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

Jósef Sigurdsson[†]

November 20, 2024

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.

*I am indebted to Emi Nakamura, Arash Nekoei, Torsten Persson and Jón Steinsson for their invaluable advice, guidance and encouragement. For their helpful comments, suggestions and discussions, I also thank Ingvild Almås, Josh Angrist, Richard Blundell, Konrad Burchardi, Saman Darougheh, Mitch Downey, Peter Fredriksson, Karl Harmenberg, Xavier Jaravel, Per Krusell, Hannes Malmberg, Peter Nilsson, Matt Notowidigdo, Robert Östling, Jon de Quidt, Emmanuel Saez, Benjamin Schoefer, Robert Shimer, Fabian Sinn, David Strömberg, Dario Tortarolo, Danny Yagan, participants in the joint labor and public economics session at the 2019 NBER Summer Institute, and numerous other seminar and conference participants. I thank the staff at Statistics Iceland for their collaboration and hospitality over multiple visits between fall 2016 and spring 2018 during the process of data construction. In particular, I thank Hrafnhildur Arnkelsdóttir and Margrét K. Indriðadóttir for their assistance in making this project possible. Special thanks go to Ómar Harðarsson, Stefán Jansen, Hildur Sigurðardóttir, Eyjólfur Sigurðsson and other staff members at Statistics Iceland for their valuable discussions. I thank the Carl Mannerfelt Foundation for financial support.

[†]Stockholm University, CEPR, CESifo, and IZA. Address: Stockholm University, Department of Economics, 106 91 Stockholm, Sweden. Email: josef.sigurdsson@su.se.

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 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 and there is limited consensus on the size of the Frisch elasticity. Disagreement on the size is likely to reflect different views on the frictions that attenuate observed labor supply responses relative to unconstrained responses solely determined by preferences. While some evidence suggests that the Frisch elasticity is small or even close to zero (e.g. Martinez, Saez, and Siegenthaler, 2021), evidence 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)—implies 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 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 0.11. 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 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

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.

I demonstrate that adjustment frictions play 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, 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

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 VI provides a detailed summary and meta-analysis of previous work.

[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).

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

I The Tax-Free Year and Background

A 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 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

[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.

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

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 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

[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.*"(*Morgunblaðið*, December 7, 1986.).

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

in Section III, I perform a series of tests to evaluate this claim, finding the results to be robust.

B 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.

II 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]

A 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

[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).

[11]I also use data on educational attainment from Statistics Iceland's Education Registry, see Statistics Iceland (2008). Further details about the data, including summary statistics, are in Appendix C.

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 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.

B 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.[12] 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.[13] 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.

III The Intensive Margin

A 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%, 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

[12]For further information about the tax return data, see, e.g., Sigurðsson and Tómasson (2008).

[13]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 A). This method predicts actual tax liabilities with great precision.

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.[14] 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 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.

[14]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.

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.

B 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).[15] 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.[16] The tests indicate that trends in labor incomes were not statistically significantly different across brackets in the years before the tax-free year and the coefficients are small relative to those for the tax-free year, in most cases close to zero.[17] 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

[15] 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.

[16] 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.

[17] 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.

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 I, 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 I also plot in the figure, has 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 and weeks worked. Each column-by-row entry in the table corresponds to a regression estimate. First, the top row of column (1) provides an estimate 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.[18] 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 middle row, is a DID estimate of equation (1) on log labor income, which is estimated to be 0.073. Second, presented in the bottom 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 0.168. 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.[19]

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.[20] 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).[21]

It is important to highlight what these results imply and what to expect. As discussed in Section II, 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 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.[22] The increase in weeks worked therefore most likely reflects

[18] 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.

[19] In Appendix G I use standard theory to evaluate whether the magnitude of these estimates is reasonable. Using a dynamic labor supply model and parameter estimates from the literature, I show that the implied Hicksian elasticity is similar to that calculated in Chetty (2012) when pooling across existing studies.

[20] Appendix Figure A.7 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.

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

[22] About 40–45% of workers work overtime in the average month. The corresponding share is 60–65% when including

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.

I refer to the estimates in Table 1 as representing the intensive margin responses to the tax-free year. This designation arises because the research design is inherently unable to uncover labor market entry responses. However, the estimates might capture the differential effect of the tax-free year on labor market exit, such as delayed retirement. In Appendix Table A.5, I explore this possibility and report a small (0.024-0.031) and statistically imprecise effect on employment, defined as earning positive income.[23] Given the limitations of the tax-bracket difference-in-differences design in capturing the full extensive margin response, I employ a complementary research design in Section IV. This design not only allows for the identification of delayed exit but also, crucially, for entry into the labor market.

Real labor supply responses, not a reporting phenomenon. Can we interpret the estimated earnings elasticity as labor supply elasticity? 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 but may also be able to increase their income in the tax-free year by misreporting capital income as labor income, or by shifting income across years. 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 (0.10 vs. 0.25). This suggests that differences in earnings elasticity 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, I estimate equation (2) separately for each sub-component on the pay slip and report the results in Appendix Table A.2. The results do not exhibit an unexpected pattern. Increases in wages and salaries make up 93% of the increase in payments and potential suspects for income shifting, such as sales commissions, bonuses, and gifts, make up only 0.5%.[24]

irregular hours, such as nights and weekends (Sigurdsson and Sigurdardottir, 2016).

[23] Figure A.6 illustrates the DID estimates for the tax-free year alongside placebo estimates for years preceding and following it. Following a convention in the literature, employment is defined as an indicator variable for having positive labor income in a given year. In Appendix Table A.5, I also present estimates using alternative definitions based on minimum earning thresholds or weeks worked.

[24] These results are consistent with evidence from other Nordic countries, indicating limited tax avoidance in labor earn-

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, capital income was taxed in 1987 when labor income was tax free. Although this does not provide a pure placebo test, the effect on capital income allows for investigating potential misreporting, which would manifest itself as a negative effect on capital income. Appendix Table A.3 reports a positive but small effect on capital income, amounting to 3.2% of the treatment effect on labor income.[25]

Robustness. There is extensive literature estimating the elasticity of taxable income 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. First, the tax rate that individuals face following tax reform is a function of income which is the outcome of interest and, thus, endogenous. Second, with rising income inequality, it may be difficult to isolate the long-term effects of tax changes, 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 years following tax reforms, 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. 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.

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

ings because of third-party reporting by firms (Kleven et al., 2011).

[25]Other, more circumstantial, evidence supports the interpretation of real labor supply responses. 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. In line with that, Appendix Figure A.12 documents that workers in Iceland took less sick leave in 1987. To the extent that this evidence indicates that workers were working hard in 1987, it aligns with evidence of an increased likelihood of heart attack among middle-aged and old men, in particular among the self-employed, during 1987.

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.[26] 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 prediction).[27] 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.[28]

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 0.3 to 0.46 depending on specification. Also, 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 the permanent changes made to the tax system in 1988 when the progressive tax schedule was replaced with a flat tax rate. 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 such effects may be limited. 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 I, 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 new tax law was passed by Parliament in late March 1987, workers had been aware of the 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 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, Figure 2 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

[26]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.

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

[28]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.

IV 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.

A 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.[29] 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.[30]

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.[31] Within

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

[30] 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.

[31] 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.[32] 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 municipality, 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:

[32] 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.

B Results

Figure 8 reports the estimated employment elasticity by age and the population as a whole.[33] 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.[34] 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 0.51 and at 0.48 for those that were 61-67 years old.[35] 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.[36]

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,

[33] 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.

[34] 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.

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

[36] 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 III, 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.[37] 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.[38] 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 0.85. 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 $\log(1 - \tau_{ik})$

[37] 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.

[38] In addition, during a tax-free year, there may be an incentive for firms to front-load investment to increase their activity in a period of increased labor supply. Appendix Figure A.11 documents increased growth in the capital stock—primarily machines, equipment, and buildings (plants)—in 1987 compared with the years before and after.

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 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 III. 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.

V 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.

A 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.[39]

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.[40] I assign this measure of the flexibility of remuneration struc-

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

[40] 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

ture to the workers in the main data set based on their pre-reform occupation. Occupations with the 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.

B 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 III 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.[41]

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

workers by occupation paid by the hour or that has a fixed salary but paid for overtime.

[41] 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.

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 be written in terms of its subcomponents as

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

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 III).

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.[42]

VI Why Do Elasticity Estimates Differ So Much Across Settings?

Frisch elasticity estimates vary significantly across different settings.[43] While estimates for prime-age men, which constitute the majority of existing evidence, tend to be small, estimates for specific occupations such as bicycle messengers, taxi drivers, and fishermen are often relatively large. This raises the question: why do elasticity estimates differ so much across settings?

Significant differences also emerge in responses to tax holidays. In a recent study, Martinez, Saez, and Siegenthaler (2021) estimated the Frisch elasticity using a tax reform in Switzerland that introduced two-year tax holidays staggered across Swiss cantons. The authors found a small intensive margin elasticity of 0.025 and observed no extensive margin response. The contrast between the elasticity estimates from the tax holidays in Iceland and Switzerland—particularly the limited responsiveness of Swiss workers—is somewhat surprising at first glance. As I have demonstrated, the difference in extensive margin elasticities arises from the fact that employment responses in Iceland come exclusively from young first-time workers and those close to retirement, groups that are ex-

[42]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.

[43]In Appendix E, I provide a summary of previous estimates of the intensive-margin and extensive-margin Frisch elasticities.

cluded from the Martinez, Saez, and Siegenthaler (2021) analysis. However, the intensive margin elasticity estimates for Iceland are an order of magnitude larger than those for Switzerland, where they are close to zero. Given that the tax holidays in both Iceland and Switzerland created strong incentives for workers to temporarily increase their labor supply, what explains the divergent responses?

Several factors may account for these differences, including the salience of the reforms and the framing of their announcement. However, I argue that variations in labor market flexibility offer the most plausible explanation. The first piece of evidence supporting this argument comes from a recent study. In Sigurdsson (2023a), I investigate a tax holiday in Norway resulting from a tax reform similar to those in Iceland and Switzerland. Using survey data, I document that over 80 percent of Norwegian adults were aware of the tax holiday, aligning with the evidence presented in the current paper and Martinez, Saez, and Siegenthaler (2021) that tax holidays are salient events. In Norway, a quarter of the working-age population reported working more hours in response. Among those aware of the tax holiday but not responding to it, the majority cited friction in adjusting working time, in one form or another, as the primary reason.

The second, more concrete piece of evidence supporting this argument stems from examining the relationship between measures of labor market flexibility and the size of Frisch elasticity estimates. I define labor market flexibility in terms of the speed of adjustment to external shocks or changing macroeconomic conditions (Pissarides, 1997). This flexibility can be further categorized 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).

Following this categorization, Figure 11 presents a range of measures of labor market flexibility for selected OECD countries. Panel (a) displays monthly flow probabilities into and out of unemployment, representing the "fluidity" measure of labor market flexibility. The US stands out with the most fluid labor market, followed by Iceland. Notably, worker flows in Iceland are two to three times larger than in Switzerland. Panel (b) focuses on the cyclicality of hours per worker and their contribution to the cyclical variation in total hours. If workers can adjust their hours flexibly, we would expect hours per worker to move with the business cycle and account for a significant share of changes in total hours. In Iceland and the US, this is indeed the case, but to a much lesser extent in Switzerland. The correlations between the cyclical components of hours per worker and total hours are 0.86 and 0.84 in Iceland and the US, respectively. In Iceland, hours per worker explain about 45% of the cyclical variation in total hours, more than twice as much as in Switzerland.[44] Panel (c) presents two measures of institutional flexibility. The y-axis shows the replacement rate of unemployment benefits based on workers' previous earnings in the first year of unemployment. The x-axis displays the average of the indices in the *OECD Indicators of Employment Protection*, where a higher index indicates stricter employment protection. On both dimensions, the US stands out as having the most flexible institutional framework. Iceland's replacement rate aligns with the OECD average, while its employment protection is less stringent than in most other European countries. On

[44]In their chapter in the Handbook of Labor Economics, Rogerson and Shimer (2011) highlight the extreme case of Switzerland, where most of the cyclical movement in total hours is accounted for by shifts between non-participation and employment at a fixed number of hours per worker.

this institutional metric, Switzerland appears more flexible than micro-level measures suggest.

In summary, across various measures of labor market flexibility, the Icelandic labor market appears notably more flexible than the Swiss labor market and closely aligns with that of the US. Panel (d) of Figure 11 explores the correlation between labor market flexibility and intensive-margin Frisch elasticity, using the cyclicality of working hours as the measure of flexibility. Using this measure allows for comparisons both across countries and across occupations. I include estimates of intensive-margin Frisch elasticity for the countries with available Frisch elasticity estimates: Iceland, Switzerland, and the US.[45] The figure also includes estimates for specific occupations within Iceland, as presented in Figure 9. A consistent pattern emerges: the size of the Frisch elasticity is positively correlated with the flexibility of working hours, both across countries and within occupations. Iceland and the US represent one end of this spectrum, with similar Frisch elasticity estimates and flexibility levels, while Switzerland stands at the opposite end, exhibiting smaller elasticity and less flexibility.

VII 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 op-

[45] Tortarolo, Cruces, and Castillo (2020) analyze a 2.5-year income tax holiday in Argentina and find a minimal labor supply response. This finding is consistent with the broader cross-country analysis, suggesting that Argentina's labor market is relatively rigid compared to OECD countries. However, due to the reform generating both substitution and income effects, the elasticity estimate may be closer to a Marshallian than a Frisch elasticity.

portunities 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.

References

ALTONJI, J. G. AND C. H. PAXSON (1988): "Labor supply preferences, hours constraints, and hours-wage trade-offs," *Journal of labor economics*, 6, 254–276.

——— (1992): "Labor supply, hours constraints, and job mobility," *Journal of Human Resources*, 27, 256–279.

ANGRIST, J. D. (1991): "Grouped-data estimation and testing in simple labor-supply models," *Journal of Econometrics*, 47, 243–266.

ASHENFELTER, O. (1984): "Macroeconomic analyses and microeconomic analyses of labor supply," in *Carnegie-Rochester Conference Series on Public Policy*, Elsevier, vol. 21, 117–156.

BIANCHI, M., B. R. GUDMUNDSSON, AND G. ZOEGA (2001): "Iceland's Natural Experiment in Supply-Side Economics," *American Economic Review*, 91, 1564–1579.

BORUSYAK, K., X. JARAVEL, AND J. SPIESS (2023): "Revisiting Event Study Designs: Robust and Efficient Estimation," *Review of Economic Studies*, Forthcoming.

CENTRAL BANK OF ICELAND (2018): *The Economy of Iceland*, Central Bank of Iceland.

CHETTY, R. (2012): "Bounds on elasticities with optimization frictions: A synthesis of micro and macro evidence on labor supply," *Econometrica*, 80, 969–1018.

CHETTY, R., A. GUREN, D. MANOLI, AND A. WEBER (2013): "Does indivisible labor explain the difference between micro and macro elasticities? A meta-analysis of extensive margin elasticities," *NBER Macroeconomics Annual*, 27, 1–56.

COGAN, J. F. (1981): "Fixed Costs and Labor Supply," *Econometrica: Journal of the Econometric Society*, 945–963.

CONWAY, K. S. AND J. KIMMEL (1998): "Male labor supply estimates and the decision to moonlight," *Labour Economics*, 5, 135–166.

DICKENS, W. T. AND S. J. LUNDBERG (1993): "Hours Restrictions and Labor Supply," *International Economic Review*, 169–192.

FARBER, H. S. (2015): "Why you can't find a taxi in the rain and other labor supply lessons from cab drivers," *The Quarterly Journal of Economics*, 130, 1975–2026.

FEHR, E. AND L. GOETTE (2007): "Do workers work more if wages are high? Evidence from a randomized field experiment," *American Economic Review*, 97, 298–317.

FELDSTEIN, M. (1995): "The effect of marginal tax rates on taxable income: a panel study of the 1986 Tax Reform Act," *Journal of Political Economy*, 103, 551–572.

GELBER, A. M. (2014): "Taxation and the earnings of husbands and wives: evidence from Sweden," *Review of Economics and Statistics*, 96, 287–305.

GOLDIN, C. AND L. F. KATZ (2016): "A most egalitarian profession: pharmacy and the evolution of a family-friendly occupation," *Journal of Labor Economics*, 34, 705–746.

HALL, J. V. AND A. B. KRUEGER (2018): "An analysis of the labor market for Uber's driver-partners in the United States," *ILR Review*, 71, 705–732.

HAM, J. C. (1982): "Estimation of a labour supply model with censoring due to unemployment and underemployment," *The Review of Economic Studies*, 49, 335–354.

HANSEN, G. D. (1985): "Indivisible labor and the business cycle," *Journal of Monetary Economics*, 16, 309–327.

HOBIJN, B. AND A. SAHIN (2007): "Job-Finding and Separation Rates in the OECD," *FRB of New York Staff Report*.

——— (2009): "Job-finding and separation rates in the OECD," *Economics Letters*, 104, 107–111.

IACUS, S. M., G. KING, AND G. PORRO (2012): "Causal inference without balance checking: Coarsened exact matching," *Political Analysis*, 20, 1–24.

IMROHOROĞLU, S. AND S. KITAO (2012): "Social Security reforms: Benefit claiming, labor force participation, and long-run sustainability," *American Economic Journal: Macroeconomics*, 4, 96–127.

JAKOBSEN, K. M. AND J. E. SØGAARD (2022): "Identifying behavioral responses to tax reforms: New insights and a new approach," *Journal of public Economics*, 212, 104691.

KINDLUND, H. AND D. BITERMAN (2002): ". Work and the labour market," *International Journal of Social Welfare*, 11.

KLEVEN, H. J., M. B. KNUDSEN, C. T. KREINER, S. PEDERSEN, AND E. SAEZ (2011): "Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark," *Econometrica*, 79, 651–692.

LOONEY, A. AND M. SINGHAL (2006): "The effect of anticipated tax changes on intertemporal labor supply and the realization of taxable income," NBER Working Paper No. 12417.

LUCAS, R. E. AND L. A. RAPPING (1969): "Real wages, employment, and inflation," *Journal of political Economy*, 77, 721–754.

MACURDY, T. E. (1981): "An empirical model of labor supply in a life-cycle setting," *Journal of Political Economy*, 89, 1059–1085.

MARTINEZ, I. Z., E. SAEZ, AND M. SIEGENTHALER (2021): "Intertemporal Labor Supply Substitution? Evidence from the Swiss Income Tax Holidays," *American Economic Review*, 111, 506–546.

OECD (1991): *OECD Economic Surveys: Iceland 1991*, Paris: OECD Publishing.

——— (2007): *OECD Economic Surveys: Iceland 2007*, Paris: OECD Publishing.

ÓLAFSDÓTTIR, T., B. HRAFNKELSSON, G. THORGEIRSSON, AND T. L. ÁSGEIRSDÓTTIR (2016): "The tax-free year in Iceland: A natural experiment to explore the impact of a short-term increase in labor supply on the risk of heart attacks," *Journal of Health Economics*, 49, 14–27.

OLGEIRSSON, F. G. (2013): *Í þágu þjóðar. Saga skatta og skattkerfisbreytinga á Íslandi 1877-2012*, Reykjavik, Iceland: Directorate of Internal Revenue.

PAXSON, C. H. AND N. SICHERMAN (1996): "The dynamics of dual job holding and job mobility," *Journal of Labor Economics*, 14, 357–393.

PISSARIDES, C. (1997): "The need for labor-market flexibility in a European economic and monetary union," *Swedish Economic Policy Review*, 4, 513–546.

ROGERSON, R. (1988): "Indivisible labor, lotteries and equilibrium," *Journal of Monetary Economics*, 21, 3–16.

ROGERSON, R. AND R. SHIMER (2011): "Search in macroeconomic models of the labor market," in *Handbook of Labor Economics*, Elsevier, vol. 4, 619–700.

ROGERSON, R. AND J. WALLENIUS (2009): "Micro and macro elasticities in a life cycle model with taxes," *Journal of Economic Theory*, 144, 2277–2292.

SAEZ, E. (2003): "The effect of marginal tax rates on income: a panel study of 'bracket creep'," *Journal of Public Economics*, 87, 1231–1258.

SAEZ, E., J. SLEMROD, AND S. H. GIERTZ (2012): "The elasticity of taxable income with respect to marginal tax rates: A critical review," *Journal of Economic Literature*, 50, 3–50.

SHISHKO, R. AND B. ROSTKER (1976): "The economics of multiple job holding," *The American Economic Review*, 298–308.

SIGURÐSSON, E. AND H. TÓMASSON (2008): "langsniðslíkön og langsniðsgögn við rannsóknir á tekju/launaþróun," in *Rannsóknir í félagsvísindum; Viðskipta- og hagfræðideild*, ed. by I. Hannibalsson, vol. IX, 101–113.

SIGURDSSON, J. (2011): "Unemployment dynamics and cyclical fluctuations in the Icelandic labour market," Tech. rep., Department of Economics, Central bank of Iceland.

——— (2023a): "The Norwegian Tax Holiday: Salience, Labor Supply Responses, and Frictions," Tech. rep., Stockholm University.

——— (2023b): "Transitory Earnings Opportunities and Educational Scarring of Young Men," Working paper, Stockholm University.

SIGURDSSON, J. AND R. SIGURDARDOTTIR (2016): "Time-dependent or state-dependent wage-setting? Evidence from periods of macroeconomic instability," *Journal of Monetary Economics*, 78, 50–66.

SNÆVARR, S. (1993): *Haglýsing Íslands*, Reykjavik: Heimskringla, Haskolaforlag Mals og menningar.

STATISTICS ICELAND (2008): "ÍSNÁM2008 - Íslensk náms- og menntunarflokkun," Reykjavik.

STEFÁNSSON, A. (2019): "Labor supply response to a tax holiday: The take-home from a large and salient shock," Mimeo, Uppsala University.

SUN, L. AND J. M. SHAPIRO (2022): "A linear panel model with heterogeneous coefficients and variation in exposure," *Journal of Economic Perspectives*, 36, 193–204.

TAZHITDINOVA, A. (2021): "Increasing hours worked: moonlighting responses to a large tax reform," *American Economic Journal: Economic Policy*, Forthcoming.

THOMSEN, P. M. (2018): "Ragnarök: Iceland's Crisis, its Successful Stabilization Program, and the Role of the IMF," Speech, International Monetary Fund.

TORTAROLO, D., G. A. CRUCES, AND V. CASTILLO (2020): "It takes two to tango: Labour responses to an income tax holiday in Argentina," .

WEBER, C. E. (2014): "Toward obtaining a consistent estimate of the elasticity of taxable income using difference-in-differences," *Journal of Public Economics*, 117, 90–103.

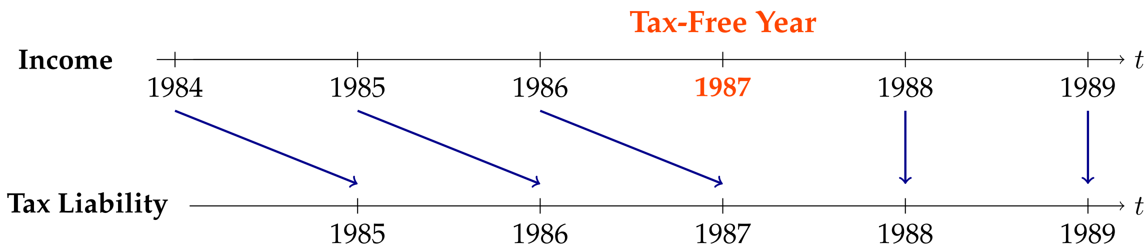


Figure 1: Income Tax System Before and After the Tax Reform

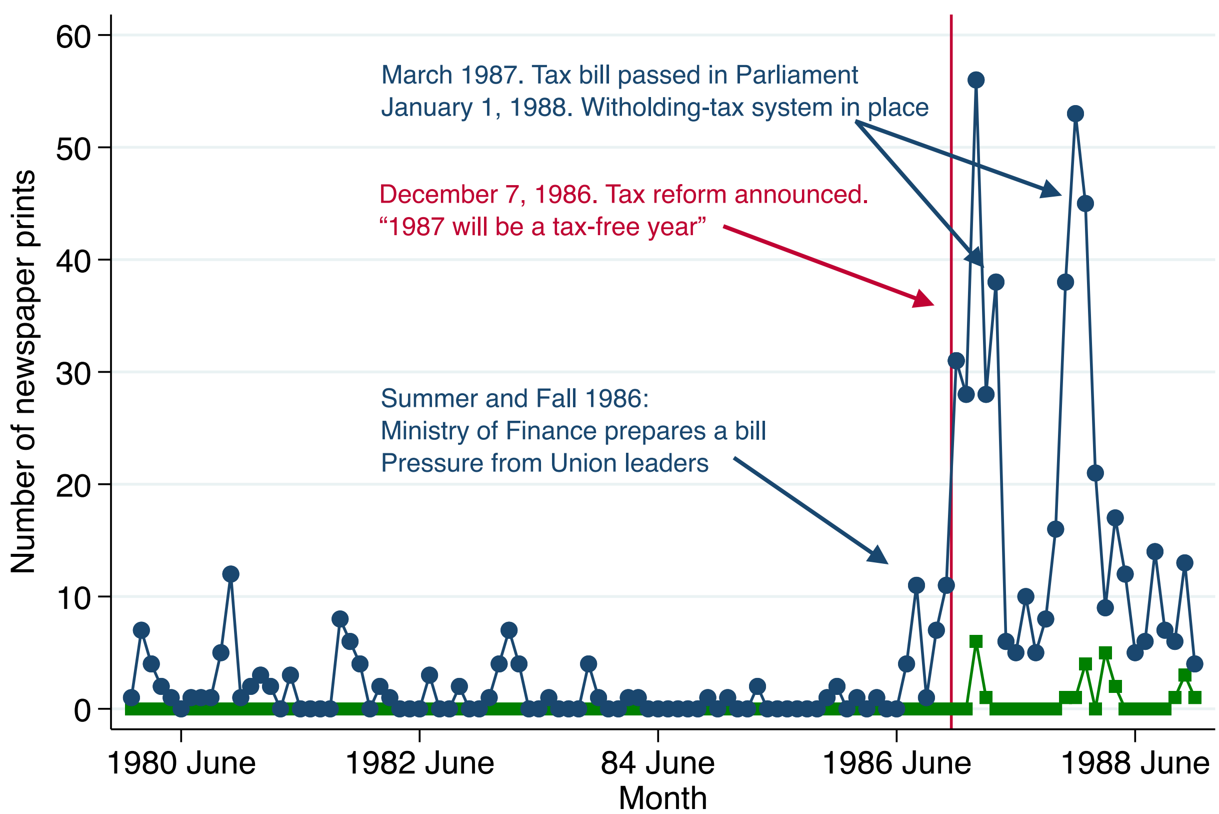
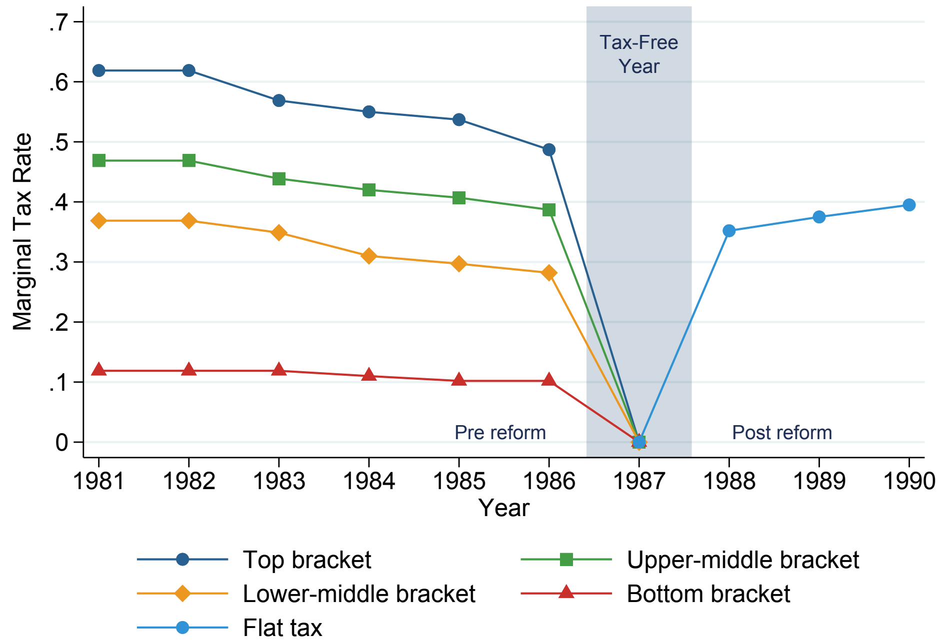
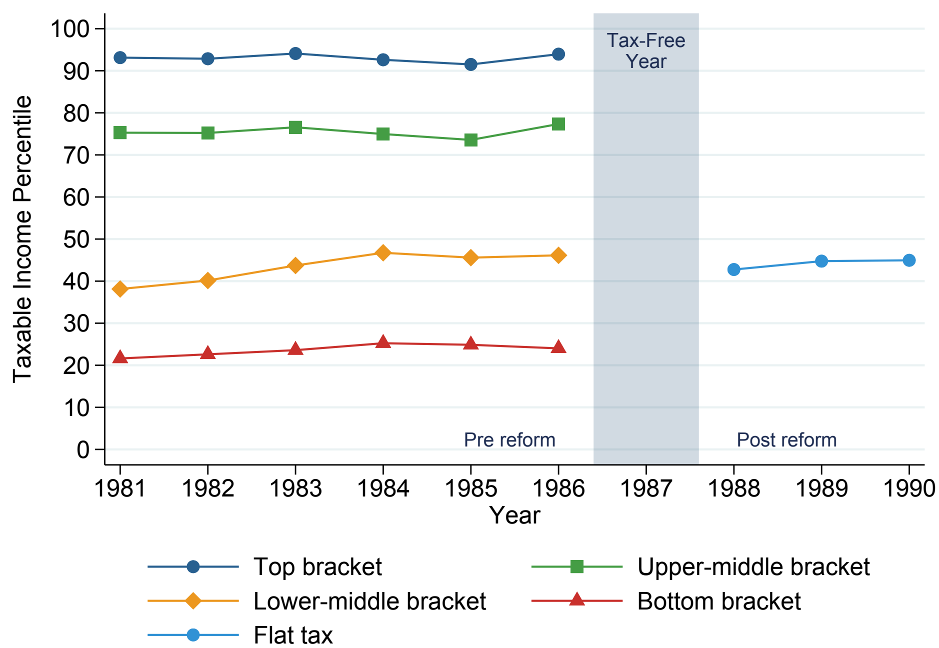


Figure 2: Number of Printed Newspapers Mentioning Withholding Tax

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.



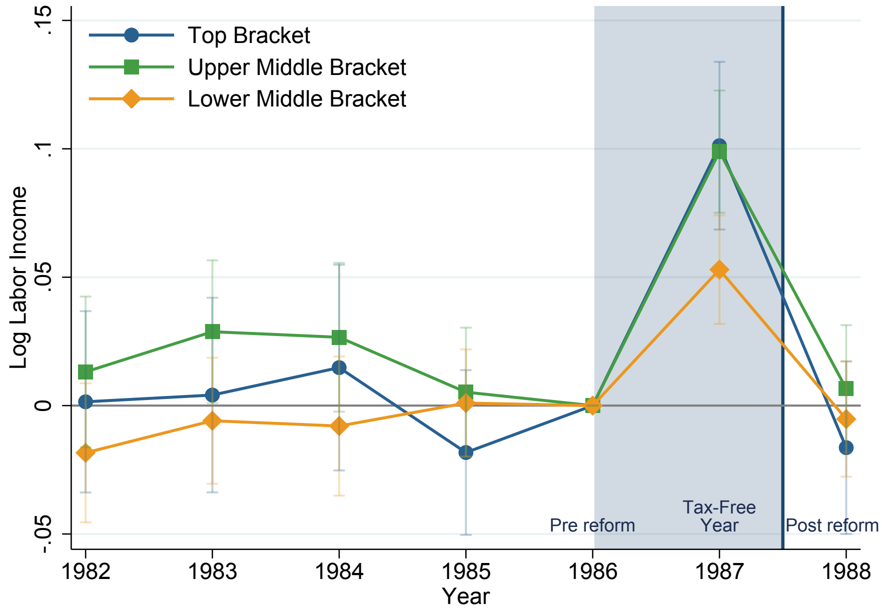
(a) Marginal tax rate by tax bracket



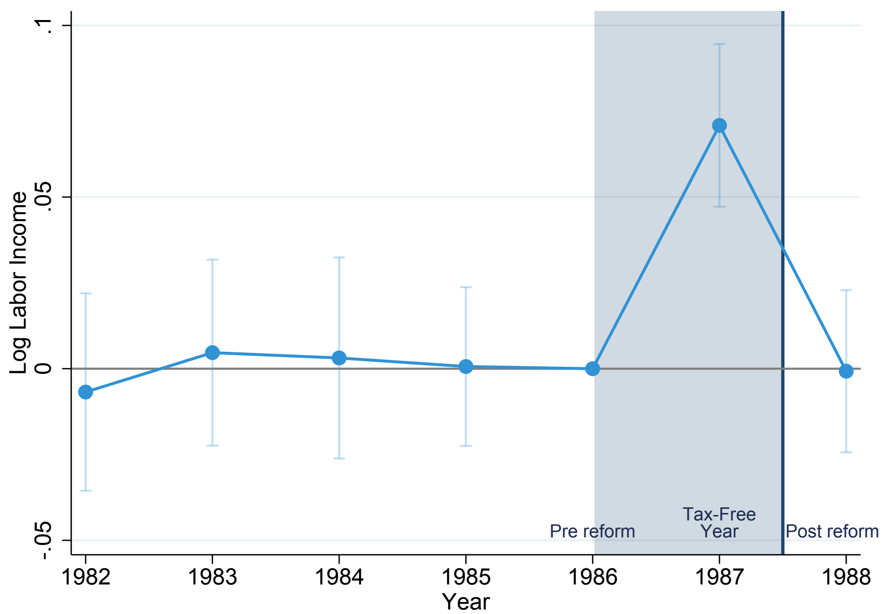
(b) Tax bracket thresholds in percentiles of income

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 contributions, cemetery charges, church tax and contributions to the construction fund for the elderly, are 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



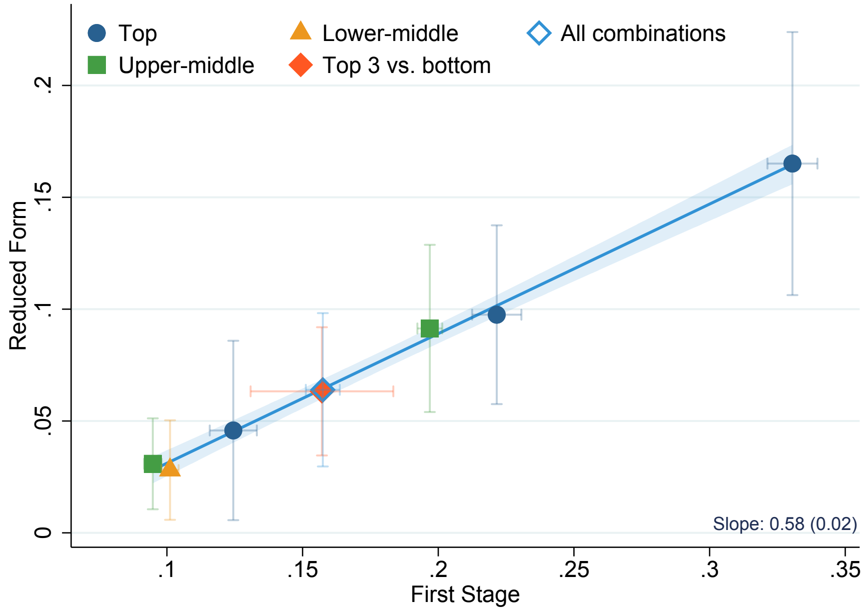
(b) Top three brackets vs. bottom bracket

Figure 4: Tax-Bracket Difference-in-Differences

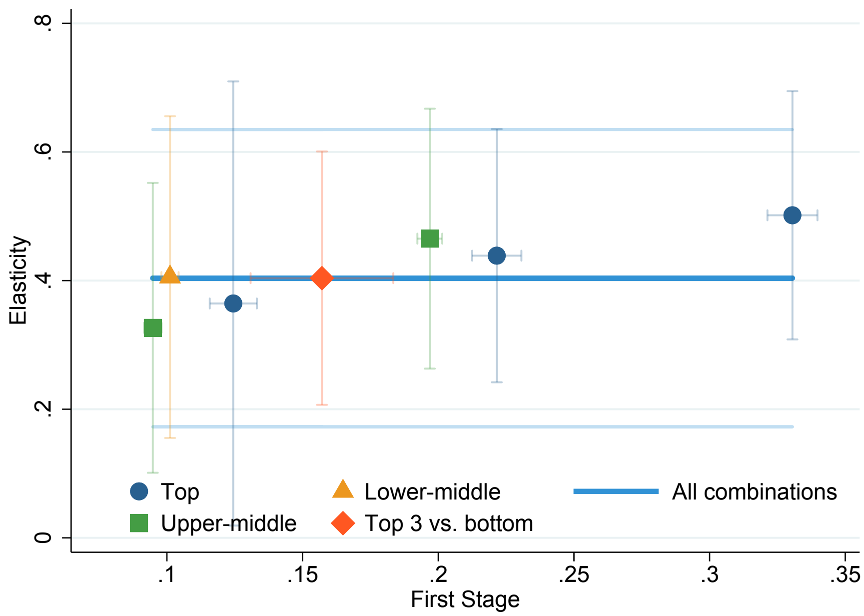
Notes: The figures plot estimates from a dynamic DID version of equation (1), estimated in the following regression

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

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).

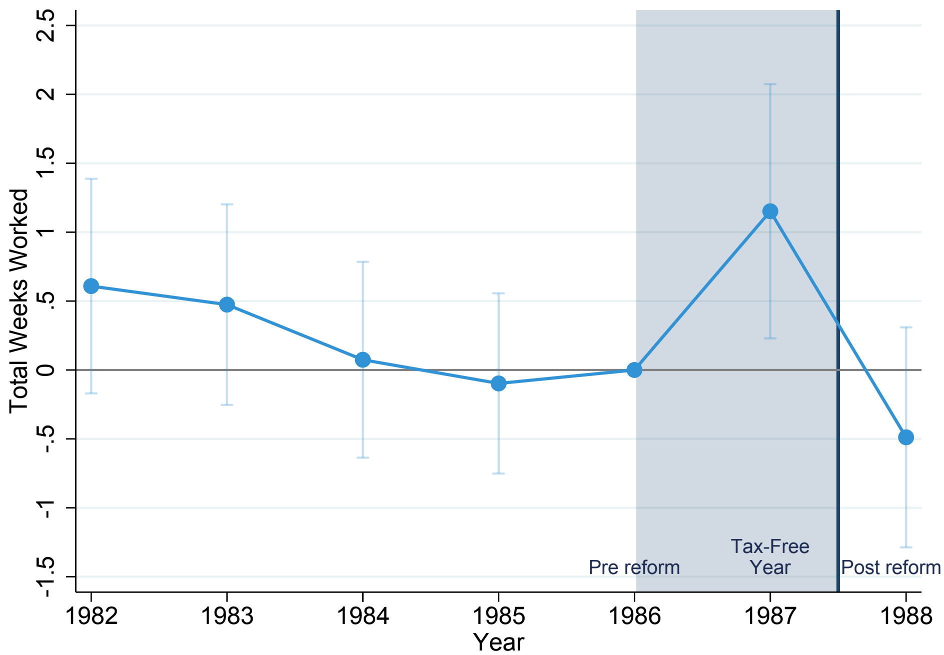
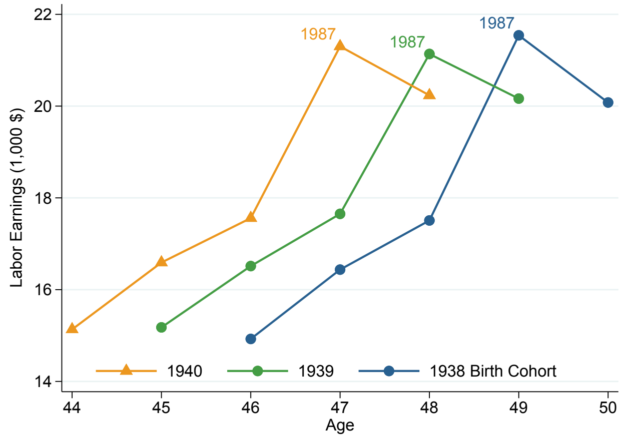
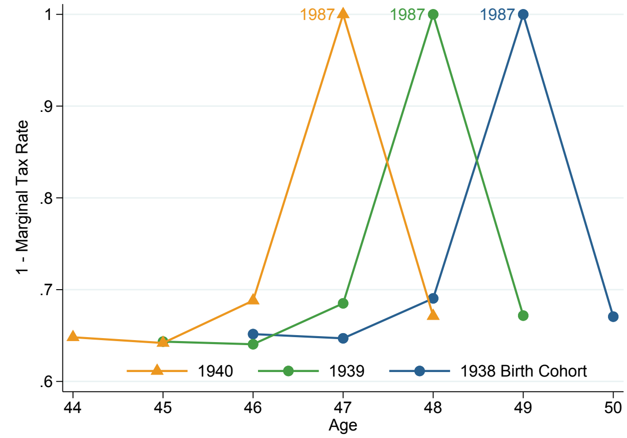


Figure 6: Weeks Worked

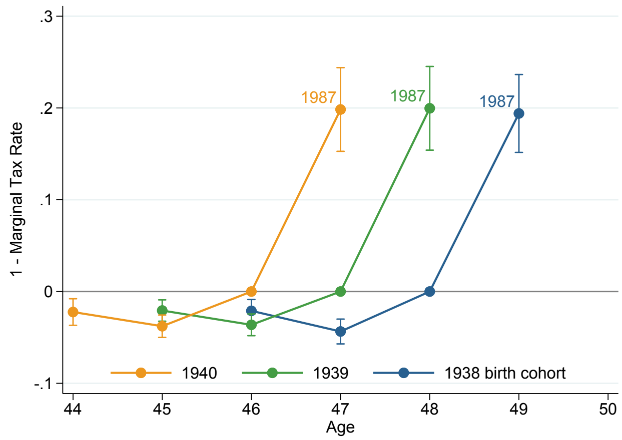
Notes: The figure documents the effect of the tax-free year on weeks worked. It plots coefficients from the dynamic version of regression (1) where the outcome variable is the total weeks worked per year. Details on the regression specification are in the note to Figure 4. The treatment group consists 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.



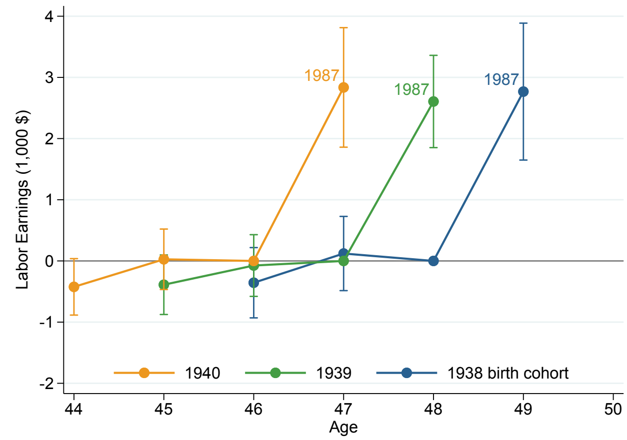
(a) Tax rates: Levels



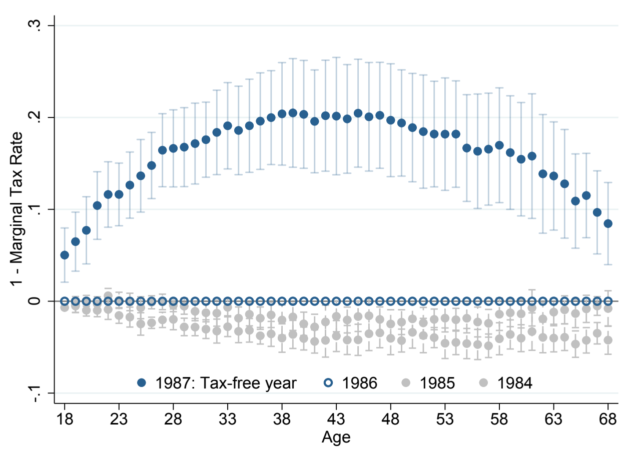
(b) Labor income: Levels



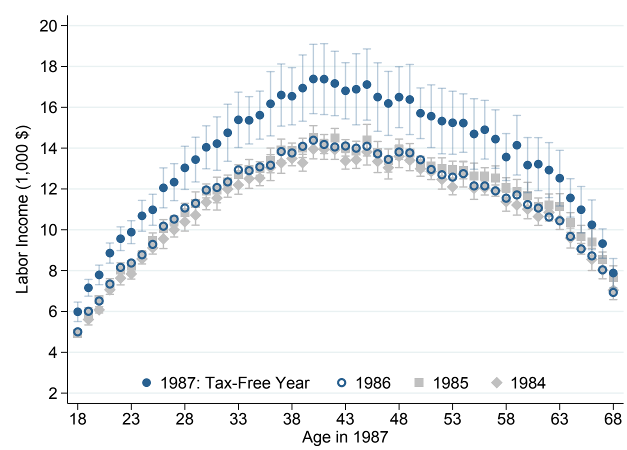
(c) Tax rates: Matched-cohort differences



(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 municipality, and vertical bars plot the 95% confidence intervals.

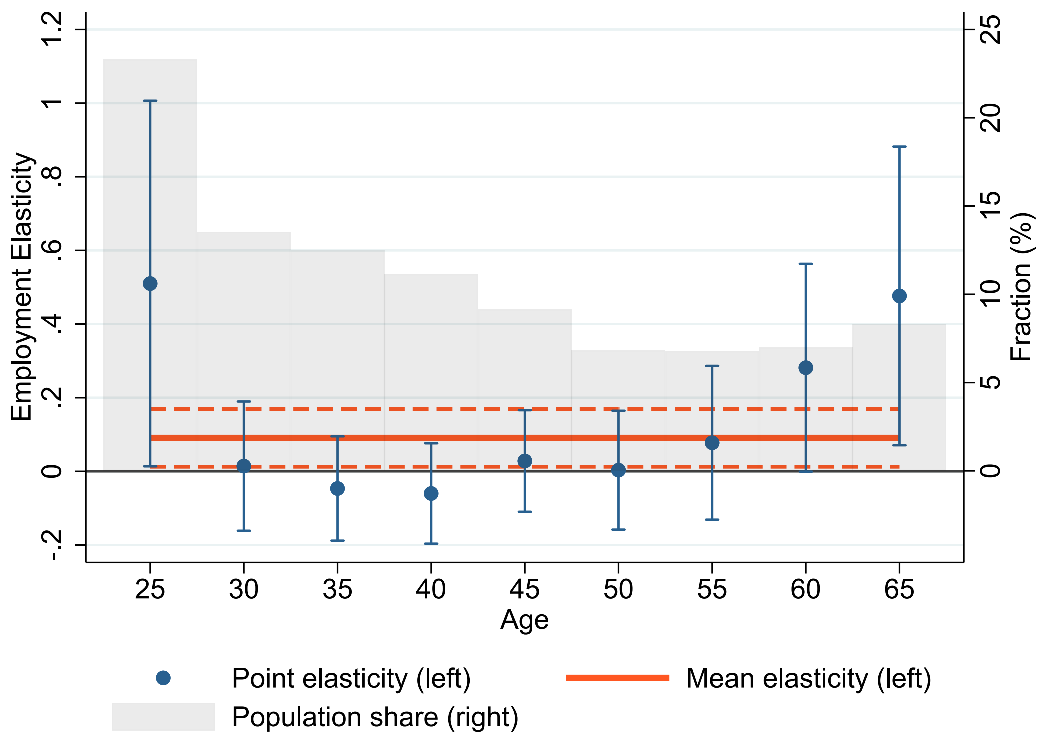
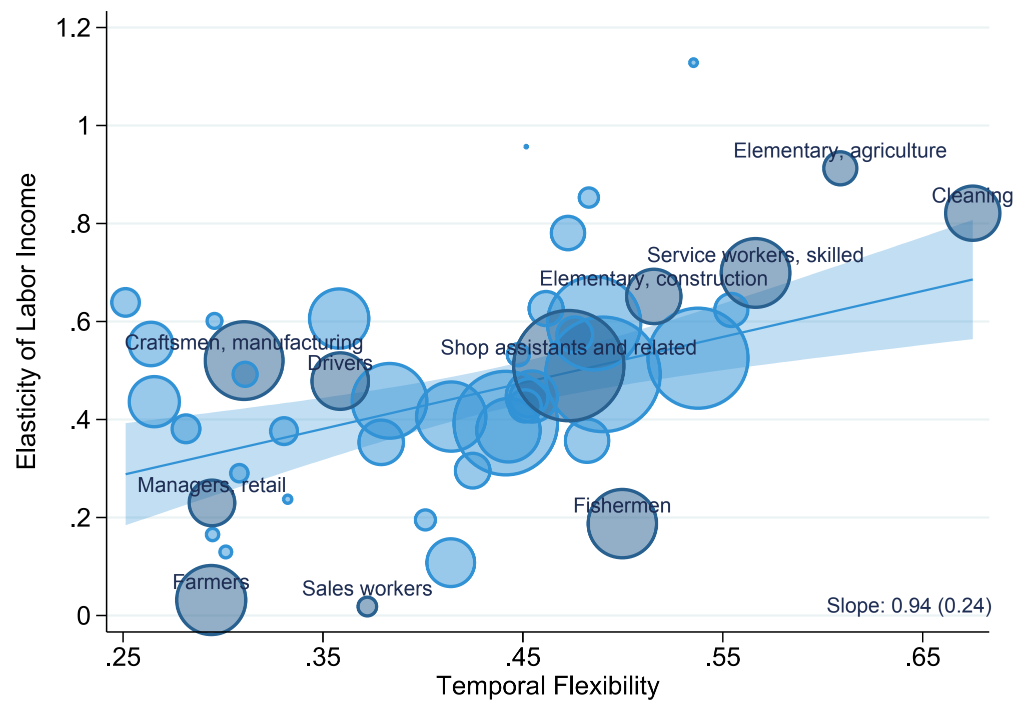
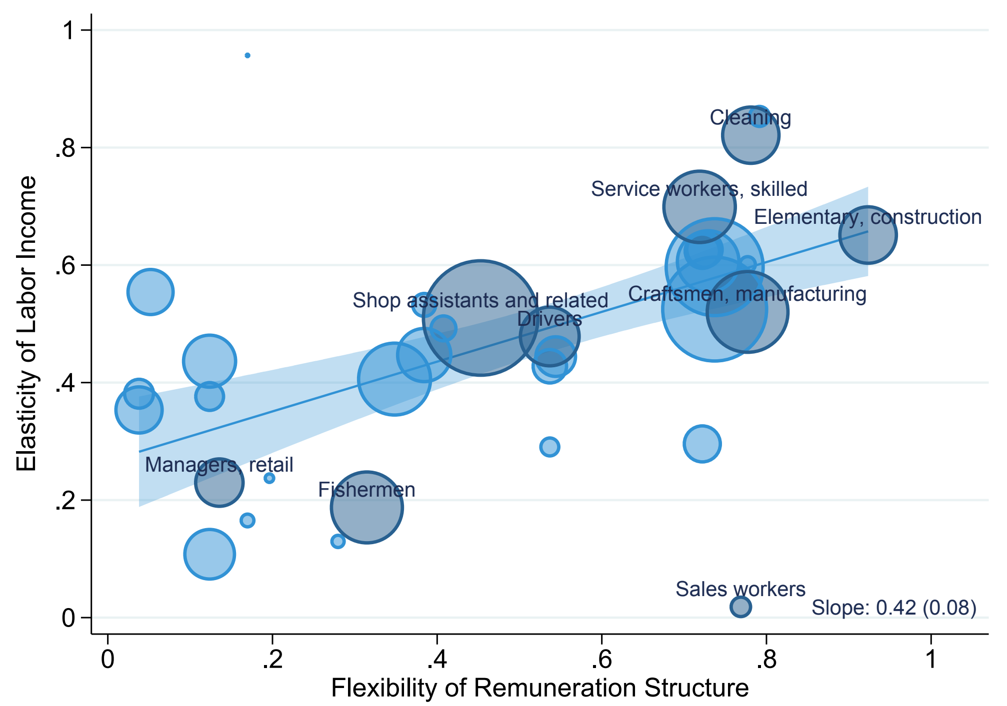


Figure 8: Extensive Margin Elasticity by Age

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 municipality, 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 III, 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.

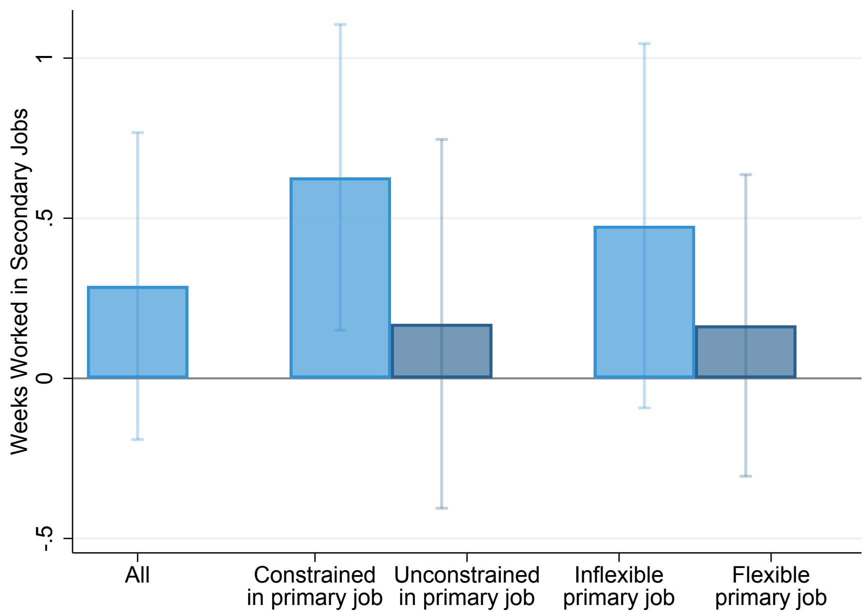
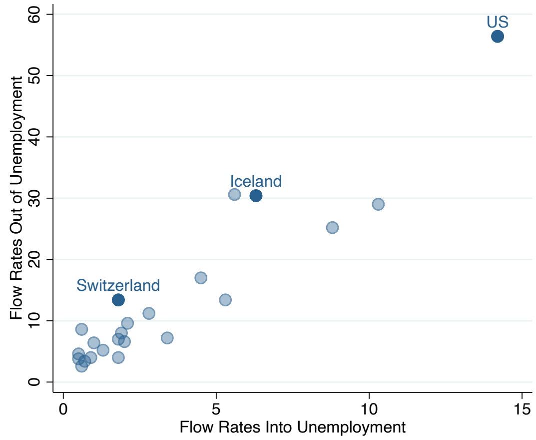
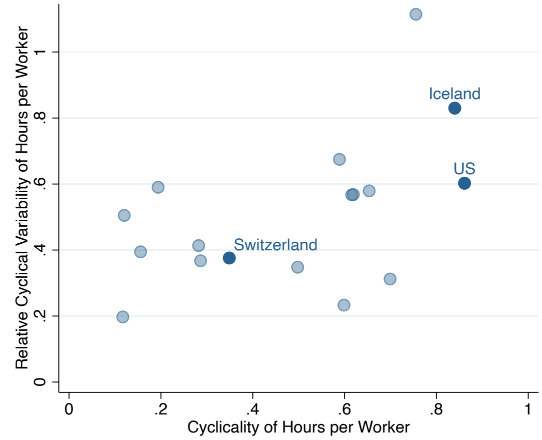


Figure 10: Effect on Weeks Worked in Secondary Jobs

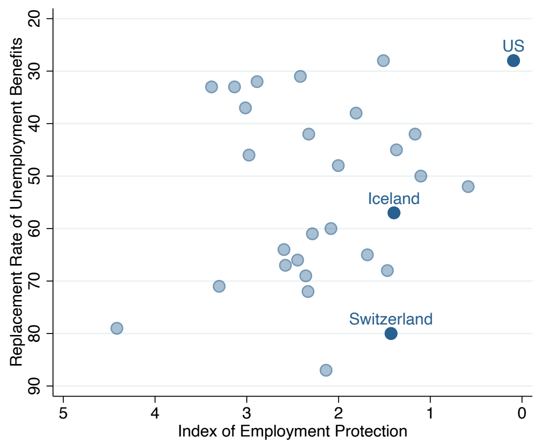
Notes: The figure presents the estimated effect on weeks worked in secondary jobs. The plots coefficient estimates from an estimation of equation (1), where the treatment group consists of workers in the three top tax brackets and the control group of those in the bottom bracket. The outcome variable 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 A, 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.



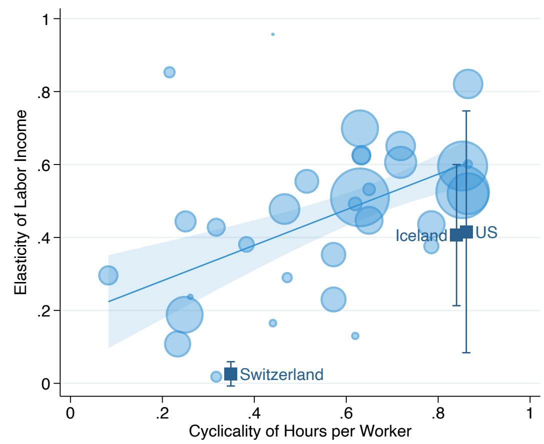
(a) Flexibility of employment



(b) Flexibility of working hours



(c) Flexibility of institutions



(d) Frisch elasticity estimates by flexibility in working hours

Figure 11: Measures of labor market flexibility across OECD countries

Notes: Panel (a) plots on the x-axis the flow probabilities into unemployment from employment and non-employment, 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 worker. 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 the 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 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 are reversed so that moving out along the axis implies more flexibility. Panel (d) plots intensive margin Frisch elasticity estimates by country and occupation against cyclicality of hours per worker, plotted on the x-axis of panel (b). 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 and Switzerland are, respectively, from the current paper and Martinez, Saez, and Siegenthaler (2021). 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.

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

	Log labor income		Weeks worked	
	(1)	(2)	(3)	(4)
2SLS DID ($\frac{dy}{d\log(1-\tau)}$)	0.434	0.407	6.583	5.369
	(0.067)	(0.099)	(2.403)	(3.101)
Reduced form (dy)	0.073	0.064	1.107	1.249
	(0.011)	(0.015)	(0.413)	(0.491)
First stage ($d\log(1-\tau)$)	0.168	0.157	0.168	0.157
	(0.013)	(0.013)	(0.013)	(0.013)
Mean of outcome variable	—	—	49.37	49.37
Weighted	No	Yes	No	Yes
Observations	176,298	165,044	174,204	163,084

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	0.460	0.300	0.353	0.339	0.307
	(0.099)	(0.133)	(0.096)	(0.079)	(0.102)	(0.093)
Reduced form ($d\log y$)	0.064	0.075	0.049	0.068	0.053	0.049
	(0.015)	(0.021)	(0.016)	(0.016)	(0.016)	(0.014)
First stage ($d\log(1-\tau)$)	0.157	0.161	0.164	0.191	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	154,879	144,206	65,967	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 the 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	0.846	6.490	0.008	0.346	2.186
	(0.025)	(0.028)	(0.634)	(0.012)	(0.031)	(0.719)
Reduced form (dy)	0.005	0.163	1.261	0.000	0.105	0.642
	(0.002)	(0.005)	(0.122)	(0.002)	(0.004)	(0.197)
First stage $(d\log(1-\tau))$	0.098	0.182	0.194	0.156	0.280	0.281
	(0.002)	(0.003)	(0.003)	(0.011)	(0.015)	(0.015)
Mean dependent variable	0.618	—	40.71	0.871	—	47.74
Elasticity	0.090	—	0.159	0.009	—	0.046
Observations	551,438	513,090	503,177	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-elasticity (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 municipality, are in parentheses.