full-time jobs in 1986



(b) Part-time jobs in 1986



Notes: The figure presents wage responses by the full-time and part-time subgroups of the low-income group (100,000 $\leq$ LI_{i86} $<$ 150,000). Full-time jobs are defined as working more than 30 hours per week. Outcome Y_{it} is the log of real gross hourly wages for a November job that individual i holds in year t. The left panels plot $\overline{Y_{it}} - \overline{Y_{i86}}$ for $t = 83, ..., 93$ by treatment status, where $\overline{Y_{it}}$ denotes mean Y_{it} over i. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1). The right panels plot the point estimates of β_t for $t = 83, ..., 93$ with their 95% confidence intervals from the fixed-effect DID model specified by (2). Standard errors are clustered at the individual level.

Appendix

A Background of the 1987 tax reform

Like other Scandinavian countries, Denmark has a high tax burden. According to Kleven (2014), its ratio of tax revenue to GDP in 2012 was 48 percent, which is higher than that in other developed countries such as Germany (36 percent), the United Kingdom (35 percent), and the United States (25 percent). Denmark collects approximately half of its revenue from individual income taxes.

Before the 1987 reform, the Danish income tax system was characterized by high marginal tax rates and narrow tax bases. As the left panel of Table 2 shows, the top marginal tax rate was as high as 73 percent. Regarding narrow tax bases, as Table 3 shows, capital income is negative on average because of interest payments on debt, such as mortgage loans. Negative capital income thus narrowed the tax bases calculated as LI + CI – D.

Given these points, the reform was designed to broaden the tax bases and narrow the difference in marginal tax rates across the three brackets. First, it changed the tax bases of the middle and top brackets from LI + CI – D to LI + [CI>0] and LI + [CI>60k], respectively. The reform thus broadened the tax bases by reducing the tax value of negative capital income and itemized deductions. Second, Figure F.1 in Appendix F plots marginal tax rates on labor income (LI) as a function of LI before and after the 1987 reform. The tax rates and bracket cutoffs are listed in Table 2. For simplicity, we here assume single individuals with zero capital income and deductions (CI = D = 0). It is clear from the figure that the reform lowered the top and middle tax rates but raised the bottom tax rate, which narrowed the difference in marginal tax rates across the three brackets.[5]

B Overview of the tax simulator

This appendix explains the inputs of the simulator (including data sources), all income concepts necessary for the simulations, and the outputs of the simulator used for the empirical analysis.

B.1 Inputs of the tax simulator

The tax simulator takes as input information on the Danish income tax system (e.g., statutory tax rates) and information on individual income and demographic characteristics. The former information is primarily obtained from the website of the Danish Ministry of Taxation at `https://www.skm.dk`. The latter information is obtained from population-wide Danish administrative datasets. We refer to these

[5] Although the reform flattened the tax schedule, it was approximately ex-ante revenue neutral by introducing green taxes levied on the consumption of natural resources.

datasets by their filenames on the server of Statistics Denmark used for the computations (ECONAU project 707275 via Aarhus University). The filenames are INDK, INDH, and PERSONER.

The three datasets are annual panels constructed from registers including tax returns, cover all legal residents in Denmark aged 15–74 (on the 31st of December each year) since 1980, have a common individual ID, and contain a wide range of information. INDK and INDH contain administrative records on income tax assessments and public transfers, such as unemployment benefits; we use variables regarding individual income and joint taxation. PERSONER contains information on demographic characteristics; we use variables regarding the municipality of residence, marital status, and the ID of his or her spouse (if married).

We construct a dataset used for the simulations as follows. We first link INDK, INDH, and PERSONER using the individual ID and a year variable; thus, the unit of observation is person-year. We then create a variable regarding spousal income using the individual ID, his or her income, and the spousal ID. To this dataset, we next add information on regional taxes (e.g., statutory tax rates) using the municipality ID and a year variable. Information on national taxes is coded in the simulator.

B.2 All income concepts

The constructed dataset contains the individual-level precise measures of five income concepts necessary for the simulations. Table 1 lists three key income concepts in the Danish income tax system: labor income (LI), capital income (CI), and itemized deductions (D). For accurate simulations, we need two additional income concepts of minor importance for our sample. The first is personal income, which is labor income plus transfers minus pension contributions. The second is stock income, which is dividends and realized capital gains from shares (Kleven and Schultz, 2014). In the main text, we omit these two income concepts for ease of exposition.

B.3 Outputs of the tax simulator

Using all the necessary income concepts (LI, CI, ..., LI^{w}, CI^{w}, ...), demographic characteristics, and information on the Danish income tax system, we simulate bracket locations and effective marginal tax rates for individuals each year over the sample period 1983–1993. We describe and use the bracket locations in Section 3 to define treated and control individuals. We describe and use the effective marginal tax rates in Section 5 to compute elasticities. Finally, we link the dataset containing these input and output variables to IDA and job spell data using the common individual ID and a year variable (see Section 4 for IDA and job spell data).

C Distributions of pre-reform labor income among single males

Figure F.2 in Appendix F plots the kernel density estimates of pre-reform labor income LI_{i86} by treatment status among single males. Except for marital status, the sample is the same as in Figure 1. The dashed line represents a cutoff for the middle bracket under the inflation-adjusted 1987 tax system, which is consistent with $\widetilde{\text{M}}^{87}(z_{i86})$ in (1). Among single males, the two distributions do not sufficiently overlap because of a lack of variation to exploit; the treated individuals simply have higher LI_{i86} and thus are mechanically pushed upward to the middle bracket under the 1987 tax system, i.e., $\widetilde{\text{M}}^{87}(z_{i86})$. Figures 1 and F.2 make it clear that by exploiting the joint taxation and variation in wives' income, we can find treated and control individuals with overlapping distributions of LI_{i86}.

Note that it is challenging to control for pre-reform labor income LI_{i86} robustly when two distributions do not sufficiently overlap. In this case, linear regression relies on extrapolation and becomes sensitive to the specifications of control variables (Abadie et al., 2015; Imbens, 2015). For example, the literature on the elasticity of taxable income finds many estimates sensitive to the specifications of pre-reform income because researchers often compare a certain income group affected by a tax reform to an unaffected higher- or lower-income group (Saez et al., 2012).

D Computation of key outcome variables: wages and hours

Gross hourly wages. Gross hourly wages in IDA are computed as annual earnings from a November job divided by annual hours worked for that job. Note that labor income (LI) is the sum of annual earnings from November and non-November jobs, and primary and non-primary jobs.

Annual earnings (i.e., the numerator) are reported to the tax authorities by employers for income tax purposes and are subject to minimal misreporting and measurement errors. Annual earnings include regular pay, overtime pay, bonuses, vacation pay, and illness allowances, but not employer pension contributions.

Annual hours (i.e., the denominator) in IDA are estimated from annual pension contribution records (called ATP for "Arbejdsmarkedets Tillægspension") by exploiting the fact that mandatory employer contributions to a supplementary pension scheme depend only on hours worked by individual employees (Lund and Vejlin, 2016). Annual hours do not include overtime work, vacation, and periods of absence due to illness; among these missing components, overtime work will be the most important.

We argue that the lack of overtime work is not a serious concern for three reasons. First, Lund and Vejlin (2016) document that the estimated hourly wages are precise by comparing them to hourly wages obtained from another register called "Lønstatistik" (Wage and Salary Statistics). Second, for their sample from IDA in 2006, Hummels et al. (2014) have data on overtime work and document that annual hours including and excluding overtime work are highly correlated. Third, the lack of overtime work will pose a threat to identification in the DID design if overtime work correlates with both outcome dynamics (e.g., wage dynamics) and our instrumental variable (i.e., wives' labor income LI^{w}_{i86}); however, in Section 5, we provide evidence against this correlation by showing parallel pre-reform outcome dynamics and by conducting a placebo test.

Daily hours worked. Daily hours worked in the "November-job" data are computed as annual hours worked for a November job divided by annual days worked for that job. Annual hours (i.e., the numerator) are estimated similarly to annual hours in IDA (described above); indeed, they are highly correlated. Annual days (i.e., the denominator) are calculated from the start and end dates of each job.[6]

E Conceptual framework for the accumulating effects

This appendix section clarifies the meaning of the accumulating effects by constructing a simple theoretical model. Our model features human capital accumulation under income taxation.

The economy lasts for two periods ($t = 1, 2$). At the beginning of period one, a worker is on the job and has initial human capital k_0. Since our sample includes only males strongly attached to the labor market, we do not model decisions on labor market participation. Furthermore, hours of work are fixed and normalized to one, i.e., $h_t = 1$ for $t = 1, 2$.

Subject to income taxes, he invests i_1 amounts of money in human capital with a non-deductible cost $c(i_1)$. During period one, his human capital k_0 upgrades to k_1 following the law of motion $k_1 = F(i_1, k_0)$, where $F(\cdot, \cdot)$ is a human capital production function. Firm behavior and wage determination are exogenous (i.e., a partial equilibrium model); therefore, hourly wages are given by $w_1 = w(k_1)$. His net income at the end of period one is $(1 - \tau)w_1$ under a linear marginal tax rate τ.

He repeats the action (i_t) over two periods ($t = 1, 2$). We assume a zero discount rate and quasi-linear utility; hence, he has no incentive for private saving. Then, his problem at the beginning of period one is $\max_{i_1,i_2}(1 - \tau)w_1 - c(i_1) + (1 - \tau)w_2 - c(i_2)$ subject to $w_t = w(k_t)$ and $k_t = F(i_t, k_{t-1})$ for $t = 1, 2$. Under standard assumptions on $w(\cdot)$, $F(\cdot, \cdot)$, and $c(\cdot)$, the first-order condition stipulates $(1 - \tau)w'(k_t)\frac{\partial k_t}{\partial i_t} = c'(i_t)$, which provides the following intuitive comparative statics: $\frac{\partial i_t}{\partial \tau} < 0$. Higher marginal tax rates reduce incentives for investment by depressing returns from it.

[6] We cannot use IDA to compute daily hours worked because it does not contain the start and end dates of jobs.

The interest lies in wage dynamics when the government permanently raises the tax rate at the beginning of period one, i.e., $\frac{dw_1}{d\tau}\big|_{t=1}$ and $\frac{dw_2}{d\tau}\big|_{t=1}$. Simple algebra leads to

$$\left.\frac{dw_1}{d\tau}\right|_{t=1} = \underbrace{w'(k_1)\frac{\partial k_1}{\partial i_1} \cdot \frac{\partial i_1}{\partial \tau}}_{\text{distortion in } t=1} < 0$$

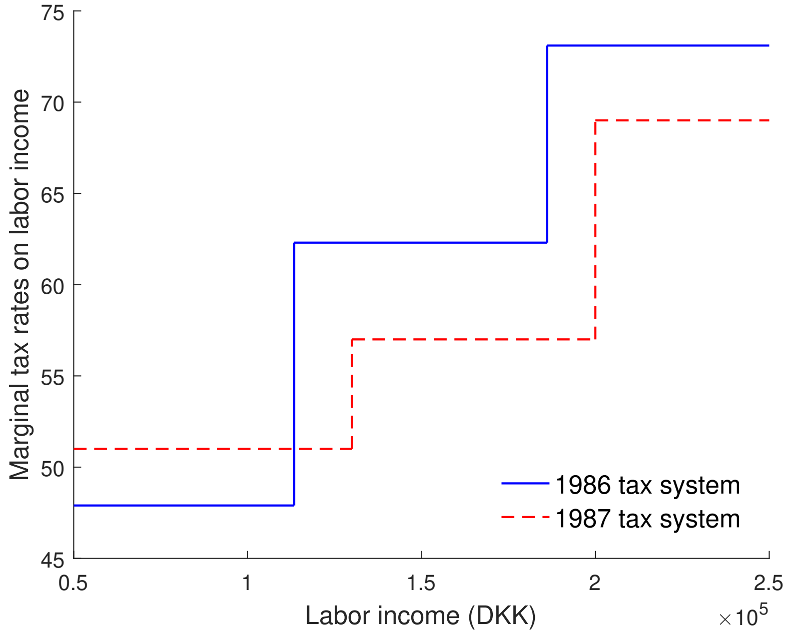
$$\left.\frac{dw_2}{d\tau}\right|_{t=1} = \underbrace{w'(k_2)\frac{\partial k_2}{\partial i_2} \cdot \frac{\partial i_2}{\partial \tau}}_{\text{distortion in } t=2} + \underbrace{w'(k_2)\frac{\partial k_2}{\partial k_1} \cdot \frac{\partial k_1}{\partial i_1} \cdot \frac{\partial i_1}{\partial \tau}}_{\text{distortion from } t=1} < 0.$$

The interpretation of $\frac{dw_1}{d\tau}\big|_{t=1}$ is straightforward: a worker reduces investment in period one, which creates a contemporaneous negative effect on wages. Let us move on to $\frac{dw_2}{d\tau}\big|_{t=1}$. The contemporaneous effect also emerges in the first term on the right-hand side. In addition, the negative effect created in period one carries over to period two, which is captured by the second term and potentially leads to accumulating effects, i.e., $\frac{dw_2}{d\tau}\big|_{t=1}$ is larger than $\frac{dw_1}{d\tau}\big|_{t=1}$ in magnitude.

We clarify three points. First, we can extend the periods to more than two. Second, we can include a decision on hours of work h_t with a cost $d(h_t)$. In this case, human capital accumulation can occur also through learning-by-doing by modifying the human capital production function as $k_t = F(i_t, k_{t-1}, h_t)$. Finally, k_t can be any capital that contributes to wages. One such example is a job ladder (k_t) with on-the-job search (i_t). In all cases, the insight still holds: distortion can accumulate on wages through investment repeated over multiple periods.

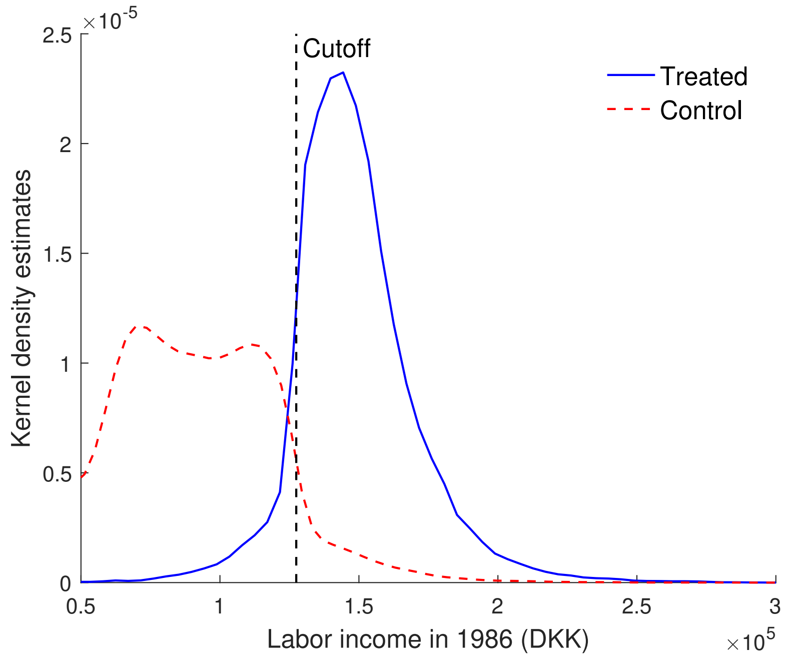
F Additional figures

Figure F.1: Overview of the 1987 tax reform



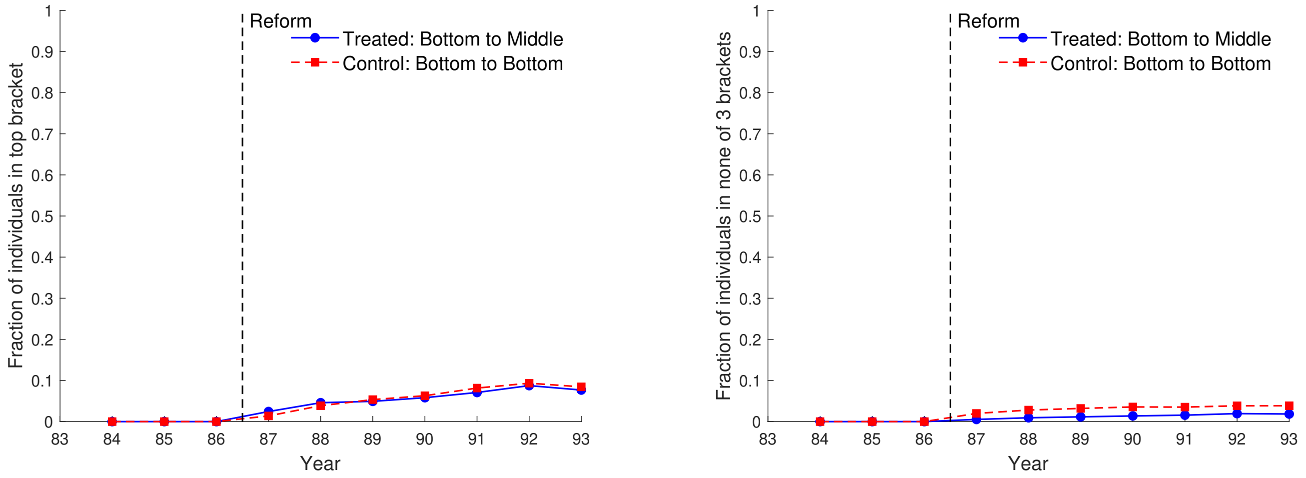
Notes: The figure plots marginal tax rates on labor income (LI) as a function of LI before and after the 1987 reform. The tax rates and bracket cutoffs are listed in Table 2. For simplicity, we here assume single individuals with zero capital income and deductions (CI = D = 0). DKK 1 in 1986 approximately equals USD 0.3 in 2016.

Figure F.2: Distributions of pre-reform labor income LI_{i86} (single males)



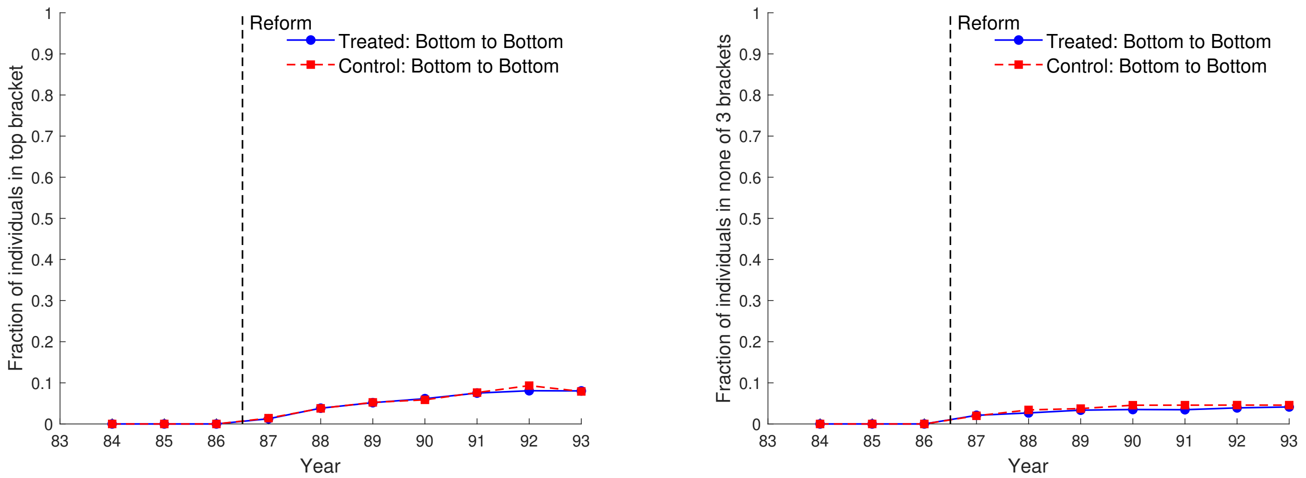
Notes: The figure plots the kernel density estimates of pre-reform labor income LI_{i86} by treatment status among single males. Except for marital status, the sample is the same as in Figure 1. The dashed line represents a cutoff for the middle bracket under the inflation-adjusted 1987 tax system. The estimation is based on a `ksdensity` function in MATLAB with default settings. DKK 1 in 1986 approximately equals USD 0.3 in 2016. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. The treated and control individuals are defined by (1).

Figure F.3: Bracket locations among the low-income group (other brackets)



Notes: The figure presents bracket locations among the low-income group (100,000 $\leq$ LI$_{i86}$ $<$ 150,000). The left (right) panel plots the fractions of individuals located in the top bracket (none of the three brackets, respectively) by treatment status. Bracket locations in 1983 are missing due to data limitations. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. The treated and control individuals are defined by (1).

Figure F.4: Bracket locations among the placebo group (other brackets)



Notes: The figure presents bracket locations among the placebo group. The left (right) panel plots the fractions of individuals located in the top bracket (none of the three brackets, respectively) by treatment status. Bracket locations in 1983 are missing due to data limitations. The sample is males who were (i) younger than 50 years old in 1986 and (ii) employed (on the 28th of November) in all the pre-reform years 1983–1986. Furthermore, (iii) they were married in 1986, and (iv) their wives had (strictly) positive labor income in 1986. They are restricted to the low-income group (100,000 $\leq$ LI$_{i86}$ $<$ 150,000). The placebo-treated and placebo-control individuals are defined by (4).