Labor Supply Responses and Adjustment Frictions: A Tax-Free Year in Iceland*

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Abstract

Labor income earned in Iceland in 1987 went untaxed. I use this episode to study labor supply responses to temporary wage changes. Using a population-wide dataset of earnings and working time and two identification strategies, I estimate intensive and extensive margin Frisch elasticities of 0.4 and 0.09, respectively. Workers with the ability to adjust drive these average responses: extensive margin by young and close-to-retirement cohorts and intensive margin responses by workers in temporally flexible jobs, though secondary jobs contribute to one-tenth of the response. The results suggest that adjustment frictions may similarly explain differences in elasticities within and across countries.

JEL Classification: E65, H24, H31, J21, J22.

Keywords: Intertemporal labor supply, Frisch elasticity, Adjustment frictions.

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1 Introduction

One of the longest standing questions in economics asks how workers adjust their labor supply in response to temporary changes in pay, as typically summarized by the intertemporal elasticity of substitution in labor supply, or the *Frisch elasticity*. This elasticity is pivotal for a wide range of issues, from understanding the drivers of cyclical movements in employment and wages (Lucas and Rapping, 1969) to determining optimal taxes on capital and labor income (Conesa, Kitao, and Krueger, 2009; Stantcheva, 2017) to evaluating the consequences of reforms to public policy (Imrohoroğlu and Kitao, 2012).

Problematically, obtaining a causal estimate of the Frisch elasticity is notoriously difficult, as it requires an exogenous transitory change in wages that generates limited income effects. These are hard to find. As a result, the quasi-experimental evidence remains scarce, but the consensus emerging from existing evidence is that the Frisch elasticity is about zero (e.g. Banerjee and Duflo, 2019). This would have defining implications for policy and our understanding of economic behavior. As the Frisch elasticity is an upper bound on the steady state (Hicksian) elasticity, a zero Frisch elasticity implies that there are no labor supply responses to any change in wages or taxes. Furthermore, rationalizing the relative cyclical movements in employment and wages hinges on intertemporal labor supply behavior. Indeed, macroeconomic models with labor market clearing require a relatively large Frisch elasticity—somewhere in the range of 2 to 4—to match the actual data (King and Rebelo, 1999).

In this paper, I shed new light on how workers respond to transitory wage changes. To do this, I exploit a tax reform in Iceland resulting in a year free of labor income taxes. As background, in 1986 the Icelandic government announced a tax reform, replacing the existing system whereby the current year's taxes were based on the previous year's income with a pay-as-you-earn withholding-based system. In the transition, and to ensure that workers would not have to pay taxes simultaneously on their 1986 and 1987 earnings, there were no taxes collected on 1987 labor incomes. As illustrated in Figure 1, the income earned in 1987 was then effectively tax free. This tax-free year created a strong and salient incentive for the intertemporal substitution of work, but a minimal income effect, for two reasons. First, there was no windfall gain for taxpayers, as those earning the same amount in 1987 as in 1986 did not discern any change in their cash flows. Second, the reform only implied a small change in lifetime income. Consequently, this tax-free year in Iceland offers a rare natural experiment for estimating the Frisch elasticity.

To exploit this experiment, I construct a new population-wide dataset using new data on the entire universe of workers and firms from pay slips stored at Statistics Iceland, which I convert into a machine-readable data set. Information on all pay and all working time in all jobs makes this an ideal data set to study labor supply behavior. Combining this with individual data from tax returns, I obtain a new employer-employee panel data set for the entire Icelandic workforce from 1981 until today. These rich data enable me to reveal the details of labor-supply adjustment along multiple dimensions.

In the analysis, I employ two complementary research designs to identify the labor supply elasticities along the intensive (i.e. working hours among those working) and extensive (i.e. employment and labor force participation) margins. First, building on seminal work in Feldstein (1995), I exploit cross-sectional variation in the size of tax cuts arising from the progressivity of the tax schedule. More precisely, while all workers were given a tax-free year in 1987, workers in a higher tax bracket receiving a larger increase in after-tax wages were expected to respond more strongly than those in a lower tax bracket. Relating these dose responses to the differences in the intensities of the marginal tax rate changes enables me to identify the labor supply elasticities. A key advantage of this design is the ability to difference out aggregate trends and macroeconomic shocks.

Using the tax bracket difference-in-differences design, I estimate responses in both labor income and working time, with an intensive margin earnings elasticity of 0.4 and a working time elasticity of 0.13. These results imply that a third of the overall response stems from additional weeks of full-time work. This includes transitions from part- to full-time employment, the exchange of vacation time for working time, and weeks worked in secondary jobs. The remaining two-thirds can be attributed to additional earnings within full-time weeks, encompassing overtime and increased work effort.

I also establish that the increased earnings reflect labor supply rather than reporting responses. First, I consider wage earners and self-employed workers separately, identifying larger earnings responses for the self-employed. The self-employed may have more flexibility in adjusting their hours. However, they might also be able to increase their tax-free income through misreporting. While the latter may explain some of the differences, the self-employed also have larger working time responses by the same magnitude, indicating that these differences likely reflect differences in flexibility. Second, my estimates cannot be explained by income shifting because discretionary payments, such as bonuses, make up less than 1% of the earnings effect. Third, my estimates are unlikely to reflect misreporting as there is no evidence of a reduction in reported capital income, despite capital income being (unlike labor income) subject to taxes in 1987. Finally, I document some additional circumstantial evidence of increased hours worked, such as in the form of a decline in sick leave hours.

As the tax-bracket research design exploits the variation in tax rates across groups of workers employed before the reform, by construction it cannot identify labor market entry. This is an important limitation as obtaining an estimate of the extensive margin elasticity is crucial for evaluating the aggregate response in hours worked to temporary changes in pay. If the labor supply is indivisible, temporary changes in wages or taxes can lead to large changes in aggregate hours through an adjustment at the extensive margin, irrespective of the hours' elasticity of those employed (Hansen, 1985; Rogerson, 1988; Rogerson and Wallenius, 2009).

To overcome these issues, I develop a research design that leverages two features of the current setting. First, as the tax reform was unanticipated, the timing of the tax-free year was plausibly exogenous from the perspective of an individual life cycle. Second, as the tax-free year led to a transitory increase in pay, by borrowing the intuition from the seminal paper of MaCurdy (1981), the labor supply elasticities can be identified by comparing two similar individuals, at the same point in their life cycles, but in a period when one receives an unexpected wage shock and the other does not.[1] As an example, I can estimate the extensive margin elasticity for a near-retirement 67-year-

[1] The idea of grouping individuals into cohorts on similar life-cycle trends to estimate labor supply elasticities originates

old individual in 1987 by matching that individual with another who is otherwise observationally similar, both in terms of characteristics and labor supply, but reached the retirement age of 67 in 1986 when there was no tax-free year.

Using the life-cycle research design, I estimate an extensive margin elasticity of 0.09. This average elasticity, which is rather modest relative to most comparable evidence (Chetty et al., 2013), masks important heterogeneity. The employment response originated almost exclusively among those close to retirement and cohorts younger than 25 years, still in school or out of the labor force for some other reason. For the prime-age population, those aged between 25 and 60 years, I estimate a zero elasticity.

Disagreement on the size of the Frisch elasticities is likely to reflect different views on the frictions that attenuate observed labor supply responses relative to unconstrained responses solely determined by preferences. Results from settings where workers are free to choose their hours worked—such as bicycle messengers in Zurich (Fehr and Goette, 2007) and taxi drivers in New York (Farber, 2015)—imply relatively large Frisch elasticities. Generalizing from these estimates and reconciling evidence across studies inherently relies on understanding how frictions distort the labor supply responses of the average worker. However, how important adjustment frictions are in shaping aggregate hours responses has remained elusive owing to limited direct evidence. In this regard, my analysis yields new insights.

I demonstrate that flexibility plays an important role in shaping labor supply responses, influencing both their magnitude and adjustment margins. Specifically, I find that the intensive-margin responses are strongest among workers in jobs with greater temporal flexibility, i.e. those with a priori superior ability to adjust their hours, and those with labor market contracts that build in compensation for marginal hours worked. Less likely bound by constraints in hours, these workers can receive compensation for any additional hours worked in their primary jobs. In addition, individuals with less labor market attachment working less than full time before the reform—including younger cohorts and workers close to retirement—are the most responsive and drive the extensive margin response. The largest responses are therefore concentrated among precisely those groups predicted by theory, a pattern for which existing evidence has hitherto been limited.

Even when working hours in a specific job are rigid, individual workers may increase their hours by changing jobs or holding multiple jobs. I document increased work in secondary jobs during the tax-free year. This response is driven by workers constrained in their primary jobs, in line with the theoretical predictions (Shishko and Rostker, 1976; Paxson and Sicherman, 1996). When decomposing the overall intensive margin response, I find that one-third of the increase in weeks worked and one-tenth of the total increase in labor earnings stem from work in secondary jobs. The remainder results from increased hours and earnings in continuing primary jobs.

My estimates of average Frisch elasticities most closely relate to those in two existing studies.[2] First, Bianchi, Gudmundsson, and Zoega (2001) consider labor supply during the same tax-free year,

in the pioneering work of Ashenfelter (1984) and later employed in Angrist (1991) using a grouping instrumental variables approach. I thank Josh Angrist for pointing this out. The method employed in the current paper differs from these earlier studies by combining cohort grouping and a natural experiment, where the former generates comparable groups on similar life-cycle trends and the latter provides the identifying variation.

[2]Section 7 provides a detailed summary and meta-analysis of previous work.

but only among a small random sample of workers.[3] They find strong responses by comparing outcomes during the tax-free year to those in the preceding and following years. When translated to an elasticity, their estimates imply an average intensive-margin elasticity of 0.77. This is twice as large as my estimate. On this basis, I conclude that it is important to isolate responses to the tax-free year from the influences of pre-trends and macroeconomic shocks.

Second, in a contemporaneous study, Martinez, Saez, and Siegenthaler (2021) analyze the labor supply responses to a two-year tax holiday in Switzerland. This tax holiday resulted from the transition from an income tax system where current taxes depended on income in the previous two years to an annual pay-as-you-earn system. They estimate an average intensive margin elasticity of 0.025 and identify no extensive margin responses. My results suggest that all of the employment responses and a disproportionate proportion of the hours and earnings response to temporary tax cuts arise from young first-time workers and those close to retirement. Both groups are excluded from the analysis in Martinez, Saez, and Siegenthaler (2021). The remaining differences likely arise from differences in labor market flexibility. Flexibility in working hours, as measured by the cyclicality of hours per worker, is more than twice as high in Iceland as in Switzerland, and worker flows—measuring the fluidity of the labor market—more than three times as high. Other measures of labor market flexibility tell a similar story: the flexibility of the Icelandic labor market is much more similar to that of the US labor market than to continental Europe. I document a positive correlation between the flexibility of working hours and the size of the Frisch elasticity, both across occupations and across countries where estimates are available, indicating that similar forces are at work in shaping differences in labor supply elasticities within and across countries.[4]

The paper proceeds as follows. Section 2 describes the empirical setting and the reform that gave rise to the tax-free year while Section 3 describes the data set constructed. Sections 4 and 5 document the labor supply responses at the intensive and extensive margins using two complementary research designs, respectively. Section 6 analyzes and illustrates how heterogeneous adjustment frictions shape these labor supply responses. Section 7 discusses the Frisch elasticity estimates and places them in the context of existing work. Section 8 concludes the paper. I relegate some additional background material and auxiliary analyses to an online appendix.

2 The Tax-Free Year and Background

2.1 Income Tax System and Tax Reform

On January 1, 1988, Iceland adopted a withholding-based pay-as-you-earn income tax system, similar to what is now in place in most advanced economies. Prior to the reform, income taxes were collected

[3]Other studies of the Icelandic tax-free year include that by Ólafsdóttir, Hrafnkelsson, Thorgeirsson, and Ásgeirsdóttir (2016), who study the health consequences of increased work during the tax-free year, and that by Stefánsson (2019), who studies labor supply and reevaluates the evidence in Bianchi, Gudmundsson, and Zoega (2001).

[4]In Sigurdsson (2023a) I study a tax holiday in Norway and present evidence that further corroborates this notion. I document that 80% of the working-age population was aware of the tax holiday and a quarter responded by increasing their labor supply. Furthermore, for those aware of the tax holiday but did not adjust their hours, the majority cited friction in adjusting the time spent working, broadly speaking, as the main reason for not responding.

with a one-year lag, with the tax liability and tax payments due every month in year t computed based on year $t-1$ income. This system resembled those in place in most developed countries before adopting a modern pay-as-you-earn tax system. When announcing the tax reform, the authorities also announced that labor income earned in 1987 would be untaxed. As Figure 1 depicts, this implies that while people were paying taxes every year, including in 1987 when they paid taxes based on their income earned in 1986, they would take home tax-free whatever they earned in 1987 that was above and beyond what they had earned in 1986.[5]

The key features of the reform for the purpose of my analysis are that it generated a large, salient and unanticipated increase in wages that lasted only a single year. On December 6, 1986, the Finance Minister announced the tax reform. The Ministry of Finance had begun preparing the reform in early fall 1986 and later that same fall there was the decision for it to take place in January 1988. The reform was therefore unanticipated by taxpayers. Figure 2 plots the monthly count of the number of newspapers mentioning a withholding-based or pay-as-you-earn tax system between January 1980 and December 1988. As shown, there was no discussion of a reform of this kind in the years before its announcement, whereas 30–40% of the newspapers printed in the weeks following the announcement had coverage of the reform.[6]

The reform was very salient. Newspapers printed headlines such as "*A Tax-Free Year*" and "*Pay-As-You-Earn Tax System In 1988 – All Income In 1987 Tax-Free*", and in the media, politicians and union leaders emphasized the opportunity that the tax-free year would bring.[7] In addition, the tax authorities sent out advertisements and explanatory flyers, as exemplified in Appendix Figures A.3 and A.4. These also advertised that a prerequisite for tax freedom was that workers filed their taxes for 1987 as usual. This was important, as other taxes such as those on capital income and wealth, and benefits, were unchanged in 1987. From the perspective of my study, the quality of administrative data in 1987, such as tax returns, was uninfluenced by the reform.

The tax-free year generated a strong incentive for intertemporal substitution. The average tax rate fell to zero from about 10%, increasing the incentive for employment (the extensive margin). At the intensive margin, the changes in incentives were even stronger, as the after-tax wage increased by about 20% on average. However, while the whole population received an increase in wages, some workers received a larger tax cut because of the progressivity of the Icelandic tax system. Furthermore, the tax-free year did not create an income effect for individuals who were myopic in their decision-making in that there was no windfall gain for taxpayers, as those earning the same in 1987 as they earned in 1986 did not see any change in their cash flows.[8]

The only change to the tax system made in 1987 was that income taxes were temporarily set to zero. However, the reform was accompanied by a simplification of the tax system that was put in place after the tax-free year. These changes were being worked out during the first months of 1987 as part of adapting the old tax system to tax withholding. The simplifications consisted of two main

[5] Appendix A details the Icelandic tax system before and after the tax-free year.

[6] Further discussion of the reform and the timeline of events is in Appendix B.

[7] In an interview, the chairman of one of the largest labor unions was quoted as saying: "*Now it is time for everyone outside the labor market to enter, and for all workers to earn tax-free income. There exists work for everyone who wants to work.*"(see *Morgunblaðið*, December 7, 1986.).

[8] Similarly, the reform did not influence the government budget, as the tax revenue flows were uninterrupted.

changes. First, the reform abolished a large share of tax deductions. Second, a flat tax replaced the progressive tax schedule. To summarize, the reform changed both the tax base and the tax rate, the aim being to simplify the tax system, but leave the average tax burden unchanged.

I argue that these changes are unlikely to influence the responses to the tax-free year and the estimates of the Frisch elasticities. The effects on later taxes were not as obvious and clear-cut as the tax-free year. Understanding the effect on tax payments would involve understanding the interaction of tax deductions, tax allowances, and tax rates that influenced the tax burden in opposing directions. Relatedly, these changes were much less salient than the tax-free year. Figure 2 provides evidence that a change to a flat tax received limited media attention. Moreover, explanatory material from the tax authorities emphasized that income in 1987 was tax free and showed the changes in the structure of tax collection in 1988, but contained no information about changes in the tax schedule. As discussed in Section 4, I perform a series of tests to evaluate this claim, finding the results to be robust.

2.2 Icelandic Labor Market in an International Context

The Icelandic labor market is quite flexible, characterized by low unemployment, flexible hours, and variable participation and wages (OECD, 1991, 2007).[9] In this sense, its characteristics are more similar to the US than to continental Europe (Central Bank of Iceland, 2018). This flexibility has long played a key role in the rapid adjustment of the Icelandic macroeconomy to shocks.[10]

Labor force participation in Iceland is high, exceeding 80% of the working-age population. The overall participation grew steadily until the mid-1980s, primarily because of the increased participation of women, who by the beginning of the 1990s accounted for close to half of the labor force, although a smaller share of total hours. Relative to other OECD countries, female participation in Iceland is among the highest, as are participation rates among the young and elderly.

Icelandic firms also have considerable flexibility in laying off workers when compared with firms in other OECD countries. Firms can easily adjust their level of labor input over the business cycle, either by hiring and firing workers or by adjusting the number of hours of current employees. The latter margin is important, as evidenced by changes in hours per worker accounting for about half of the variation in employment over the business cycle.

Nonetheless, the Icelandic labor market is highly unionized. Collective bargaining between the umbrella unions on both sides of the market decides general employee rights and minimum wages. However, this sets the base for wage bargaining at lower levels, such as in sectors and firms, where the flexibility to account for local conditions is greater. Therefore, despite this centralization, real wages are very flexible in Iceland when compared with many other European countries.

[9]For an overview of the Icelandic economy, including the characteristics of the labor market, see e.g. (Central Bank of Iceland, 2018) and various previous issues of *Economy of Iceland*.

[10]As an example of this emphasis, the Director of the European Department of the International Monetary Fund (IMF) noted in a recent speech that "*Iceland had a history of quickly adjusting to shocks, not least because of labor market flexibility.*" (Thomsen, 2018).

3 Data

For this project, I construct a new administrative data set for the universe of the Icelandic working-age population back to 1981. The data set has two main sources: an employer–employee data set constructed from newly digitized pay slips, and individual tax records. I describe these below.[11]

3.1 Pay Slips: Employer–Employee Data

At the end of each year, all employers are obliged to compile a pay slip for each employee of their establishment or for every job if the employee holds more than one job at the same establishment. This applies to all firms and establishments, including self-employed workers. Employers send copies of these pay slips both to the respective employee and to the Directorate of Internal Revenue. Information from pay slips then serves as inputs for many purposes, such as for individual income taxation, the computation of accident insurance and the computation of firm payroll taxes.

Since the early 1990s, an increasing number of employers compile and send pay slips to the Directorate of Internal Revenue in a machine-readable format, and currently almost all are electronic. Before that time, in the 1980s and the early 1990s, all pay slips were in paper format. The records were then stored in various forms, including on magnetic tape cartridges and mainframe tapes. In collaboration with Statistics Iceland, I converted all pay slips back to 1981 into data in a machine-readable form. The resulting product is a panel data set covering the universe of jobs in Iceland, connecting all employers and their employees, for each year from 1981 to 2015.

Pay slips contain information on all labor earnings and related compensation. This includes wage payments, contractor payments, piecework pay in fishing, pension payments, bonuses and commission, remuneration to a company's board members and accountants, travel allowances and other allowances (car, clothes, food, etc.). Each of these components is on a separate pay slip for a given job. In addition, and importantly for the current project on labor supply, the pay slips also contain information about working time in each job. Time is in weeks worked, with the reference week amounting to 40 working hours. Employers are obligated to report the number of weeks employees worked on a given job based on their actual working time during the year and employment arrangement, such as part-time employment. The same is true for self-employed workers, who must report working time in the same way for themselves as well as for their spouses and any children who may work for them. A worker can at most be recorded working 52 weeks on a given job during the year. However, workers can hold more than one job, and therefore be registered as working more than 52 weeks in a year. For example, a full-time employee holding a single job and working at least 40 hours per week is recorded as working for 52 weeks. Elsewhere, another worker holding two part-time jobs working 20 hours per week in parallel would be recorded as working 26 weeks in each job (reported separately) and 52 weeks in total.

The reason why employers (and self-employed workers) were required to report the working time of their employees was twofold. First, the calculation of a worker's accident insurance fees depended on the number of weeks an employee worked during the year. Second, the payroll tax levied on firms

[11] Further details about the data, including summary statistics, are in Appendix C.

to fund the public unemployment insurance system hinged on the total number of weeks worked by all workers in a given firm each year. Therefore, weeks registered in pay slips reflected the number of weeks worked during the year rather than the number of weeks employed. In addition, these are the only universal data on labor input by sector and occupation for which official statistics are constructed, which places further pressure on their correct filing.

Lastly, each pay slip includes a unique personal identifier of the worker and a unique firm identifier. In addition to the detailed information on payments and working time, pay slips also include demographic and structural information, such as on workers' occupations and firms' sectors.

3.2 Individual Tax Returns

The second primary data source I use in this paper is a panel of individual tax returns. As is the case for the data set constructed from pay slips, these data extend back to 1981, with the data sets easily linked via the unique personal identifier. Individual tax returns have information on all income, including labor income, financial income, pension, social security, and transfer payments as well as other sources of income. These data also record all tax payments, both at the national and local levels, as well as any deductions and tax allowances. I use these detailed data to construct the marginal tax rates.[12] Because Iceland levied a wealth tax during most of the sample period, in periods when a wealth tax was not levied, the structure of tax returns has not been altered and the data set includes detailed information on all assets and liabilities back to 1981. In addition, the tax records include a range of demographic variables, as well as family identifiers linking married or cohabiting couples.

4 The Intensive Margin

4.1 Research Design

In general, identifying the causal effect of the tax-free year on labor supply requires a proper counterfactual of what would have happened in its absence. Alternatively, if the population is treated with different 'doses' of tax cuts, causal effects can be identified from the differential treatment intensity, provided they generate differential responses. In the current context, while the entire Icelandic population was given a tax-free year in 1987, nonlinearities in the pre-reform tax schedule generated substantial differences in the changes in after-tax wages. I exploit these features in a difference-in-differences (DID) research design, relating the intensity at which workers' after-tax wages were influenced by the tax-free year and the dose-response in labor supply.

The tax schedule before the reform was progressive with four brackets, consisting of three national-level brackets and a local-level municipal tax. Figure 3, panel (a), plots the evolution of tax rates by tax brackets during the 1980s. In 1986, taxpayers in the bottom bracket living in Reykjavik faced a marginal tax rate of 10.2%, corresponding to the municipal tax rate, while taxpayers in the top bracket faced a marginal tax of roughly 48.7%. The municipal tax rate ranged between 5% and 11.5%,

[12] As the marginal tax rates are not directly observed in the individual tax returns, I build a tax calculator for the Icelandic tax system to construct marginal tax rates (Appendix C.1). This method predicts actual tax liabilities with great precision.

averaging at 9.6%. As the figure depicts, while tax rates had been on a slightly decreasing trend, the difference across brackets had remained stable. Tax rates were frequently reviewed in relation to the government's budget and tax-bracket thresholds, which were set in nominal values, were generally reviewed and updated each year to account for inflation. As Figure 3, panel (b), documents, this resulted in tax-bracket thresholds corresponding to roughly the same income percentile throughout the 1980s, and therefore the income groups in each bracket were stable and similar over time.

Assigning treatment status. The empirical strategy used to estimate the elasticities is to relate the differential labor supply responses of workers in higher vs. lower tax brackets to their differential tax relief. As the tax rates faced each year are endogenous to labor income, which is the outcome of interest, I follow Feldstein (1995) and later work by assigning treatment status based on a lagged tax bracket, which is unrelated to current income. Given that the income and other factors influencing the tax bracket position are persistent from one year to the next, the tax bracket position is also persistent. This persistence is documented in Appendix Figure A.5. As a result, a lagged tax bracket serves as a valid and strong instrument for the current tax bracket.

Sample and restrictions. To analyze a sample of comparable workers facing different tax rates, I restrict the sample of the working-age population (those aged 16–70 years) in two ways. First, I employ a balanced sample of individuals observed in all years from 1981 to 1987. As everyone aged 16 years and older is required to file taxes, independent of their labor market status, this excludes workers who die, those who emigrate from Iceland, and young people not observed during the pretreatment period and for whom I cannot assess the trends in labor supply. Second, for each of the pre-reform years, I restrict the sample to workers with labor earnings greater than or equal to the base income threshold, roughly corresponding to the lowest minimum wage earnings for a low-skilled worker according to collective bargaining agreements.[13] Restricting the sample in this way corresponds to restricting the sample to those with labor income above the 20th percentile, including zeros, leaving a sample of workers in one of the four brackets (see Figure 3), excluding workers paying taxes primarily on capital income.

Estimating equation. I estimate the reduced-form labor supply responses to the tax-free year using the following DID regression specification

$$y_{it} = b_{i,t-1} + \delta_t + \eta \cdot B_{i,t-1} \cdot \delta_{t=1987} + \boldsymbol{X}'_{it}\gamma + \mu_{it} \tag{1}$$

where y_{it} is the outcome of interest of individual i in year t, $b_{i,t-1}$ is an indicator function for tax brackets in year $t-1$, and δ_t are time fixed effects included to control for time effects affecting all individuals. The identification of the labor supply response to the tax-free year is brought by η, the coefficient on the interaction of the treatment status $B_{i,t-1}$, which is an indicator of being in a high tax bracket in a given comparison, interacted with a dummy for the tax-free year of 1987. Depending

[13]Similar restrictions are frequently imposed in studies of the core labor force, see e.g. Kindlund and Biterman (2002). The base income threshold equals $1.6\times$ guaranteed income (*tekjutrygging*), where guaranteed income is a reference amount used in calculations of various kinds for income support provided by the government and municipalities, such as for the elderly and disabled. Using the guaranteed income as a reference point has the advantage, when compared with, e.g. minimum-wage earnings by sectors and occupations, of being updated each year to account for inflation.

on specification, $B_{i,t-1}$ is an indicator for being in one of the top three tax brackets. I estimate this equation using data for 1986 and 1987, except when evaluating pre-trends. The regression controls for individual characteristics, collected in the vector $\boldsymbol{X}_{it}$, which includes individual characteristics such as gender, age, education, marital status, number of children, an indicator for living in the capital area, and occupation, all defined in pre-reform levels. The error term is denoted by μ_{it} and captures other determinants of the labor supply. I cluster standard errors at the tax-bracket by municipality level, i.e. at the level of the identifying variation. This allows for arbitrary correlation across workers subject to the same tax rates.

To obtain an elasticity estimate, I relate differential labor supply responses (i.e. the dose response) to the differential increase in the after-tax wage generated by the tax-free year. Intuitively, in its simplest form, the elasticity estimate corresponds to the Wald estimator, which is the ratio of the reduced form and first stage, which can be obtained by estimating equation (1) with the tax rate as the outcome. Following this logic, I employ the following two-stage least squares (2SLS) regression specification

$$y_{it} = b_{i,t-1} + \delta_t + \varepsilon \cdot \log(1 - \tau_{it}) + \boldsymbol{X}'_{it}\gamma + \nu_{it} \tag{2}$$

where τ_{it} is individual i's marginal tax rate in year t. Instrumenting $\log(1-\tau_{it})$ with the reduced-form interaction $B_{i,t-1} \cdot \delta_{t=1987}$, the coefficient ε identifies the elasticity to a change in the net-of-tax wage.

4.2 Results

Graphical evidence and validity of identifying assumptions. The first key identifying assumption underlying the empirical design is that the labor supply of workers across the tax-bracket distribution would have run parallel in the absence of a tax-free year. To formally test the plausibility of this assumption, I estimate a version of the DID regression (1), where I interact the treatment status with a full set of time dummies for the years 1982-1988, normalized to zero in 1986. The results are presented in Figure 4. Panel (a) presents estimates separately for workers in each of the three top tax brackets where workers in the bottom tax bracket serve as the control group. While no group is unaffected by the reform, the bottom bracket is least affected and therefore serves as the natural control group (Sun and Shapiro, 2022).[14] Panel (b) presents estimates where the treatment group consists of workers in one of the three top tax brackets, providing a weighted average of their responses. The set of pre-reform coefficients tests for constant trends in labor income across pairs of tax brackets, with each coefficient corresponding to a placebo test for the given year.[15] The tests indicate that trends in labor incomes were not statistically significantly different across brackets in the years before the tax-free

[14] Sun and Shapiro (2022) emphasize that settings that exploit differences across units in exposure to a policy as identifying variation in a two-way fixed effects model can fail to identify the average effect of the policy if there is unmodeled heterogeneity in effects across units. As a solution, they propose to always use one unit that is closest to or completely unaffected by the policy as the common control group for all affected units in a DID setting. This results in an estimate that is centered around the average treatment effect across the affected units.

[15] This approach is similar in nature to Jakobsen and Søgaard (2022) who compare trend differences in income across affected and unaffected parts of the income distribution in a study of long-run effects of tax reforms.

year and the coefficients are small relative to those for the tax-free year, in most cases close to zero.[16] The graph also presents a difference-in-differences estimate for the post-reform year 1988, where, as in 1987, individuals are assigned to tax brackets based on their bracket position in 1986. While this estimate should not necessarily be viewed as a placebo test, the size of this coefficient influences the interpretation of the elasticity estimates in two ways. First, the reform may have had persistent effects on the labor supply that extended beyond the tax-free year itself. As the Frisch elasticity measures temporary labor supply responses to a transitory increase in wages, long-lasting responses would call into question the interpretation of the estimated elasticity. Second, the changes that were made to the tax system in 1988 may have affected the labor supply from 1988 onwards. If so, one might worry whether these changes, which, as described in Section 2, were announced during 1987, may have influenced responses to the tax-free year. I find no differences across brackets in labor supply in 1988, alleviating these concerns.

The second key identifying assumption is that differences in labor supply responses across tax brackets are proportional to their differences in changes in tax rates. Figure 4 reveals some non-linearity in responses, showing that changes in labor earnings of the upper-middle bracket are almost as large as those of the top tax bracket, despite facing about 10 percentage points less cut in taxes. However, these estimates do not account for differences in the composition of workers across brackets in terms of demographics and characteristics. Suppose workers in a high bracket respond systematically differently to a given tax cut than those in a lower bracket, e.g. due to differences in preferences or frictions in adjusting labor supply. In that case, this heterogeneity will lead to a bias in the estimated elasticity. To account for the heterogeneous composition of workers across brackets, I match workers across brackets on demographics and pre-reform characteristics, weighing the groups to reflect the same distribution of characteristics. In practice, I use nonparametric coarsened exact matching (Iacus, King, and Porro, 2012) to coarsely match individuals across brackets by gender, age in 5-year intervals, three groups of education, marital status, and number of children (0, 1, or 2 or more children). I drop individuals with characteristics that do not belong to the common support of characteristics across brackets and weigh the groups being compared to account for differences in strata sizes.

Under the homogeneity—or linearity—assumption, the relationship between the reduced form and the first stage should be constant (linear) across pairs of tax brackets and the implied labor supply elasticities be the same. To evaluate this assumption, I perform the matching described above for each of the six pairs of tax brackets and estimate weighted difference-in-differences. I present the results in Figure 5. Panel (a) plots the relationship between the reduced-form and first-stage estimates. The results support the dose-response assumption: the figure shows a linear relationship between the reduced-form and first-stage estimates. The regression line through the six pairs of estimates, which

[16] Figure 4 demonstrates how this identification strategy is useful in dealing with possible effects of macroeconomic shocks. Following a spiral of inflation in prices and wage, fueled by oil price increases and foreign inflation, combined with wage indexation, inflation reached its historical record high in 1983 of more than 80%. In response, the government passed a law banning wage indexation and deflated the exchange rate (Snævarr, 1993). What followed was a sharp recession. Documented in Appendix Figure A.9, GDP growth was negative in 1983, for the first time since the 1960s, but reverted back in 1984. Figure 4 highlights the importance of the DID design, as it is mostly able to 'difference out' the effects of this sharp macroeconomic shock.

I also plot in the figure, has an intercept of approximately zero (-0.03) and a slope of 0.58. This implies that increasing the reduction in marginal tax rates by 10 percentage points increases the labor supply response by 5.8 percentage points. Panel (b) of Figure 5 plots the elasticity estimates for each of the six pairs of tax brackets. In further support of the dose-response assumption, the elasticity estimates are broadly in line and not statistically different from each other. The figure also includes two other sets of estimates. First, an estimate where the treatment group consists of workers in one of the three top tax brackets and the control group is the least-affected bottom bracket, as in Figure 4, panel (b), and second, a weighted average of all of the six tax-bracket pairs, using all of the identifying variation available. The two elasticity estimates are both 0.4 and virtually indistinguishable. Together with the evidence presented in Figure 4, this evidence supports the validity of the empirical strategy as well as the choice to compare workers in the top three tax brackets to the bottom bracket as the main comparison, which I will follow in much of the subsequent analysis.

Regression results. Table 1 presents estimates of the effects of the tax-free year on labor income, weeks worked, and employment. Each column-by-row entry in the table corresponds to a regression estimate. First, the top row of column (1) provides estimates of the elasticity of labor income, estimated using a 2SLS estimation of equation (2). The elasticity estimate is 0.434 and is highly statistically significant at the 1% level.[17] This estimate implies that a 10% increase in the after-tax wage causes labor earnings to increase by roughly 4% on average. Conceptually, the elasticity estimate consists of two components. First, the reduced form, presented in the third row, is a DID estimate of equation (1) on log labor income, which is estimated to be 0.077. Second, presented in the middle row, is the first stage, which is a similar DID estimate where the outcome variable is the log net-of-tax rate, estimated to be 0.207. The elasticity is essentially the ratio of the reduced form to the first stage, but here estimated using 2SLS. As emphasized and illustrated above, it is important to account for heterogeneity in demographic characteristics across tax brackets. When doing so in column (2), I obtain an elasticity estimate of 0.407.

Next, I estimate the effect on annual weeks worked. Figure 6, panel (a), plots the DID estimates for the tax-free year as well as estimates for the years before and after. As for labor income, the figure reports a strong response in weeks worked, implying that workers in the top three tax brackets increased their working time by roughly one additional week compared to those in the bottom bracket. The corresponding regression estimates are presented in columns (3) and (4) of Table 1. The treatment effect, estimated using 2SLS, is between 5.4 and 6.6 additional weeks, implying an elasticity of 0.11-0.13 when scaled by the pre-reform average (49.37 weeks).[18]

It is important to highlight what these results imply and what to expect. As discussed in Section 3, the working time recorded on the pay slips is in terms of weeks worked. This reflects time spent working, not the duration of employment, with a standard week corresponding to 40 hours. The caveat is that the cap is 52 weeks per job. Workers can be recorded as working less than 52 weeks in a given year, e.g. if not working all weeks of the year or if working part-time, i.e. less than 40

[17] Appendix Table A.4 evaluates the robustness of the size of standard errors to alternative clustering. This table reports that the statistical precision of the estimates is robust to substantially coarser geographical clustering, including a nine-region level, as well as clustering at the age by tax-bracket level.

[18] This implied elasticity is similar to that obtained from a specification for logarithms of weeks worked, which is 0.137.

hours per week. However, they can be recorded as working more than 52 weeks only if they hold more than one job. Therefore, an additional week reflects the exchange of vacation for working time, more full-time employment, and work in secondary jobs. However, this measure does not capture overtime and other changes in working time beyond the 40-hour work week, which in Iceland is an important margin for labor adjustment.[19] The increase in weeks worked therefore most likely reflects a lower bound of the total hours adjustment to the tax-free year, which are captured in full in the earnings response. With this in mind, the estimated labor supply responses can be decomposed into two components. The estimates imply that about 33% of the overall response is brought about by more weeks worked—through less vacation time, more full-time employment, and secondary jobs—and the remaining 67% by more earnings within those weeks, through overtime hours and greater work effort.

Finally, I estimate the effect on employment or the extensive margin.[20] Figure 6, panel (b), plots the DID estimates for the tax-free year and placebo estimates for years before and after, showing a positive but small and statistically imprecise effect on employment. Columns (5) and (6) in Table 1 report the corresponding regression estimates, documenting an employment elasticity of 0.023-0.036. When interpreting these results, it is important to bear in mind a few features of the research design. Recall that I identify the labor supply responses from the differential responses of workers in different tax brackets. Hence, by construction, the research design is unable to uncover labor market-entry responses. Instead, this research design reveals the effect of the tax-free year on labor market exit, such as delayed retirement or delayed education. However, it is possible that more individuals entered the labor market in response to the temporary incentive created by the tax-free year, relative to what they would not have done in a normal (taxed) year. I revisit this question in Section 5. In light of these results, I, therefore, refer to the estimates in Table 1 as reflecting to a large extent the intensive margin responses to the tax-free year.

Real labor supply responses, not a reporting phenomenon. A critical reader may ask the important question: Can we interpret the estimated earnings elasticity as labor supply elasticity? While it is clear that the finding of an effect on weeks worked stems from additional time spent working, the earnings effect may incorporate some form of reporting response or tax avoidance. I conduct further analysis along several dimensions to shed light on this question, demonstrating that the findings reflect, at least largely, real labor supply responses.

First, I estimate responses separately for wage earners and those who are self-employed or business owners, defined as those having at least one job as self-employed. Self-employed individuals are likely to have greater flexibility in adjusting their labor supply and hence, we might expect to find larger responses for them. However, self-employed workers may also be able to increase their income in the tax-free year through tax avoidance, e.g. by misreporting capital income as labor income, or by shifting income from other years to the tax-free year. Such avoidance is less likely to be possible

[19] About 40–45% of workers work overtime in the average month. The corresponding share is 60–65% when including irregular hours, such as nights and weekends (Sigurdsson and Sigurdardottir, 2016).

[20] In line with a convention in the literature, employment is an indicator variable for having positive labor income in a given year. In Appendix Table A.5 and Appendix Figure A.6 I present estimates using alternative definitions based on minimum earning thresholds or weeks worked.

for employed workers, as their employers have no direct incentive to collude. Appendix Table A.1 reports estimates for these groups separately. For wage earners, the labor income elasticity is almost the same as for the whole sample (0.4) while the elasticity is larger for the self-employed (0.45-0.58). However, there are even larger differences in the elasticity of working time. The implied elasticity of weeks worked among the self-employed is 0.25 (15.3/61.8) while the elasticity for wage earners is 0.10 (4.9/47.4). While I cannot rule out that some of the differences in the increase in labor income between these groups arise from reporting behavior, these findings indicate that they may to a large extent reflect differences in hours flexibility.

Second, I examine whether income shifting can explain the estimated increase in income. During the tax-free year, workers may have negotiated with their employer to adjust their compensation in some way or to front-load some payments. While such behavior is likely to be more difficult and costly to achieve through wages and salaries, e.g. due to payroll taxes, other forms of payments may have been used. To investigate this possibility, I estimate equation (2) separately for each sub-component on the pay slip (in real $ values) and report the effect relative to the total. The results are reported in Appendix Table A.2. Overall, the results do not exhibit an unexpected pattern. Increases in wages and salaries make up 93% of the increase in payments and most of the remainder consists of payments such as fringe benefits and travel allowances, which are likely linked to more work. Potential suspects for income shifting, such as sales commissions and bonuses, as well as gifts, make up only 0.5%.[21]

Third, I estimate the effect on capital income. While labor and capital income were taxed according to the same tax schedule both pre- and post-reform, in 1987 capital income was taxed as before while labor income was tax free. Although this does not provide a pure placebo test, estimating the effect on capital income allows for investigating potential misreporting and tax avoidance. The reporting behavior would manifest itself as a negative effect on capital income, as taxpayers report more of their capital income as labor earnings in the tax-free year. A negative effect on capital income would therefore indicate that at least part of the estimated earnings elasticity is masking reporting behavior. However, we might still expect an effect on capital income. Because a large part of capital income, such as business income and dividends, is an implicit function of labor supply in the economy, there may be equilibrium effects on capital income resulting from an increased labor supply. Appendix Table A.3 reports a positive but small effect on capital income, amounting to 3.2% of the treatment effect on labor income. This contradicts the hypothesis of misreporting.

Lastly, there is other, more circumstantial, evidence implying that the Icelandic population was working very hard during the tax-free year. When there is a strong temporary incentive to work, individuals have the incentive to avoid or postpone other activities that take time away from work. While a natural example is leisure activity, workers might also be more reluctant to stay at home when they or their family are ill. Figure A.12 documents that workers in Iceland took less sick leave in 1987. The average share of hours spent on sickness leave of total paid hours was 2.4% in both the years before and after 1987 but fell to 1.6% during 1987.[22]

[21]These results are consistent with evidence from other Nordic countries, indicating limited tax avoidance in labor earnings because of third-party reporting by firms (Kleven et al., 2011).

[22]In Figure A.12 I also report that fewer people were receiving sickness benefits in 1987 than in the years before. To

Robustness. There is extensive literature estimating the elasticity of taxable income (ETI) employing, as I do, research designs that exploit tax reforms in combination with a progressive income tax schedule (Saez et al., 2012). This literature has highlighted three key problems (Gruber and Saez, 2002). First, the tax rate that individuals face following tax reform, and therefore the treatment, is a function of income which is the outcome of interest and, thus, endogenous. Second, if the income distribution is continually widening, e.g. due to factors such as skill-biased technical change, globalization, and other secular increases in inequality, it may be difficult to disentangle the long-term effects of tax changes from these trends, particularly at the top of the income distribution. Third, due to mean-reverting transitory income shocks, high-income individuals tend, on average, to see their income decline in the following year, and the reverse is true for low-income individuals.

The tax-free year offers a setting that has several advantages that allow me to overcome these issues. First, all taxes were reduced to zero in 1987, implying that treatment is only a function of pre-reform income. Second, the analysis is concerned with short-term responses to a temporary tax cut affecting taxes across the entire income distribution. This alleviates concerns related to long-term trends such as the evolution of inequality. Third, the variation in marginal tax rates is not only a function of the level of labor income, as in many settings, but owing to multiple tax deductions and tax credits, there was a substantial overlap in the earnings distributions across tax brackets.

Still, the presence of temporary mean-reverting income shocks may lead to a bias in the income elasticity estimates. For example, some individuals who were in a high tax bracket in the previous year were there because of a positive income shock that reverts to the mean in the current year, generating a downward bias in the earnings elasticity. Similarly, some individuals in a low tax bracket were there because of a negative income shock.

The empirical tax literature has demonstrated how mean reversion can be dealt with by using further lags of pre-reform information to assign treatment status (e.g. Weber, 2014). Following these insights, I proceeded to construct more stable treatment and control groups by using further lags of tax-bracket position and other pre-reform information to assign workers to groups. In doing so, I proceed in two ways. First, I perform a prediction exercise, where I predict workers' tax brackets (treatment status) using a rich set of individual characteristics. For each year, the prediction is based on an estimation of a multinomial logit model where the outcome variable is a categorical variable for the tax brackets. The predictors include indicator variables for tax brackets in the previous one to three years, depending on specification, and a full set of dummies for the previous year's percentile in the income distribution to proxy for distance from tax bracket thresholds, across which temporary shocks might push individual workers. The model also includes individual characteristics including dummies for age, gender, marital status, the number of children, and a dummy for living in the capital area.[23] For each year, I assign workers to tax brackets based on the predicted probabilities from this model estimated on data for all years except the one being considered (i.e. out-of-sample pre-

the extent that this evidence indicates that workers were working very hard in 1987, it is in accordance with a study by Ólafsdóttir et al. (2016), which finds there was an increased likelihood among middle-aged and old men in 1987 and 1988 of having heart attack, in particular among the self-employed.

[23]The pseudo R^2 from the multinomial model estimates are in the range of 0.40–0.45, depending on the year, compared with about 0.30–0.35 when only the previous year's tax bracket is included.

diction).[24] Second, as an alternative approach, I assign workers' treatment status restricting to those who stayed in the same bracket for three consecutive years before 1987, while excluding others.[25]

Table 2, columns (1) to (3), presents the estimates of labor income elasticity using these two approaches to assigning treatment status. The magnitudes are broadly similar to the main elasticity estimates, ranging from 0.33 to 0.43 depending on specification. Furthermore, as presented in Appendix Figure A.8, I found no false positives in the years before or after the tax-free year using these specifications.

Another potential concern is whether the permanent changes made to the tax system in 1988 spill over to labor supply during 1987, affecting the elasticity estimates. As Figure 3a documents, in 1988 the progressive tax schedule was replaced with a flat tax rate, leading to a reduction in tax rates for those previously in the top and upper-middle tax brackets, while increasing tax rates for those in the lower-middle and bottom brackets. In principle, this may have generated income effects that would confound the Frisch elasticity estimated based on responses in 1987.

There are two arguments for why there may be limited effects of the permanent reform spilling over to my elasticity estimates. First, while the new withholding-based tax system and the resulting tax-free year were announced in December 1986, no announcement was made on changes to the tax schedule. As described in Section 2, the technical and legal aspects of the new withholding-based tax system were worked out by the government and the tax authorities in the first few months of 1987 to simplify the tax system and ease the transition (Olgeirsson, 2013). When the bill that spelled out the new tax law was passed by Parliament in late March 1987, workers had been aware of the much-advertised tax-free year for several months. Second, relative to the simple and salient nature of the tax-free year, many of the implications of the new tax code for marginal tax rates were much less clear. In particular, an important part of the tax reform was the removal of tax deductions, which affected the tax base and therefore the marginal tax rates. For most taxpayers, assessing how changes in tax deductions and allowances would affect their marginal tax rates was likely to have been a complicated task. In addition to this complicated nature of the changes in the tax code, Figure 2 also suggests that the changes in the tax schedule appear to have been much less salient than the tax-free year.

To evaluate the robustness of the elasticity estimates to this concern, I control for taxes that individuals faced in 1988 in an attempt to approximate the potential income effect resulting from the permanent change in taxes. This is an imperfect proxy. Not only does it assume perfect foresight and knowledge, but this control also captures some of the variation identifying the elasticity. Table 2, columns (4) and (5), report results when controlling for the marginal tax rate or the taxes paid in 1988, respectively. Adding these controls lowers the elasticity estimate to 0.31-0.34. This suggests that the labor supply responses may have been affected by income effects. However, other evidence suggests that the permanent change may have had limited effects. First, Figures 4 and 6 show that there was

[24] I require that the bracket position is predicted with at least 50% probability. The results are robust to requiring higher levels of prediction accuracy.

[25] One drawback of these approaches is that in addition to reducing potential bias due to mean reversion, they restrict the sample to workers with stable incomes. Those who are on a positive trend, such as younger workers, or a negative trend, such as older workers, are excluded if they transition between brackets. If these workers have more or less elastic labor supply compared to those with stable incomes, excluding those will affect the elasticity estimate.

no effect on labor income, weeks worked, or employment in 1988. Second, Figure 5 documents that elasticity estimates are similar across different bracket compositions. For example, the elasticity estimate from comparing the top and the upper-middle brackets is of about the same size as the elasticity estimate from comparing the upper-middle and lower-middle brackets. This is true despite the more treated group in the former comparison receiving a larger permanent change in tax rates from 1988 onwards as shown in Figure 3 and would therefore be expected to incur a larger income effect.

5 The Extensive Margin

This tax-bracket difference-in-differences design employed in the previous section exploits variation in tax rates across groups of workers employed before the reform. It therefore has the drawback that, by construction, it cannot identify labor market entry responses. This is an important limitation as obtaining an estimate of the extensive margin elasticity is crucial for evaluating the aggregate response in hours worked to temporary changes in pay. In this section, I develop a complementary research design that has the advantage of being able to identify labor supply responses along the extensive margin, both entry and delayed exit.

5.1 Research Design

Motivation. The empirical strategy is to compare people of a certain age in the tax-free year to similar workers with the same life-cycle labor supply trends but who are of that same age in another year. This idea borrows the intuition from the seminal work by MaCurdy (1981), who demonstrated how the Frisch labor supply elasticity is identified from a transitory deviation in hours from the life-cycle labor supply profile in response to a transitory change in wages.[26] The research design leverages two features. First, from the individual perspective, at which age a worker experienced the tax-free year was as good as random. Second, in the absence of the tax-free year, the labor supply of similar individuals was likely to follow similar paths over their life cycle. Therefore, for a given worker experiencing a tax-free year, workers in other birth cohorts with similar characteristics, when observed at the same age, are likely to constitute a good counterfactual.[27]

Matching procedure. I construct control groups by matching individuals from each birth cohort to individuals of the same age and characteristics in other birth cohorts. For each birth cohort, I selected the control group from the adjacent birth cohort born one year earlier. This limits the set of potentially matched workers to those who are most likely to be comparable in other aspects than where they are in their life cycle, and, importantly, restricts the control group within each birth-cohort pair to individuals who do not experience a treatment until after the end of the sample period.[28] Within

[26] Appendix D outlines the MaCurdy (1981) model and builds on the model to illustrate the empirical strategy.

[27] Estimation of labor supply elasticities using grouping of individuals on similar life-cycle trends was pioneered by Ashenfelter (1984) and later applied by Angrist (1991) in a grouping instrumental variables approach. The method used in this section differs from this earlier work in that it combines cohort grouping and a natural experiment, where the former generates comparable groups on similar life-cycle trends and the latter provides the identifying variation.

[28] This setup allows me to circumvent the problems discussed in Borusyak et al. (2023) related to event study designs where the control group eventually becomes treated within the sample period.

adjacent cohort pairs, I further match on a set of characteristics that may correlate with life-cycle trends in the labor supply. These include gender, marital status, number of children, completed education coarsened into three levels, and tax bracket. I perform one-to-one matching, dropping cases where no match is found and selecting at random in cases of multiple matches. Given the general set of characteristics, I have broad support and can match more than 99% of the sample.

The matching procedure provides a sample of the treatment and control groups that are comparable in factors confounded with trends in labor supply behavior. However, the research design does not impose the assumption that labor supply is at an equal level across comparison groups. Rather, it assumes that they follow common life-cycle trends and deviations from these trends during the tax-free year identify the labor supply responses to the tax cut.

Estimating equation. The sample consists of individuals i belonging to birth cohorts c, where c denotes year of birth. Age is defined as $a = t - c$, where t is "calendar time". Denote the age at which a birth cohort experiences the tax-free year treatment by $A_c = 1987 - c$. As in the MaCurdy (1981) model, the relevant concept of time in this empirical framework is lifetime, i.e. age. In that context, it is useful to refer to *age cohorts* as the group of individuals observed at the same points in their lifetime.

As detailed above, workers at age a from cohort c are matched to workers of the same age a from the adjacent birth cohort $c - 1$. Matched cohort pairs $\{c, c-1\}$, i.e. age cohorts, are denoted by g. Within each age cohort g, I define "event time" as $k = a - A_c$, or age relative to age at the event of treatment. Then, define the treatment indicator as $D_{gk} = 1$ if $a = A_c$, but zero otherwise. All age cohorts are observed during and before the treatment event. Importantly, this implies that the treatment indicator D_{gk} uniquely defines the treatment group (c) and the treatment period within each age cohort, as the control group ($c - 1$) does not experience the treatment until after the end of the study period. Using this notation, the estimating equation for the reduced-form labor supply effects is:

$$y_{ik} = \alpha_{ig} + \delta_k + \eta \cdot D_{gk} + \boldsymbol{X}'_{ik}\gamma + \mu_{ik} \tag{3}$$

where y_{ik} measures the outcome of interest for individual i at event time k, α_{ig} are match-group fixed effects, i.e. fixed effects for each cell (or block) within which individuals are matched, which absorbs the average differences between the treatment and the control groups, and δ_k are event-time fixed effects. The coefficient η captures the average treatment effect on labor supply. The vector $\boldsymbol{X}_{ik}$ collects potential characteristics that we may want to control for, but that are not used in the matching process.[29] The error term, μ_{ik}, captures other determinants of labor supply. I cluster standard errors at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, for standard errors to reflect year-to-year variation in employment.

To obtain an estimate of labor supply elasticity, I instead estimate the following equation:

[29] Due to the "curse of dimensionality", the matching procedure delivers fewer matches the larger the set of characteristics matched on. I therefore choose a general set of characteristics to match on, retaining a high matching rate.

$$y_{ik} = \alpha_{ig} + \delta_k + \varepsilon \cdot \log(1 - \tau_{ik}) + \boldsymbol{X}'_{ik}\gamma + \nu_{ik} \tag{4}$$

where the logarithm of the net-of-tax rate, $\log(1 - \tau_{ik})$, is instrumented by the treatment indicator D_{gk}. The coefficient ε measures the labor supply elasticity.

Graphical evidence and validity of identifying assumptions. The identifying assumptions underlying this research design are, first, that, in the absence of a tax-free year, the labor supply of similar individuals in adjacent cohorts would have followed a common life-cycle path. Deviations from this path during the tax-free year will then identify the labor supply responses to the tax cut. The second and related assumption is that labor supply only deviates from these life-cycle trends in 1987 because of responses to the tax-free year.

Figure 7 provides graphical evidence to illustrate how the research design works and evaluate the plausibility of the identifying assumptions. The figure plots the evolution of tax rates and labor income across birth cohorts around the time of the tax-free year. Panel (a) plots the evolution of the net-of-tax rate for three birth cohorts—born in 1940, 1939, and 1938—to illustrate the staggering of when the birth cohorts experienced the tax-free year over their lifetime. Panel (b) plots a companion graph for the evolution of labor income. This figure shows how the adjacent cohorts followed similar trends before 1987, displaying a clear temporary divergence from that trend in the tax-free year before then reverting to the trend.

The research design does not impose the assumption that labor supply is at an equal level across comparison groups. Rather, it assumes that they follow common life-cycle trends. By differencing out these trends, the deviation from the trend during the tax-free year identifies the labor supply response to the tax cut. I illustrate this in panels (c) and (d). I first match individuals across adjacent birth cohorts, as described above, which ensures a sample of the treatment and control groups that are comparable in factors related to trends in labor supply behavior. Then I estimate equation (3) for each cohort over the event time, k, including the tax-free year and three years prior, $k \in [-3, 0]$, normalizing differences to zero in $k = -1$. For net-of-tax rates, panel (c) shows small differences in tax rates before the tax-free year, reflecting small changes in the tax schedule, but highlighting the large jump in net-of-tax wages in 1987. For labor income, panel (d) similarly shows that comparable individuals from adjacent cohorts were on parallel life-cycle trends before the tax-free year, but deviated starkly from those trends during that year. Panels (e) and (f) extend the illustration in panels (c) and (d) to all birth cohorts 1919-1970. To aid graphical representation, the figure for labor income is scaled by adding to the coefficients the average labor income of the given cohort in 1986, and both figures plot on the x-axis the age in 1987. In this way, the figures become visually comparable to the original model graphs in MaCurdy (1981), showing the earnings life-cycle and deviations from that at the time of transitory increase in net-of-tax wages. Differences in pre-reform years represent placebos tests. It is reassuring to see no significant deviations from the life-cycle trends in the years before the tax-free year, neither statistically of economically when compared to the deviation visible during the tax-free year.

While the evidence presented in Figure 7 lends support to the first identifying assumption, a potential threat to identification would be if there were shocks contemporaneous to the tax-free year that influence the outcome of the treatment group relative to the control group. Importantly, no other reforms coincided with the tax-free year, such as changes to social security or taxes on firms. However, an example of such threats would be aggregate shocks to labor demand leading to an increased labor input in equilibrium and reverse causality. Below I evaluate the robustness of the results to such concerns.

5.2 Results

Figure 8 reports the estimated employment elasticity by age and the population as a whole.[30] Given that an individual's decision whether to enter or exit the labor market is likely based on the total financial incentives for working—which in turn are influenced by the disincentives generated by the tax burden the worker expects to bear if employed—the employment semi-elasticity relates the employment probability to the average tax rate individuals face if working rather than the tax paid on the marginal dollar earned.[31] To obtain an elasticity estimate, I scale the semi-elasticity estimates by the employment rate of the relevant group in 1986.

For the population, I estimate a modest average employment elasticity of 0.09, as presented in Table 3, column (1). However, this average estimate masks important heterogeneity. Figure 8 plots the employment elasticity estimates by age. I estimate a close to zero elasticity across the prime-age population, while a large and statistically significant for the youngest cohorts and those cohorts close to and the around statutory retirement age of 67. For 18-25 year olds the elasticity is estimated at 0.59 and at 0.44 for those that were 61-67 years old.[32] These results are therefore broadly in line with life-cycle models with a nonconvexity in the mapping from hours to labor services, giving rise to discontinuous labor market entry of young cohorts and retirement of old cohorts (Rogerson and Wallenius, 2009). These results highlight an important heterogeneity. Essentially, young first-time workers, who were still in school or out of the labor force for other reasons, and workers close to retirement drive the employment response. This heterogeneity is informative for calibrating macro models featuring indivisible labor or understanding fluctuations in employment over the business cycle, as this evidence suggests that extensive margin responses will depend on the marginal density at the tails of the working life cycle.[33]

The life-cycle research design can similarly be used to estimate the response in labor income to the tax-free year. When interpreting these estimates, two factors are important to emphasize. First,

[30] I define employment as an indicator for having labor income above a threshold corresponding to 1.6×guaranteed income, which is a reference amount used in calculations of various kinds for governmental income support. This roughly corresponds to the lowest minimum wage earnings according to collective bargaining agreements. Using this reference point has the advantage of being updated each year to account for changes in prices and wages. In Appendix Table A.8, I report estimates using alternative definitions of employment, either based on non-zero earnings or weeks worked.

[31] Individuals' average tax rate is the ratio of income tax payments to the income tax base, i.e. total taxable income net of deductions. The employment semi-elasticity estimates relate the employment rate to the net-of-average tax rate.

[32] Appendix Table A.6 summarizes the employment elasticity estimates for the population and three main age groups.

[33] Some of these responses appear not to have been transitory. In Sigurdsson (2023b), I study the effects of a temporarily increased opportunity cost of schooling generated by the tax-free year on educational attainment. Comparing individuals above and below compulsory schooling age, I find evidence of reduced enrollment and increased dropout from upper-secondary school during the tax-free year, resulting in a permanent loss in educational attainment.

this estimate will capture both the intensive and the extensive margin response. In comparison, as documented in Section 4, the tax bracket DID is mainly able to identify the intensive margin. Second, as the method exploits a combination of cross-sectional and time-series variation, it will also incorporate all macroeconomic effects in the tax-free year, including equilibrium effects. Ex-ante, it is unclear whether these contribute positively or negatively to the aggregate elasticity estimate. On the one hand, if labor demand is not perfectly elastic, strong labor supply responses may lead wages to fall. This would dampen the labor supply response and attenuate the estimated elasticity using the life-cycle design compared to the tax bracket DID if these effects are common across tax brackets.[34] On the other hand, workers receiving large tax cuts may spend less time on leisure but also home production, such as home cleaning, cooking, and childcare. This may then generate demand for labor inputs in occupations providing these services, thus facilitating more work for those who desire to work longer hours during a tax holiday and amplifying the overall labor supply response. The tax bracket DID method 'differences out' all such aggregate time effects. With these two factors in mind, the labor income elasticity estimated using the life-cycle design reflects the aggregate elasticity to the tax-free year, capturing the intensive margin, extensive margin, and possible equilibrium effects.

Table 3, column (2), presents estimated effects on labor income using the life-cycle design. In line with both positive extensive margin effects and possible equilibrium effects, the aggregate income elasticity is 0.84. Appendix Figure A.13, panel (a), plots the labor income elasticity by age. As expected, the pattern is similar to that for the extensive margin elasticity—largest for the youngest and oldest cohorts but flat across the prime-age population. In column (3) of Table 3 I present the estimated effects of weeks worked of 6.5 additional weeks worked, corresponding to an elasticity of 0.16 when this semi-elasticity is evaluated relative to the pre-reform average number of weeks worked.

As explained above, one of the identifying assumptions for the life-cycle design is that labor supply only deviates from these life-cycle trends in 1987 because of responses to the tax-free year. Since this assumption is hard to verify, a common approach to address concerns regarding its validity would be to add time-fixed effects to isolate the labor supply response from any time- or equilibrium effects. However, this requires identifying variation within a group of adjacent birth cohorts. One source of such variation is differences in pre-reform tax rates. While this can difference out equilibrium effects and any other time effects, it also mechanically restricts responses to those in the labor market before the reform, therefore excluding the extensive margin. Exploiting this cross-sectional variation together with the cohort life-cycle differences leads to a triple-differences design, essentially combining regression equations (4) and (2), yielding the following regression equation:

$$y_{ik} = \alpha_{ig} + \delta_k + \varepsilon \cdot \log(1-\tau_{ik}) + b_{i,k-1} + \alpha_{ig} \cdot b_{i,k-1} + \beta_D D_{gk} + \beta_B B_{i,k-1} + \boldsymbol{X}'_{ik}\gamma + \nu_{ik} \tag{5}$$

where ε measures the elasticity of labor income when the logarithm of the net-of-tax rate $\log(1-\tau_{ik})$ is instrumented with the triple-difference interaction term $D_{gk} \times B_{i,k-1}$. The elasticity is identified from the variation in labor supply specific to the treated birth cohorts (relative to the control birth

[34] Appendix Figure A.10 plots hourly wage rates by occupation through the 1980s according to survey data. The evidence presented in this figure lends little support for a reduction in wage rates in 1987 and shows similar movements across occupations.

cohorts), for the workers in the top three tax brackets (relative to those in the bottom tax bracket), during the tax-free year (relative to the years before).

Table 3, column (4), presents estimated effects on labor income using the triple-differences design. The population labor income elasticity is estimated at 0.35. This estimate is therefore similar in magnitude to the intensive-margin elasticity estimate presented in Section 4. In Appendix Figure A.13, panel (b), I plot these estimates by age, showing a much flatter age profile of elasticities than estimated under the life-cycle design, consistent with differences reflecting the inclusion of extensive margin responses to a large extent. Column (5) of Table 3 presents estimated effects on weeks worked, documenting an effect of 2.2 additional weeks worked, or an elasticity of 0.05. Lastly, as reported in column (3), this method estimates no positive effect on employment.

Robustness. I have conducted further analyses along several dimensions to evaluate the robustness of the results reported above. Although the life-cycle design allows for identifying labor supply elasticities from differences across individuals likely to be on common life-cycle trends, I cannot rule out the possibility of aggregate shocks, other than the tax-free year, affecting the estimates. While such transitory shocks are more likely to affect hours and earnings, hence the focus on extensive margin using this research design, I cannot rule out such impacts on employment. Being a small open economy, external shocks have traditionally driven macroeconomic volatility in Iceland, such as through exports or shocks in its natural resources, e.g. biological shocks in the fish supply. At the time of the tax reform, the Icelandic economy had been in an upswing where a key driver of the growing economy was a booming fishing sector (see Appendix Figure A.9). Marine exports had been growing strongly following a positive terms-of-trade shock, mainly due to higher fish prices in nearby markets. While on a downward trend throughout much of the 20th century, fishing and fish processing constituted about 15% of GDP in the 1980s and this sector employed about the same share of workers. Therefore, there may be a concern that some form of export or fishing sector shock influences the results. To evaluate this claim, Appendix Table A.7 presents results for samples excluding all workers and firms in these sectors, first excluding the fishing and fish-processing sectors and, second, tradable sectors. In both cases, the magnitude of estimates are similar, if anything stronger, than implied by my main estimates.

6 Adjustment Frictions Shape Labor Supply Responses

The canonical model of labor supply assumes that workers hold a single job in which they can flexibly choose their hours of work. Hence, workers are always on their labor supply curve, and preferences determine the response of hours worked to wage changes. A growing literature casts doubt on this assumption, proposing that workers face frictions such as adjustment costs (Cogan, 1981; Ham, 1982), hours constraints (Altonji and Paxson, 1988; Dickens and Lundberg, 1993) and costs of changing jobs (Altonji and Paxson, 1992). As a result, estimates of short-run labor supply elasticities will be muted relative to the underlying structural elasticity (Chetty, 2012). In what follows, I document how adjustment frictions influence the heterogeneity of intertemporal labor supply responses. In turn, I examine how temporal flexibility in workers' current employment arrangement influences their responses and how workers can overcome frictions through secondary jobs.

6.1 Temporal Flexibility and Hours Constraints

Jobs appear to vary greatly in the temporal flexibility they offer. Some occupations, such as taxi and ride-hailing drivers, can flexibly choose to work another hour or another day (Hall and Krueger, 2018). For other occupations, such as pharmacists, temporal flexibility arises from the ease of changing the number of shifts worked and transitioning between part- and full-time employment Goldin and Katz (2016). In these cases, temporal flexibility leads to a large dispersion in working time within the occupation as workers choose the number of hours they work to match their preferences. In many jobs, however, workers have limited or no ability to vary their hours and, in particular, to be paid for working an additional hour.

Motivated by this, I construct a measure of temporal flexibility based on the dispersion in working time within occupations. More precisely, I measure temporal flexibility using the coefficient of variation (CV) in working time within occupations:

$$CV(W_{ot}) = \frac{\sigma_{ot}}{\mu_{ot}}, \quad \sigma_{ot} = \left[\frac{1}{N_{ot}-1}\sum_{i=1}^{N_{ot}}(W_{iot}-\mu_{ot})^2\right]^{\frac{1}{2}}, \quad \mu_{ot} = \frac{1}{N_{ot}}\sum_{i=1}^{N_{ot}} W_{iot} \tag{6}$$

where W_{iot} is the number of weeks worked by individual i in occupation o in year t, N_{ot} is the number of jobs in occupation o in year t, and μ_{ot}, σ_{ot} are, respectively, the average and standard deviation of weeks worked in occupation o in year t. I calculate $CV(W_{ot})$ for three years prior to the tax-free year and include the average in the analysis.[35]

How should we interpret this metric? If there is much dispersion in working time, e.g. many workers work only part-time while others work full-time, the occupation displays high temporal flexibility. However, if the dispersion is low, e.g. if the occupation only allows for full-time employment at a fixed number of hours, the occupation has low temporal flexibility. In other words, the occupations with higher temporal flexibility are those that offer a broader menu in terms of employment arrangements. According to this measure, occupations with the most temporal flexibility are elementary workers in the service sector (e.g. restaurant workers), workers in cleaning and related activities, and elementary workers in agriculture. The least flexible occupations are managers in retail, construction, and manufacturing.

As a second measure, I proxy the constraints in hours according to whether a worker holds a job with a fixed contracted monthly salary and hours or one with the option of working paid overtime. Using an employer-employee data set with comprehensive information, including daytime and overtime hours (see e.g. Sigurdsson and Sigurdardottir, 2016, for details), I identify workers who work paid overtime in an average month.[36] I assign this measure of the flexibility of remuneration structure to the workers in the main data set based on their pre-reform occupation. Occupations with the

[35] Appendix Figure A.14 plots the distribution of $CV(W_{ot})$ in the sample.

[36] Unfortunately, these data do not cover all sectors and occupations and only extend back to 1998. As a result, I cannot directly merge them with the main data set at the level of individuals or firms. Therefore, I measure the average share of workers by occupation paid by the hour or that has a fixed salary but paid for overtime.

least flexibility according to this measure are professionals (e.g. engineers) and managers while those with the most flexibility are cleaners and elementary workers in construction.

Figure 9 plots the occupation-level labor income elasticity against the measure of temporal flexibility, panel (a), and flexibility of remuneration structure, panel (b). Elasticities are obtained using the tax bracket DID method by matching on pre-reform characteristics and estimating regression equation (2) interacted with an indicator of pre-reform occupation. Both figures depict a positive and statistically significant correlation, implying that workers in occupations with more flexibility have larger elasticities than those in less flexible jobs. Appendix Table A.9 summarizes the labor supply elasticity estimates by workers with different degrees of hours constraints according to these measures. In addition, the table presents estimates using a measure based on the actual pre-reform working time of workers. I define workers to be hours constrained in their primary job if they are recorded as having worked exactly 52 weeks in that job in the previous year. This measure is likely to capture similar features as the measure based on overtime work. Indeed, the cross-sectional correlation between the two measures is 0.75. The results document significantly larger elasticities for those workers who are not hours-constrained in their primary job according to this measure.

6.2 Overcoming Hours Constraints: Secondary Jobs

The previous section documented substantially larger labor supply responses among workers in jobs with more temporal flexibility. Interestingly, however, I find significant responses even for those workers most likely to face hours constraints. How are they able to overcome these frictions?

While hours may be rigid within jobs, they may be flexible across jobs. As a result, constrained workers may choose to change jobs to adjust their labor supply to a new desired level. Although job changes may be an operating margin for long-term adjustment, it is likely to be too costly a margin for temporary adjustment. Alternatively, therefore, workers may choose to take up secondary jobs—i.e. to *moonlight*—as a way of overcoming hours constraints (Shishko and Rostker, 1976; Paxson and Sicherman, 1996; Conway and Kimmel, 1998).

Figure 10 presents estimated effects on weeks worked on secondary jobs. The figure documents an increase in working time on secondary jobs, although the results are statistically imprecise. This result is in line with a share of the responses in weeks worked in Section 4 reflecting more work on secondary jobs. I then estimate these responses separately by workers who faced different degrees of hours constraints and temporal flexibility in their primary jobs. Workers who were likely to have faced hours constraints and low temporal flexibility in their primary jobs worked on average an additional half a week on secondary jobs.[37]

To evaluate the aggregate implications of these findings, I compute how much weight secondary jobs carry in explaining the overall labor supply response. More precisely, I decompose the total labor supply effect into the contributions from continuing primary jobs, new primary jobs, and secondary jobs. Total labor supply, E_T, measured either at the level of real labor earnings or weeks worked, can

[37] These results are in line with those in Tazhitdinova (2021), who studies a tax reform in Germany that allowed workers to hold secondary jobs tax-free. She finds a large increase in the take-up of secondary jobs and documents that workers take up secondary jobs to overcome hours constraints in their primary jobs.

be written in terms of its subcomponents as

$$E_T = E_p + E_s \tag{7}$$
$$E_T = E_p^{\text{Cont}} + \gamma \cdot (E_p^{\text{New}} - E_p^{\text{Cont}}) + E_s$$

where E_p^{Cont} is a continuing primary job, γ is the propensity of primary job change and E_s are secondary jobs. The total effect of the tax reform ($d\tau$) can then be decomposed as follows

$$dE_T = \underbrace{dE_p^{Cont}}_{\text{Continuing primary job}} + \underbrace{\gamma \cdot (dE_p^{\text{New}} - dE_p^{\text{Cont}}) + d\gamma \cdot (E_p^{\text{New}} - E_p^{\text{Cont}})}_{\text{Primary job change}} + \underbrace{dE_s}_{\text{Secondary jobs}} \tag{8}$$

where each component can be estimated using the tax-bracket DID framework (Section 4).

The decomposition reveals that 34% of the additional weeks worked were created by more time on secondary jobs while the remainder arises from increased working time in primary jobs (less vacation time, full-time employment, etc.). Similarly, 7% of the total earnings effect stems from work on secondary jobs with the remaining 93% being accounted for by increased earnings on primary jobs. The decomposition also reveals that primary job changes account for only 0.2% of the effect on labor earnings and even contribute negatively to the change in weeks worked, consistent with a search cost in terms of foregone working time.[38]

7 Relation to Past Work

To obtain a reference point for evaluating the magnitude of the Frisch elasticity estimates, first I conduct a meta-analysis of previous estimates. I then highlight the most likely reasons behind the differences between my estimates and those of the closest studies before providing an alternative evaluation of the size of the estimates building on economic theory and existing parameter estimates.

7.1 Intensive Margin Frisch Elasticity

Figure 11, panel (a), summarizes past estimates of intensive margin Frisch elasticities.[39] The figure is organized in three sections by the samples studied, from left to right: the population (as either a whole or a representative sample), prime-aged men, and specific occupational groups. For reference, I also plot my estimates for the corresponding sample.

Close studies. My analysis lies closest to two earlier studies. Using a random sample of 9,300 individuals, Bianchi, Gudmundsson, and Zoega (2001) study labor supply during the Icelandic tax-free year and compare it to the year before and the year after. Their paper is an important contribution

[38]As highlighted by equation (8), the contribution from job changes is a result of two opposing forces. First, I estimate a decreased propensity for job change during the tax-free year. Second, those workers who do change jobs, however, increase their labor supply, possibly because they can overcome constraints in hours in the previous job. According to the decomposition results, these two effects almost exactly cancel each other.

[39]See Appendix Table A.14 for details of the studies included in Figure 11. The figure attempts to provide an informative comparison rather than an exhaustive survey. Earlier surveys include Blundell and MaCurdy (1999), Keane (2011), Chetty (2012) and Chetty et al. (2013).

highlighting the Icelandic tax-free year as a unique natural experiment. To date, it still remains one of the few informative data points on intertemporal labor supply (Chetty et al., 2013). However, owing to limited data availability, their estimates are based on *average* tax rates in 1986 while the relevant measure for calculating the intensive margin elasticity is the *marginal* tax rate. To facilitate comparability of their estimates to this and other studies in the literature, I compute the average marginal tax rates for the groups they study and transform their estimated earnings responses into an intensive margin elasticity of 0.77 (see Appendix E for details).[40] Of most note, this estimate is more than twice as large as what I estimate in Section 4. This contrast underlines the importance of separating responses to the tax-free year from the influences of pre-trends, the business cycle, and subsequent changes to the tax system.

The other close study is where Martinez, Saez, and Siegenthaler (2021) estimate a Frisch elasticity using a tax reform in Switzerland, much like that leading to the tax-free year in Iceland. In the late 1990s and early 2000s, Switzerland changed its base for income taxation from the previous *two* years' income to pay-as-you-earn. As a result, the reform led to a two-year tax holiday, but this took place at different times across geographic regions. Using the staggering of the reform, the authors estimate a small intensive margin elasticity of 0.025 with a small standard error. As I explain below, different populations and differences in the flexibility of the two labor markets likely explain this difference.

In more recent work, Stefánsson (2019) revisits and extends the analysis in Bianchi, Gudmundsson, and Zoega (2001) using population-level income data. Using difference-in-differences across income groups at the upper end of the income distribution, he estimates an earnings elasticity of about 0.07. This is different from my methodology, which exploits differences in marginal tax rates across tax brackets. As explained in Appendix A, marginal tax rates were not only a function of the level of labor income but owing to multiple tax deductions and tax credits there was a substantial overlap in the earnings distributions across tax brackets.

Other earlier work. Most of the existing evidence on Frisch elasticity, including the seminal studies by MaCurdy (1981) and Altonji (1986), draw on regressions of the working hours on wages of prime-age men. As Figure 11, panel (a), illustrates, the elasticities in these studies are very imprecisely estimated and often statistically insignificant. This may be due to several reasons. First, the instrumental variable approach used in much of this literature is based on individual characteristics, traditionally age and education, as predictors of changes in wages. While this literature brought the insight that these factors can be good predictors of the level of wages, later work has found them to perform poorly in predicting wage changes, leading to weak instruments (Keane, 2011). Second, prior work has emphasized issues concerning the measurement of wages and hours in the Panel Study of Income Dynamics (PSID) used in much of this literature, which may lead to either a positive or a negative bias (Heckman, 1993; French, 2004a).[41]

[40] I follow essentially the same procedure as in Chetty et al. (2013) when calculating the elasticity. The difference is that my calculations are based on individual-level marginal tax rates whereas the calculations in Chetty et al. (2013) are based on the average across the progressive tax bracket schedule, assuming an equal share of taxpayers in each bracket. The fact that there is much more mass of taxpayers at lower brackets than higher tax brackets explains why their calculations yield a much lower intensive margin elasticity (0.37) than what I calculate.

[41] In addition to the quasi-experimental literature surveyed in this section, extensive literature estimates Frisch elasticity

The empirical challenge of estimating Frisch elasticity and the presence of adjustment frictions has motivated several studies that estimate elasticities for particular occupations, such as bicycle messengers (Fehr and Goette, 2007) and taxi drivers (Farber, 2015), for whom exogenous changes in wages are plausible and who are flexible in choosing their daily labor supply. As summarized in Figure 11, panel (a), these studies tend to estimate relatively large labor supply elasticities. The size of the elasticities are roughly similar but even larger than what I estimate for the groups with the most temporal flexibility (Figure 9). While these studies provide clear causal estimates in an environment with minimum frictions, it is challenging to generalize their findings to the situation where average workers respond to transitory and business cycle variations in wages.

7.2 Extensive Margin Frisch Elasticity

Figure 11, panel (b), summarizes the estimates of extensive margin Frisch elasticity. Compared with the intensive margin, the existing studies of these are much fewer. My extensive margin elasticity estimate falls far below the estimate by Carrington (1996), who studied employment in Alaska during an oil pipeline boom in the 1970s.[42] The figure underlines the drivers of my elasticity estimate. Similar to prior evidence, I identify an employment response for those at and around retirement age (age 60 years and older). The strongest employment responses, however, are among the youngest cohorts (below age 25 years), for which no comparable estimates exist. For the prime-aged, which is also the population studied in Martinez, Saez, and Siegenthaler (2021), the extensive margin elasticity is zero.

7.3 Why Do the Icelandic and Swiss Elasticity Estimates Differ So Much?

The difference between the elasticity estimates from the tax holidays in Iceland and Switzerland—in particular the irresponsiveness of Swiss workers—is at first sight somewhat surprising. As I have illustrated, the difference in extensive margin elasticities stems from the fact that employment responses arise exclusively from young first-time workers and those close to retirement. Both groups are excluded from the Martinez, Saez, and Siegenthaler (2021) analysis. Nonetheless, my intensive margin elasticity estimates are an order of magnitude larger than for Switzerland, where they are close to zero. Given the tax holidays in Iceland and Switzerland both created strong incentives for workers to temporarily increase their labor supply, why did they generate such different responses?

Several factors may explain the differences, such as the salience of the reforms and the framing of their announcement. However, I argue that differences in labor market flexibility are the most plausible explanation. The first evidence in support of this notion comes from prior research. In Sigurdsson (2023a) I study a tax holiday in Norway that resulted from a tax reform similar to those in Iceland and Switzerland. There I document that more than 80 percent of Norwegian adults were aware of the tax holiday. This is consistent with the evidence presented in this paper and Martinez, Saez, and Siegenthaler (2021) that tax holidays are salient events. In Norway, a quarter of the working-age pop-

using structural methods. I survey prominent papers in this literature in Appendix Figure A.15.

[42] It is interesting to note that the median age in the Alaskan population in 1970 was just 22.9 years (Carrington (1996), Table 1). Therefore, the estimate in Carrington (1996) is perhaps more comparable to that for young cohorts in Iceland.

ulation responded by working more hours. For those aware of the tax holiday but did not respond to it, most said that friction in adjusting working time, in one form or another, was the main reason.

The second and more concrete evidence in support of this notion comes from relating measures of flexibility for labor supply adjustment and size of responses. The Icelandic labor market is relatively more flexible than the Swiss labor market and others in continental Europe, and indeed closer to that found in the US labor market. I now illustrate how the measures of flexibility correlate with the size of the intensive margin elasticity estimates, both between and within countries. I define labor market flexibility in terms of the speed of adjustment to external shocks or changing macroeconomic conditions (Pissarides, 1997). Flexibility can then be divided into flexibility at the micro-level (reflected by working time flexibility, worker flows between labor market states, and job flows) and at the macro or institutional level (as reflected by labor regulations and wage flexibility). In Appendix F I collect several measures of labor market flexibility for OECD countries. Whether measured as worker flows (being the "fluidity" of the labor market), cyclicality and the importance of hours per worker, or as wage flexibility or the flexibility of institutions, all measures are highly correlated (see Appendix Figure A.16). Furthermore, on all metrics, the Icelandic labor market is substantially more flexible than the Swiss labor market and closer to that of the US. For example, the cyclicality of hours per worker, measuring the flexibility of hours, is more than twice as high in Iceland than in Switzerland, while flow rates in and out of unemployment in Iceland are three times higher.[43]

Figure 11, panel (c), reveals that flexibility of working hours is positively correlated with the intensive margin Frisch elasticity, across both countries and occupations. The figure measures flexibility by the correlation of hours per worker and total hours, i.e. the cyclicality of working hours, which I select for two reasons. First, if workers have the flexibility to adjust their hours and the intensive margin is operative, we would expect hours per worker to move with the business cycle and to explain a significant share of the changes in total hours. Second, I can use this measure of flexibility for both countries and subgroups within countries, such as occupations, facilitating a broader comparison. As there are few available elasticity estimates across countries, the figure only plots the estimates for Iceland, Switzerland, Norway, and the US.[44] In addition, the figure plots the estimates for occupations within Iceland, as reported in Figure 9. The Frisch elasticity estimate and the hours of flexibility in Iceland and the US are broadly similar, although the variation across US estimates is substantial. Switzerland falls at the other end of the spectrum compared to Iceland and the US.[45] The positive correlation indicated by the cross-country comparison is confirmed by the cross-occupation

[43]Both the cyclicality of hours per worker and their cyclical importance, measured by relative standard deviations, are higher in Iceland and the US than in Switzerland. In Iceland, hours per worker explain about 45% of the cyclical variation in total hours, which is more than twice as much as in Switzerland. Indeed, Rogerson and Shimer (2011) note: "An extreme example is Switzerland, where [...] most of the cyclical movement in total hours is accounted for by movements between non-participation and employment at a fixed number of hours per worker."

[44]Tortarolo, Cruces, and Castillo (2020) study a 2.5-year-long income tax holiday in Argentina and estimate a very small labor supply response. Overall, in line with the cross-country analysis of labor market flexibility and labor supply elasticity, Argentina has a very rigid labor market compared to OECD countries. The estimate is therefore consistent with the pattern in Figure 11. However, since this reform generated both a substitution and an income effect, the elasticity estimate is, perhaps, closer to a Marshallian than Frisch.

[45]The elasticity estimate for Norway is based on cross-country difference in differences and therefore differs from the elasticity estimates for the other countries in that it may capture both intensive and extensive margin responses. See Sigurdsson (2023a) for details.

comparison.

7.4 Is the Order of Magnitude of the Elasticity Estimate Reasonable?

Reliable and comparable estimates of the intensive margin Frisch elasticity are few and the existing evidence is mixed. As a result, inferring whether the size of my estimates is reasonable through such a comparison may not be conclusive. An alternative approach is to use theory to evaluate whether the estimates are consistent with those of other parameters in the standard dynamic labor supply model.

Using economic theory and prior estimates, I can provide a prediction of the Hicksian elasticity implied by my estimate of the Frisch elasticity.[46] A dynamic labor supply model with time-separable utility in consumption and leisure provides the following relationship between the intensive margin Frisch elasticity and other key parameters in the model (Ziliak and Kniesner, 1999; Browning, 2005):

$$\varepsilon_{\text{Frisch}} = \varepsilon_{\text{Hicks}} + \rho \cdot mpe^2 \frac{A}{wh} \tag{9}$$

where ρ is the intertemporal substitution in consumption (EIS), mpe is the marginal propensity to earn (MPE) out of unearned income, i.e. the income effect, and $\frac{A}{wh}$ is the ratio of wealth to labor income.[47] Figure 11, panel (d), maps my Frisch elasticity estimate into the Hicksian (and Marshallian) elasticity on the y-axis and IES on the x-axis for given values of the other parameters in equation (9). The most prominent estimates of the MPE are based on estimates of the effect of winning a lottery, e.g. Imbens, Rubin, and Sacerdote (2001), Cesarini, Lindqvist, Notowidigdo, and Östling (2017), and Golosov, Graber, Mogstad, and Novgorodsky (2023), and receiving an inheritance (Nekoei and Seim, 2021). In my calculations, I use an MPE of 0.11 implied by the estimates in Imbens, Rubin, and Sacerdote (2001).[48] Then, I use data from individual tax returns to calculate a $\frac{A}{wh}$ ratio of 3.01, which is the median ratio for workers aged 25-67 in 1986. A value of the IES then pins down the implied Hicksian in the figure. The figure marks two estimates of IES: first, an average IES of 0.5 across the 169 studies surveyed in Havránek (2015), and second, an average IES of 0.9 across 33 studies published in the top-5 general interest journals. The implied Hicksian elasticity lies between 0.37 and 0.39, which is close to the Hicksian elasticity of 0.33 calculated in Chetty (2012) when pooling across existing studies. The implied Marshallian elasticity of 0.04-0.06 is also in line with previous estimates, such as by Kleven and Schultz (2014) who estimate an elasticity of 0.05 by pooling over a series of tax reforms in Denmark. The Frisch elasticity estimated in the current paper is therefore consistent in magnitude with other parameter estimates where evidence is more abundant.

[46] Standard theory of dynamic labor supply yields an important conclusion about the relationship between the Frisch, Hicks and Marshallian elasticities, namely that the Frisch elasticity is larger than the Hicks elasticity, which is, in turn, larger than the Marshallian elasticity (MaCurdy, 1981). This already implies that obtaining an estimate of the Hicks or Marshallian elasticities yields a lower bound on the Frisch elasticity.

[47] Similarly, via the Slutsky equation, the model yields the following relation between the Marshallian and the Frisch elasticities: $\varepsilon_{\text{Marshallian}} = \varepsilon_{\text{Frisch}} + \frac{A}{wh} \cdot mpe(1 - \rho \cdot mpe)$.

[48] While MPE cannot be separately estimated from the marginal propensity to save (MPS), Imbens, Rubin, and Sacerdote (2001) consider a setting where lottery winnings are paid out as installments over 20 years, enabling them to argue for an MPS close to 1 (they use 0.9). Studies of heirs find larger MPE than found for lottery winners (Nekoei and Seim, 2021).

8 Conclusion

Understanding how labor supply responds to temporary changes in wages has been a longstanding research program in micro and macroeconomics. The magnitude of this response, as measured by the Frisch elasticity, is crucial for our understanding of business cycles and labor markets and key for designing and evaluating many public policies.

Exploiting a tax-free year in Iceland as a natural experiment, I find that people do indeed respond to this temporary but strong and salient incentive. However, the responses appear not only to have been influenced by the size of the incentives that people faced but also by their ability to adjust labor supply. In terms of intensive margin responses, I document that workers in the most flexible jobs and employment arrangements display the strongest responses. However, those who face hours constraints in their primary jobs seem to have been able to partially alleviate these through working on secondary jobs. In terms of the extensive margin, I find that while the employment responses are on average small, young first-time workers and workers close to retirement drive them almost entirely.

Previous work has illustrated how relatively small frictions can explain that observed labor supply responses to permanent changes in wages are often near zero (Chetty, 2012). In line with this, Gelber (2014) estimates relatively large labor supply elasticities to an extensive tax reform that dramatically lowered marginal income tax rates in Sweden in the early 1990s. However, the salience of incentives is also likely to be important. Events such as the "*tax-free year*" in Iceland, or the "*tax reform of the century*" in Sweden, are likely to have been very salient to most people and simple to understand. In addition, union leaders, politicians, and media in Iceland emphasized the unique opportunity the reform provided for people to work at higher pay for one year. In comparison, as emphasized by (Martinez, Saez, and Siegenthaler, 2021), the tax holidays in Switzerland and the opportunities they provided may not have been salient in the same way. Taken together, the lessons learned about labor supply by studying natural experiments are likely shaped by the salience of the incentives they generate, the size of those incentives, and the ability workers have to respond to them.

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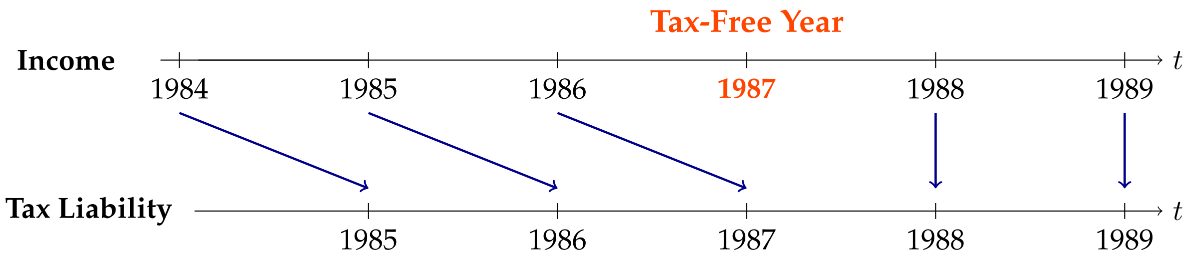


Figure 1: Income Tax System Before and After the Tax Reform

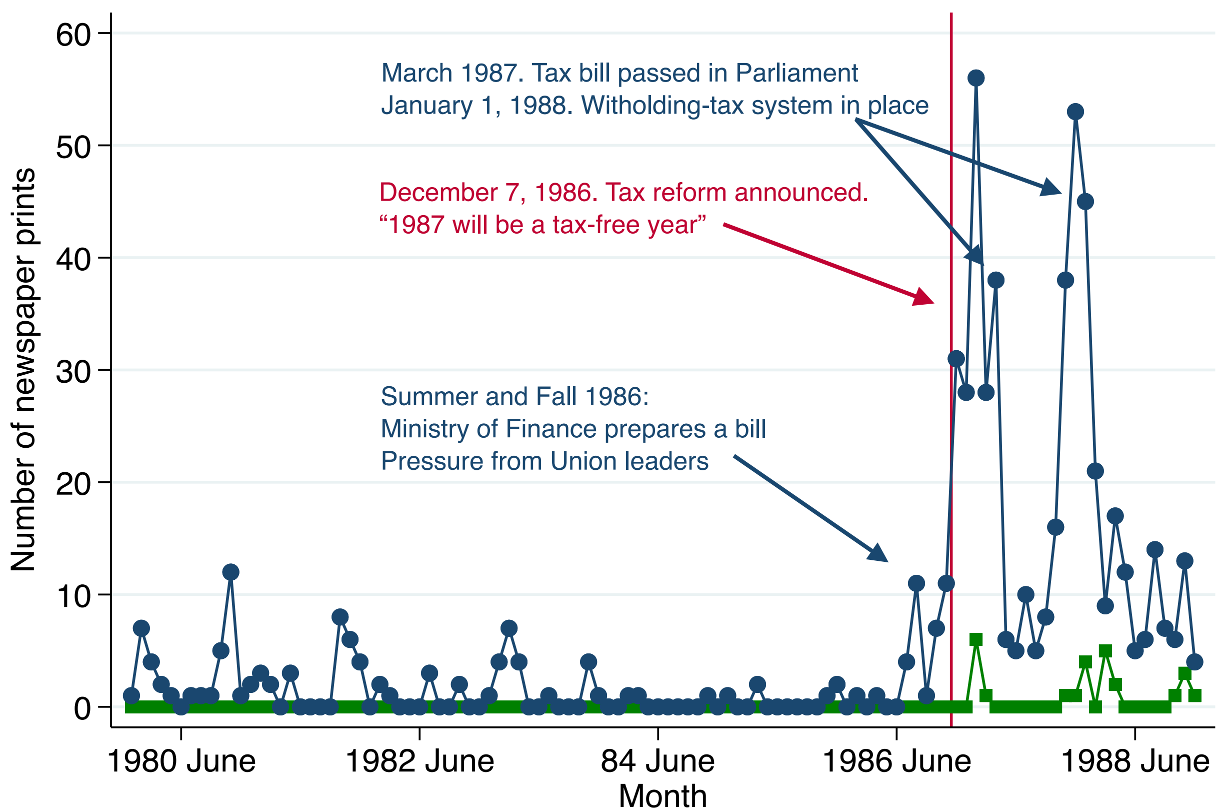
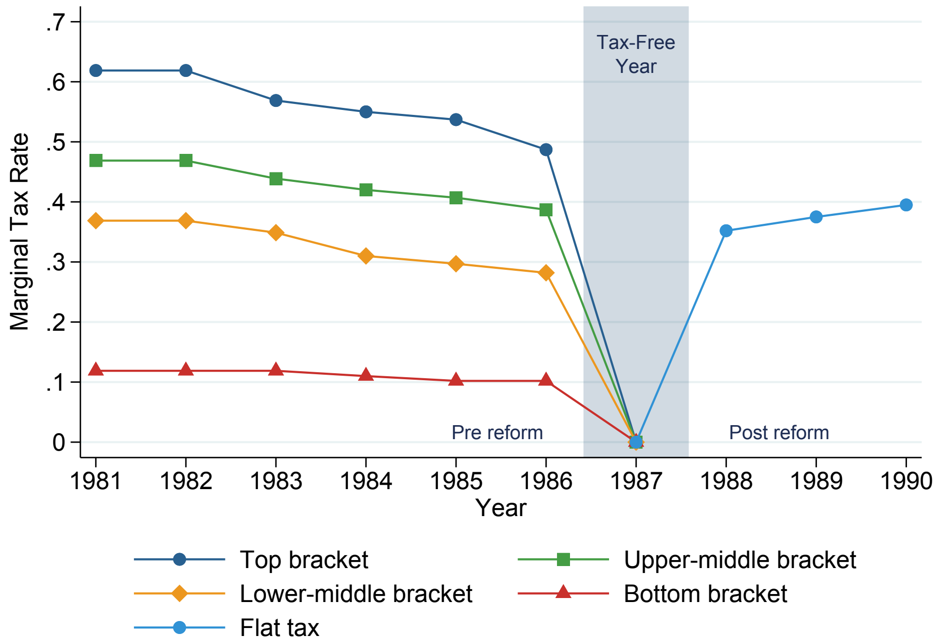
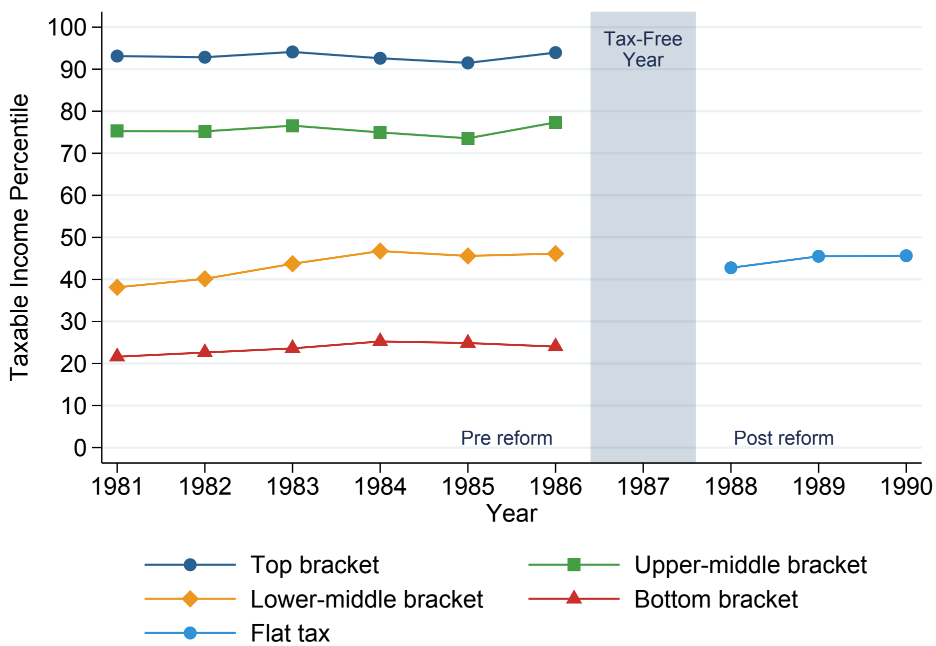


Figure 2: Number of Printed Newspapers Mentioning Withholding Tax

Notes: The figure plots in blue dots the number of printed newspapers mentioning a withholding-based pay-as-you-earn tax system each month during the period January 1980 to December 1988. Appendix B provides a detailed timeline of events. The keywords searched for were “Staðgreiðsla skatta” and “Staðgreiðslukerfi skatta”. In green squares, I plot a similar count of newspapers mentioning a flat tax system, as adopted in 1988. The keywords searched for were “eitt skatthlutfall”, “eitt skattþrep” and “flatur skattur”. The count is based on searches in the Icelandic newspaper database Tímarit.is for the six main newspapers (*Alþýðublaðið, Dagblaðið Vísir (DV), Dagur, Morgunblaðið, Tíminn, Þjóðviljinn*). The total number of printed newspapers per month is about 145 on average.



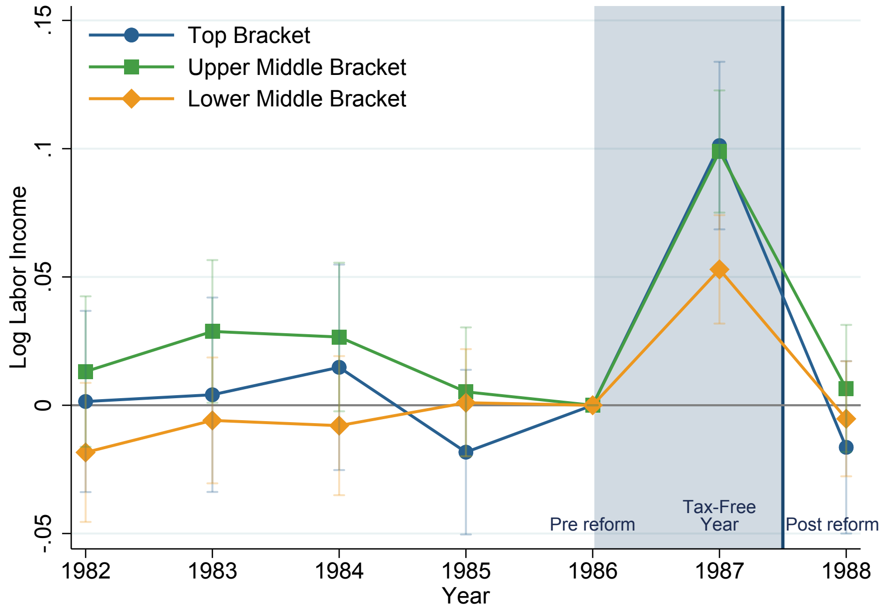
(a) Marginal tax rate by tax bracket



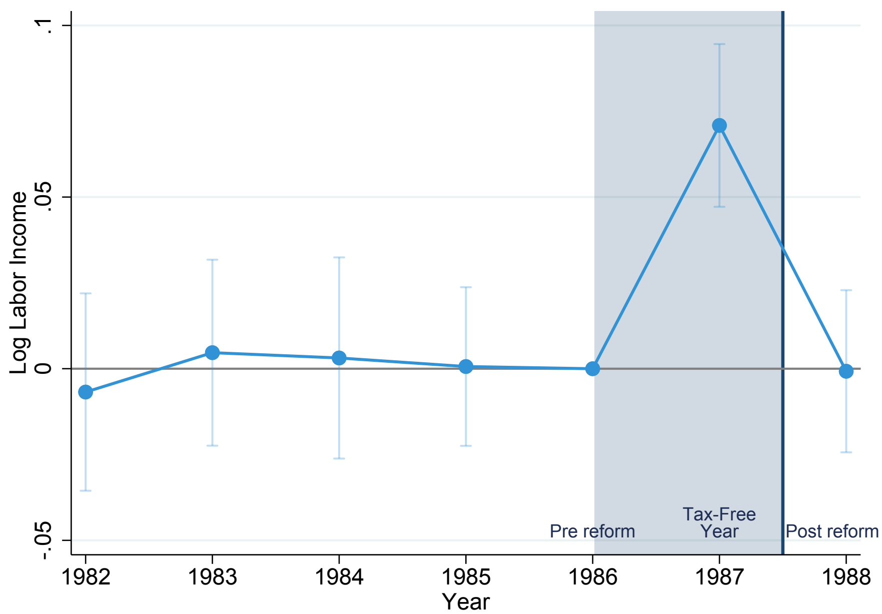
(b) Tax bracket thresholds in percentiles of income

Figure 3: Marginal Tax rates and Tax-Bracket Thresholds

Notes: The figure documents marginal tax rates and tax bracket thresholds before and after the tax-free year. Panel (a) shows the evolution of statutory marginal tax rates by tax bracket, where the local-level tax rate is the tax rate in Reykjavik, the capital city. For comparison, in 1986, the municipal tax rate was 10.2% in Reykjavik but ranged between 5% and 11.5% across municipalities. Small lump-sum and flat income taxes, such as health insurance contribution, cemetery charge, church tax and contribution to the construction fund for the elderly, excluded. Panel (b) shows the evolution of tax bracket thresholds, set in nominal values and updated regularly by the Icelandic Parliament to account for changes in prices and wages. The thresholds are the percentile of the taxable income distribution each year. Calculations assume that workers deduct the statutory minimum of 10% from their national-level income tax base each year. For more details on the Icelandic tax system and tax deductions, see Appendix A.



(a) Top-, Upper-middle, and Lower-middle brackets vs. bottom bracket



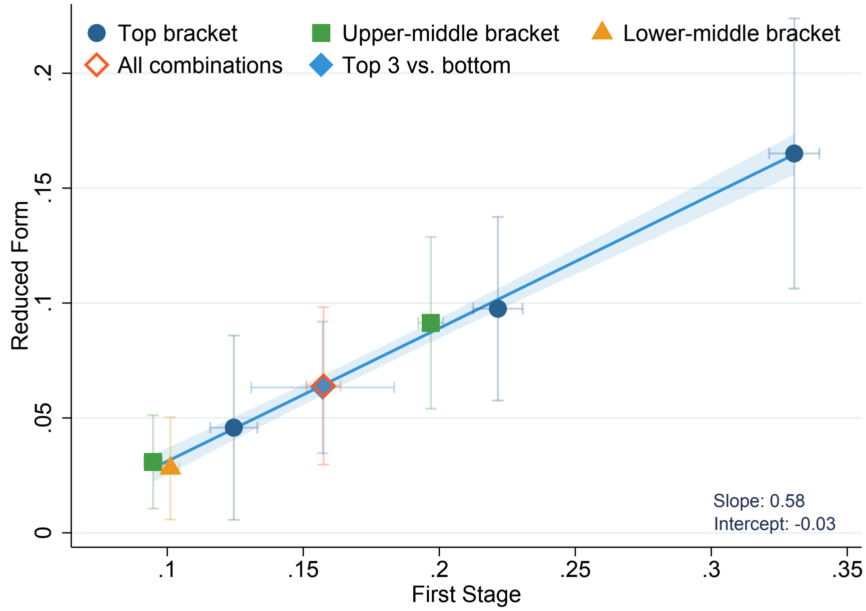
(b) Top three brackets vs. bottom bracket

Figure 4: Tax-Bracket Difference-in-Differences

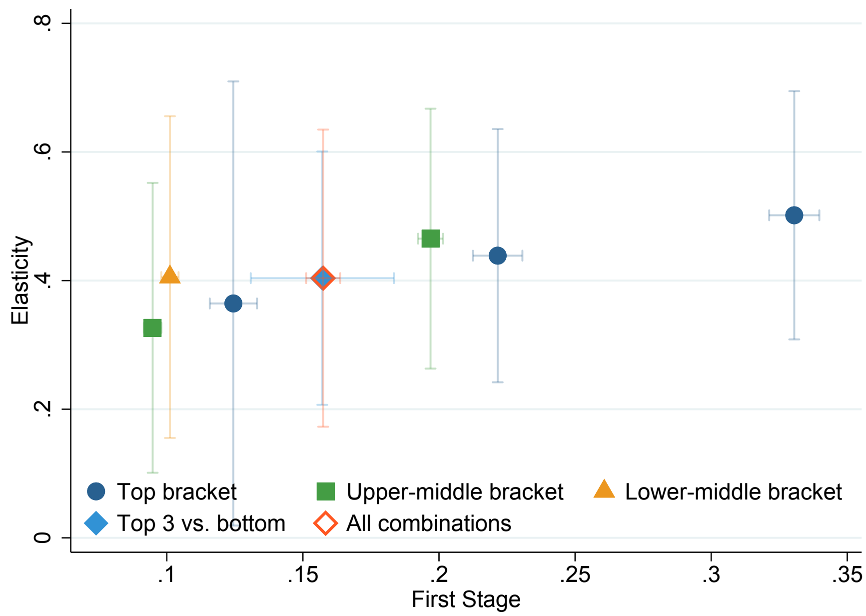
Notes: The figures plot estimates from a dynamic DID version of equation (1), estimated in the following regression

$$y_{it} = b_{i,t-1} + \delta_t + \eta_t \cdot \sum_{t=1982}^{1988} B_{i,t-1} \cdot \delta_t + \boldsymbol{X}'_{it}\gamma + \mu_{it},$$

where the outcome variable is log labor income. These plot the coefficients η_t, where $B_{i,t-1} \times \delta_{t=1986}$ is normalized to zero. Panel (a) plots estimates separately for each of the top three tax brackets. That is, for each of the three top brackets, $B_{i,t-1}$ is equal to one if workers belong to that bracket, but zero if they belong to the bottom bracket. Panel (b) plots estimates pooling across the three top brackets, i.e. $B_{i,t-1}$ is equal to one if workers belong to one of the three top brackets, but zero if they belong to the bottom bracket. The shaded area marks the period used to estimate the response to the tax-reform—the 1987 tax-free year compared to the pre-reform year 1986—and the labels highlight the pre and post-reform periods. The regressions control for gender, marital status, age, education, number of children, indicator for living in the capital area, and occupation in the previous year. Standard errors are clustered at the tax-bracket by municipality level for each year and the vertical bars plot the 95% confidence intervals.



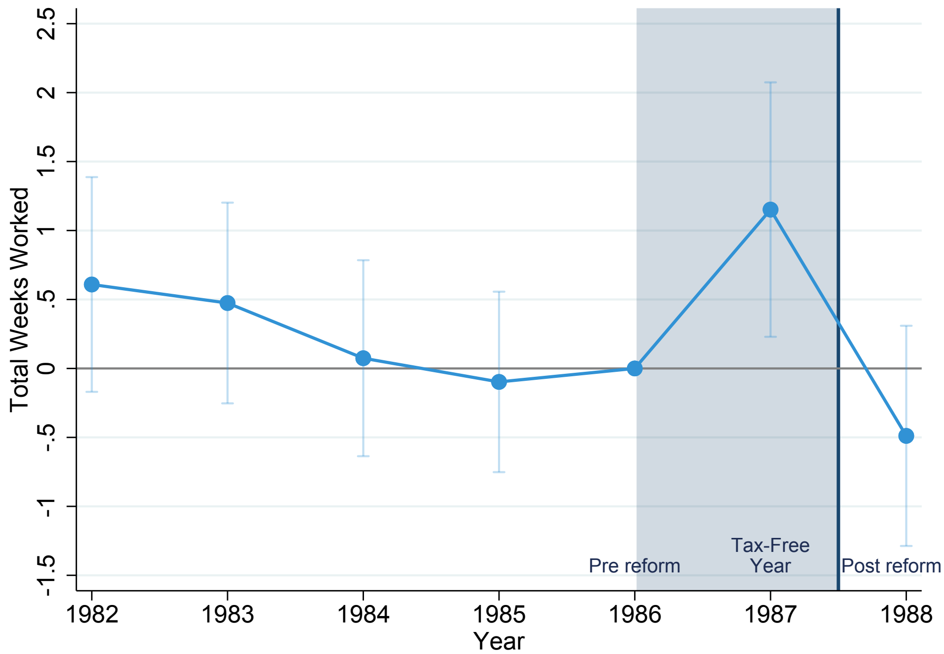
(a) Size of labor supply response (reduced form) by size of tax cut (first stage)



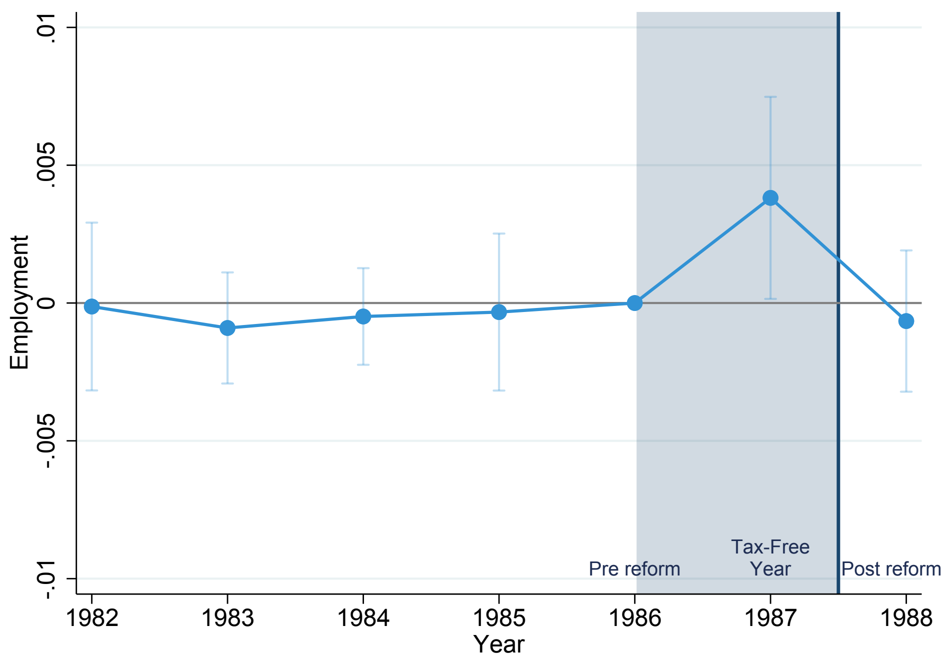
(b) Labor supply elasticity by size of tax cut (first stage)

Figure 5: Estimates of Labor Supply Responses by Size of Tax Cut

Notes: The figure plots estimates of labor supply responses by the size of the tax cut. Panel (a) plots the reduced-form estimates by the size of the first stage for each tax bracket combination as well as estimates for the top three tax brackets compared to the bottom bracket. In addition, it plots the average across all bracket combinations (6 estimates), weighted by the number of observations underlying each estimate. Each estimate is obtained from a difference-in-difference (DID) regression (1) comparing log labor earnings of workers in two brackets in 1987 (tax-free year) compared to 1986 (pre-reform). To allow for treatment effect heterogeneity, I match workers across brackets by gender, age in 5-year intervals, three groups of education, marital status, and number of children (0, 1, 2+). I weigh the regressions to account for differences in strata sizes across each pair of brackets being compared. Standard errors are clustered at the tax-bracket by municipality level and the vertical and horizontal bars plot the 95% confidence intervals of the reduced-form and first-stage estimates, respectively. Panel (b) plots the labor income elasticity estimates by the size of the first stage for each tax bracket combination. Each estimate is obtained from a two-stage least squares DID regression (2) where the outcome is log labor earnings. The matching procedure and clustering of standard errors is as in panel (a).



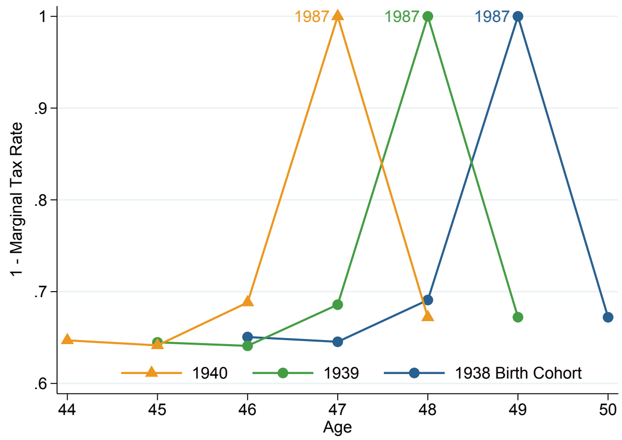
(a) Weeks worked



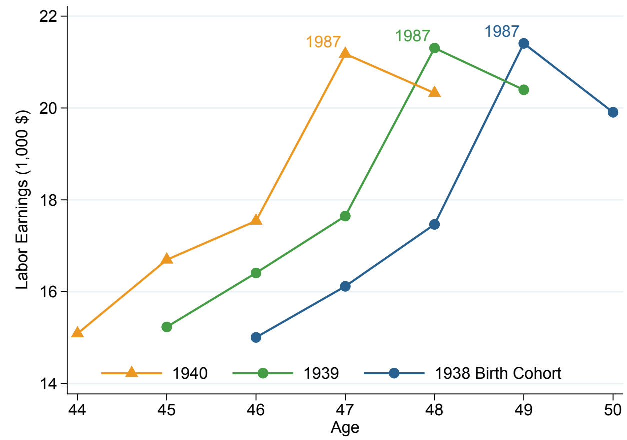
(b) Employment

Figure 6: Weeks Worked and Employment

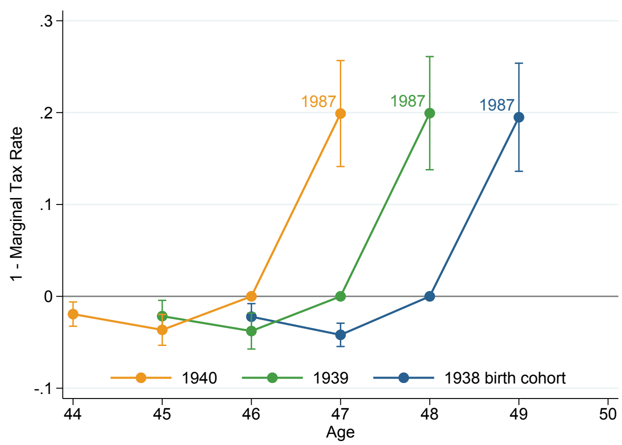
Notes: The figure documents the effect of the tax-free year on weeks worked and employment. Panel (a) plots coefficients from the dynamic version of regression (1) where the outcome variable is the total weeks worked per year. Details on the regression specification are in the note to Figure 4. The treatment group consists of workers in the three top tax brackets and the control group of those in the bottom bracket. The shaded area marks the period used to estimate the response to the tax-reform—the 1987 tax-free year compared to the pre-reform year 1986—and the labels highlight the pre and post-reform periods. The regressions control for gender, marital status, age, education, number of children, indicator for living in the capital area, and occupation in the previous year. Standard errors are clustered at the tax-bracket by municipality level and the vertical bars plot the 95% confidence intervals. Panel (b) similarly plots the coefficients from the same regression where the outcome is employment, defined as earning income above zero in the given year.



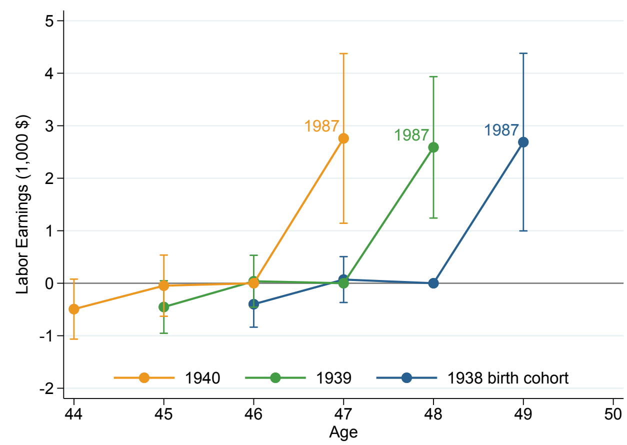
(a) Tax rates: Levels



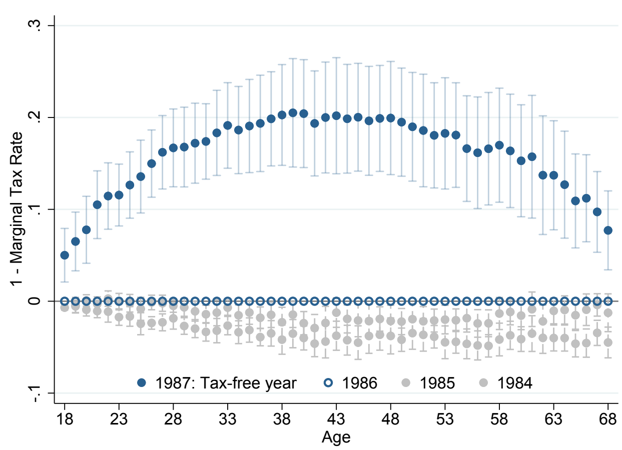
(b) Labor income: Levels



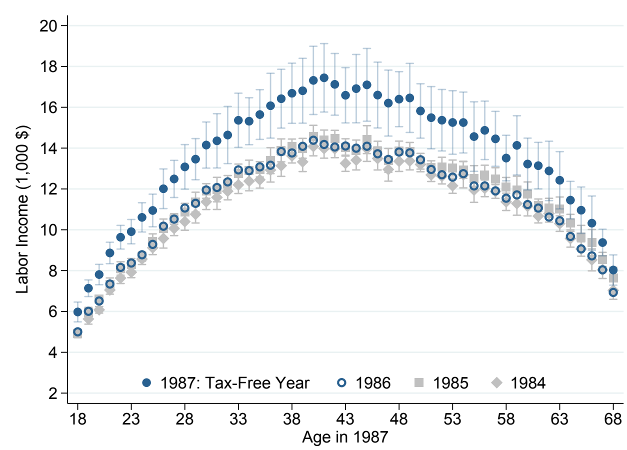
(c) Tax rates: Matched-cohort differences



(d) Labor income: Matched-cohort differences



(e) Tax rates: Matched-cohort differences



(f) Labor income: Matched-cohort differences

Figure 7: Life-Cycle Differences in Tax Rates and Labor Income

Notes: The figure provides a graphical illustration of the life-cycle differences research design by showing the evolution of tax rates and labor income across birth cohorts. Panel (a) plots the evolution of marginal tax rates for three birth cohorts (1940, 1939, and 1938), illustrating the staggering of when the birth cohorts experienced the tax-free year over their lifetime. Panel (b) plots the evolution of labor income, in 1,000 USD of 1986, for the three birth cohorts. This figure depicts similar trends among the three cohorts in the years before 1987 but a clear temporary divergence from that trend in the tax-free year. Panels (c) and (d) plot the matched-cohort differences in tax rates and labor income, respectively, for the same three cohorts. That is, individuals from each cohort are matched to individuals from the adjacent birth cohort, born one year earlier, following the procedure described in the main text. The figures plot coefficient estimates from regression equation (3), which removes the common life-cycle trends. Cohort differences are normalized to zero at their age in the year before the relevant cohorts experienced the tax-free year. Panels (e) and (f) plot the matched-cohort differences in tax rates and labor income, respectively, for all birth cohorts 1919-1970. Different from panels (c) and (d), these figures plot on the x-axis the age in 1987. Standard errors are clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, and vertical bars plot the 95% confidence intervals.

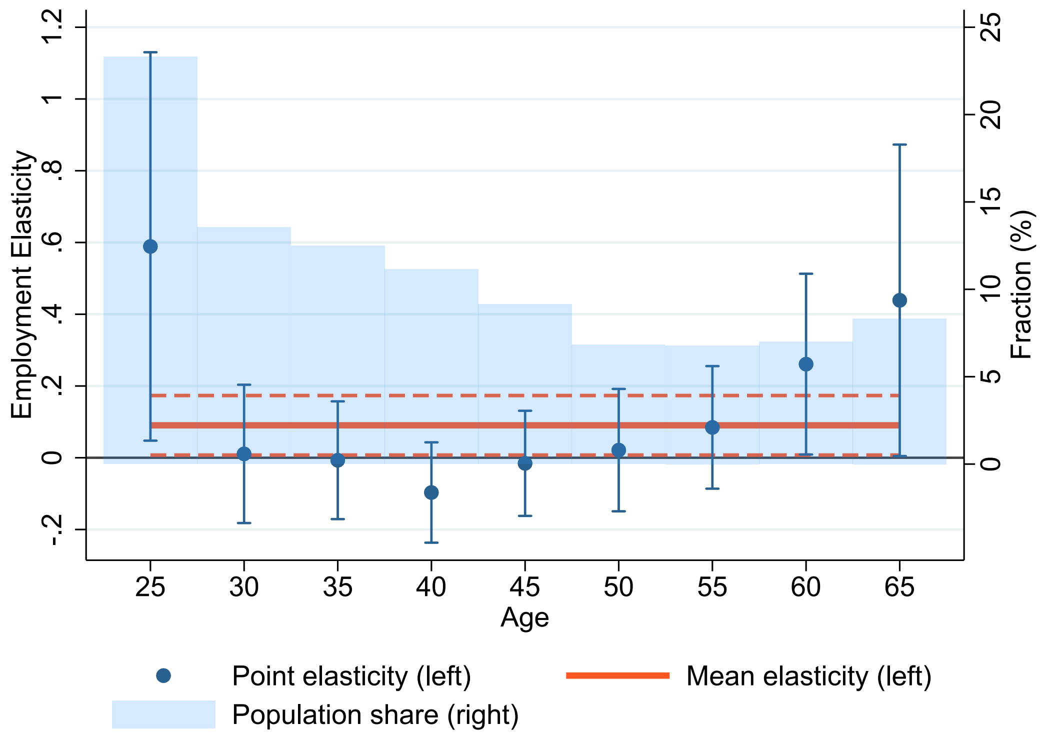
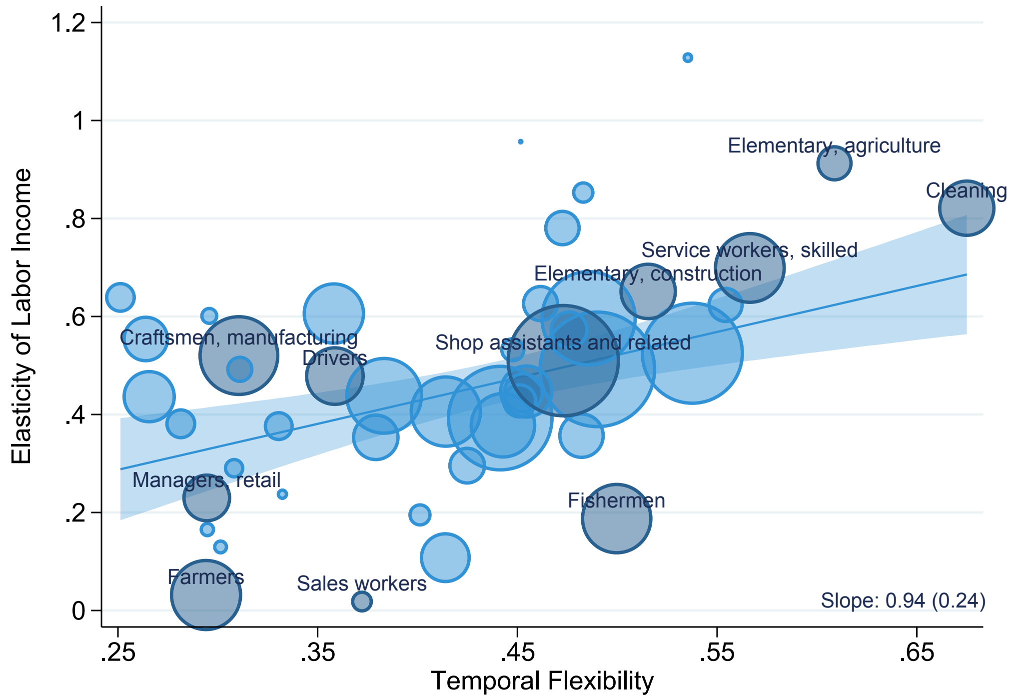
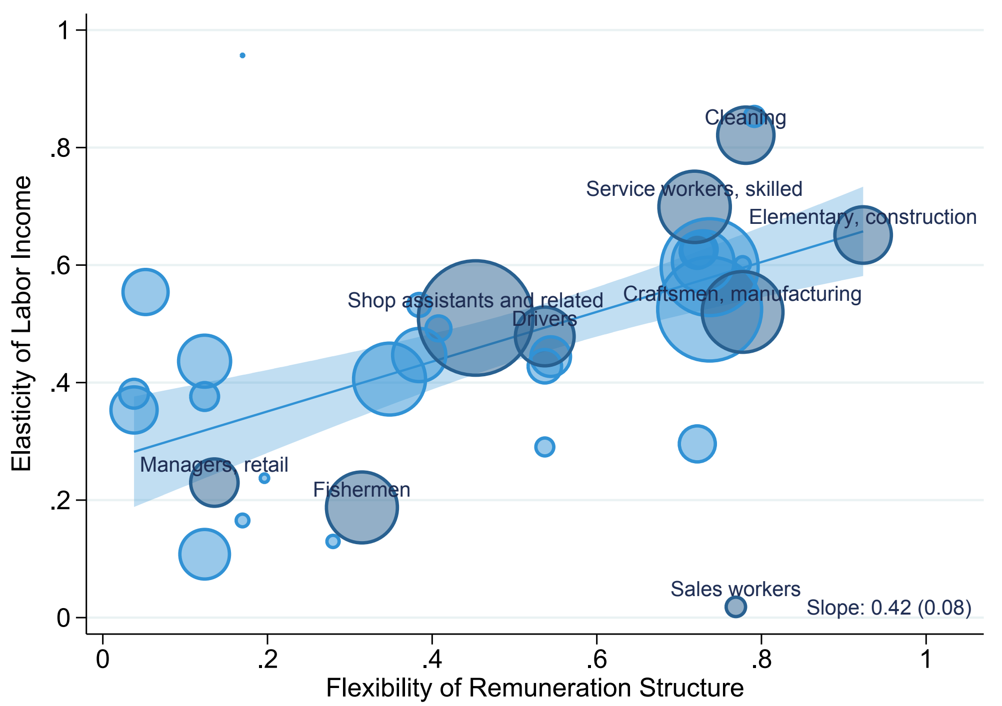


Figure 8: Extensive Margin Elasticity by Age

Notes: The figure plots the estimated employment elasticity by age. I estimate semi-elasticities using equation (4) for a set of cohorts and scale the estimates by the average employment rate of these cohorts in 1986. I group cohorts by age in 1987 and present estimates by age group, such that "25" refers to those at age 18-25; "30" to age 26-30; "35" to age 31-35; "40" to age 36-40; "45" to age 41-45; "50" to age 46-50; "55" to age 51-55; "60" to age 56-60; "65" to age 61-67. Standard errors are clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, and vertical bars plot the 95% confidence intervals. The horizontal line plots the average elasticity for the population and the dashed line the corresponding 95% confidence intervals. The shaded area (bars) is the population distribution, where each bar corresponds to the share of the working-age population (in %).



(a) Earnings elasticity by temporal flexibility



(b) Earnings elasticity by flexibility of remuneration structure

Figure 9: Labor Income Elasticities by Job Flexibility

Notes: Each panel plots the elasticity of labor income elasticity for workers by occupation against a measure of adjustment frictions for that occupation. In panel (a), "*temporal flexibility*" is measured using the coefficient of variation in weeks worked, i.e. the occupation-level dispersion in working time; see main text for details. In panel (b), "*Flexibility of Remuneration Structure*" is the share of workers within an occupation who work and are paid by the marginal hour; see main text for details. Occupation-level elasticities are estimated using the tax-bracket research design, described in Section 4, where I interact the treatment indicator with the occupation indicator and control for occupation fixed effects. The treatment group consists of workers in the three top tax brackets and the control group of those in the bottom bracket. The size of the dots in each graph is proportional to the number of workers in each occupation. The figure reports the slope of the regression line through elasticity estimates, weighted by occupation size.

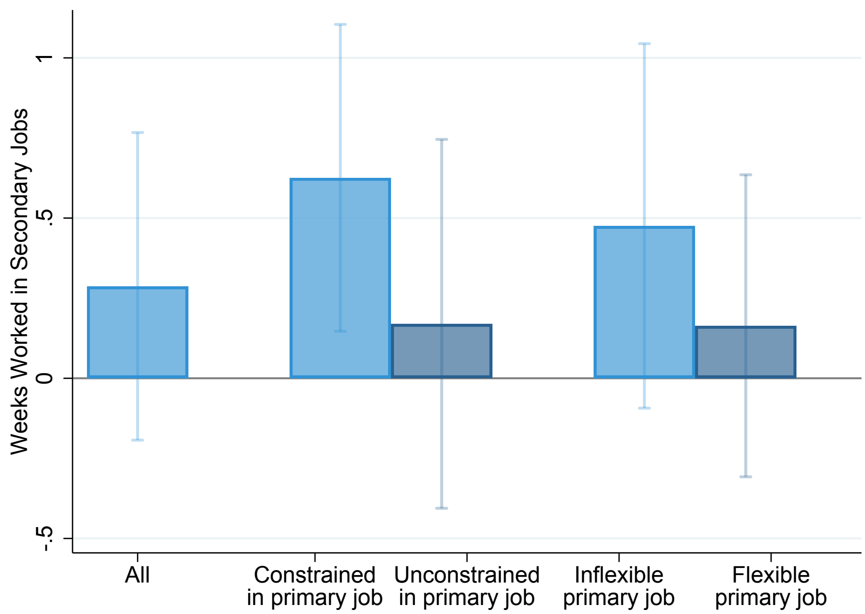
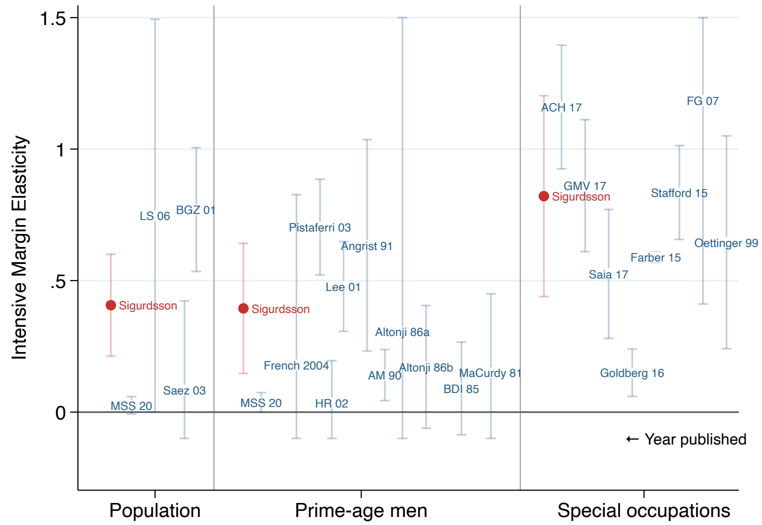
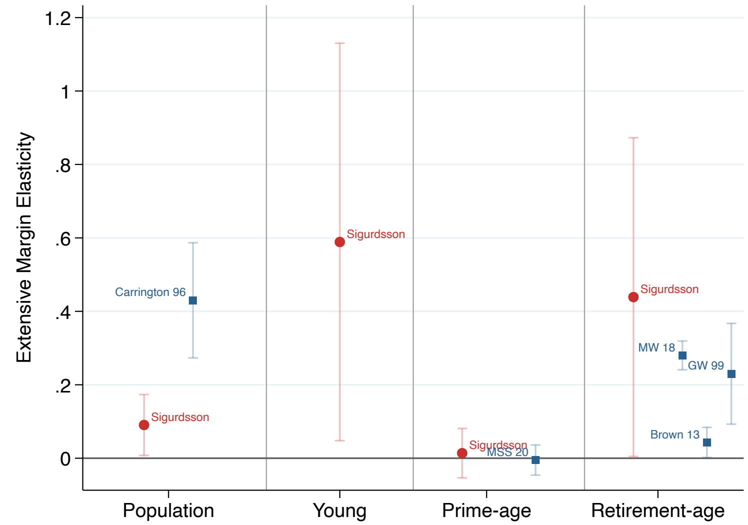


Figure 10: Effect on Weeks Worked in Secondary Jobs

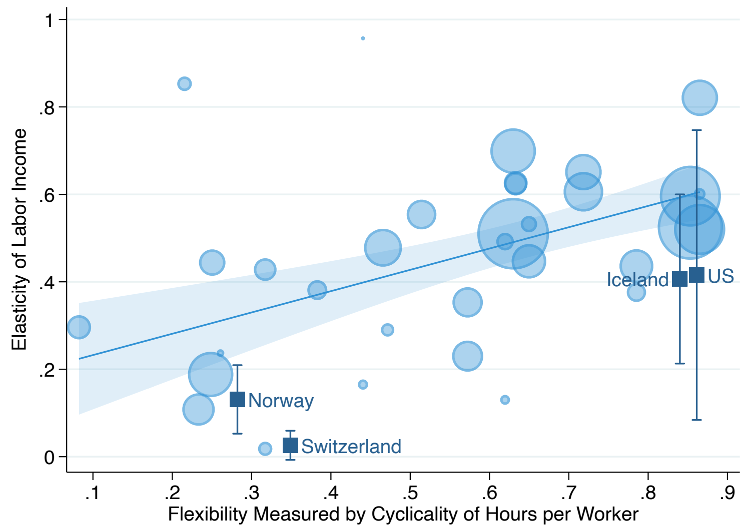
Notes: The figure presents the estimated effect on weeks worked in secondary jobs. The plots coefficient estimates from an estimation of equation (1), where the treatment group consists of workers in the three top tax brackets and the control group of those in the bottom bracket. The outcome variable is the number of weeks worked across jobs other than the primary job, defined as that where the worker earns the highest income. *"Inflexible primary job"* is an indicator for holding a primary job in an occupation with below-median "temporal flexibility", as measured in Section 6.1, but zero otherwise. *"Constrained in primary job"* is an indicator for working 52 weeks in the primary job in the prior year, but zero otherwise. Estimates by subgroups were obtained by interacting group indicators with the log of the net-of-tax rate as well as the respective instrumental variables. The regressions control for gender, marital status, age, education, number of children, indicator for living in the capital area, and occupation in the previous year. Standard errors are clustered at the tax-bracket by municipality level for each year and the vertical bars plot the 95% confidence intervals.



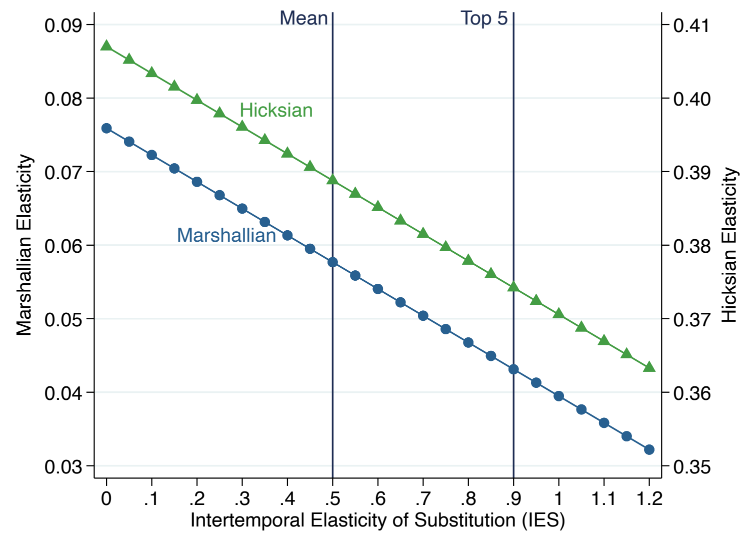
(a) Intensive margin Frisch elasticity



(b) Extensive margin Frisch elasticity



(c) Frisch elasticity estimates by flexibility in hours



(d) From Frisch to Hicksian and Marshallian Elasticities

Figure 11: Summary of Frisch Elasticity Estimates

Notes: Panel (a) summarizes prior estimates of the intensive margin Frisch elasticity and panel (b) summarizes extensive margin elasticity estimates. My estimates are in red dots. For the subgroup of special occupations, my elasticity estimate refers to the cleaning occupation, which has the highest temporal flexibility (Figure 9). Point estimates refer to the authors' main, representative, or preferred specification. The 95% confidence intervals are either based on reported standard errors or computed using the delta method. For details, see Appendix Table A.14. Panel (c) plots intensive margin Frisch elasticity estimates by country and occupation against the correlation between total hours and hours per worker. The solid squares reflect country-level elasticity estimates. For the US the square is the average across the two US estimates and the vertical bar spans the higher estimate (Looney and Singhal, 2006) and the lower estimate (Saez, 2003). Elasticity estimates for Iceland, Switzerland, and Norway are, respectively, from the current paper, Martinez, Saez, and Siegenthaler (2021), and Sigurdsson (2023a). Measures of cyclicality are from Sigurdsson (2011) and Rogerson and Shimer (2011); see Appendix F for details. The transparent circles are elasticities by occupation, as in Figure 9, plotted against the occupation-level correlation between total hours and hours per worker. The latter are constructed using administrative data on working hours and employment in the private sector. I compute the occupation-level correlations in the same way as for countries. The size of the dots is proportional to the number of workers in each occupation. Panel (d) reports values of the Hicksian elasticity, Marshallian elasticity, and intertemporal elasticity of substitution consistent with my estimate of intensive margin Frisch elasticity; see main text for details. The line "*Mean*" marks the average across EIS estimates summarized in Havránek (2015) and "*Top 5*" marks the average IES estimate across studies published in the top-five general interest journals.

Table 1: Effects on Labor Income, Weeks Worked, and Employment in 1987

	Log labor income		Weeks worked		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DID ($\frac{dy}{d\log(1-\tau)}$)	0.434	0.407	6.583	5.369	0.023	0.036
	(0.067)	(0.099)	(2.403)	(3.101)	(0.013)	(0.016)
Reduced form (dy)	0.073	0.064		1.249	0.004	0.006
	(0.011)	(0.015)		(0.491)	(0.002)	(0.002)
First stage ($d\log(1-\tau)$)	0.168	0.157	0.168	0.157	0.168	0.157
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Mean of outcome variable	—	—	49.37	49.37	0.993	0.993
Weighted	No	Yes	No	Yes	No	Yes
Observations	176,298	165,044	174,204	163,084	182,870	170,806

Notes: The table presents the results from difference-in-differences (DID) regressions, where each row and column entry corresponds to one regression estimate. The sample period for each regression is 1986-1987. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable (y) is defined in the top panel and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DID estimation of equation (1), where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage DID estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. "Weighted" refers to the regressions being weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table 2: Robustness of Effects on Labor Income in 1987

	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.407	0.431	0.329	0.357	0.339	0.307
	(0.099)	(0.137)	(0.104)	(0.079)	(0.102)	(0.093)
Reduced form ($d\log y$)	0.064	0.070	0.054	0.068	0.053	0.049
	(0.015)	(0.022)	(0.017)	(0.016)	(0.016)	(0.014)
First stage ($d\log(1-\tau)$)	0.161	0.161	0.163	0.189	0.158	0.159
	(0.013)	(0.013)	(0.013)	(0.018)	(0.013)	(0.014)
Treatment status	Baseline	Predicted - 1y	Predicted - 3y	Persistent - 3y	Baseline	Baseline
Controls	Baseline	Baseline	Baseline	Baseline	1988 MRT	1988 Tax paid
Weighted	Yes	Yes	Yes	Yes	Yes	Yes
Observations	165,044	162,758	151,905	67,910	165,044	165,044

Notes: The table presents the results from difference-in-differences (DID) regressions, where each row and column entry corresponds to one regression estimate. The sample period for each regression is 1986-1987. The top row presents results from a 2SLS estimation of equation (2), where the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DID estimation of equation (1). The bottom row presents results from a first-stage DID estimation of equation (1). "Predicted" refers to the tax-bracket being predicted when defining treatment status using information lagged for 1 or 3 years, and "Persistent" refers to defining treatment status based on being in the same bracket for past 3 years. Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. "Weighted" refers to the regressions being weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table 3: Effects of the Tax-Free Year on Extensive Margin and Aggregate Labor Supply in 1987

	Life-cycle differences			Triple differences		
	Employment (1)	Log Income (2)	Weeks (3)	Employment (4)	Log Income (5)	Weeks (6)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.056	0.843	6.547	0.008	0.346	2.186
	(0.026)	(0.033)	(0.782)	(0.012)	(0.031)	(0.719)
Reduced form (dy)	0.005	0.162	1.182	0.000	0.105	0.642
	(0.003)	(0.005)	(0.150)	(0.002)	(0.197)	(0.002)
First stage ($d\log(1-\tau)$)	0.098	0.182	0.182	0.156	0.280	0.281
	(0.003)	(0.005)	(0.005)	(0.011)	(0.015)	(0.015)
Mean dependent variable	0.618	—	40.71	0.871	—	47.74
Elasticity	0.090	—	0.161	0.009	—	0.046
Observations	551,383	533,770	551,383	369,164	367,411	359,450

Notes: The table presents results from life-cycle differences regressions, equation (4), and triple-differences regressions, equation (5), where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation, where the dependent variable (y) is defined in the top panel, and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form estimation, where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage estimation, where the outcome variable is the logarithm of one minus the marginal tax rate in columns (2), (3), (5), and (6) and one minus the average tax rate in columns (1)–(4). Regressions control for match-strata fixed effects, i.e. group fixed effects where each group is a cell used in coarsened exact cohort matching. Elasticity is calculated as the ratio of the semi-elasticitiy (top row) and the mean of the dependent variable. Robust standard errors clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, are in parentheses.

Online Appendix of:

Labor Supply Responses and Adjustment Frictions: A Tax-Free Year in Iceland

Jósef Sigurdsson

January 23, 2024

A Overview of the Icelandic Income Tax System

Up until and throughout 1987, income taxes in Iceland were collected with a one-year lag. That is, the tax payments made throughout every year were based on the income earned in the year before. In practice, early each year, an income tax return was filed for the income earned the previous year, including other components such as deductions to be made, assets and liabilities for the calculation of wealth taxes, etc. The outstanding tax liability was then computed based on this information. Throughout the year, taxes were then paid in ten equal payments on the first day of each month of the calendar year, except January and July. At the beginning of the year, and before taxes had been computed, taxpayers paid a fixed share (decided by the Directorate of Internal Revenue, DIR) of their payments in the preceding year. Once the tax returns had been compiled and the correct tax payment had been computed, the difference between the outstanding tax liability and the tax installment payments already made was divided equally between the remaining months of the year to find the monthly payment. After the reform, taxes on income earned in year t were collected during year t through "withholding at source". That is, employers deducted taxes from their employees' paycheck and remitted them to the government.

Although this system had some advantages, such as easing the work of the tax authorities in taking into account a range of tax deductions and allowances to arrive at the correct tax liability, it had obvious drawbacks, for both taxpayers and the collectors of tax revenue. Taxpayers with variable or cyclical income, such as those employed in the fishing sector or in agriculture, faced a countercyclical variation in their tax burden relative to their current income. From the perspective of the government and the municipalities, this system could be a handicap, as their revenues were misaligned with, e.g. the price level of their current expenses.

Income taxes in Iceland are levied at two levels: a national tax and a local municipal tax. As described in Section 2, during 1987, all taxes on labor income at both levels were set to zero. The tax schedule prior to the reform consisted of three national-level brackets and a municipal tax. In addition, there were a few small and lump-sum income taxes, such as the health insurance contribution, cemetery charge, church tax and contribution to the construction fund for the elderly. All taxable income—both labor and capital income—was taxed equally and in the same way at the national and municipal levels.[49] Before arriving at the tax base, multiple deductions could be made. As these deductions differed substantially between the national and municipal levels, the tax base for the two levels was different. The components that were deductible at both levels included fringe benefits; travel allowances; purchases of tools, machines and instruments; mandatory savings; child support; and education-related costs. At the national level there were various other deductions such as a special fisher's deduction, deductions for each day spent at sea, special deductions for the costs of starting a family ("wedding deduction"), interest expenses, pension savings, union membership fees, charitable gifts, etc. Moreover, in exchange for a subset of these options for deduction, the tax law offered taxpayers the option to instead deduct a fixed 10% from the national-level tax base, an option many exploited. While including both labor and capital income as the national-level tax base, pension and social security benefits were not part of the municipal tax base but were included in the national-level tax base. To summarize, the tax base at the municipal level tended to be higher than that at the national level. Because of those features, the progressive income tax schedule consisted of four brackets, consisting of three national-level brackets and a municipal tax. In addition, each worker had a personal tax allowance, both at the municipal and national levels, deducted from the computed tax payments. At the national level, this amount was fixed and was the same for everyone, but the municipal allowance depended on marital status and the number of children. The allowance at both levels was deducted from the outstanding tax liability.

Since 1978, Iceland has had an individual tax system, such that married and cohabiting individuals have been taxed as single units, not jointly. Therefore, each spouse files his/her own tax return, and has a separate tax allowance and deductions. However, the tax system has some joint aspects that were incorporated into the tax system with the aim of lowering the tax burden of two-adult households with a single earner and households with low-income secondary earners. First, married and cohabiting individuals were allowed to transfer to their spouses both their personal tax allowance and tax deductions that remained unaccounted for after their own income taxes had been paid in full.[50] Second, married or cohabiting workers whose spouses were out of the labor force or with a very low income could increase the amount taxed in the first bracket by up to half of what remained after their spouses' income was fully accounted for.

The tax rates were frequently reviewed in relation to the government's budget. Although national-level tax rates had been on a slight decreasing trend throughout the 1980s, as documented in Figure 3a, the difference across brackets had remained stable. Moreover, the tax bracket thresholds, which were set in nominal values and reviewed and updated yearly to account for changes in prices and wages, represent roughly the same income percentile over time, as shown in Figure 3b in the main text. The figure also documents that the bottom-bracket threshold, below which individuals do not pay the national-level income tax, corresponds to roughly the 40th percentile of income throughout the pre-reform period. However, as the tax base for the municipal tax was different and generally higher than the national-level tax base, the share of workers who fall below the bottom-bracket threshold pay the municipal tax.

[49] A separate taxation of labor and capital income was introduced in 1997.

[50] Following the reform, however, the share of the personal tax allowance that was transferable between spouses was reduced from 100% to 80%.

Due to the reform, many of the deductions that were an integral part of the old tax system were abolished. These included a deduction for newly married couples, mandatory pension savings, union membership fees, interest payments on loans and mortgages, various work-related deductions and a 10% fixed deduction. Deductions from the municipal tax were abolished, but the tax rates were lowered such that the municipal tax revenue was almost unaffected. As a result, the tax base at the national and municipal levels became the same after the reform. In addition, other adjustments were made to the tax system, such as replacing the interest payment deduction with an interest allowance and a housing allowance for first housing purchases, paying out child benefits directly instead of being integrated into the tax system, and incorporating minor fees such as fees to the church and cemeteries into the main income tax, all of which simplified the tax system and made it easier to manage for the authorities. In exchange for the deductions in the old system, the personal tax allowance was increased by half and now served as a single source of tax deduction, with the aim of keeping the tax burden the same in the new and simplified system.[51]

B The Tax Reform and the Timeline of Events

On January 1, 1988, Iceland took up a withholding-based pay-as-you-earn income tax system. Prior to the reform, income taxes were collected with a one-year lag. That is, as depicted in Figure 1, the tax liability and tax payments due every month in year t were computed based on income in year $t-1$. This system was similar to that in place in most Western countries prior to adopting the modern pay-as-you-earn tax systems.[52] When the tax reform was announced on December 6, 1986, it was also announced that during the transition year of 1987, labor income would not be taxed. As Figure 1 depicts, this implies that while people were paying taxes every year, including in 1987 when they paid taxes based on their income earned the year before, all income earned in 1987 was tax free. Therefore, the reform did not influence the government's budget, as the tax revenue flows were uninterrupted, and nor did it generate a cash-flow effect on workers.[53] However, as all marginal income earned in 1987 was tax free, the reform generated a strong incentive for intertemporal substitution: work more during the tax-free year and less in other years.

On December 6, 1986, the Finance Minister announced a tax reform to take place in January 1988 when a system where taxes were collected with a one-year lag would be replaced with a pay-as-you-earn withholding tax system. An important part in understanding the implications of the tax-free year is understanding how and when the Icelandic population learned about this change. As evidence on when the population learned about the reform, Figure 2 plots the monthly count of

[51] In 1988, the personal tax allowance equaled 22.6% of the average income compared with 12.7% in 1986.

[52] The US transitioned to a withholding-based PAYE system in 1943, when the Current Tax Payment Act was passed, and the UK reformed its system in 1944 after trials in 1940/41. Sweden passed a law establishing a PAYE system in 1945 that was implemented two years later. Similarly, Norway passed a law in 1952 but the reform took place in 1957 and Ireland passed a law in 1959 with a reform the following year. More recently, Switzerland transitioned to a PAYE system in 1999–2003. France is the last holdout of the Western countries, but a reform is currently underway.

[53] The modern income tax system was established in 1877. The tax laws, specifying progressive taxes collected with a lag, were passed four years after Iceland's constitution was proclaimed and the country was granted home rule, after having been part of Denmark until 1874. When giving a tax-free year in 1987, the government was essentially giving up one year's tax revenue, which will be evident that it was lost by examining the Treasury's position on "Judgment Day".

the number of newspapers mentioning a withholding-based or pay-as-you-earn tax system between January 1980 and December 1988, i.e. almost seven years before the announcement.

When the reform was announced, and for a long time before, there was a broad political consensus that tax reform was needed. The first records of a pay-as-you-earn system being discussed in the Icelandic Parliament date back to the mid-1960s (Olgeirsson, 2013). Neighboring countries, such as Norway, Sweden, the US, the UK and Ireland, had already introduced such a system in the 1940s and 1950s. Icelandic politicians, as well as the labor unions, publicly highlighted the defects of the existing system and the benefits of introducing a withholding-based system. However, discussions and attempts in 1978 and 1981 were unsuccessful, mainly because adopting a withholding-based tax system using the existing tax code was technically complicated or infeasible due to the structure of the tax system, which had a range of deductions and transfers that would complicate the calculations and likely lead to large differences between the income tax withheld during the year and the tax payable at the end of the year (Olgeirsson, 2013).

In the fall of 1986, the Ministry of Finance began preparing a tax reform. In November, the Finance Minister formed a committee to work on a proposal revising the income tax system. Around the same time, in late November and early December 1986, national-level union bargaining on general employee rights and minimum wages was in progress. Traditionally, the bargaining often effectively takes a form of tripartite negotiations, with the government usually having an input at later stages to close the contracts.

On December 6, 1986, new collective agreements were signed and the Finance Minister announced the tax reform, which was the government's input to a settlement. The pay-as-you-earn tax system was scheduled to be implemented on January 1, 1988. The Finance Minister ordered the aforementioned tax-reform committee to prioritize proposing simplifying changes to the income tax system that would be necessary for an implementation of a withholding-based tax system. To avoid a heavy tax burden and "double taxation" during the transition to the new system, i.e. that workers would pay taxes on both income earned in 1986 and 1987 using their 1987 income, it was decided that all labor income earned in 1987 would be exempt from taxes.[54] Naturally, the reform received much media attention in the following days and weeks. Newspapers printed headlines such as "*A Tax-Free Year*" and "*Pay-as-you-earn tax system in 1988 – all income in 1987 tax-free*". Politicians and union leaders emphasized the opportunity that this reform provided, and in an interview, the chairman of one of the largest labor unions was quoted as saying "*Now it is time for everyone outside the labor market to enter, and for all workers to earn tax-free income. There is work for everyone that wants to work.*"[55]

Based on the proposals set forth by the tax-reform committee, four parliamentary bills were prepared in the first weeks of 1987. These served the purpose of paving the way and preparing the transition to a pay-as-you-earn tax system, either directly or indirectly by simplifying parts of the tax

[54]Although policy makers are likely to want to make some adjustments to tax payments during a transition, a tax-free year was not the only option. There are two options for such adjustments: forgive outstanding (or some) tax liabilities in the transition period, or collect no (or lower) taxes on income earned during the transition period. When the US established a withholding-based tax system in 1943, the adjustment took the form of the forgiveness of most outstanding tax liabilities. According to the Current Tax Payment Act of 1943, 75% of the 1942 tax liability was canceled with the remainder being due in two equal payments on March 15, 1944 and March 15, 1945 (Paul, 1954).

[55]See *Morgunblaðið*, December 7, 1986.

system necessary for the transition. A specific law was passed specifying that labor income earned in 1987 should not be taxed, and a law on the timing of the transition taking place on January 1, 1988, as had been scheduled when the reform was first announced. During March 16–18, 1987, all bills necessary for the new tax system were passed by the Parliament and signed into law.

In practice, workers and firms were to collect information as usual and file taxes at the beginning of 1988 as in earlier years. The tax authorities sent out advertisements emphasizing that the requirement for enjoying a tax-free year was to file taxes as usual, and they produced flyers explaining the new tax system and that income earned in 1987 was tax free (see Appendix Figures A.3 and A.4). For those who would not file their taxes, their income would be approximated based on their income in the year before and they would be taxed as in a normal year. Reporting information as usual was also important because other taxes, such as on capital income and wealth, and benefits were unchanged in 1987; the only change in that year was that income taxes were set to zero.[56]

While the general rule was that all labor income in 1987 should be exempt from taxes, some attempts were made to prevent an abuse of the reform. The documents and explanations associated with the law explicitly expressed a very positive view and encouragement of the legislature towards workers, exploiting the opportunity that the reform provided to increase their disposable income in 1987 by increasing their labor supply by any or all means. However, a clear aim was that any abuse of the reform by entrepreneurs or firm owners should be prevented. The law therefore specified two exceptions to the general rule. First, increased earnings in 1987 that were not due to more work or changes in employment arrangements, such as promotion, but rather reflecting transfers of income from other years should be taxed as usual. Second, inflation-adjusted increases in earnings of self-employed workers and business owners exceeding 25% should be taxed as usual. Studying the records, however, I find that these measures seem to have played only a limited de facto role.[57]

C Data and Measurement

The following appendices provide a further description of the data and measures provided in the main text.

C.1 Tax Calculator

Marginal tax rates are not directly observed in individuals' tax returns. Marginal tax rates and in which tax bracket individuals' next krona of income falls are crucial for my analysis. As there exists no tax simulation model for Iceland, such as the NBER TAXSIM model which computes marginal tax rates in the US, I constructed a tax calculator for the Icelandic tax system. The calculator uses details

[56] After the tax returns had been processed, the tax office computed how much of the income taxes due should be waived based on reported labor and capital income. For workers with no taxable capital income, this share would be 100%.

[57] Based on administrative tax records, there were only 255 cases where individuals had excess income taxed on these grounds. One potential implication of these clauses, as well as an interpretation of the fact of so few cases of income being taxed as transferred income, is that self-employed workers and business owners cluster (or bunch) at their permitted income growth of 25%. When studying this possibility, I find limited evidence of bunching, indicating that these conditions were in most cases not strictly binding.

of the Icelandic tax system in each year, taking into account all tax deductions as well as family aspects of the tax system, such as transfers of tax allowance and extensions of tax brackets due to low spousal income.

The total marginal tax rate is calculated as the sum of the municipal income tax rate (*útsvar*) and the national income tax rate. The individual's marginal tax rate is found as follows. The municipal tax is a flat tax rate, which therefore corresponds to a marginal tax rate on the municipal-level tax base after accounting for deductions. At the national level, there were three tax brackets until 1986 and a flat tax rate in 1988 and onwards. In order to compute the marginal tax rate, I first compute the income tax base by summing over all relevant measures of income and withdrawing all relevant deductions. All necessary information is reported separately in tax returns (and the final tax base in 1985 onwards). Then, the income tax in each bracket is calculated based on the individual's tax base. Married and cohabiting individuals whose spouses have a sufficiently low income, or are out of the labor force, can increase the amount taxed in the first tax bracket by up to 50%. The calculation of taxation in each bracket accounts for this. From the total income tax calculated, I withdraw their own tax allowances and, in some cases, transferred allowances between married and cohabiting individuals. This provides the total income tax payable and, depending on in which tax bracket the next krona earned would be taxed, the marginal tax rate.

Empirically, the tax calculator is accurate and in the years prior to the 1987 reform, it predicts actual liabilities within 10 ISK ($\approx$ \$0.25) for 97.5% of tax filers. The discrepancy is largely because of inaccurate information related to moving, within or outside Iceland, as the accuracy increases to 99.5% when I restrict my attention to national-level taxes only.

To calculate the average tax rate, I divide the national and municipal income tax payable by the respective tax base (accounting for differences in deductions at the national and municipal levels). The total average tax rate for an individual is then the sum of the two.

C.2 Summary Statistics

Table A.10 presents summary statistics in 1986 for the population of 16–70-year-olds as a whole for all wage earners and for self-employed individuals. The average age in the population is 38 years and 45% of the population are women. About 36% have a junior college degree (post-compulsory schooling) and 10% have a university degree. Among those with nonzero labor earnings, the average weeks worked is 41. The average marginal tax rate was 19% and the average tax rate—computed as the average tax payments divided by the tax base—was roughly 11%.

C.3 Occupation and Sector Classification

Pay slips include information about occupation according to a two-digit classification. There are 74 separate occupation classes recorded. The occupation classification is based on the International Labor Organization's (ILO) International Standard Classification of Occupations (ISCO), version ISCO-88. More details on the classification are provided in documentation on ILO's website. Table A.11 documents the structure of the classification and lists the broader occupation groups.

The pay slips also record the sector for each firm. In total there are 189 separate sector classes recorded. The sector classification is based on the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC). Details about the classification are provided in documentation on UN's website. Table A.12 documents the structure of the sector classification.

C.4 Education Classification

In the analysis, we use data on educational attainment from Statistics Iceland's Education Register. This source contains yearly data on the highest level of education completed in that year. The data set is categories of education attained according to the Icelandic national standard for the classification of educational attainment, *ÍSMENNT2011*, which builds on the international standard classification of education, *ISCED 2011*, but taking into account education attained by Icelandic students from the early 20th century onwards. This classification, as the ISCED, divides education attained into nine categories, out of which six are further subdivided leading to a complete set of 31 educational classes.

D Life-Cycle Labor Supply Model and Identification Strategy

Section 5 in the main text develops an identification strategy motivated by the life-cycle labor supply model of MaCurdy (1981). For the purposes of explaining and illustrating the intuition behind this method, this section in the appendix lays out the MaCurdy (1981) model and discusses in turn labor supply responses to evolutionary wage changes, anticipated transitory wage changes, and an unexpected transitory wage change (tax-free year). I then illustrate how the model informs about how labor supply elasticities can be estimated.

D.1 Model

In this model, individual i lives for $T+1$ periods, where in each period the individual has a time endowment of $\bar{L}$, faces no restriction of borrowing at the rate r_t, and the rate of time preference is denoted by ρ. Then the individual's optimization problem can be stated as follows:

$$\max_{\{C_{it},L_{it}\}} \sum_{t=1}^{T} \frac{1}{(1+\rho)^{t-1}} U_{it}(C_{it}, L_{it}), \quad N_{it} = \bar{L} - L_{it} \tag{10}$$

subject to

$$A_{it} = (1+r_t)A_{it-1} + w_{it}N_{it} - C_{it} \tag{11}$$

where A_{it} is the net wealth in each period. Assume that individual i's within-period utility can be described with the following additively separable function:

$$U_{it}(C_{it}, L_{it}) = \gamma_{C_i t} C_{it}^{\alpha_C} - \gamma_{N_{it}} N_{it}^{\alpha_N}, \quad N_{it} = \bar{L} - L_{it} \tag{12}$$

Note that α_C and α_N are constant and common across all workers, while γ_{C_it} and γ_{N_it} are individual- and age-specific parameters describing the tastes for consumption and leisure. It is assumed that (the log of) taste for leisure is

$$\log \gamma_{N_{it}} = \sigma_i + \mu_{it} \tag{13}$$

where μ_{it} is a random error term (i.i.d., mean zero). The Frisch labor supply equation can then be written as

$$\log N_{it} = \frac{1}{\alpha_N - 1} \left(\log \lambda_{it} - \log \alpha_N + \log w_{it} - \sigma_i + \mu_{it} \right) \tag{14}$$

The Frisch consumption demand function can be written in a similar fashion. In (14), λ_{it} is the Lagrange multiplier on wealth. From the envelope theorem, we have that

$$\lambda_{it} = \frac{1 + r_{t+1}}{1 + \rho} \lambda_{it+1} \tag{15}$$

Taking logs and using the approximation around zero that $\log(1 + x) \approx x$, we have

$$\log \lambda_{it} \approx r_{t+1} - \rho + \lambda_{it+1} \tag{16}$$

Using the above approximation, the labor supply equation (14) can be written as follows

$$\log N_{it} = F_i + bt - \varepsilon R_t + \varepsilon \log w_{it} + u_{it} \tag{17}$$

where

$$F_i = \frac{1}{\alpha_N - 1} \left(\log \lambda_i - \sigma_i - \log \alpha_N \right), \quad \varepsilon = \frac{1}{\alpha_N - 1}, \quad b = \sigma \rho, \quad u_{it} = -\sigma \mu_{it}$$

As in MaCurdy (1981), let us assume a linear approximation of F_i, such that

$$F_i = Z_i \theta + \sum_{t=1}^{T} \gamma_t \log w_{it} + A_{i0} \theta + \alpha_i \tag{18}$$

where Z_i is a vector of individual characteristics and α_i is a residual. Moreover, let us assume that wages follow a quadratic lifetime path:

$$w_{it} = \pi_{0i} + \pi_{1i} t + \pi_{2i} t^2 + \nu_{it} \tag{19}$$

where $\pi_{0i}, \pi_{1i}, \pi_{2i}$ are linear functions of the form

$$\pi_{ji} = M_i g_j, \quad j = 0, 1, 2,$$

with M_i being a vector of determinants of wages that are exogenous and constant over the lifetime, such as education, g_j are vectors of parameters, and ν_{it} is an error term. Substituting (19) into (20)

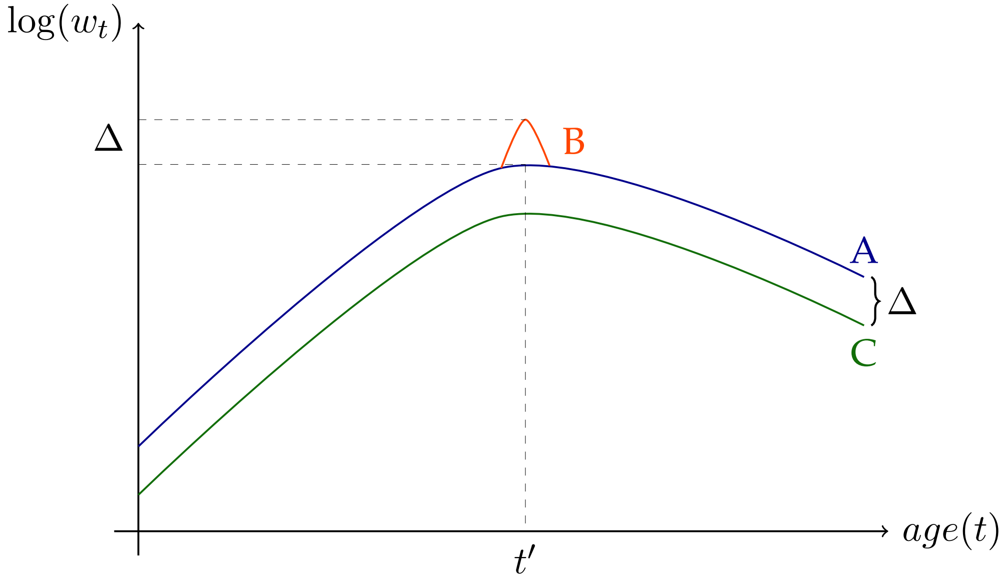


Figure A.1: Evolutionary and transitory wage changes over the life cycle

yields

$$F_i = Z_i\theta + \pi_{0i}\bar{\gamma_0} + \pi_{1i}\bar{\gamma_1} + \pi_{2i}\bar{\gamma_2} + A_{i0}\theta + \xi_i \tag{20}$$

$$\bar{\gamma_j} = \sum_{t=1}^{T} \gamma_t t^j, \quad j = 0, 1, 2.$$

D.2 Labor Supply Responses to Evolutionary and Transitory Wage Changes

I now consider the labor supply responses to wage changes. In such an analysis, it is important to distinguish between wage changes that are anticipated (known as *evolutionary* wage changes) and those that are unanticipated (so-called *parametric* wage changes). As we will see, this is a useful distinction given that anticipated changes only generate substitution effects while the latter generate both substitution and income or wealth effects. This analysis is therefore helpful in understanding which parameters can be estimated using natural experiments such as tax reforms to generate a variation in after-tax wages.

Figure A.1 plots wage paths over the life cycle, according to the process in (19). Consider an individual whose wage path can be described by path A. As he becomes older, individual A's wages increase, to which the individual responds by adjusting hours. Such evolutionary wage changes are known to the individual as the wage path, and therefore generate a substitution effect and no income effect. The parameter governing these responses is ε, which is the intertemporal (λ-constant) *Frisch* elasticity of substitution. While this is an elasticity that determines responses to an evolutionary change in wages, it can also be interpreted as determining responses to a particular type of parametric change, i.e. one associated with a wage increase at time t' but holding the marginal utility of wealth constant.

As such perfectly anticipated evolutionary wage changes are difficult to identify and observe, let us consider two scenarios an econometrician might encounter. First, let us compare two individuals,

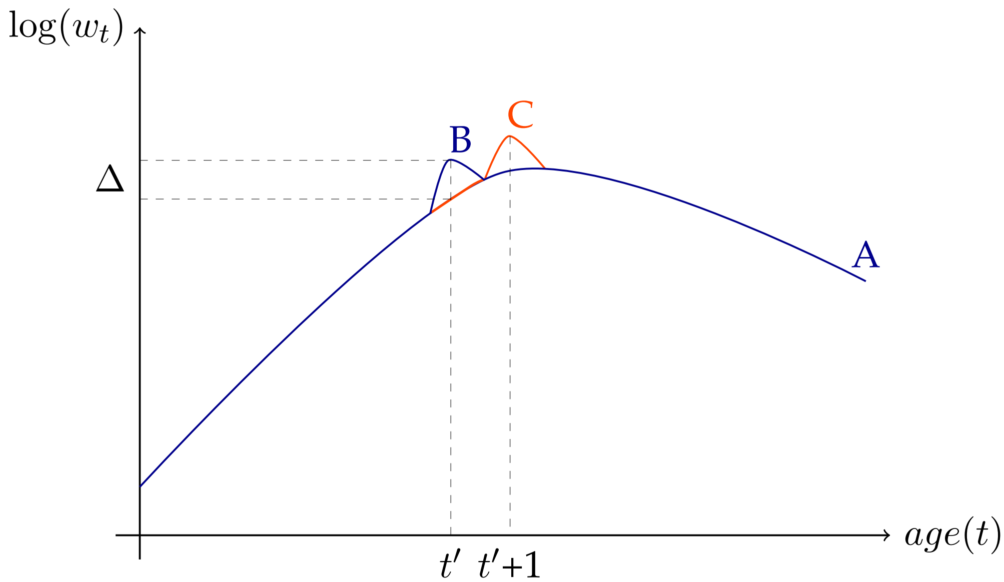


Figure A.2: Tax-free year at different periods over the life cycle

for whom the evolution of wages can be described by paths A and B in Figure A.1, where they are equal at all periods t except at t' when they differ by Δ (e.g. due to a tax-free year). This is a parametric change in wages, as this is a shift in the (known) life-cycle path A. This has two effects on the individual's labor supply. First, it generates an intertemporal substitution effect: labor supply in period t' will exceed that in all other periods $t \neq t'$ by $\Delta\varepsilon$. Second, there is an income effect: the individual will set a value of F_B that is lower than that of F_A by $\gamma_{t'}\Delta$. As a result, the labor supply of an individual facing path B compared with path A will be lower in all periods $t \neq t'$ by some constant. In total, the effect on labor supply at time t' is $(\varepsilon + \gamma_{t'})\Delta$. Given the income effect and substitution effect are of opposite sign, the labor supply response to a one-period wage increase is smaller than that predicted by the Frisch elasticity ε.

As a second comparison, let us compare individuals with paths A and C in Figure A.1. Moving from path C to A is equivalent to increasing the intercept π_0 of path A by, say, Δ. As before, there are two effects, a substitution effect of $\Delta\varepsilon$ for every period, and a wealth effect of $\sum_{t=1}^{T} \gamma_t \Delta = \bar{\gamma}\Delta$.

Any temporary variation in wages that is not perfectly predictable does not allow us to identify the Frisch elasticity; such changes always generate an income effect. Therefore, the observed labor supply elasticity estimated from a transitory wage change is $(\varepsilon + \gamma_{t'}) \leqslant \varepsilon$, where equality only holds when utility is linear in consumption, implying no income effect. However, comparing the two "experiments" considered above, the temporary one-period increase in wages (e.g. the tax-free year) only generates a very small income effect compared with that generated by a permanent shift in the wage profile (e.g. a permanent change in taxes). Transitory wage increases therefore allow us to measure elasticities close to the Frisch substitution elasticity.

D.3 Labor Supply Responses to a Tax-Free Year

The intuition from the MaCurdy (1981) model can be used to motivate the empirical strategy I develop in Section 5 to estimate labor supply responses to the tax-free year. Figure A.2 presents a stylized graphical example to help describing the intuition behind the empirical approach.

The comparison between the life-cycle wage profiles of two individuals, A and B, in Figure A.2 is identical to that in Figure A.1. A comparison of the labor supply of A and B before and during the wage increase faced by B allows for estimating the Frisch elasticity ε, net of an income effect. To be precise, as during the tax-free year income remains unchanged at the same labor supply as the year before, the reform does not generate an income effect in the same way as a one-period wage increase. Therefore, this reform allows for estimating an elasticity closer to the Frisch elasticity ε.

In my empirical setting, there exists no comparison such as that between A and B. However, as individuals experience the tax-free year at different points over their lifetime, my setting allows for an alternative comparison, enabling me to estimate the labor supply elasticities. To illustrate the comparison, Figure A.2 plots a wage profile for individual C, who is identical to B except that the individual experiences the wage increase when one year older, at age t'+1. As documented by the figure, at age t', individual C is the counterfactual for B, as they follow the same life-cycle paths. Therefore, the Frisch elasticity ε can be estimated by relating the wage increase Δ to the difference in labor supply of B and C at age t', when C has not yet received the wage increase.

E Calculation Based on Bianchi, Gudmundsson, and Zoega (2001)

Bianchi, Gudmundsson, and Zoega (2001) study labor supply during the Icelandic tax-free year among a random sample of 9,300 individuals by comparing labor income and weeks worked in the tax-free year to the year before and the year after. Their estimates carry much weight in the literature as estimates of the intensive-margin Frisch elasticity are few. However, their estimates are based on the *average* tax rates in 1986 while the relevant measure for measuring the intensive margin elasticity is the *marginal* tax rate. Therefore, to facilitate a comparison of their elasticity estimates and others in the literature, it is necessary to convert their estimates into an elasticity using marginal tax rates. I proceed by calculating an intensive margin elasticity using the estimates from Bianchi, Gudmundsson, and Zoega (2001) and my individual-level data on marginal tax rates.

I base my calculations on the estimates in Table 6 in Bianchi, Gudmundsson, and Zoega (2001). Since the numbers are for individuals working in 1986, it implies that the resulting elasticity can be interpreted as the intensive margin elasticity. The table reports the percentage change in labor income for men and women in 1987 relative to the average in 1986 and 1988. This therefore gives a reduced-form estimate, or the numerator for the elasticity calculation. To obtain the denominator, I calculate the average marginal tax rate across these two groups—employed men and women—in my data, where, as explained in Appendix C, marginal tax rates are calculated using microdata and a tax calculator. The intensive marginal elasticity can then be calculated as the percentage change in labor income for men and women divided by the corresponding group averages in the logarithm of the net of marginal tax rates. To obtain a population-level estimate, I construct a weighted average across men and women using the sample size numbers in Table 6 in Bianchi, Gudmundsson, and Zoega (2001). The resulting elasticity estimate is 0.77. The standard error for this estimate is computed from the standard errors reported in Bianchi, Gudmundsson, and Zoega (2001) using the Delta method and is 0.12.

The procedure described above is essentially the same as the procedure used in Chetty et al. (2013). They calculate an elasticity of 0.37. The large difference lies in the fact that since they did not have access to individual-level data on tax rates, their calculations are based on the average marginal tax rate across the progressive tax bracket schedule, assuming an equal share of taxpayers in each bracket. Since there is much more mass at lower tax rates, this assumption yields a much larger denominator than what I calculate using averages of individual-level marginal tax rates. Therefore, the elasticity that they calculate is substantially smaller than what I calculate.

F Measures of Labor Market Flexibility Across Countries

Guided by a general definition, we can divide labor market flexibility into micro-level flexibility and institutional- or macro-level, flexibility. The former refers to worker flows between labor market states, job flows and working time flexibility, while the latter refers to labor regulations and wage flexibility.

Following this categorization, I collected several measures of labor market flexibility for a set of OECD countries, including Iceland, Switzerland and the US. Figure A.16 presents four subfigures that display the general pattern in this international comparison. As shown, Iceland has a flexible labor market, much more so than Switzerland and other countries in continental Europe, and one that is closer to the US labor market. In addition, Figure A.16 demonstrates that these measures, while different, are correlated.

First, Figure A.16a depicts monthly flow probabilities into and out of unemployment. According to this "fluidity" measure of labor market flexibility, the US stands out as having the most fluid labor market, followed by Iceland. In fact, as shown, worker flows in Iceland are two to three times larger than in Switzerland. Hobijn and Sahin (2009) document similar differences for job flows. In addition, the monthly job-finding rate in Iceland is 30.5% compared with 56.3% in the US and 13.4% in Switzerland.

Second, Figure A.16b presents statistics on the cyclicality of hours per worker and their relative contribution to the cyclical variation in total hours. If workers have the flexibility to adjust their hours and the intensive margin is operative, we would expect hours per worker to move with the business cycle and to explain a significant share of changes in total hours. As Figure A.16b reveals, this is true in Iceland and in the US, but to a much lesser extent in Switzerland. In Iceland and the US, the cyclical components of hours per worker are highly correlated with the cyclical component of total hours, with correlations of 0.86 and 0.84, respectively. Similarly, in Iceland, the ratio of the standard deviation in hours to the standard deviation in employment is 0.83. This implies that hours per worker explain about 45% of the cyclical variation in total hours, which is more than twice as much as in Switzerland. Indeed, Rogerson and Shimer (2011) note that "An extreme example is

Switzerland, where [...] most of the cyclical movement in total hours is accounted for by movements between non-participation and employment at a fixed number of hours per worker."

Third, Figure A.16c details wage flexibility. The figure plots the coefficient on the unemployment rate gap from a regression of the growth of real labor compensation on a constant, the unemployment rate gap (the difference between unemployment and NAIRU), a long moving average of labor productivity growth, and lagged real labor compensation growth. According to this measure, among the OECD countries, real wage flexibility is highest in Iceland.

Fourth, Figure A.16d plots two different measures of institutional flexibility. On the y-axis, it plots the replacement rate of unemployment benefits of workers' previous earnings in the first year of unemployment. On the x-axis, it plots the average of the indices in the *OECD Indicators of Employment Protection*, where a higher index implies stricter employment protection. The replacement rate in Iceland is around the country average, while employment protection in Iceland is less than in most other European countries. Unsurprisingly, the US stands out on both dimensions as having a more flexible institutional framework.

G Supplementary Figures

Figure A.3: Advertisement: "Road to Tax Freedom"

Figure A.4: Explanation of the withholding tax system and 1987 being a tax-free year

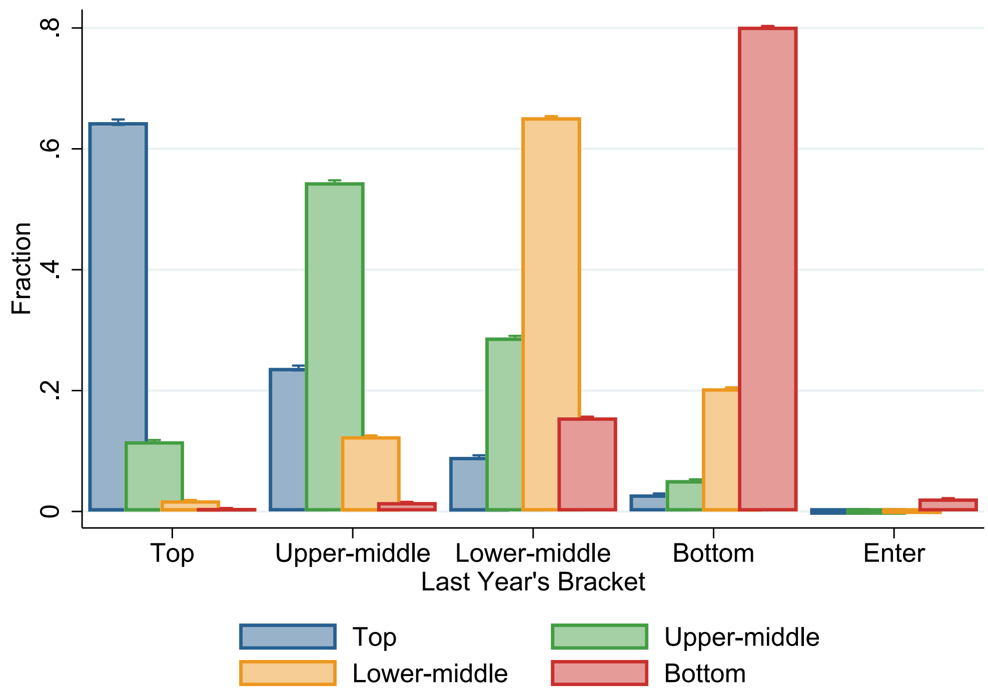


Figure A.5: Transitions between tax brackets, 1982–1986

Notes: The figure plots the average transition rate between tax bracket during the pre-reform period, 1982-1986. That is, every year I compute the rate of transition from a given tax bracket to all other brackets and the rate of stays within the same bracket. I then compute averages of the resulting transition matrix and plot in the figure.

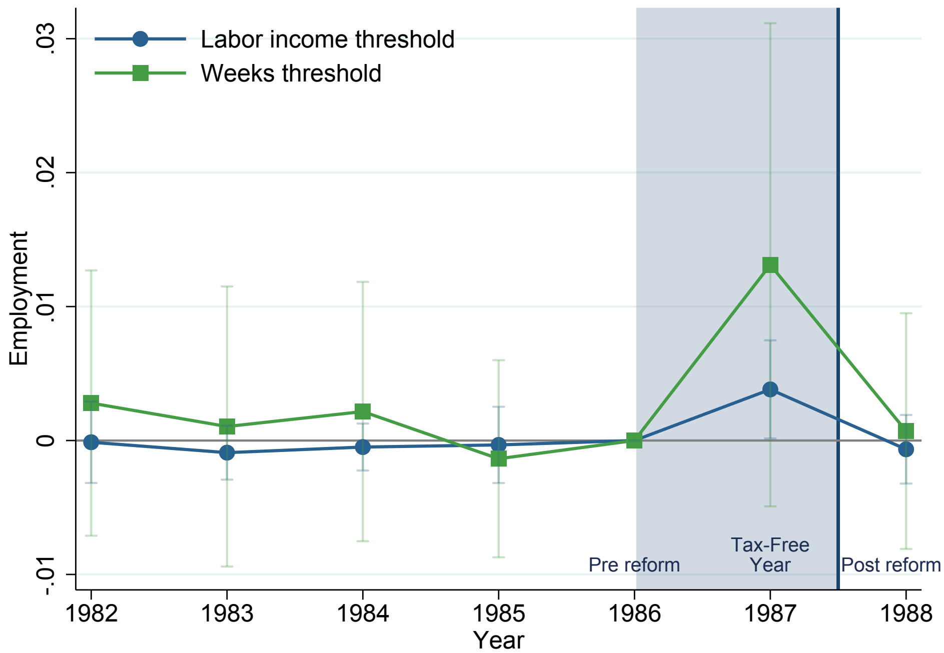
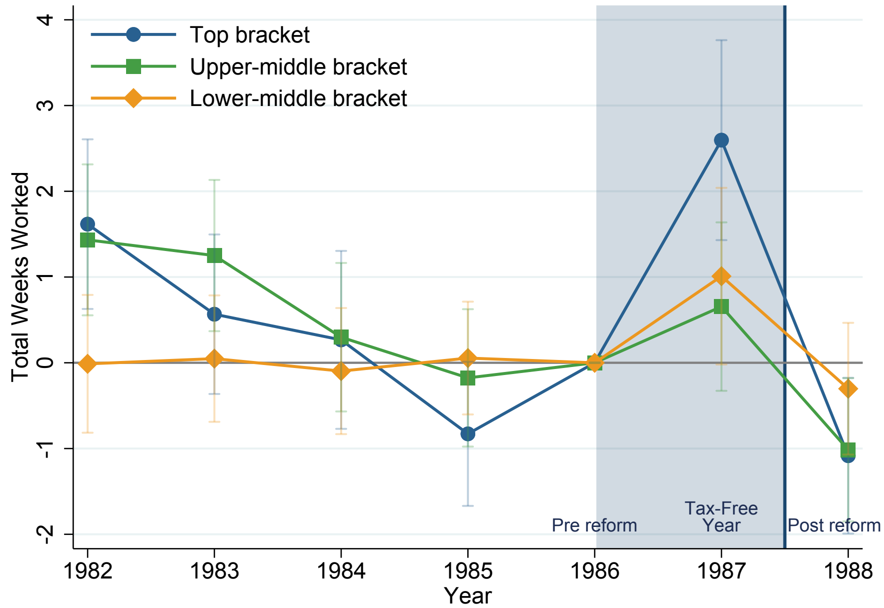
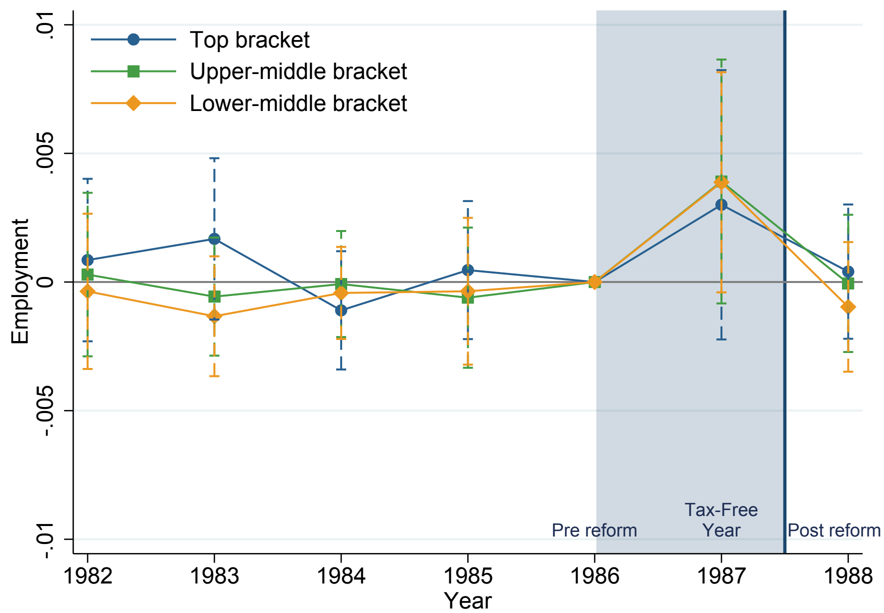


Figure A.6: Employment

Notes: The figure documents the effect of the tax-free year on employment. Employment is defined either as earning income above zero or working one month or more in a given year. Details on the regression specification are in the note to Figure 4. The treatment group consists of workers in the three top tax brackets and the control group of those in the bottom bracket. The shaded area marks the period used to estimate the response to the tax-reform—the 1987 tax-free year compared to the pre-reform year 1986—and the labels highlight the pre and post-reform periods. The regressions control for gender, marital status, age, education, number of children, indicator for living in the capital area, and occupation in the previous year. Standard errors are clustered at the tax-bracket by municipality level and the vertical bars plot the 95% confidence intervals.



(a) Weeks Worked



(b) Employment

Figure A.7: Weeks Worked and Employment: Tax-Bracket Difference-in-Differences

Notes: The figure plots estimates from a dynamic DID version of equation (1), as explained in the note to Figure 4. The outcome in panel (a) is weeks worked and in panel (b) employment is defined as earning income above zero. Estimates are plotted separately for each of the top three tax brackets where the bottom bracket is the control group. The shaded area marks the period used to estimate the response to the tax-reform—the 1987 tax-free year compared to the pre-reform year 1986—and the labels highlight the pre and post-reform periods. The regressions control for gender, marital status, age, education, number of children, indicator for living in the capital area, and occupation in the previous year. Standard errors are clustered at the tax-bracket by municipality level for each year and the vertical bars plot the 95% confidence intervals.

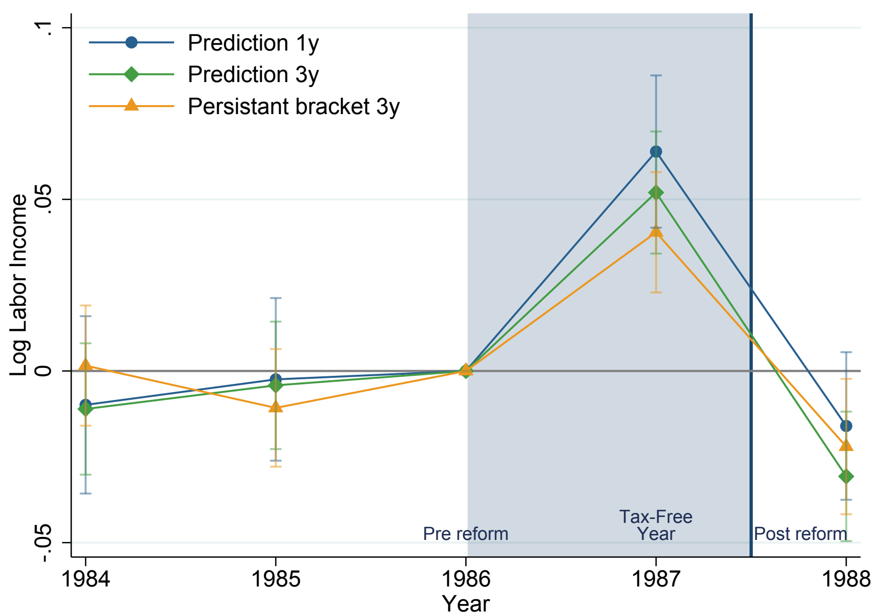
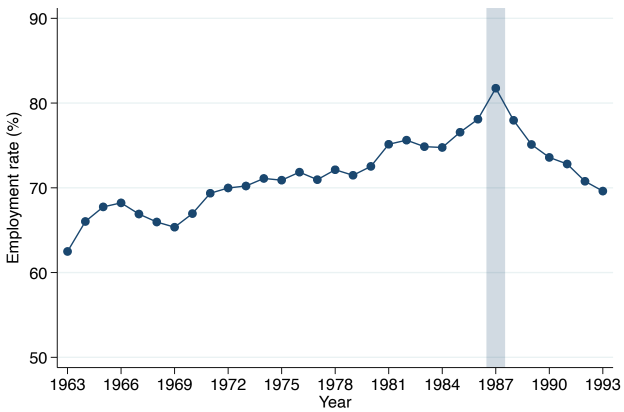


Figure A.8: Sensitivity to Assignment of Treatment Status

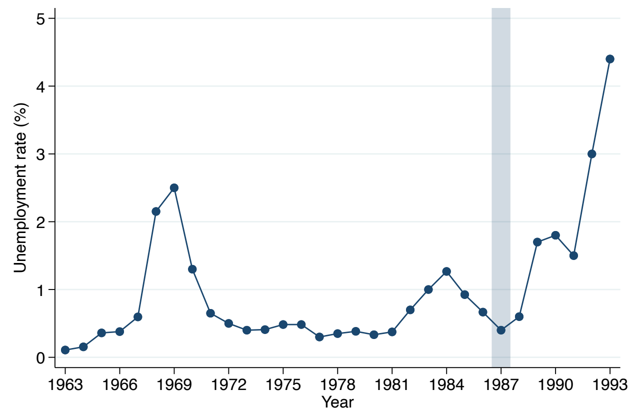
Notes: The figures present estimates from a dynamic DD version of equation (1), estimated in the following regression

$$y_{it} = bracket_{i,t-1} + \delta_t + \eta_t \cdot B_{i,t-1} \times \delta_t + \boldsymbol{X}'_{it}\gamma + \mu_{it},$$

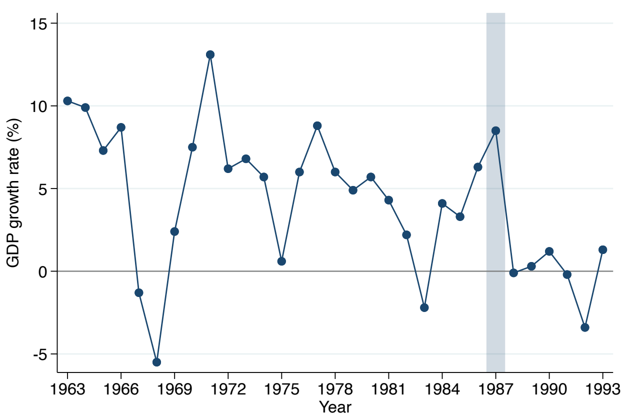
where the outcome variable is log labor income. These plot the coefficients η_t, where $B_{i,t-1} \times \delta_{t=1986}$ is normalized to zero, and the tax bracket position is predicted using three lags of tax-bracket position along with other characteristics, as described in the text. Standard errors are clustered at the individual level and the vertical bars plot the 95% confidence intervals.



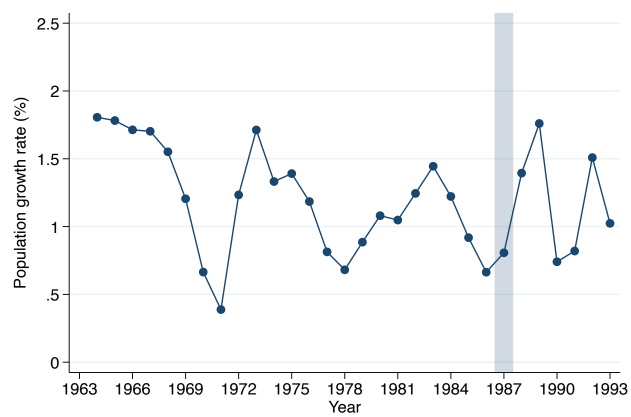
(a) Employment



(b) Unemployment



(c) GDP growth



(d) Population growth

Figure A.9: Employment, unemployment, GDP growth, and population growth

Notes: The figure shows the evolution of macroeconomic aggregates for the period 1963-1993. The vertical bar marks the tax-free year of 1987. Panel (a) plots the employment rate, measured by Statistics Iceland as the ratio of total man-years (full-time equivalent workers) to the working age population. Panel (b) plots the unemployment rate, measured as registered unemployment at the Directorate of Labor. Panel (c) plots the yearly growth rate in real GDP, measured by Statistics Iceland. Panel (d) plots the yearly population growth rate.

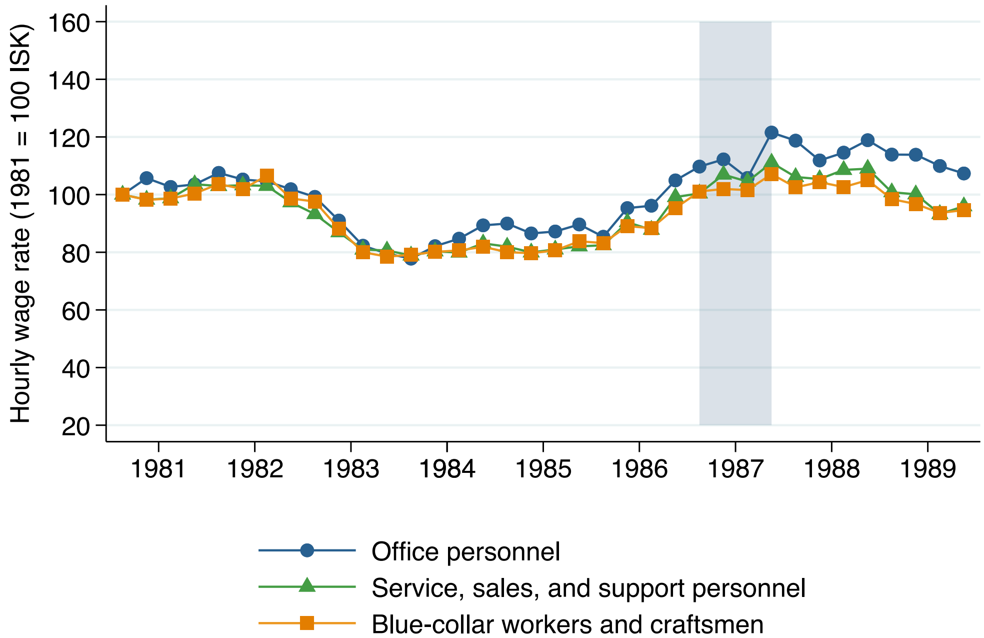


Figure A.10: Hourly wage rates by occupation

Notes: The figure plots the average hourly wage rate, normalized to 100 Icelandic krona (ISK) in the first quarter of 1981, in three broad occupation groups corresponding to office, service and sales, and support personnel. The shaded area corresponds to the period from the first to fourth quarters of 1987. Data on wages are drawn from a survey on paid hourly wage rate collected by the Wage Research Committee (*Kjararannsóknanefnd*) on wages in the private sector.

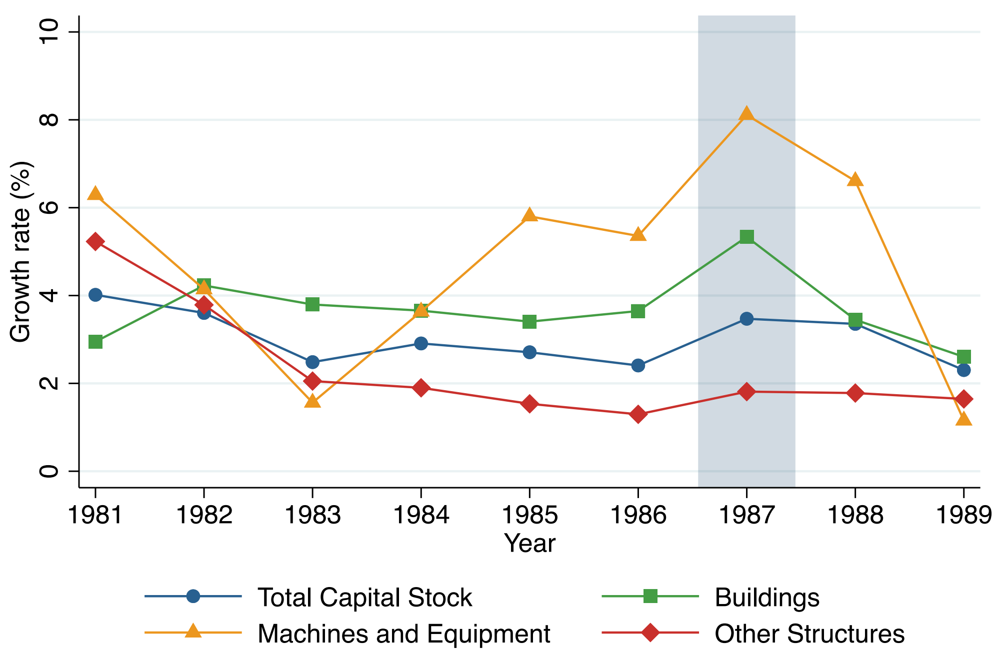
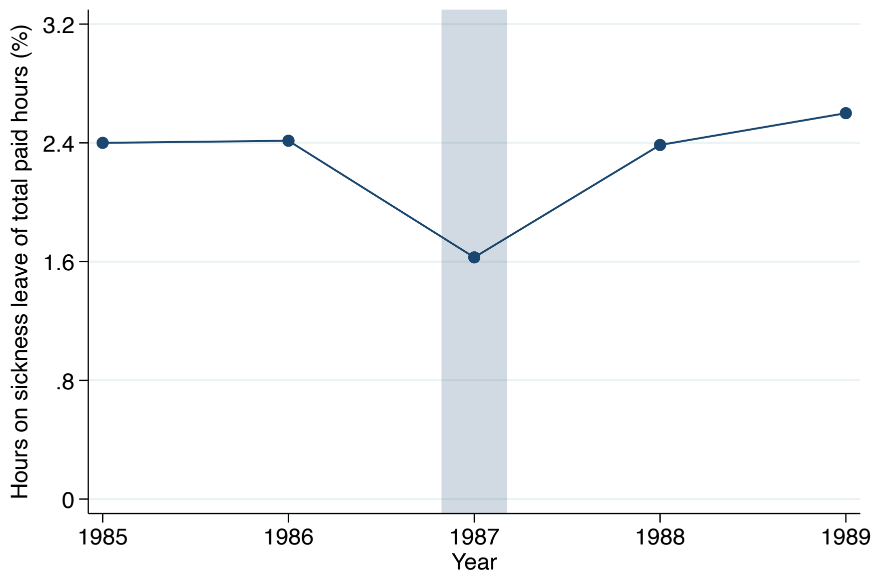
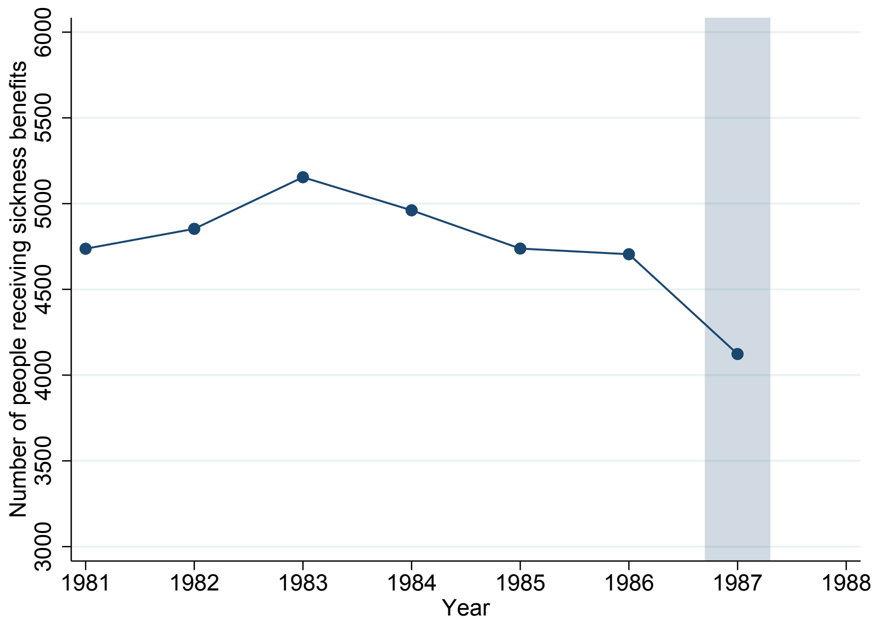


Figure A.11: Growth rate of the capital stock

Notes: The figure plots the yearly growth rate in the capital stock and capital stock subcategories. Data are from Statistics Iceland.



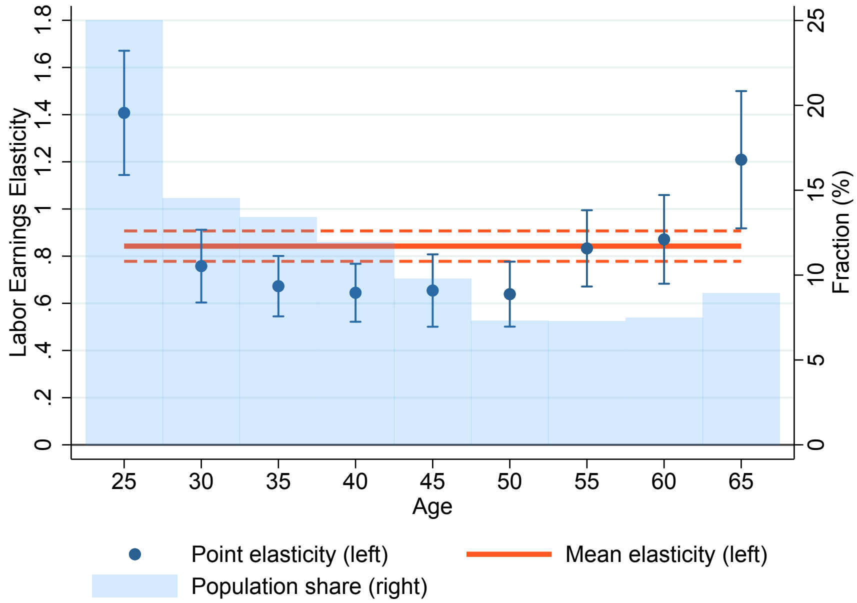
(a) Sick leave, in hours of work



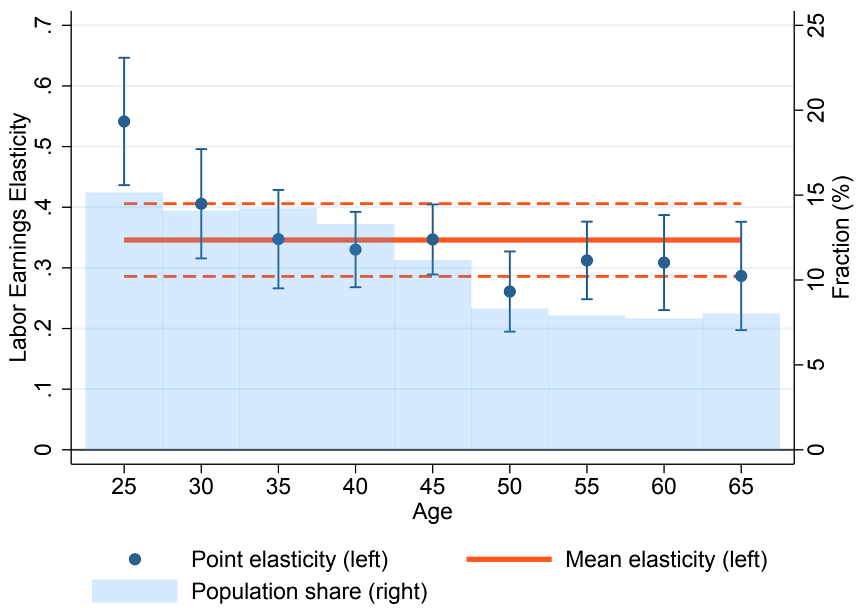
(b) Recipients of sickness benefits

Figure A.12: Sick leave from work and recipients of sickness benefits

Notes: Panel (a) plots the number of hours of sickness leave as a share of total paid hours (in %), based on survey data collected by the Wage Research Committee (*Kjararannsóknanefnd*). The numbers are sample averages. Panel (b) plots the number of people (tax filers) receiving sickness benefits in the given year. These benefits were reported in tax returns until 1987 and were deductible from taxes. From 1988 onwards, under the withholding tax system, these were no longer reported.



(a) Life-Cycle Differences Estimates



(b) Triple-Differences Estmates

Figure A.13: Elasticity of Labor Earnings by Age

Notes: The figure plots the estimated labor income elasticity by age. The elasticities in panel (a) are estimates using the life-cycle differences, equation (4). The elasticities in panel (b) are estimated using a triple differences design, equation (5). I group cohorts by age in 1987 and present estimates by age group, such that "25" refers to those at age 18-25; "30" to age 26-30; "35" to age 31-35; "40" to age 36-40; "45" to age 41-45; "50" to age 46-50; "55" to age 51-55; "60" to age 56-60; "65" to age 61-67. Standard errors are clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, and vertical bars plot the 95% confidence intervals. The horizontal line plots the average elasticity for the population and the dashed line the corresponding 95% confidence intervals. The shaded area (bars) is the population distribution, where each bar corresponds to the share of the working-age population (in %).

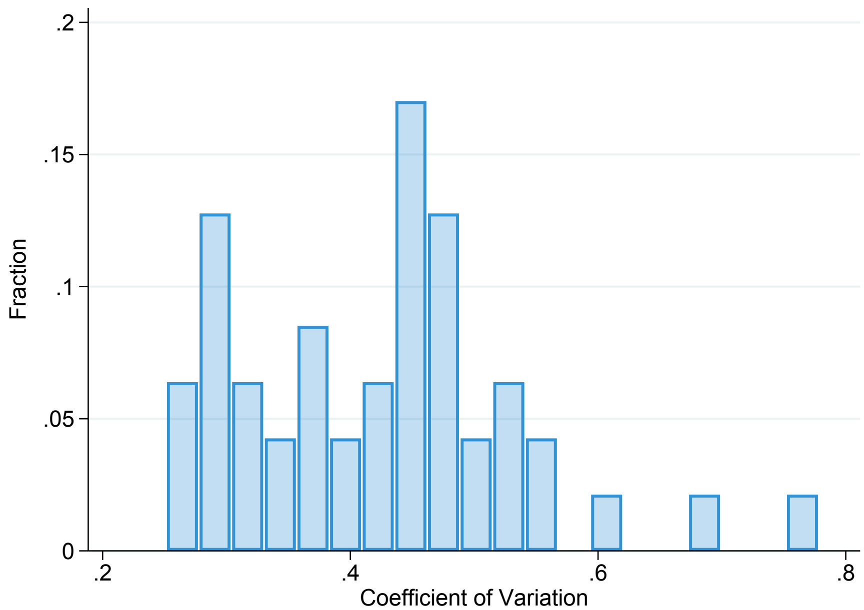


Figure A.14: Relative variability in weeks worked by occupation

Notes: The figure plots the histogram of the coefficient of variation of weeks worked by occupation, measured using equation (6).

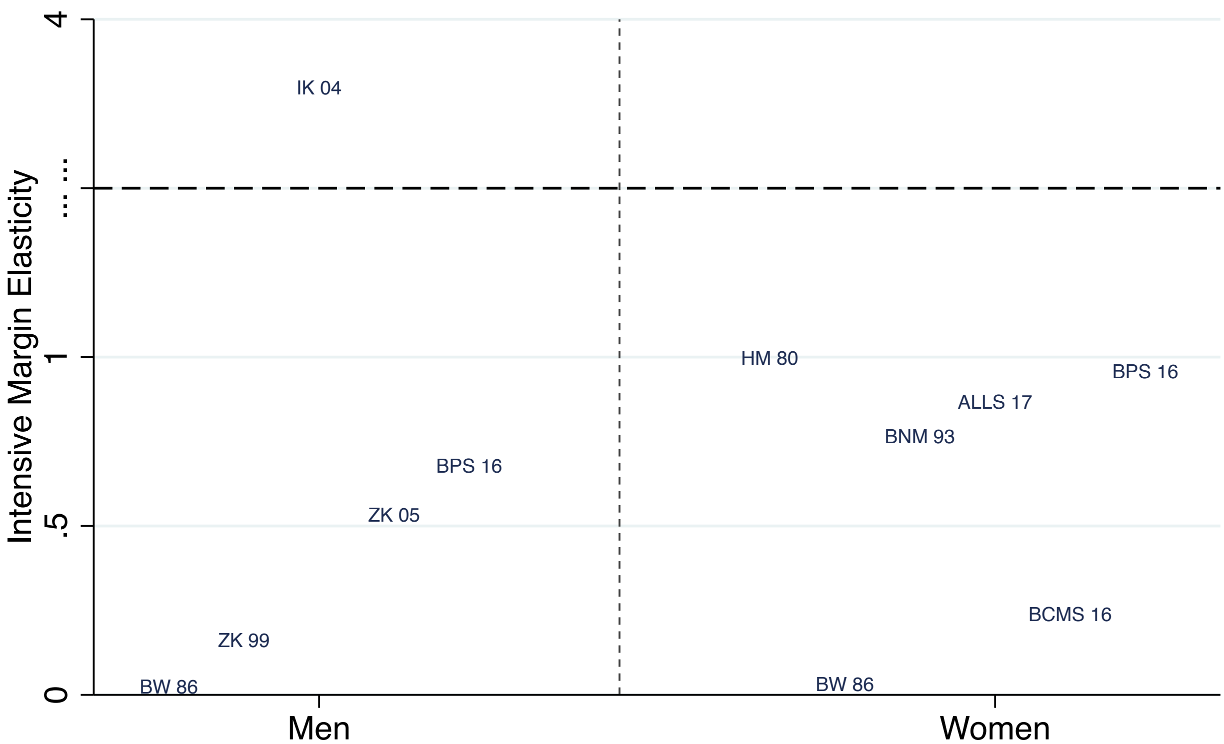
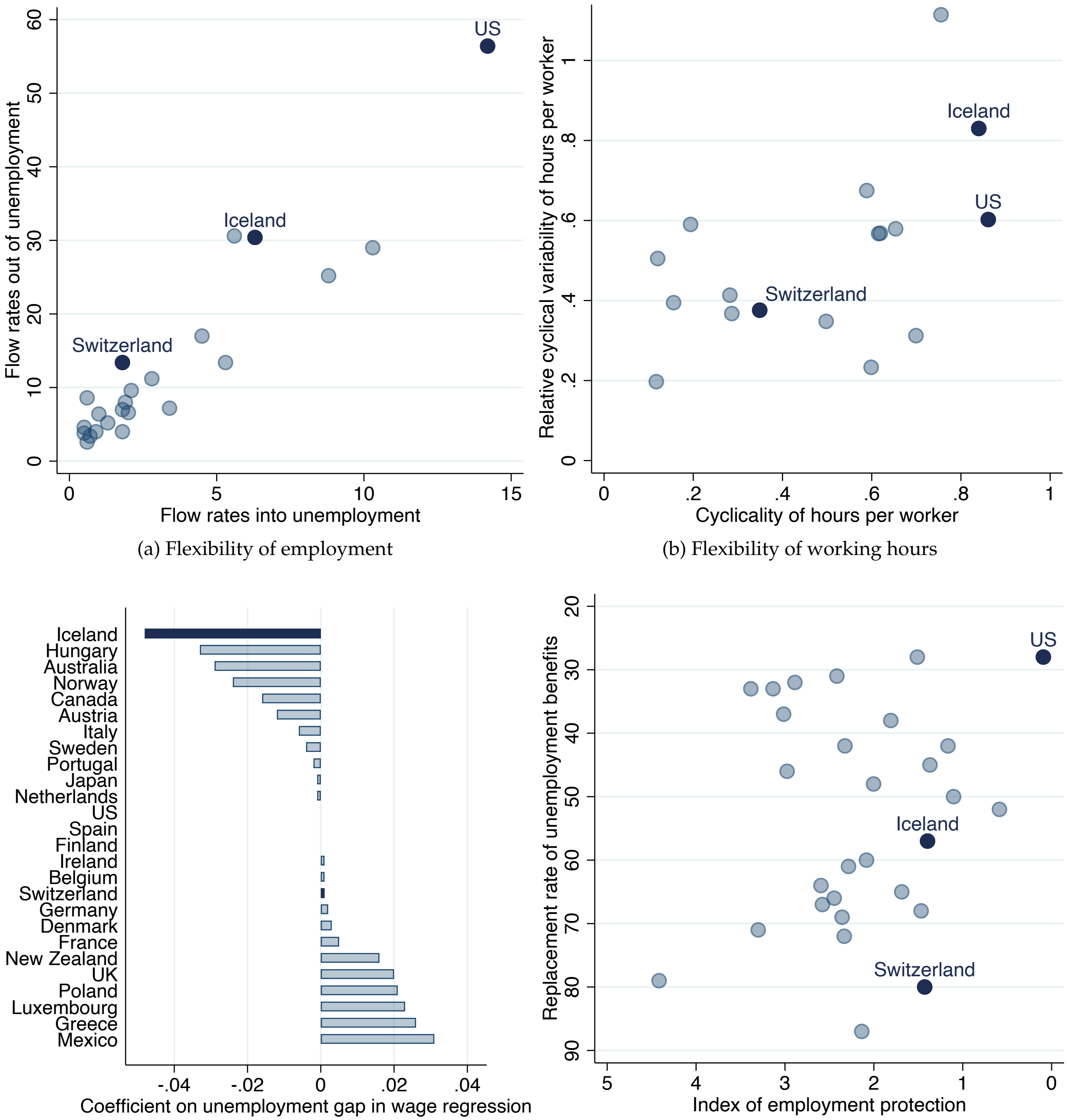


Figure A.15: Summary of structural estimates of intensive margin elasticities

Notes: The figure plots parameter estimates of the intensive margin Frisch elasticity. As most papers focus on either men or women, or report estimates separately, elasticities are reported by gender. The labels are as follows: "BW 86": Blundell and Walker (1986), "ZK 99": Ziliak and Kniesner (1999), "IK 04": Imai and Keane (2004), "ZK 05": Ziliak and Kniesner (2005), "BPS 16": Blundell et al. (2016b), "HM 80": Heckman and MaCurdy (1980), "BNM 93": Blundell et al. (1993), "ALLS 17": Attanasio et al. (2018), "BCMS 16": Blundell et al. (2016a).



(a) Flexibility of employment

(b) Flexibility of working hours

(c) Wage flexibility

(d) Flexibility of institutions

Figure A.16: Measures of labor market flexibility across OECD countries

Notes: Panel (a) plots on the x-axis the flow probabilities into unemployment (U) from employment (E) and nonemployment (N), and on the y-axis the flow probabilities out of unemployment for a selection of OECD countries. Measures of worker flows are from Hobijn and Sahin (2007, 2009) using harmonized OECD data. Panel (b) plots on the x-axis the relative standard deviation of hours per worker to the standard deviation of employment. On the y-axis, the figure plots the correlation between total hours and hours per workers. Total hours worked, th, are defined (in logarithmic terms) as $th = h + n$, where h is the average number of hours worked per worker, and n is the number of people employed (both divided by the size of the labor force). The time series are detrended using the Hodrick–Prescott (HP) filter so that th, h, and n reflect the cyclical components. Measures of cyclicality of hours for Iceland are from Sigurdsson (2011) and from Rogerson and Shimer (2011) for other countries using data from the OECD database. Panel (c) plots as a measure of wage flexibility the coefficient on the unemployment rate gap from a regression of the growth of real labor compensation on a constant, the unemployment rate gap (difference between unemployment and NAIRU), a long moving average of labor productivity growth, and lagged real labor compensation growth. See OECD (2011) for details. Panel (d) plots on the y-axis the replacement rate of unemployment benefits of workers' previous earnings in the first year of unemployment, as of 2007. The x-axis plots the average across indices in the OECD *Indicators of Employment Protection* in 2007, where a higher index implies stricter employment protection. Both axes in panel (d) are reversed so that moving out along the axis implies more flexibility.

H Supplementary Tables

Table A.1: Effect of Tax-Free Year on Labor Income and Weeks Worked: Employees vs. Self-Employed

	Log labor income				Weeks worked			
	Wage earners		Self-employed		Wage earners		Self-employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.403	0.404	0.576	0.446	5.423	4.865	15.022	15.306
	(0.067)	(0.107)	(0.122)	(0.120)	(2.174)	(2.596)	(5.020)	(6.052)
Reduced form (dy)	0.167	0.063	0.100	0.075	0.907	0.756	2.594	2.582
	(0.013)	(0.016)	(0.021)	(0.020)	(0.357)	(0.392)	(0.879)	(1.026)
First stage ($d\log(1-\tau)$)	0.205	0.191	0.213	0.204	0.205	0.191	0.213	0.204
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
Mean of outcome variable	—	—	—	—	47.38	47.38	61.76	61.76
Weighted	No	Yes	No	Yes	No	Yes	No	Yes
Observations	146,593	137,147	26,242	24,759	147,109	137,602	26,299	24,807

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. Columns (1)–(2) and (5)–(6) report estimates for wage earners and columns (3)–(4) and (7)–(8) report estimates for the sample of business owners and workers with income from self-employment. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable (y) is defined in the top panel and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. "Weighted" refers to the regressions being weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table A.2: Effect on Earnings and Employment-Related Income

Wages and salaries	93%
Fringe benefits, travel allowances, etc.	2.1%
Drivers' payments	2.6%
Gifts from employer	0.1%
Pension payment from employer	0.5%
Bonuses, sales commission, etc.	0.4%
Board remuneration	1.3%
Sum	100%

Notes: The table presents results from a 2SLS estimation of equation (2), where the dependent variable is that stated in each row, in 1981$. Estimates are presented as the share of total employment-related income. Each regression controls for gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years.

Table A.3: Effect of Tax-Free Year on Capital Income

	All (1)	Wage earners (2)	Self-employed (3)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	492	507	532
	(239)	(239)	(473)
Reduced form (dy)	64	61	53
	(24)	(23)	(25)
First stage $(d\log(1-\tau))$	0.207	0.208	0.193
	(0.001)	(0.001)	(0.001)
Mean of outcome variable	91.9	86.5	117.6
Share of treatment effect on labor earnings	0.032	0.032	0.035
Weighted	Yes	Yes	Yes
Observations	166,427	137,807	24,807

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is real taxable capital income in 1981$ and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is real taxable capital income in 1981$. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. "Weighted" refers to the regressions being weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. "Share of treatment effect on labor earnings" refers to the ratio of the top row to a similar estimate of real labor earnings in 1981$. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table A.4: Effects on Labor Income in 1987 – Alternative Standard Error Clustering

	(1)	(2)	(3)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.407	0.407	0.407	0.407
	(0.099)	(0.101)	(0.113)	(0.081)
Reduced form $(d\log y)$	0.064	0.064	0.064	0.064
	(0.015)	(0.015)	(0.017)	(0.012)
First stage $(d\log(1-\tau))$	0.157	0.157	0.157	0.157
	(0.013)	(0.013)	(0.015)	(0.006)
Clustering	Municipality × Bracket	Close municipalities × Bracket	Regions × Bracket	Age × Bracket
Weighted	Yes	Yes	Yes	Yes
Observations	165,044	165,044	165,044	165,044

Notes: The table presents results using alternative clustering of standard errors that in the main text. The estimates are results from difference-in-differences (DID) regressions, where each row and column entry corresponds to one regression estimate. Column (1) repeats Column (2) in Table 1. Column (2) clusters standard errors at the level of geographically close municipalities by tax bracket, where municipalities are aggregated to a 2-digit level. Column (3) clusters standard errors at the level of the nine geographic regions of Iceland by tax bracket. Column (4) clusters standard errors at the level of taxpayer's age by tax bracket. The sample period for each regression is 1986-1987. The top row presents results from a 2SLS estimation of equation (2), where the net-of-tax rate $(\log(1-\tau))$ is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DID estimation of equation (1). The bottom row presents results from a first-stage DID estimation of equation (1). Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. Clustered robust standard errors are in parentheses.

Table A.5: Robustness of Effects on Employment in 1987

	Income > 0		Income > Threshold		Weeks > 4		Weeks > 12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS DID ($\frac{dy}{d\log(1-\tau)}$)	0.023	0.036	0.028	0.059	0.071	0.080	0.084	0.094
	(0.013)	(0.016)	(0.022)	(0.027)	(0.046)	(0.059)	(0.047)	(0.056)
Reduced form (dy)	0.004	0.006	0.005	0.009	0.012	0.012	0.014	0.015
	(0.002)	(0.002)	(0.004)	(0.004)	(0.008)	(0.009)	(0.008)	(0.009)
First stage ($d\log(1-\tau)$)	0.168	0.157	0.168	0.157	0.168	0.157	0.168	0.157
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Mean of outcome variable	0.993	0.993	0.979	0.979	0.954	0.954	0.937	0.937
Weighted	No	Yes	No	Yes	No	Yes	No	Yes
Observations	182,870	170,806	182,870	170,806	182,870	170,806	182,870	170,806

Notes: The table presents estimated effects on employment under different definitions. The sample period for each regression is 1986-1987. The outcome variable, defined in the top panel of each column, is either a condition on income earned or weeks worked in a given year. The income threshold corresponds to 1.6×guaranteed income, which is a reference amount used in calculations of various kinds for governmental income support. This roughly corresponds to the lowest minimum wage earnings according to collective bargaining agreements. The top row presents results from a 2SLS estimation of equation (2), where the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DID estimation of equation (1). The bottom row presents results from a first-stage DID estimation of equation (1). Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. "Weighted" refers to the regressions being weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table A.6: Effects of the Tax-Free Year on Extensive Margin

	All (1)	Young (2)	Prime age (3)	Old (4)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.056	0.243	0.009	0.255
	(0.026)	(0.113)	(0.024)	(0.127)
Reduced form (dy)	0.005	0.013	0.001	0.018
	(0.003)	(0.006)	(0.003)	(0.009)
First stage ($d\log(1-\tau)$)	0.098	0.053	0.117	0.071
	(0.003)	(0.005)	(0.003)	(0.006)
Mean dependent variable	0.618	0.413	0.694	0.580
Elasticity	0.090	0.589	0.0130	0.439
Observations	551,383	131,611	373,221	46,551

Notes: The table presents results from life-cycle difference regressions, where each row and column entry corresponds to one regression estimate. "*Young*" are individuals younger than 25 years old in 1987, "*Prime age*" are individuals between 25 and 59 years old, and "*Old*" are individuals 60 years and older. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is employment and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents reduced form estimates based on equation (3). The bottom row presents first-stage regression estimates based on equation (3), where the outcome variable is the logarithm of one minus the average tax rate in columns. Regressions control for match-strata fixed effects, i.e. group fixed effects where each group is a cell used in coarsened exact cohort matching. Elasticity is calculated as the ratio of the semi-elasticitiy (top row) and the mean of the dependent variable. Robust standard errors clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, are in parentheses.

Table A.7: Effects of the Tax-Free Year on Extensive Margin — Robustness To Sector Shocks

	All (1)	Young (2)	Prime age (3)	Old (4)
	Population			
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.056	0.243	0.009	0.255
	(0.026)	(0.113)	(0.024)	(0.127)
Mean dependent variable	0.618	0.413	0.694	0.580
Elasticity	0.0902	0.589	0.0130	0.439
Observations	551,383	131,611	373,221	46,551
	No Fishing Sector			
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.058	0.265	0.011	0.245
	(0.028)	(0.127)	(0.025)	(0.127)
Mean dependent variable	0.603	0.388	0.682	0.576
Elasticity	0.0967	0.683	0.0164	0.425
Observations	523,888	123,889	354,110	45,889
	No Tradable Sector			
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.089	0.413	0.034	0.214
	(0.029)	(0.151)	(0.026)	(0.125)
Mean dependent variable	0.646	0.386	0.730	0.671
Elasticity	0.137	1.068	0.0472	0.319
Observations	431,876	100,534	297,224	34,118

Notes: The table presents results from life-cycle difference regressions, where each row and column entry corresponds to one regression estimate. "*No Fishing Sector*" excludes all firms and workers employed in the fishing sector, including both fishing and fish-processing. "*No Tradable Sector*" excludes all firms and workers employed in the tradable sector, defined as employment in exporting firms in fishing, agriculture, and manufacturing firms. "*Young*" are individuals younger than 25 years old in 1987, "*Prime age*" are individuals between 25 and 59 years old, and "*Old*" are individuals 60 years and older. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is employment and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents reduced form estimates based on equation (3). The bottom row presents first-stage regression estimates based on equation (3), where the outcome variable is the logarithm of one minus the average tax rate in columns. Regressions control for match-strata fixed effects, i.e. group fixed effects where each group is a cell used in coarsened exact cohort matching. Elasticity is calculated as the ratio of the semi-elasticitiy (top row) and the mean of the dependent variable. Robust standard errors clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, are in parentheses.

Table A.8: Robustness of Effects of the Tax-Free Year on Extensive Margin

	Income > Threshold	Income > 0	Weeks > 4	Weeks > 12
	(1)	(2)	(3)	(4)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.056	0.014	0.156	0.166
	(0.026)	(0.013)	(0.028)	(0.034)
Reduced form (dy)	0.005	0.007	0.007	0.007
	(0.003)	(0.003)	(0.003)	(0.003)
First stage ($d\log(1-\tau)$)	0.098	0.098	0.098	0.098
	(0.003)	(0.003)	(0.003)	(0.003)
Mean dependent variable	0.618	0.947	0.880	0.824
Elasticity	0.090	0.014	0.177	0.202
Observations	551,383	551,383	551,383	551,383

Notes: The table presents results from life-cycle difference regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is employment and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents reduced form estimates based on equation (3). The bottom row presents first-stage regression estimates based on equation (3), where the outcome variable is the logarithm of one minus the average tax rate in columns. Regressions control for match-strata fixed effects, i.e. group fixed effects where each group is a cell used in coarsened exact cohort matching. Elasticity is calculated as the ratio of the semi-elasticitiy (top row) and the mean of the dependent variable. Robust standard errors clustered at the demographic group level, i.e. by gender, age, education, and whether living in the capital region or the rest of Iceland, are in parentheses.

Table A.9: Heterogeneous Labor Supply Responses by Flexibility of Employment Arrangement

	Temporal flexibility		Constrained in in primary job		Hours flexibility	
	Low (1)	High (2)	Yes (3)	No (4)	Low (5)	High (6)
	A. Labor Income					
2SLS DD estimate	0.408	0.445	0.337	0.449	0.381	0.556
	(0.097)	(0.088)	(0.103)	(0.102)	(0.101)	(0.105)
Observations	169,313		165,044		165,044	
	B. Weeks Worked					
2SLS DD estimate	3.738	8.426	3.310	6.704	4.006	11.917
	(4.163)	(4.465)	(2.903)	(3.488)	(2.896)	(4.097)
Mean weeks pre-reform	53.62	46.71	49.38	49.37	53.68	46.03
Observations	166,018		163,084		163,084	

Notes: The table presents results from a 2SLS estimation of equation (2), where each row and column entry corresponds to one regression estimate. The dependent variable is indicated above each panel. Estimates by subgroups are obtained by interacting group indicators with the log of net-of-tax rate and the instrument in regression (2). *Temporal flexibility* splits the sample by a measure of relative variability in weeks worked within an occupation; see the main text for details. "Low" flexibility refers to workers below median of the distribution over the job flexibility measure and "High" refers those above median. *"Constrained in primary job"* is an indicator that equals one (*"Yes"*) if working 52 weeks in the primary job prior to the tax-free year, and zero (*"No"*) for those working 51 weeks or less. *Hours flexibility* splits the sample by occupations based on the share of workers with fixed-salary contracts, where "Low" share refers to occupation with a fixed-salary share below median of the distribution and "High" share refers to occupations above median. All regressions are weighted to have the same distribution of demographics in the treatment and control groups; see main text for details. Controls are gender, age, education, marital status, whether living in the capital area or not, the number of children aged 0–18 years, and pre-reform occupation. Robust standard errors clustered at the tax-bracket by municipality level are in parentheses.

Table A.10: Summary Statistics for the Icelandic Working-Age Population and Subsamples

	Population (1)	Working population (2)	Self-employed (3)
Demographics			
Age	37.67	36.97	42.80
Female (%)	46.33	47.31	15.18
Married (%)	57.45	57.51	70.70
Number of children	0.76	0.78	1.01
Capital area (%)	56.45	55.50	43.94
Junior college (%)	35.86	36.94	42.23
University degree (%)	9.71	9.79	13.34
Income and Working Time			
Wage earnings ($)	10,807	11,728	13,888
Capital income ($)	91	86	121
Other income ($)	477	357	341
Weeks worked (all jobs)	37.96	41.20	58.43
Tax Rates and Brackets			
Marginal tax rate (in %)	17.82	19.00	23.34
Average tax rate (in %)	10.21	10.89	13.84
Municipal tax rate (in %)	10.27	10.27	10.26
Number of individuals	162,804	150,013	18,220

Notes: Table entries are means for the group defined in the column header in 1986. Column 1 includes the population of all tax filers aged 16–70. Column 2 includes individuals with nonzero labor earnings. Column 3 includes the subpopulation working in self-employment, either as a primary or secondary job. The number of children is those aged 0–18 years. Capital area is the share living in Reykjavik and the surrounding area. Monetary values are in real 1981 US dollars. Capital income is taxable capital income.

Table A.11: Occupation Classification

Group	Occupation category	No. of subcategories
1	Legislators, senior officials and managers	17
2.	Professionals	5
3.	Technicians and associate professionals	8
4.	Clerks	7
5.	Service workers and shop and market sales workers	9
6.	Plant and machine operators and assemblers	1
7.	Skilled agriculture and fishery workers	7
8.	Craft and related trades workers	11
9.	Elementary occupations	9
0.	Armed Forces	0
		74

Notes: The occupation classification is based on the International Labor Organization's (ILO) International Standard Classification of Occupations (ISCO), version ISCO-88. For a detailed description of the classification, see ILO's website.

Table A.12: Sector Classification

Group	Sector category	No. of subcategories
1	Activities of extraterritorial organizations and bodies	2
2	Agriculture and forestry	10
3	Fishing	6
4	Manufacturing	64
5	Mining and quarrying	2
6	Construction	16
7	Other service activities	6
8	Electricity, gas, steam, and air conditioning supply	2
9	Water supply; sewage, waste management and remediation activities	2
10	Wholesale and retail trade; repairs of motor vehicles and motorcycles	19
11	Financial and insurance activities	5
12	Real estate activities	2
13	Rental and leasing activities	2
14	Transportation and storage	10
15	Public administration and defense; compulsory social security	6
16	Education	4
17	Human health and social work activities	11
18	Arts, entertainment and recreation	8
19	Professional, scientific and technical activities	9
20	Activities of households as employers	1
21	Accommodation and food service activities	2
		189

Notes: The sector classification is based on the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC). For a detailed description of the classification, see UN's website.

Table A.13: Education Classification According to Statistics Iceland's Education Register

Level	Description	Broad category	No. of subcategories
0	Less than primary education		1
1	Primary education	Compulsory education	1
2	Lower secondary education	Compulsory education	8
3	Upper secondary education	Junior college	8
4	Post-secondary non-tertiary education		5
5	Short-cycle tertiary education	University education	2
6	Bachelor's or equivalent level	University education	3
7	Master's or equivalent level	University education	2
8	Doctoral or equivalent level	University education	1
			31

Table A.14: Details and Sources for Figure 11

Study	Label	Group	Variation	Notes
Intensive margin – Figure 11c				
Martinez, Saez, and Siegenthaler (2021)	MMS 20	Population	Taxes	Table 2, column (2)
Looney and Singhal (2006)	LS 06	Population	Taxes	Table 5, column (3). SIPP and NBER tax panel.
Saez (2003)	Saez 03	Population	Taxes	Table 5, column (3). Elasticity of wage income.
Bianchi, Gudmundsson, and Zoega (2001)	BGZ 01	Population	Taxes	Based on Table 6 and own calculations. See footnote in Section 7.1 for details.
Martinez, Saez, and Siegenthaler (2021)	MMS 20	Prime-age men	Taxes	Appendix Table A2, column (2).
French (2004b)	French 04	Prime-age men	Wages	Table 3 (median of estimates). PSID, Men.
Pistaferri (2003)	Pistaferri 03	Prime-age men	Wages	Table 2. Men aged 26–59.
Ham and Reilly (2002)	HR 02	Prime-age men	Wages	Table 1, column (4). PSID, men of age 23–60.
Lee (2001)	Lee 01	Prime-age men	Wages	Table 2. PSID, men aged 25–60.
Angrist (1991)	Angrist 01	Prime-age men	Wages	Table 2. PSID, men of age 21–64.
Altug and Miller (1990)	AM 90	Prime-age men	Wages	See Keane (2011) for calculation of elasticity. PSID, Household-heads of age 25–46.
Altonji (1986)	Altonji 86a	Prime-age men	Wages	Table 2, column (7). PSID, men aged 25–60.
Altonji (1986)	Altonji 86b	Prime-age men	Wages	Table 4, column (3). PSID, men aged 25–60.
Browning, Deaton, and Irish (1985)	BDI 85	Prime-age men	Wages	See Keane (2011) for calculation of elasticity.
MaCurdy (1981)	MaCurdy 81	Prime-age men	Wages	Table 1, column (1). PSID, men of age 25–46.
Angrist, Caldwell, and Hall (2020)	ACH 17	Uber drivers	Wages	Table 5, column (1).
Giné et al. (2017)	GMV 17	Boat owners	Wages	Table 6, column (3).
Saia (2017)	Saia 17	Pizza deliverers	Wages	Table A1.
Goldberg (2016)	Goldberg 16	Agricultural workers	Wages	Table 4, column (1). Standard errors calculated as elasticity is calculated by author.
Farber (2015)	Farber 15	Taxi drivers	Wages	Table 6.
Stafford (2015)	Stafford 15	Lobster hunters	Wages	Table 2.
Fehr and Goette (2007)	FG 07	Bicycle messengers	Wages	Table 3 and text. Average of two estimates.
Oettinger (1999)	Oettinger 99	Baseball stadium vendors	Wages	Table 6, column (5).
Extensive margin – Figure 11d				
Martinez, Saez, and Siegenthaler (2021)	MMS 18	Population	Taxes	Table 2, column (1).
Carrington (1996)	Carrington 96	Population	Wages	Calculated based on estimates in Table 2. See Chetty et al. (2013) for details.
Manoli and Weber (2016)	MW 16	Retirement-age	Pension	Table 3. Full sample, 6 months from threshold.
Brown (2013)	Brown 13	Retirement-age	Pension	Table 4, column (4).
Gruber and Wise (1999)	GW 99	Retirement-age	Taxes	Calculated using data reported in Table 1. See Chetty et al. (2013) for details.

Notes: Estimates refer to the authors' main, representative, or preferred specification. Confidence intervals either based on reported standard errors or computed using the delta method estimates in MaCurdy (1983) of 6.25, as reported in Keane (2011), and negative elasticities in Camerer, Babcock, Loewenstein, and Thaler (1997), are excluded for visual purposes.