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

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Abstract

Labor income earned in Iceland in 1987 went untaxed. I use this episode to study labor supply responses to temporary wage changes. I construct a new population-wide dataset of earnings and working time from pay slips and use two identification strategies to estimate intensive and extensive margin Frisch elasticities of 0.37 and 0.10, respectively. Workers with the ability to adjust drive these average responses: extensive margin responses by young and close-to-retirement cohorts and intensive margin responses by workers in temporally flexible jobs. However, constrained workers take up secondary jobs, which contribute to one-tenth of the overall response. Importantly, married women with children and the wives of men in temporally inflexible jobs respond more strongly than other women do. Within families, wives respond more than do their husbands, who themselves respond negatively to their wives' tax cuts. This is consistent with substitutability in nonmarket time. Overall, my results suggest that adjustment frictions reduce aggregate labor supply responses to tax cuts and can similarly explain differences in elasticities within and across countries.

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

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

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1 Introduction

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

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

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

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

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

Using the tax bracket difference-in-differences, I estimate responses in both labor earnings and working time, with an intensive margin earnings elasticity of 0.37 and a working time elasticity of 0.10. Decomposition of this into different margins reveals that 30% 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 70% is accounted for by additional earnings within full-time weeks, such as through hours of overtime and greater effort on the job.

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 builds on 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-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 0.10. This average response, which is rather modest relative to most comparable evidence (Chetty et al., 2013), masks important heterogeneity in that the employment responses predominantly originate 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 find a very precisely estimated zero elasticity.

Disagreement on the size of the Frisch elasticities is likely to reflect different views on the frictions that attenuate observed labor supply responses relative to unconstrained responses solely determined by preferences. Results from settings where workers are free to choose their hours worked—such as bicycle messengers in Zurich (Fehr and Goette, 2007) and taxi drivers in New York (Farber, 2015)—imply relatively large Frisch elasticities. Generalizing from these estimates and reconciling evidence across studies inherently relies on understanding how frictions distort the labor supply responses of the average worker. However, how important adjustment frictions are in shaping aggregate hours responses has remained elusive owing to limited direct evidence. I leverage the unique empirical setting offered by the tax-free year in Iceland to examine how temporal flexibility in current jobs, flexibility acquired through the take-up of new and secondary jobs, and flexibility in reallocating nonworking time to market work shape the labor supply responses. Consequently, this analysis yields several new insights.

The first is that flexibility in employment arrangements is key in shaping the labor supply responses. My results demonstrate that the 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 who also drive the extensive margin response—are the most responsive. 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 an increased take-up of secondary jobs during the tax-free year. This response is entirely driven by workers constrained in their primary jobs, in line with the theoretical predictions (Shishko and Rostker, 1976; Paxson and Sicherman, 1996). In contrast,

[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. Thanks to Joshua Angrist for pointing this out. The method employed in the current paper differs from these earlier studies by combining cohort grouping and a natural experiment, where the former generates comparable groups on similar life-cycle trends and the latter provides the identifying variation.

workers changed their primary jobs less often during the tax-free year, consistent with workers being less willing to engage in time-consuming job searches. 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.

Frictions in the adjustment of labor supply not only pertain to the job and employment arrangements of workers but also more broadly to what constraints they face in reallocating more of their nonworking time to market work. As an example, if couples engage in shared childcare activities, the labor supply of one spouse will directly influence that of the other. I examine how family structure shapes the labor supply responses and document three main findings. First, married women have higher elasticities than have married men, with this difference more pronounced in families with more children. In contrast, there is no difference in the elasticities of single men and single women. Second, workers whose spouses are less temporally flexible in their jobs have larger elasticities than those whose spouses hold more flexible jobs. Third, I analyze how couples respond to changes in the marginal tax rates and labor supply of their spouses. I estimate negative cross-elasticities for husbands but no significant cross-responses for wives. Total household responses accounting for both own- and cross-responses are about 20% smaller than if the spouses had been treated in isolation. These results are consistent with coordinated spousal labor supply and substitutability in the nonmarket time of couples, whereby any increase in the time devoted to market work by wives is met with greater home production time by their husbands.[2] Moreover, the frictions that spouses face in adjusting working time influence the impact of intrafamily restrictions in the reallocation of nonmarket time. Taken together, these results imply that gender differences in labor supply documented in an extensive extant literature (McClelland and Mok, 2012) are unlikely to reflect inherent differences in preferences. Instead, they likely reflect the coordination of couples and specialization in the household, coupled with the influence of market-level frictions.

My estimates of average Frisch elasticities most closely relate to those in two existing studies.[3] First, Bianchi, Gudmundsson, and Zoega (2001) consider labor supply during the same tax-free year, but only among a small random sample of workers.[4] 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 elasticity is more than twice as large as my estimates, based on tax bracket difference-in-differences. On this basis, I conclude that it is important to isolate responses to the tax-free year from the influences of

[2] Models of unitary household labor supply predict symmetric cross-elasticities (Chiappori and Mazzocco, 2017). This contrasts with my empirical findings, thus complementing a large empirical literature testing the restrictions imposed by unitary household models (Donni and Chiappori, 2011). More generally, it is theoretically ambiguous and an open empirical question whether spousal labor supply is a complement or a substitute. My results are in line with findings of substitutability in spousal labor supply in response to job loss (e.g. Lundberg, 1985) and the non-receipt of disability benefits (Autor, Kostøl, Mogstad, and Setzler, 2019), and that substitutability in childcare time is important in shaping the labor supply responses of couples (Blundell, Pistaferri, and Saporta-Eksten, 2018).

[3] In Section 7 I provide a detailed summary and meta-analysis of previous work.

[4] 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).

pre-trends and macroeconomic shocks. Furthermore, what distinguishes my study from theirs is a focus on how adjustment frictions influence the heterogeneity of labor supply responses, which provides new insights into the anatomy of the Frisch elasticity.

Second, in a contemporaneous study, Martinez, Saez, and Siegenthaler (2021) analyze the labor supply responses to a two-year tax holiday in Switzerland. This tax holiday resulted from the transition from an income tax system where current taxes depended on income in the previous two years to an annual pay-as-you-earn system. They estimate an average intensive margin elasticity of 0.025 and identify no extensive margin responses. My results suggest that all of the employment responses and a disproportionate proportion of the hours and earnings response to temporary tax cuts arise from young first-time workers and those close to retirement. Both groups are excluded from the analysis in Martinez, Saez, and Siegenthaler (2021). The remaining differences likely arise from differences in labor market flexibility. Flexibility in working hours, as measured by the cyclicality of hours per worker, is more than twice as high in Iceland as in Switzerland, and worker flows—measuring the fluidity of the labor market—more than three times as high. Other measures of labor market flexibility tell a similar story: the flexibility of the Icelandic labor market is much more similar to that of the US labor market than to continental Europe. I document a positive correlation between the flexibility of working hours and the size of the Frisch elasticity, both across occupations and across countries where estimates are available, indicating that similar forces are at work in shaping differences in labor supply elasticities within and across countries.[5]

Sigurdsson (2021a) presents evidence that further corroborates this notion. Studying a tax holiday in Norway that resulted from a tax reform similar to those in Iceland and Switzerland, I document that a quarter of the working-age population responded by increasing their labor supply and estimate an earnings elasticity of about 0.14—halfway between the estimates from Switzerland and Iceland. In line with that, the Norwegian labor market measures to be more flexible than the Swiss labor market, but less than the Icelandic and US labor markets. Furthermore, I document that for those aware of the tax holiday but did not adjust their hours, the lion's share says that frictions in adjusting the time spent working are the main reason for not responding.

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

2 The Tax-Free Year and Background

[5] Tortarolo, Cruces, and Castillo (2020) study a 2.5-year-long income tax holiday in Argentina and estimate a very small labor supply response. In line with my cross-country analysis of labor market flexibility and labor supply elasticity, Argentina has a very rigid labor market compared to OECD countries.

2.1 Income Tax System and Tax Reform

On January 1, 1988, Iceland adopted a withholding-based pay-as-you-earn income tax system, similar to what is now in place in most advanced economies. Prior to the reform, income taxes were collected 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.[6]

The key features of the reform for the purpose of my analysis are that it generated a large, salient and unanticipated increase in wages that lasted only a single year. On December 6, 1986, the Finance Minister announced the tax reform. The Ministry of Finance had begun preparing the reform in early fall 1986 and later that same fall there was the decision for it to take place in January 1988. The reform was therefore unanticipated by taxpayers. Figure 2 plots the monthly count of the number of newspapers mentioning a withholding-based or pay-as-you-earn tax system between January 1980 and December 1988. As shown, there was no discussion of a reform of this kind in the years before its announcement, whereas 30–40% of the newspapers printed in the weeks following the announcement had coverage of the reform.[7]

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.[8] 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.[9]

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

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

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

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

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 in Section 4, I perform a series of tests to evaluate this claim, finding the results to be robust.

2.2 Icelandic Labor Market in an International Context

The Icelandic labor market is quite flexible, characterized by low unemployment, flexible hours, and variable participation and wages (OECD, 1991, 2007).[10] 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.[11]

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

[10] 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*.

[11] 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).

wages are very flexible in Iceland when compared with many other European countries.

3 Data

For this project, I construct a new administrative data set for the universe of the Icelandic working-age population back to 1981. The data set has two main sources: an employer–employee data set constructed from newly digitized pay slips, and individual tax records. I describe these below.[12]

3.1 Pay Slips: Employer–Employee Data

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

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

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

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

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.

3.2 Individual Tax Returns

The second primary data source I use in this paper is a panel of individual tax returns. As is the case for the data set constructed from pay slips, these data extend back to 1981, with the data sets easily linked via the unique personal identifier. Individual tax returns have information on all income, including labor income, financial income, pension, social security, and transfer payments as well as other sources of income. These data also record all tax payments, both at the national and local levels, as well as any deductions and tax allowances. I use these detailed data to construct the marginal tax rates.[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.

4 The Intensive Margin

4.1 Research Design

In general, to identify 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. Building on Feldstein (1995), I exploit these features in a difference-in-differences (DD) 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 3a plots the evolution of tax rates by tax brackets

[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 C.1). This method predicts actual tax liabilities with great precision.

during the 1980s. In 1986, the average worker in the bottom tax bracket faced a marginal tax rate of 10.2%, corresponding to the average municipal tax rate, while the average tax payer in the top bracket faced a marginal tax of roughly 48.7%.[14] 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; these were generally reviewed and updated each year to account for inflation. As Figure 3b documents, this resulted in tax-bracket thresholds corresponding to roughly the same income percentile throughout the 1980s and therefore the income groups in each bracket were stable and similar over time.

Assigning treatment status. The empirical strategy used to estimate the elasticities is to relate the differential labor supply responses of workers in higher vs. lower tax brackets to their differential tax relief. As the tax rates faced each year are endogenous to labor income, which is the outcome of interest, I follow Feldstein (1995) and later work by assigning treatment status based on a lagged tax bracket, which is unrelated to current income. Given that the income and other factors influencing the tax bracket position are persistent, the tax bracket position is also persistent, as 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. In the main analysis, the treatment group consists of workers facing marginal taxes in the top three brackets, while workers in the bottom bracket constitute the primary control group. To obtain a larger sample size for inference and later detailed analysis, I pool the estimates for the top three tax brackets, under the assumption that the labor supply elasticity is the same across tax brackets, providing a weighted average elasticity. In addition, I also estimate the disaggregated responses by 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 employed in the previous year, defined as having labor earnings greater than or equal to the base income threshold, roughly corresponding to minimum wage earnings for a low-skilled worker.[15] In the analysis, I define employment in the same way when estimating the extensive margin responses, i.e. having labor earnings exceeding this threshold. Restricting the sample in this way corresponds to restricting the sample to those with earnings above the 20th percentile, including zeros, leaving a sample of workers in one of the four brackets. Given the unemployment rate in Iceland was between 1% and 2% throughout the 1980s (Appendix Fig-

[14] In 1986, the municipal tax rate ranged between 5% and 11.5%.

[15] Similar restrictions are frequently imposed in studies of the core labor force, see e.g. Kindlund and Biterman (2002). The base income threshold equals $1.5\times$ guaranteed income (*tekjutrygging*), where guaranteed income is a reference amount used in calculations of various kinds for income support provided by the government and the 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.

ure A.9), this restriction mainly serves as a means of excluding those entering and exiting the labor market because of changes in life cycle, which may generate differential trends across tax brackets depending on where workers enter and exit.

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

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

where y_{it} is the outcome of interest of individual i in year t, $bracket_{i,t-1}$ is an indicator function for tax brackets in year $t-1$ (treatment status), 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 $B_{i,t-1}$, which is an indicator function for being in one of the top three tax brackets, interacted with a dummy for the tax-free year of 1987. The regression controls for individual characteristics, collected in the vector $\boldsymbol{X}_{it}$, which includes a full set of dummies for individual characteristics such as age, marital status, number of children, education and living in the capital area, are all defined in pre-reform levels. The error term is denoted by μ_{it} and captures other determinants of the labor supply. The importance of accounting for serial correlation in outcomes in a DD setting has been emphasized by Bertrand, Duflo, and Mullainathan (2004). To address this, I cluster standard errors at the individual level to allow for arbitrary correlation over time in the error term.

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

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

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

4.2 Results

Graphical evidence and validity of identifying assumptions. The key identifying assumption underlying the empirical design is that in the absence of a tax-free year, the labor supply of workers in high and low tax brackets would have run parallel. To formally test the plausibility of this assumption, I estimate a version of the DD regression (1), where the treatment status is interacted with time dummies.[16] The results for both labor earnings and weeks worked are presented in Figure 4. The set

[16] Appendix Figures A.6, A.7 and A.8 complement this by visually implementing the difference-in-differences by plotting the time series of labor earnings, weeks worked, marginal tax rates and average tax rates for the average individuals in

of pre-reform coefficients tests for parallel trends, with each coefficient corresponding to a placebo test for the given year. The tests indicate no false positives.[17] While there is no significant difference post-1987 in terms of weeks worked, there is a difference in labor earnings. There can be two reasons for this difference. First, the tax-free year may generate an effect on the labor supply that extends beyond 1987. Second, changes in the tax system in 1988 possibly influenced the labor supply in 1988 and after. As the focus of this paper is the short-term effect of a transitory tax cut, I limit the sample to 1981–1987, with 1987 being the single treatment year. While I comment briefly on responses extending beyond 1987, I reserve the analysis of permanent effects to future research.

Regression results. Table 1 presents estimates of the effects of the tax-free year on labor earnings, weeks worked and employment. Each column-by-row entry in the table corresponds to a regression estimate. The top row of column (1) provides estimates of the elasticity of earned income, defined as the sum of labor earnings in all jobs including self-employment. The elasticity estimate is 0.374 and is highly statistically significant at the 1% level. This estimate implies that a 10% increase in the after-tax wage causes labor earnings to increase by almost 4% on average. Conceptually, the elasticity estimate consists of two components. First, the reduced form, presented in the third row, which is a DD estimate of equation (1) on log labor income, which is estimated to be 0.077. Second, presented in the middle row, is the first stage, which is a similar DD estimate where the outcome variable is the log net-of-tax rate, estimated to be 0.207. The elasticity is essentially the ratio of the reduced form to the first stage, but here estimated using 2SLS.

Previous research has highlighted that DD designs can be effectively combined with matching methods to produce more robust inferences (Heckman et al., 1997; Blundell and Dias, 2009). Matching will generate more comparable treatment and control groups and DD will 'difference out' unobserved differences. To leverage these benefits, I augment the DD estimation with nonparametric coarsened exact matching (Iacus, King, and Porro, 2012). More precisely, I first match individuals coarsely on pre-reform characteristics (age, marital status, number of children, and education) and then estimate DD on the matched sample, using the weights obtained from matching. As the set of covariates used in the matching procedure is very general, I can match 99.96% of the sample in this way. The earnings elasticity estimate obtained in this manner, and reported in column (2), is about 0.4 and therefore very similar to the main specification. This robustness implies that systematic differences in the characteristics of individuals across the different brackets have only limited effects on the estimates.

the top three tax brackets relative to those in the bottom bracket. To enhance the comparison with the regression analysis, I nonparametrically weight the group-by-year distributions of the control group to align with that in the treatment group, using the frequently applied reweighting method in DiNardo, Fortin, and Lemieux (1996). The figures provide compelling evidence of differential labor supply responses among workers in the higher tax brackets.

[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. As Appendix Figures A.6, A.7 and A.8 show, this had substantial effect on earnings and hours, and somewhat disproportionally so for workers in the lower than higher tax brackets. However, Figure 4 highlights the importance of the DD design, as it is mostly able to "difference out" the effects of this sharp macroeconomic shock.

Columns (3) and (4) of Table 1 present comparable estimates for the effect on weeks worked. The variable collects total weeks worked across all jobs held by the individual. The regression estimates reflect strong responses in weeks worked. The reduced-form estimate implies that workers in the top three tax brackets increased their working time by about a week more than those in the bottom bracket. Relative to a pre-reform average, the treatment effect of five additional weeks implies an elasticity of about 0.10 (4.926/48.43).[18]

It is important to highlight what these results imply and what to expect. As discussed in Section 3, the working time recorded on the pay slips is as 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. In total, workers can work less than 52 weeks a year, e.g. if not working all weeks in the year or if part-time and not working 40 hours per week. However, they can work more than 52 weeks only if they hold more than one job. Therefore, an additional week reflects the exchange of vacation for working time, more full-time employment and work in secondary jobs. However, this measure does not capture overtime and other changes in working time beyond the 40-hour work week, which in Iceland is an important margin for labor adjustment.[19] The increase in weeks worked therefore most likely reflects a lower bound of the total hours adjustment to the tax-free year, which are captured in full in the earnings response.

With this in mind, I decompose the labor supply response into two subcomponents. The estimates imply that 30% of the overall response are brought about by more weeks worked—through less vacation time, more full-time employment, and secondary jobs—and 70% by more earnings within those weeks, through overtime hours and greater work effort.

Columns (5) and (6) in Table 1 document the estimated effect on employment, for which I find no significant effect. When interpreting this result, a few features of the research design are important to bear in mind. First, recall that I identify the labor supply responses from the differential responses of workers in different tax brackets. Hence, by construction, the research design is unable to uncover labor market-entry responses. Second, while the design is able to reveal the potential effect on labor market exit, for reasons such as delayed retirement, the estimates imply that there are no *differential responses* along the extensive margin across tax brackets. However, it is still possible that some workers delayed retirement in response to the temporary incentive created by the tax-free year, relative to what they would not have done in a normal (taxed) year. I revisit this question in Section 5, where I develop a research design that can detect extensive margin responses through both entry and exit. In light of these results, I therefore refer to the estimates in Table 1 as reflecting the intensive margin responses.

Real labor supply responses, not a reporting phenomenon. A critical reader may ask the important question: Can we interpret the estimated earnings elasticity as labor supply elasticity? While it is clear that the finding of an effect on weeks worked stems from additional time spent working, the earnings effect may incorporate some form of reporting response or tax avoidance. I conduct further

[18] As reported in Appendix Table A.2, this implied elasticity is similar to that obtained from a specification for logarithms of weeks worked.

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

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 the employed, the self-employed and business owners, defining having at least one job as self-employed. Self-employed individuals are likely to have greater flexibility in adjusting their labor supply and hence, we might expect to find larger responses for them. However, self-employed workers may also be able to increase their income in the tax-free year through tax avoidance, e.g. by misreporting capital income as labor income, or by shifting income from other years to the tax-free year. Such avoidance is less likely to be possible for employed workers, as their employers have no direct incentive to collude. Table A.1 in the appendix reports estimates for these groups separately. For wage earners, the earnings elasticity is almost the same as for the whole sample, 0.373, while the elasticity is larger for the self-employed (0.484). However, as the table shows, there are similar differences in the elasticity of working time. While I cannot rule out that some of the differences between these groups arise from reporting behavior, these findings strongly indicate that they largely reflect differences in hours flexibility. Indeed, such flexibility may be tempting for workers in less flexible jobs. In Appendix Table A.5, I investigate whether there was an increased take-up of self-employment in the tax-free year, and find a significant entry response.[20]

Second, I also examine whether income shifting likely explains the estimates. During the tax-free year, workers may have negotiated with their employer to adjust their compensation in some way or to frontload some payments. While such behavior is likely to be more difficult and costly to achieve through wages and salaries, e.g. due to payroll taxes, other forms of payments may have been used. To investigate this possibility, I estimate equation (2) separately for each subpayment on the pay slip (in real $ values) and report the effect relative to the total. The results are reported in Appendix Table A.3. Overall, the results do not exhibit an unexpected pattern. Increases in wages and salaries make up 94% of the increase in payments and most of the remainder consists of payments such as fringe benefits and travel allowances, which are likely linked to more work. Potential suspects for income shifting, such as sales commissions and bonuses, as well as gifts, make up only 0.8%.[21]

Third, I estimate the effect on capital income. While labor and capital income were taxed according to the same tax schedule both pre- and post-reform, in 1987 capital income was taxed as before while labor income was tax free. Although this does not provide a pure placebo test, estimating the effect on capital income allows for investigating potential misreporting and tax avoidance. The reporting behavior would manifest itself as a negative effect on capital income, as taxpayers report more of their capital income as labor earnings in the tax-free year. A negative effect on capital income would therefore indicate that at least part of the estimated earnings elasticity is masking reporting behavior. However, we might not expect no effect on capital income. Because a large part of capital income, such as business income and dividends, is an implicit function of labor supply in the economy, there may be equilibrium effects on capital income resulting from an increased labor supply. Appendix Table A.4 provides an estimate of a small positive effect on capital income, in being only

[20] The estimated semi-elasticity of self-employment implies that a 10% increase in the after-tax wage increases self-employment by 1 percentage point, relative to an average of 14.9%.

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

2% of the treatment effect on labor income. This contradicts the hypothesis of misreporting.

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

Robustness. In addition to the analyses described above, I perform further analyses to assess the robustness of the results. First, I evaluate the robustness of the strategy of assigning treatment status based on last year's tax bracket. While tax brackets correspond to the same income quantiles over time (Figure 3b) and individual tax bracket positions tend to be persistent (Appendix Figure A.5), analysis of pre-reform years finds no evidence of false positives (Figure 4). However, while estimates pertain to short-term responses, where switches between brackets are of less concern, a potential bias might arise due to temporary mean-reverting income shocks. For example, some in a high tax bracket in the previous year are there because of an income shock that reverts to the mean in the current year, generating a downward bias in the earnings elasticity. I evaluate the validity of this concern in Appendix D, performing exercises where I use additional lags in the tax bracket position, as well as a richer set of information, to predict the current tax bracket. This ensures more stable tax bracket positions over time. The results are very similar to the main specification, indicating limited bias from mean-reverting income shocks.

Second, I consider the differences in responses across tax brackets and evaluate the robustness of the choice of control group. In the above analysis, I pool the estimates for the top three tax brackets in a weighted average elasticity. In addition, the DD estimates assume that elasticities are homogeneous across tax brackets. If that assumption is violated, elasticity estimates will be biased. For example, if the elasticity for workers in the top bracket is lower than that for those in a bottom bracket, e.g. due to adjustment frictions, the elasticity estimate will be biased downwards relative to the true underlying elasticity. Appendix Tables A.6 and A.7 present the estimated effects on earnings and weeks worked separately by tax bracket. The results imply that the labor supply elasticities are largest for the lower-middle bracket (0.484) but smallest for the top bracket (0.236).[23] While the relative earnings response (the reduced form) increases with higher tax brackets, it does so less than the difference in the tax cuts (the first stage), resulting in smaller elasticities. One natural explanation for the smaller elasticities in the higher brackets is more frictional adjustment of working time, although this may also reflect

[22] In Figure A.12 I also report that fewer people were receiving sickness benefits in 1987 than in the years before. To the extent that this evidence indicates that workers were working very hard in 1987, it is in accordance with a recent study by Ólafsdóttir et al. (2016), which finds there was an increased likelihood among middle-aged and old men in 1987 and 1988 of having a heart attack, in particular in the self-employed group.

[23] In the Appendix I also present the results from other robustness tests. In one set of exercises, I estimate the elasticities for the top and upper-middle tax brackets, employing the lower-middle bracket as a control group. As documented in Table A.8, this results in elasticity estimates between 0.232 and 0.289, similar to the bracket average in Table A.6. Studying the elasticity of weeks worked, as reported in Table A.9 yields a similar conclusion. I also explored the sensitivity of the main estimates of earnings responses being in natural logarithms. Estimating earnings elasticities using an inverse hyperbolic sine transformation of earnings instead of logs, or $\log(\text{labor earnings} + 1)$, yields broadly similar results.

differences in preferences. To the extent that these results imply less elastic labor supply of workers in higher than lower tax brackets, this indicates that the main estimates may be biased downwards.

Third, I evaluate the impact of the permanent change in taxes in 1988 on labor supply during 1987 and my elasticity estimates. As Figure 3a documents, in 1988 the progressive tax schedule was replaced with a flat tax rate, leading to a reduction in tax rates for those previously in the top and upper-middle tax brackets, while increasing tax rates for those in the lower-middle and bottom brackets. In principle, this may have generated income effects that would confound my Frisch elasticity estimate. In Appendix E I present several tests to evaluate the robustness of my estimates to this concern. As a primary test, I estimate regression (2) controlling for different measures of the permanent change in taxes between 1986 and 1988, including changes in marginal tax rates, average tax rates, and total tax payments. Overall the results are broadly robust to these controls, implying either a statistically indistinguishable elasticity estimates or somewhat smaller estimates than according to the main specification. Other tests, such as restricting the estimation to brackets experiencing limited changes in taxes, yield similar conclusions. As I discuss further in Appendix E, there are two plausible reasons for why the impact of the 1988 reform on responses to the tax-free year may have been limited. First, while a flat tax rate replaced the progressive tax schedule, substantial changes in tax deductions influenced the tax base at the same time. As a result, it was nontrivial ex ante in 1987 to evaluate the effect of the 1988 changes on the effective tax rate. Second, in addition to having been more complicated than the tax-free year to work out, Figure 2 also suggests that these changes also appear to have been much less salient.

5 The Extensive Margin

5.1 Research Design

In this section, I develop a new research design to estimate labor supply responses to the tax-free year. Borrowing the intuition from the seminal work by MaCurdy (1981), I compare people of a certain age and life-cycle labor supply trends to similar workers of the same age before the tax-free year, exploiting the fact that the tax-free year was an unanticipated event.[24] This design complements the design and analysis in Section 4 and has the comparative advantage of being able to identify labor supply responses along the extensive margin—including potential labor market entry—which the tax-bracket design excludes by construction.

Matching procedure. 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.[25]

[24] Appendix F provides detail on the MaCurdy (1981) model as well as the intuition and graphical illustration of the empirical strategy used in this section.

[25] Estimation of labor supply elasticities using grouping of individuals on similar life-cycle trends was pioneered by

A key challenge is to pair workers experiencing a tax-free year to an appropriate comparison group with parallel trends in life-cycle labor supply. To this end, we construct a control group by implementing a "coarsened exact matching" (CEM) procedure (Iacus, King, and Porro, 2012), where each birth cohort is paired with individuals of the same age and lagged characteristics in other birth cohorts. The general argument for applying matching in observational studies is to achieve a balance in covariate distributions across treatment and control groups, with the aim of replicating a randomized experiment as closely as possible (Rosenbaum, 2002; Rubin, 2006). As opposed to methods relying on estimating a propensity score, CEM is a nonparametric procedure to achieve a sample balance ex ante. Therefore, with reference to the design of randomized experiments, the method enables me to construct "blocks" within which individuals may be expected to follow similar trends in labor supply, but where receiving treatment at a given age is plausibly random.

For each birth cohort, I selected the control group from the adjacent birth cohort born one year earlier. That is, workers at a given age in 1987 are matched to workers at the same age in 1986. I make this restriction in order to achieve three goals. First, this limits the set of workers paired to those who are most likely to be comparable in their life-cycle patterns and other aspects. Second, this allows me to restrict the sample period for both the treatment and control groups to 1987 and earlier, thus enabling the exclusion of later years where labor supply may be influenced by the tax-free year itself or subsequent changes in the tax code, thus avoiding the possible effects of the reform on the control group. Third, and most importantly, the control group within each birth-cohort pair does not experience a treatment until after the end of the sample period.[26] Within adjacent cohort pairs, I further match on a set of characteristics other than age that may correlate with trends in the labor supply. These include gender, marital status, number of children, a location dummy for living in the capital area, completed education coarsened into three levels, and lagged labor income coarsened into deciles. Given the general set of characteristics, I have broad support and am able to match 99.98% of the sample.[27]

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.

Estimating equation and identifying assumptions. 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 emphasized and illustrated in the above, the relevant concept of time in this empirical framework is

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.

[26]This setup allows me to circumvent the problems discussed in Borusyak et al. (2021) related to event study designs where the control group eventually becomes treated within the sample period.

[27]Cases where no match is found were dropped and in cases of multiple matches, observations were weighted according to the size of the treatment group. An alternative approach of one-to-one matching delivered similar results. Due to the "curse of dimensionality", the nonparametric matching procedure delivers fewer matches the larger is the set of characteristics matched on. As a robustness check, I also performed matching with additional characteristics, including occupation and sector, arriving at broadly similar results.

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 prior to 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 vector $\boldsymbol{X}_{ik}$ collects characteristics that we may want to control for, but that are not used in the matching process, such as occupation and sector fixed effects. The error term, μ_{ik}, captures other determinants of labor supply. To address potential concerns regarding serial correlation in outcomes within groups across periods (Bertrand, Duflo, and Mullainathan, 2004), I cluster standard errors μ_{ik} at the match-group level. The coefficient η in equation (3) gives the average treatment effect on labor supply for each age cohort or the average across the population. To obtain an estimate of (semi-) elasticity, estimate the following equation:

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

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.

The primary identifying assumption is that, in the absence of a tax-free year, the labor supply of similar individuals in adjacent cohorts would have followed their (common) life-cycle paths. In addition, the research design rests on the assumption that labor supply only deviates from these life-cycle trends in 1987 because of the tax-free year. Appendix Figure A.13 provides a graphical example illustrating the research design and demonstrates how adjacent cohorts follow similar life-cycle trends in labor supply, lending support to the key 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. A potential scenario would be that some sectors or occupations were affected by macroeconomic shocks in the tax-free year, which would influence their labor market outcomes, and that these would be captured by the estimates. I argue that such occupation or sector shocks are more likely to influence the earnings of employed workers rather than the decision to temporarily enter the labor market or delay retirement, which is the focus in this section. I evaluate below the robustness of the results with respect to these concerns.

5.2 Results

Figure 5 reports employment semi-elasticity estimates by cohort. As explained above, the design builds on pairwise cohort-by-cohort differences and therefore naturally produces separate elasticity estimates by cohort. Therefore, I start with presenting estimates separately for each cohort by age in 1987. 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. [28] The figure highlights a clear pattern: through prime age, I precisely estimate the employment elasticity at zero, while it is positive and statistically significant for the youngest cohorts and for those cohorts around retirement age.

Table 2 summarizes employment semi-elasticities for the population as well as three age groups. For the population, I estimate a modest average employment semi-elasticity of 0.06–0.07, implying that a 10% increase in take-home pay increases the employment rate by about 0.7 percentage points. Dividing this estimate by the pre-reform employment rate yields an extensive margin elasticity estimate of 0.10 (0.069/0.672). This elasticity is driven by youngest cohorts—of age 18 to 24—who have a semi-elasticity of 0.4, and for the oldest cohorts—of age 60 to 68—who have a semi-elasticity of 0.12.[29]

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, drive all employment responses, in addition to workers close to retirement. This heterogeneity in responses may have important consequences. If employment responses to short-run shocks primarily originate from individuals in the early stages of their life cycle shifting their time from schooling to work, then short-run shocks may have long-run effects on the economy through their effects on human capital and productivity. Furthermore, such shifts in activity may be permanent while the incentives are only temporary. In Sigurdsson (2021b), 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.

Robustness. I have conducted further analyses along several dimensions in order to evaluate the robustness of the results reported above. Although the life-cycle design allows for identifying labor

[28] I calculate individuals' average tax rate as the ratio of income tax payments to the income tax base, i.e. total taxable income net of deductions. Employment semi-elasticity estimates relate the employment rate to the net-of-average tax rate.

[29] Appendix G presents estimates of aggregate earnings responses using the life-cycle research design, as well as further analysis and tests of robustness of results.

supply elasticities from differences across individuals likely to be on common life-cycle trends, we 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.10 erform an analysis on a sample excluding all workers and firms in these sectors, first excluding the fishing and fish-processing sector and, second, tradable sectors. In both cases the magnitude of estimates are similar, if anything stronger, than implied by my main estimates.

Another concern the reader may have is whether the estimates are picking up some differential trend or shock in labor supply (or demand) around the timing of the tax reform, in the economy as a whole or for particular cohorts. Naturally, we cannot rule out or directly test this concern. However, as a way of evaluating its plausibility, we conduct placebo tests for the years leading up to the reform as "placebo tax-free years". Reassuringly, Appendix Figure A.14 documents that placebo-year coefficients are scattered around zero and are rarely statistically significant, indicating no systematic patterns or false positives.

6 Adjustment Frictions Shape Labor Supply Responses

The canonical model of labor supply assumes that workers hold a single job in which they can flexibly choose their hours of work, or, equivalently, that workers freely choose between employers offering different hours and wage packages. As a result, workers choose to work the number of hours that maximizes their utility at the given wage. As hours can vary freely, 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). This implies that for many questions for which the size of the Frisch elasticity plays an important role, knowing how labor supply responses are influenced by individuals' characteristics and constraints and how those factors shape the margins of response becomes fundamental. However, due to the lack of large-scale natural experiments and detailed microdata, previous work has been unable to examine the heterogeneity of such frictions.

In what follows, I document how adjustment frictions influence the heterogeneity of labor supply responses. In turn, I examine how temporal flexibility in workers' current employment arrangement influences their responses, how workers are able to overcome frictions through new and secondary jobs, and how coordination and ties within the family affects labor supply adjustment.[30]

6.1 Temporal Flexibility and Hours Constraints

Jobs appear to vary greatly in the temporal flexibility they offer. Some occupations, such as taxi and ride-hailing drivers, can flexibly choose to work another hour or another day (Hall and Krueger, 2018). For other occupations, such as pharmacists, such 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:

$$CV(W_{ot}) = \frac{\sigma_{ot}}{\mu_{ot}}, \quad \sigma_{ot} = \left[\frac{1}{N_{ot}-1}\sum_{i=1}^{N_{ot}}(W_{iot}-\mu_{ot})^2\right]^{\frac{1}{2}}, \quad \mu_{ot} = \frac{1}{N_{ot}}\sum_{i=1}^{N_{ot}} W_{iot} \tag{5}$$

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.[31]

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 both 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

[30] The focus and the margins analyzed in this section are motivated by the analysis in Appendix I, which uses a random forest algorithm (Breiman, 2001) to highlight the most important features shaping differences in labor supply responses.

[31] Appendix Figure A.16 plots the distribution of $CV(W_{ot})$ in the sample.

paid overtime in an average month. Unfortunately, these data do not cover all sectors and occupations and only extend back to 1998. As a result, I cannot directly merge them with the main data set at the level of individuals or firms. Therefore, I measure the average share of workers by occupation paid by the hour or that has a fixed salary but paid for overtime. I assign this measure of the flexibility of remuneration structure to the workers in the main data set based on their occupation. Occupations with the least flexibility according to this measure are professionals (e.g. engineers) and managers in the construction sector while those with the most flexibility are cleaners and elementary workers in construction.

Figure 6 plots the occupation-level earnings elasticity against these two measures of the flexibility of the occupation. Elasticities are obtained using the tax bracket DD method by estimating regression equation (2) interacted with an indicator of pre-reform occupation of employment and using weights obtained from coarsened exact matching on pre-reform characteristics. Both Figures 6a and 6b depicts a positive and statistically significant correlation, implying that workers in occupations with more flexibility have larger elasticities than those in less flexible jobs.

As an alternative to these aforementioned measures, which are both measured at the occupation level, and given that the latter cannot be computed for all workers in the sample, Appendix Table A.11 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 high or about 0.75. The results document that the earnings elasticity is about 65 percent larger for those that are not hours constrained in their primary job according to this measure. Similarly, the hours' elasticity is three times larger for those that are not hours constrained.

6.2 Overcoming Hours Constraints: Secondary Jobs and Job Changes

The previous section documented important heterogeneities in labor supply responses by temporal flexibility and flexibility in the remuneration structure. Interestingly, however, I find significant responses even for those workers most likely to be constrained. How are they able to overcome these frictions?

Secondary jobs and primary-job changes. 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).

I exploit an unusual detail of the data, where I separately observe all jobs that workers hold, to consider multiple job holding and primary job changes as possible margins of adjustment. As the data include unique firm identifiers, I can track each job over time and define a change in the primary job either as an event where the worker leaves the primary job—defined as that where the worker earns the highest income—to take up another job or if a previous secondary job becomes the primary job. I then define secondary job holding as working at least one week in a job other than the primary job. Figure 7 provides estimates of the effect of the tax-free year on holding a secondary job, estimated using regression equation (2). The figure reports a semi-elasticity of 0.052. When compared with the average pre-reform propensity, this estimate translates to the elasticity of secondary job holding of 0.18.

Figure 7 presents estimates of secondary job holding separately according to whether workers are likely to have been constrained in their primary job prior to the tax reform. I do this using two measures. First, I use my measure of temporal flexibility and split the sample according to whether a worker holds an inflexible primary job or not, defined as occupations with below-median temporal flexibility. Second, I classify workers as being constrained in their primary job if they were working full-time for 52 weeks in the primary job in the previous year.[32] The figure shows that workers who are constrained and have low flexibility in their primary job entirely drive the effect on secondary job holding. Relating this to the findings in Figure 6, the responses of those in jobs with limited flexibility and those facing hours constraints in their primary job are to some degree channeled through secondary jobs.

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. Her findings imply that the elasticity of secondary job holding is in the range of 0.35–1.48 in the short run, but even larger in the long run, and finds that workers take up secondary jobs to overcome hours constraints in their primary jobs. Other recent studies have found that workers take up secondary jobs, such as ride-hailing for Uber, because of the flexibility they provide (Hall and Krueger, 2018) and to mitigate frictions and volatility in income in their primary jobs (Farrell and Greig, 2016; Koustas, 2018). Angrist, Caldwell, and Hall (2020) estimate a large labor supply elasticity among Uber drivers, indicating that labor supply may be very elastic in secondary jobs.

Appendix Figure A.17 presents the effect on primary job change, reporting a negative effect. This result is intuitive. As the tax-free year only generated a temporary incentive, most workers were unlikely to make costly decisions such as changing their primary job. Moreover, if searching for and settling into new jobs is costly in terms of forgone working time, workers are likely to temporarily postpone otherwise planned job changes.

Decomposition. In order to evaluate the aggregate implications of secondary jobs and job changes as margins of adjustment, I evaluate how much weight secondary jobs and job changes carry in explaining the overall labor supply response. To respond to this question, I decompose the total labor supply effect into the contributions from continuing primary jobs, new primary jobs, and secondary jobs. Total labor supply, E_T, measured either at the level of real labor earnings or weeks worked, can

[32] I have estimated these responses by separating out a primary job with an ability to work paid overtime, which I can measure for a subset of the sample. This yields similar results as reported in Figure 7.

be written in terms of its subcomponents as

$$E_T = E_p + E_s \tag{6}$$
$$E_T = E_p^{\text{Cont}} + \gamma \cdot (E_p^{\text{New}} - E_p^{\text{Cont}}) + E_s$$

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

$$dE_T = \underbrace{dE_p^{Cont}}_{\text{Continuing primary job}} + \underbrace{\gamma \cdot (dE_p^{\text{New}} - dE_p^{\text{Cont}}) + d\gamma \cdot (E_p^{\text{New}} - E_p^{\text{Cont}})}_{\text{Primary job change}} + \underbrace{dE_s}_{\text{Secondary jobs}} \tag{7}$$

where each component can be estimated using the DD framework in equation (2).

Figure 7b reports the results from the decomposition based on equation (7). First, recall that in Section 4 I decomposed the intensive margin response into 30% brought about by earnings increases during additional weeks worked— reflecting less vacation time, more full-time employment, and secondary jobs—while the remainder arose from more earnings within those weeks. The decomposition based on equation (7) shows that 7% of the total earnings effect stems from work on secondary jobs with the remaining 93% being accounted for by increased earnings on continuing primary jobs. Relating this to the decomposition in Section 4 implies that 23% of the total earnings effect results from less vacation time and more full-time employment, with the remaining 70% arising from overtime hours and greater work effort. Of the additional weeks worked, 34% of the responses are created by more time on secondary jobs while the remainder arises from increased working time in continuing primary jobs (less vacation time, full-time employment, etc.).

The decomposition reveals that primary job changes account for only 0.2% of the effect on labor earnings and contribute negatively to the change in weeks worked by about 1%. This is consistent with a search cost in terms of foregone working time. As highlighted by equation (7), the contribution from job changes is a result of two opposing forces. First, as documented above, I find a decreased propensity of job change during the tax-free year. Second, those workers who do change jobs, however, increase their labor supply, possibly because they are able to overcome constraints in hours in the previous job. As the decomposition highlights, these two effects almost exactly cancel each other.

6.3 Collective Labor Supply and Family Frictions

Changes in take-home pay, whether experienced by one or more members of a family, are likely to result in coordinated family responses. Interdependencies in spousal labor supply may run through at least three channels. First, as we expect that couples enjoy spending time together, they will coordinate their working time. That is, there is a complementarity in their leisure-time allocation, implying that a change in the working time of one spouse will induce a same-sign response of the other. Second, in the spirit of Becker (1965), husbands and wives may engage in shared home production, such as childcare. As a result, if spouses are substitutes in home production, the increased labor supply of one spouse will reduce the hours worked by the other.[33] Third, in addition to these indirect effects, there may be a direct income effect if the spouse's earnings are pooled and used for shared consumption within the household.

Based on these channels, Appendix H develops a stylized model of collective labor supply, arriving at two predictions. First, within couples engaging in home production, an individual's own-wage labor supply elasticity is larger the more specialized the spouse is in home production and the more important that spouse's labor input is in the process. This is because the time allocated to home production is a closer substitute for market work than leisure. Second, labor supply elasticity to the spouse's wages—the cross-elasticity—is larger the more time the individual spends on home production, but is falling in spousal input elasticity. In other words, in households where both spouses take part in home production but where wives play the leading role, their own-wage elasticity will be larger than that of their husbands. As the presence, age, and the number of children likely influence chores, with childcare being a primary example of home production, mothers are likely to have larger elasticities than are women with fewer or no children. However, the cross-elasticity may be larger (more negative) for married men than for married women if they require relatively more time input to substitute for their wives' time. The flexibility spouses have in adjusting their hours affects all of these mechanisms. For example, if husbands are more constrained in adjusting their hours, it may be optimal for the household to adjust its labor supply such that husbands provide more home production hours while wives supply more hours to the market. In what follows, I consider how these mechanisms affected labor supply responses to the tax-free year.

Labor supply elasticities by family status. Figure 8a plots the intensive margin elasticity by gender and marital status. As shown, women are more intertemporally elastic. Interestingly, however, this is entirely driven by large elasticities for married women while there is no statistical gender difference between singles. Figure 8b plots the elasticities by the number of children, separately for men and women. While mothers have larger elasticities than women without children, the same is not true for fathers. Both the fact that married women have larger elasticities than their husbands and that elasticities are larger for mothers of more children is consistent with a model of collective labor supply with home production where wives contribute a larger share of their time to home production than their husbands do.

An extensive literature has studied gender differences in labor supply and frequently finds larger elasticities for women than for men (McClelland and Mok, 2012; Blundell and MaCurdy, 1999). My results suggest that gender differences in the Frisch elasticities are not inherent to gender per se, as displayed by equal elasticities for single men and women, but rather to the presence of children and specialization within the household. As the time allocated to home production is a closer substitute for market work than pure leisure, spouses spending relatively more time on home duties (traditionally women), will respond relatively more strongly to wage changes.

[33] Several studies have argued that home production influences the labor supply over the life cycle (Rupert, Rogerson, and Wright, 1995, 2000) and the business cycle (Benhabib, Rogerson, and Wright, 1991), implying that it may be an important factor in explaining the macro–micro discrepancy in the size of the Frisch elasticity. However, empirical evidence on spousal interdependencies in intertemporal labor supply remains scarce.

Cross-elasticities of couples and spousal constraints. How do spouses coordinate their labor supply responses? In order to answer this question, I examine how married men and women respond to changes in their spouses' marginal tax rate and labor supply. As the collection of income taxes in Iceland is at the individual level, an individual's marginal tax rate depends only on that individual's own earned income, not that of the individual's spouse. This implies that the tax-free year generated different changes in the tax rates of husbands and wives. Given these differences vary across households, such cross-elasticities can be identified using a modification of the regression equation (2).[34]

Figure 8c plots the cross-elasticities for married men and women, separately for parents of young children (0–6-year-olds). For men, the cross-elasticity is negative and much more so for fathers than for childless husbands.[35] This is in line with the predictions of the model of collective labor supply with home production. In clear contrast, women's cross-elasticities are not statistically different from zero. These results show that while men work less in response to their wives' incentive to work more, the reverse is not true.[36]

Strong responses of married women and negative cross-elasticities of men indicate that coordinated responses arise from substitution in tasks and chores within the household. Married women with children respond strongly to a temporary tax cut. As nonworking time, at least partly, is spent on home production, increased market work must be met either by increased market-produced consumption or through increased input from the spouse. Large responses of mothers may result from adjustment frictions that make it more difficult for primary earners to adjust their hours relative to secondary earners. Figure 8d evaluates this mechanism by presenting the elasticities separately for men and women by the spouse's job flexibility. Individuals— women in particular—whose spouses are constrained in their primary job respond more strongly than those whose spouses are not constrained to the same degree.[37]

What do the results in Figure 8 imply about overall household responses? To gauge this, I compare total household responses, both including and excluding, the cross-responses and spousal income effects. More precisely, I first estimate the responses in levels of income for both spouses separately and estimate the increase in total household income accounting for both own responses and

[34]See notes to Figure 8 for the regression specification and details.

[35]Cross-elasticities are identified under the exclusion restriction that the spouse's tax rate only affects an individual's labor supply via their spouse's labor supply. The estimates may, however, be influenced by income effects. I assess this in Appendix Table A.12 by including the spouse's income as an additional regressor. I estimate a negative coefficient for men, indicating a small income effect from spousal labor supply, but a positive for women. The estimated size of the cross-elasticities is robust to this inclusion.

[36]The unitary model of household labor supply, which models spouses as a single decision-making unit, makes strong predictions about cross-elasticities (Becker, 1973). More precisely, it predicts that the Slutsky matrix should be symmetric: the cross-elasticities for husbands and wives should be equal (Chiappori and Mazzocco, 2017), but this is rejected in my setting. More generally, whether the spousal labor supply is a complement or a substitute remains an open question. Studies on the "added worker" effect have found evidence of substitutability in spousal labor supply in response to job loss (Lundberg, 1985; Cullen and Gruber, 2000; Stephens, 2002) and non-receipt of disability benefits (Autor et al., 2019). Other studies have found evidence of complementarity in retirement decisions (Blau, 1998; Gustman and Steinmeier, 2000) and in responses to permanent tax reforms (Gelber, 2014; Goux et al., 2014). Recent structural work, e.g. (Blundell et al., 2018), highlights how the presence of children shapes the own- and cross-elasticities of spouses.

[37]In line with this, Appendix Table A.12 reports that the cross-elasticities are negative and large for men who are constrained in their primary job, but not statistically significant for others.

effects from their spouse's responses. Comparing this increase in total household earnings to those assuming no cross-responses or income effects implies that actual household responses are 23% smaller than if spouses had been treated in isolation.

7 Relation to Past Work

To obtain a reference point for evaluating the magnitude of the Frisch elasticity estimates, I conduct a meta-analysis of previous estimates in Figure 9.[38] I then highlight the most likely reasons behind the differences between my estimates and those of the closest studies before providing an alternative evaluation of the size of the estimates building on economic theory and existing parameter estimates.

7.1 Intensive Margin Frisch Elasticity

Figure 9a summarizes past estimates of intensive margin Frisch elasticities. The figure is organized in three sections by the samples studied, from left to right: the population (as either a whole or a representative sample), prime-aged men, and specific occupational groups. For reference, I also plot (circled in orange) my estimates for the corresponding sample.

Close studies. My analysis lies closest to two earlier studies. Using a random sample of 9,300 individuals, Bianchi, Gudmundsson, and Zoega (2001) study labor supply during the Icelandic tax-free year and compare it to the year before and the year after. Their paper is an important contribution highlighting the Icelandic tax-free year as a unique natural experiment, which to date remains one of few informative data points on intertemporal labor supply (Chetty et al., 2013). However, owing to limited data availability, their estimates are based on *average* tax rates in 1986 while the relevant measure for estimating the intensive margin elasticity is the *marginal* tax rate. To enhance comparability of their estimates to this and other studies in the literature, I compute the average marginal tax rates for the groups they study and transform their estimated earnings responses into an intensive margin elasticity of 0.77.[39] Of most note, this estimate is more than twice as large as that I report in Section 4. This contrast underlines the importance of separating responses to the tax-free year from the influences of pre-trends, the business cycle, and subsequent changes to the tax system. I take this concern seriously by using a methodology that combines difference-in-differences and the matching of comparable treatment and control groups and then differences out common trends—such as the business cycle—and any unobserved differences.

[38]See Appendix Table A.29 for details of the studies included in Figure 9. The figure attempts to provide an informative comparison rather than an exhaustive survey. Earlier surveys include Blundell and MaCurdy (1999), Keane (2011), Chetty (2012) and Chetty et al. (2013).

[39]Specifically, I use the estimates in Table 6 in Bianchi, Gudmundsson, and Zoega (2001), which reports the percentage change in earnings for men and women in 1987 relative to the average in 1986 and 1988. To calculate an elasticity, I divide these estimates by the change in net-of-marginal-tax rates (averages by group) for the same years, using my microdata and tax calculator. I then construct a weighted average across men and women and interpret this as the intensive margin elasticity, as their figures in Table 6 are for individuals working in 1986. The standard errors reported in Figure 9a are computed from the standard errors reported in Bianchi, Gudmundsson, and Zoega (2001) using the Delta method. This is the same procedure as used in Chetty et al. (2013), whereas their calculations are based on the average across the tax bracket schedule, assuming an equal share of taxpayers in each bracket. This explains why Chetty et al. (2013) calculate a much lower intensive margin elasticity than I, as my calculations use the weighted average of marginal tax rates.

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

In more recent work, Stefánsson (2019) revisits and extends the analysis in Bianchi, Gudmundsson, and Zoega (2001) using population-level income data. Using difference-in-differences across income groups at the upper end of the income distribution, Stefánsson (2019) estimates an earnings elasticity of about 0.07. However, the comparison groups there differ from those in my main analysis, which pools estimates across the lower-middle up to the top tax brackets, but when comparable, the earnings elasticity estimates are more similar (see e.g. Appendix Table A.8).

Other earlier work. Most of the existing evidence on Frisch elasticity, including the seminal studies by MaCurdy (1981) and Altonji (1986), draw on regressions of the working hours on wages of prime-age men. As Figure 9a illustrates, the elasticities in these studies are very imprecisely estimated and often statistically insignificant. This may be due to several reasons. First, the instrumental variable approach used in much of this literature is based on individual characteristics, traditionally age and education, as predictors of changes in wages. While this literature brought the insight that these factors can be good predictors of the level of wages, later work has found them to perform poorly in predicting wage changes, leading to weak instruments (Keane, 2011). Second, prior work has emphasized issues concerning the measurement of wages and hours in the Panel Study of Income Dynamics (PSID) used in much of this literature, which may lead to either a positive or a negative bias (Heckman, 1993; French, 2004a).[40]

The empirical challenge of estimating Frisch elasticity and the presence of adjustment frictions has motivated several studies that estimate elasticities for particular occupations, such as bicycle messengers and taxi drivers, for whom exogenous changes in wages are plausible and who are flexible in choosing their daily labor supply. As summarized in Figure 9a, these studies tend to reveal relatively strong labor supply responses.While these studies provide clear causal estimates in an environment with minimum frictions, it is challenging to generalize their findings to the situation where average workers respond to transitory and business cycle variations in wages.

7.2 Extensive Margin Frisch Elasticity

Figure 9b summarizes the estimates of extensive margin Frisch elasticity. Compared with the intensive margin, the existing studies of these are much fewer. My extensive margin elasticity estimate for the population is 0.10, which falls far below the estimate by Carrington (1996), who studied em-

[40] In addition to the quasi-experimental literature surveyed in this section, an extensive literature estimates Frisch elasticity using structural methods. I survey prominent papers in this literature in Appendix Figure A.19.

ployment in Alaska during an oil pipeline boom in the 1970s.[41] The figure underlines the drivers of our elasticity estimate. Similar to prior evidence, I identify an employment response for those at and around retirement age (age 60 years and older). The strongest employment responses, however, are among the youngest cohorts (below age 25 years), for which no comparable estimates exist. For the prime-aged, which is also the population studied in Martinez, Saez, and Siegenthaler (2021), the extensive margin elasticity is zero.

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

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

While several factors may explain the differences, such as the salience of the reforms and the framing of their announcement, I argue, based on recent research, that differences in labor market flexibility are the most plausible explanation.

In Sigurdsson (2021a) I study a tax holiday in Norway that resulted from a tax reform similar to those in Iceland and Switzerland. I document that tax holidays are salient—more than 80 percent of Norwegian adults were aware of it—and a quarter of the working-age population responded by working more hours. For those aware of the tax holiday but did not respond to it, most say that frictions in adjusting working time are the main reason.

The Icelandic labor market is relatively more flexible than the Swiss labor market and others in continental Europe, and indeed closer to that found in the US labor market. I now illustrate how the measures of flexibility correlate with the size of the intensive margin elasticity estimates, both between and within countries. I define labor market flexibility in terms of the speed of adjustment to external shocks or changing macroeconomic conditions (Pissarides, 1997). Flexibility can then be divided into flexibility at the micro-level (reflected by working time flexibility, worker flows between labor market states, and job flows) and at the macro or institutional level (as reflected by labor regulations and wage flexibility). In Appendix J I collect several measures of labor market flexibility for OECD countries. Whether measured as worker flows (being the "fluidity" of the labor market), cyclicality and the importance of hours per worker, or as wage flexibility or the flexibility of institutions, all measures are highly correlated (see Figure A.20). Furthermore, on all metrics, the Icelandic labor market is substantially more flexible than the Swiss labor market and closer to that of the US. For example, the cyclicality of hours per worker, measuring the flexibility of hours, is more than twice as high in Iceland than in Switzerland, while flow rates in and out of unemployment in Iceland are

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

three times higher.[42]

Figure 10 reveals that flexibility of working hours is positively correlated with the intensive margin Frisch elasticity, across both countries and occupations. The figure measures flexibility by the correlation of hours per worker and total hours, i.e. the cyclicality of working hours, which I select for two reasons. First, if workers have the flexibility to adjust their hours and the intensive margin is operative, we would expect hours per worker to move with the business cycle and to explain a significant share of the changes in total hours. Second, I can use this measure of flexibility for both countries and subgroups within countries, such as occupations, facilitating a broader comparison. As there are few comparable elasticity estimates across countries, the figure only plots the estimates for Iceland, Switzerland, and the US. The Frisch elasticity estimate and the hours of flexibility in Iceland and the US are broadly similar, although the variation across US estimates is substantial. Switzerland, however, falls at the other end of the spectrum. In Sigurdsson (2021a) I report an earnings elasticity for Norway. While this captures both intensive and extensive margin responses, the elasticity estimate is 0.14 and falls halfway between the estimates for Iceland and Switzerland, in line with the relative flexibility of the Norwegian labor market. Figure 10 also plots the estimates of the intensive margin elasticity for occupations in Iceland against the occupation-level correlation between the cyclical component of hours per worker and total hours, confirming the same positive correlation as indicated by the cross-country comparison.

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

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

Using economic theory and prior estimates, I can provide a prediction of the Hicksian elasticity implied by my estimate of the Frisch elasticity.[43] A dynamic labor supply model with time-separable utility in consumption and leisure provides the following relationship between the intensive margin Frisch elasticity and other key parameters in the model (Ziliak and Kniesner, 1999; Browning, 2005):

$$\varepsilon_{\text{Frisch}} = \varepsilon_{\text{Hicks}} + \rho \cdot mpe^2 \frac{A}{wh} \tag{8}$$

where ρ is the intertemporal substitution in consumption (EIS), mpe is the marginal propensity to

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

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

earn (MPE) out of unearned income, i.e. the income effect, and $\frac{A}{wh}$ is the ratio of wealth to labor income.[44] Appendix Figure A.21 maps my Frisch elasticity estimate into the Hicksian (and Marshallian) elasticity on the y-axis and IES on the x-axis for given values of the other parameters in equation (8). The most prominent estimates of the MPE are based on estimates of the effect of winning a lottery, e.g. Imbens, Rubin, and Sacerdote (2001) and Cesarini et al. (2017) and receiving an inheritance (Nekoei and Seim, 2021). In our calculations, we use an MPE of 0.11 implied by the estimates in Imbens, Rubin, and Sacerdote (2001).[45] Then, I use data from individual tax returns to calculate a median $\frac{A}{wh}$ ratio of 2.59. A value of the IES then pins down the implied Hicksian in Figure A.21. The figure marks two estimates of IES: first, an average IES of 0.5 across the 169 studies surveyed in Havránek (2015), and second, an average IES of 0.9 across 33 studies published in the top-5 general interest journals. The implied Hicksian elasticity lies between 0.34 and 0.36, close to the Hicksian elasticity of 0.33 which Chetty (2012) reports in a meta-analysis pooling across existing studies. The implied Marshallian elasticity 0.06 is also in line with previous estimates, such as by Kleven and Schultz (2014) who estimate an elasticity of 0.05 by pooling over a series of tax reforms in Denmark.

8 Conclusion

Understanding how labor supply responds to temporary changes in wages has been a longstanding research program in micro and macroeconomics. The size of this response, 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. The results strongly indicate that we cannot simply boil labor supply responses down to a single number and that we cannot interpret average elasticities as estimates of a deep structural parameter. This is because frictions are important in shaping labor supply adjustment and the observed responses are very heterogeneous. In terms of intensive margin responses, I document three findings that illustrate this. First, I find that workers in the most flexible jobs and employment arrangements display the strongest responses. Second, those who face hours constraints in their primary jobs are able to alleviate these by taking up secondary jobs. Third, I find that married women, particularly those with children and with husbands who are constrained in adjusting their hours, respond more strongly than do their husbands, who themselves respond negatively to their wives' tax cuts. This illustrates the influence of frictions in reallocating nonworking time to market work, e.g. because of childcare responsibilities. 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 sup-

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

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

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

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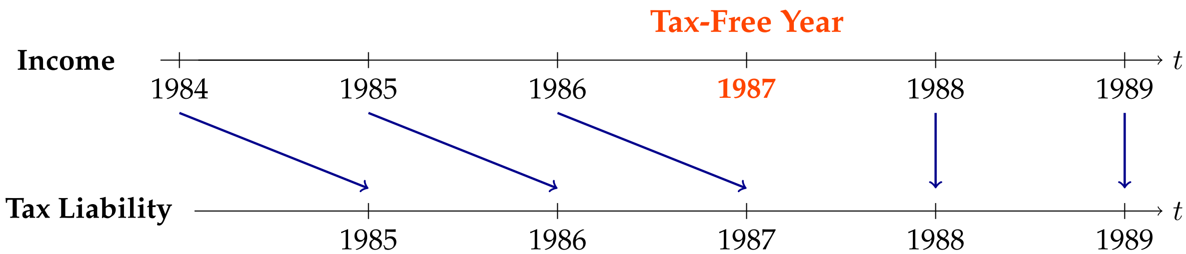


Figure 1: Income tax system before and after the tax reform

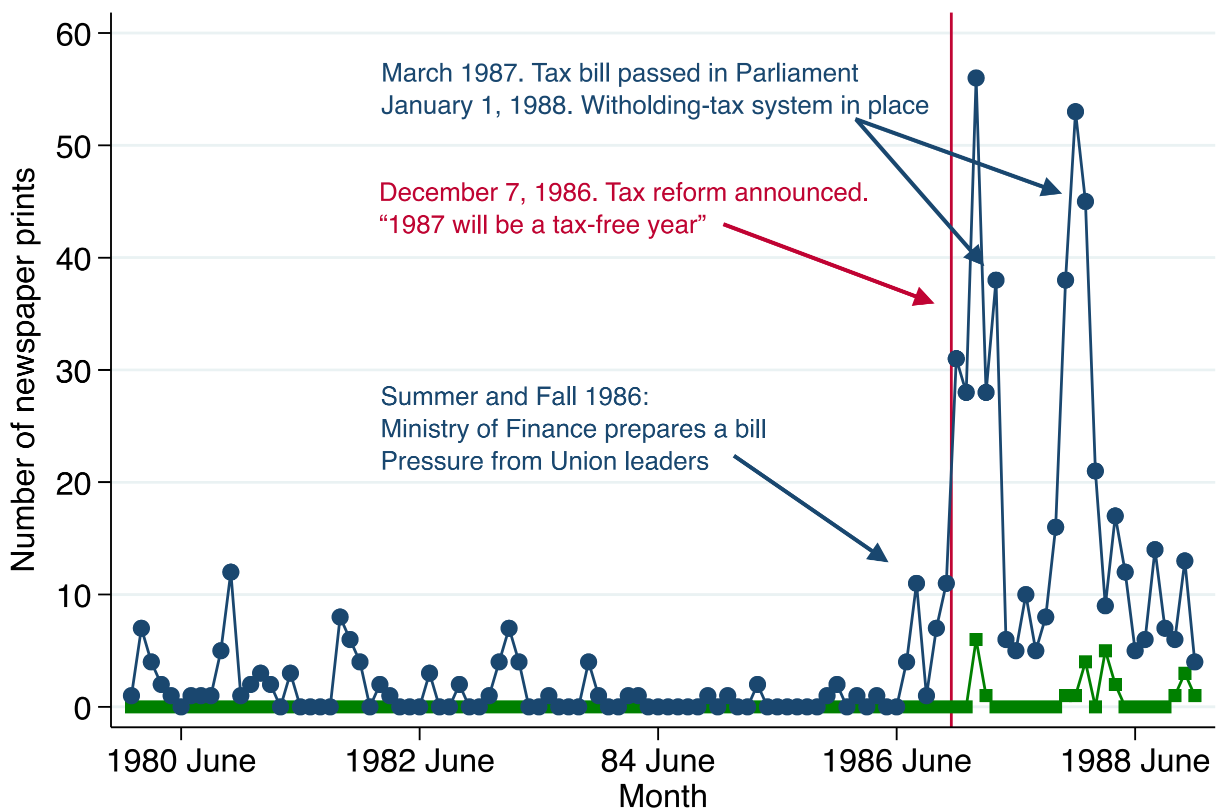
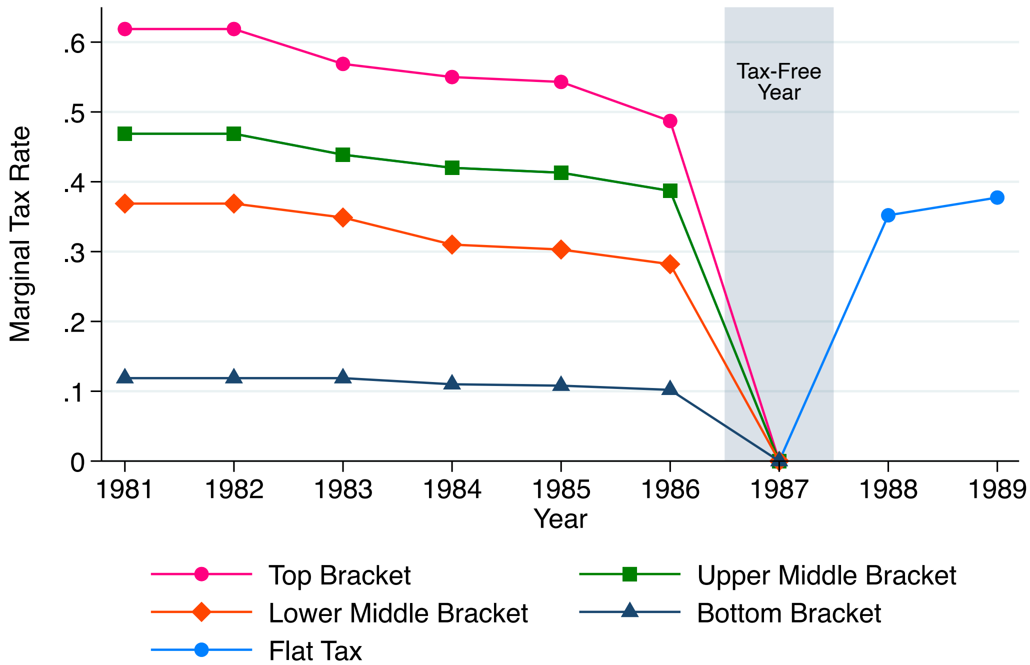
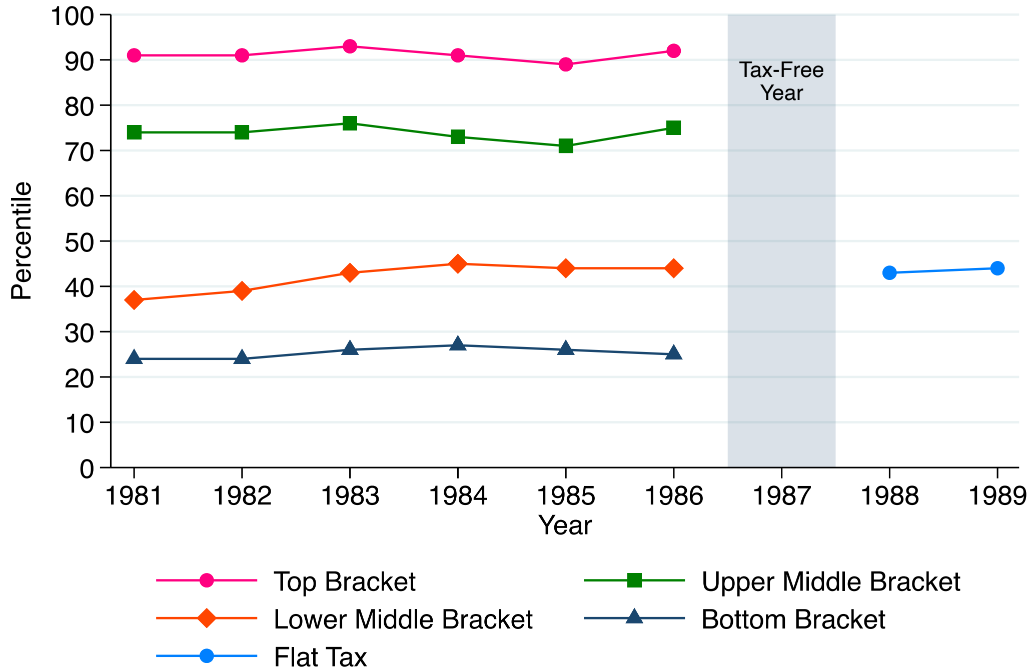


Figure 2: Number of printed newspapers mentioning withholding tax

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



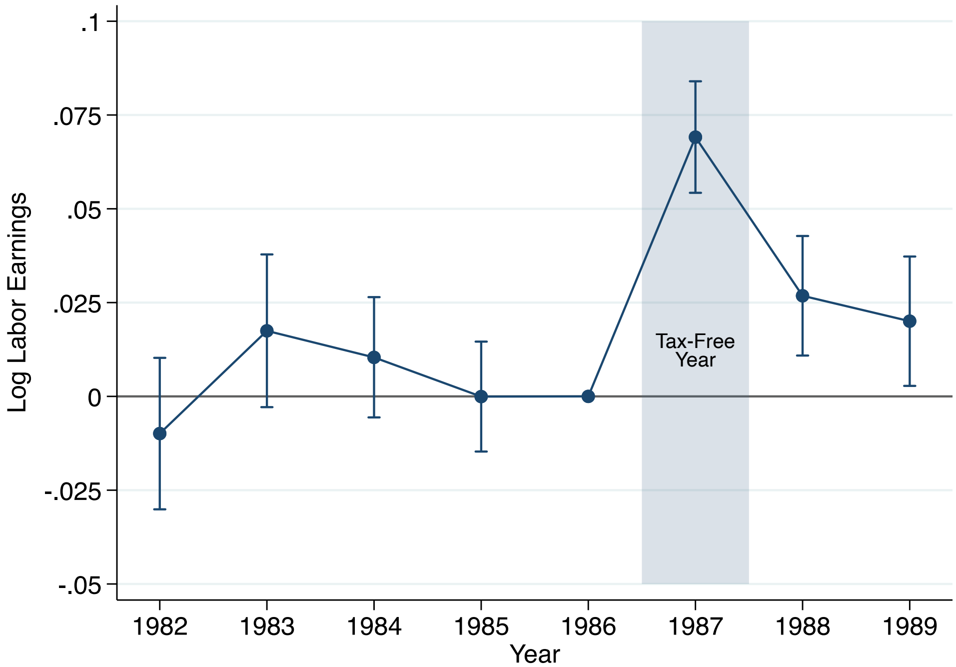
(a) Marginal tax rate by tax bracket



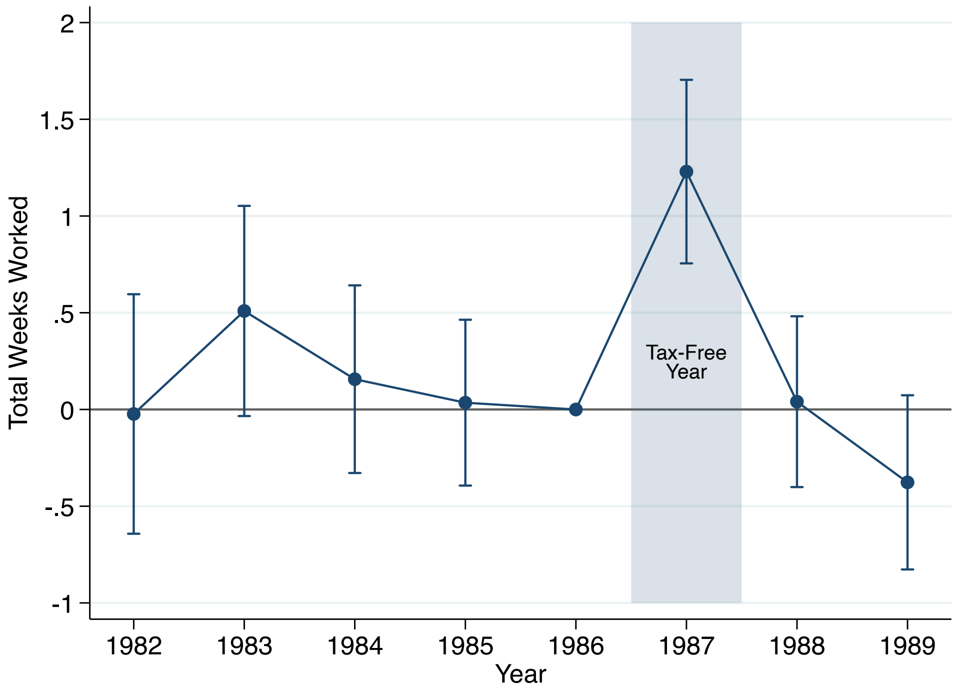
(b) Tax bracket thresholds in percentiles of income

Figure 3: Marginal tax rates and tax-bracket thresholds

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



(a) Reduced form: Labor earnings



(b) Reduced form: Weeks worked

Figure 4: Dynamic difference-in-difference and placebo tests

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

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

where the outcome variable in panel (a) is log labor earnings and in panel (b) total weeks worked. These plot the coefficients η_t, where $B_{i,t-1} \times \delta_{t=1986}$ is normalized to zero. Standard errors are clustered at the individual level and the vertical bars plot the 95% confidence intervals. Appendix Figures A.6, A.7 and A.8 provide a graphical presentation of the reduced-form evidence and the first stage.

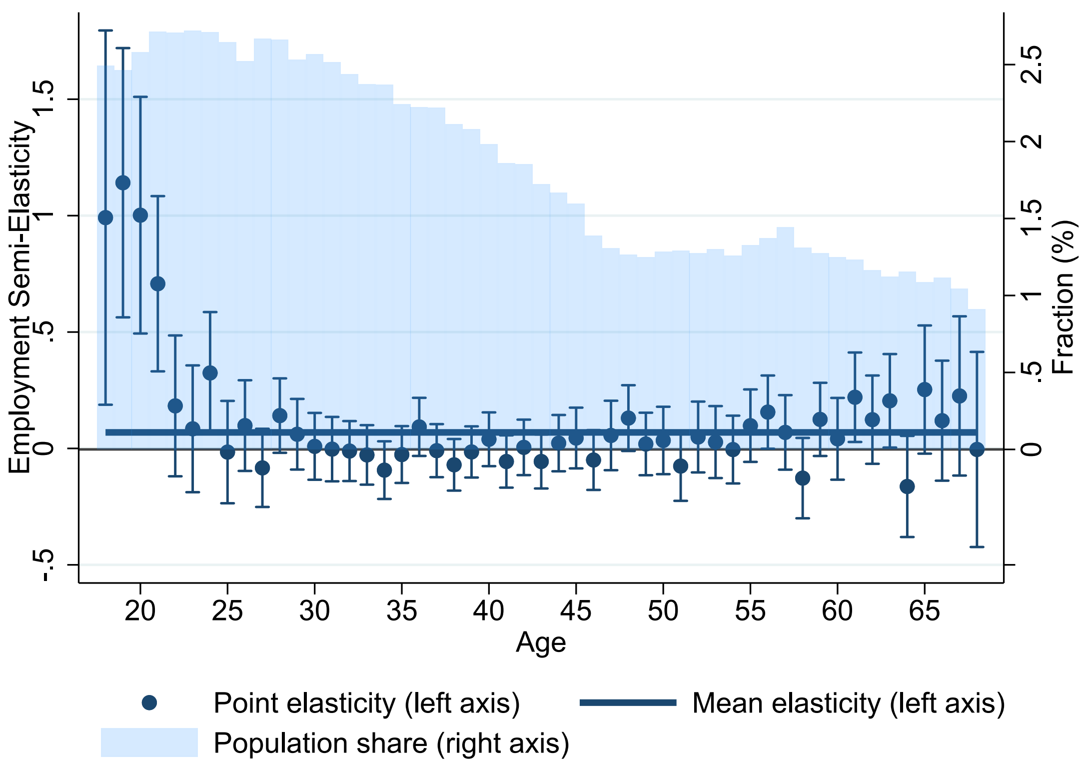
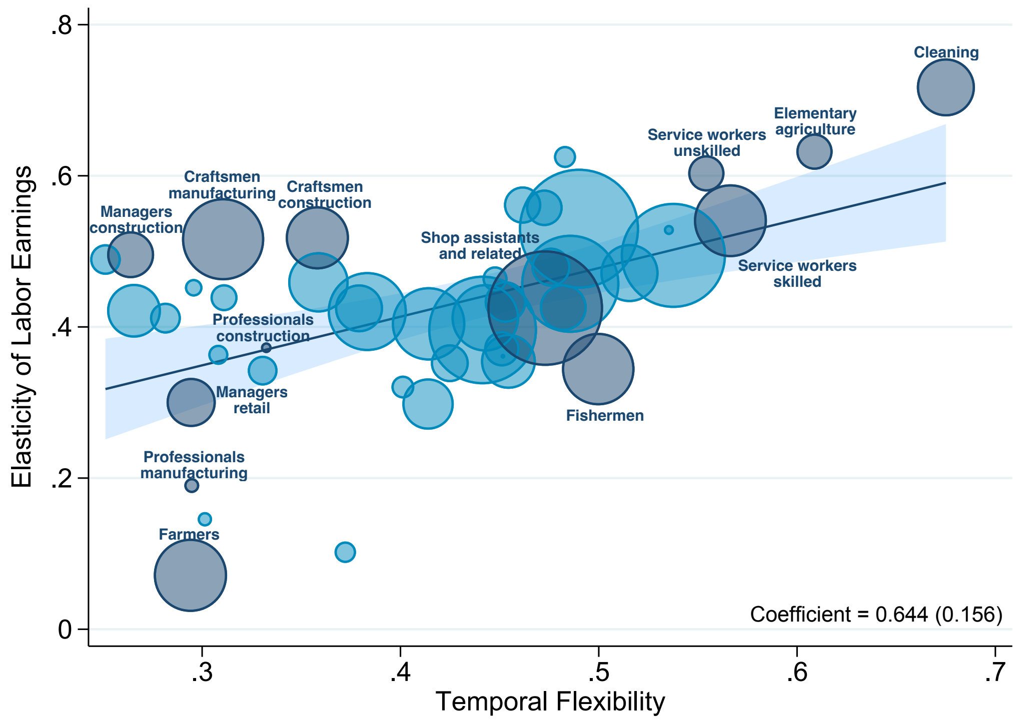
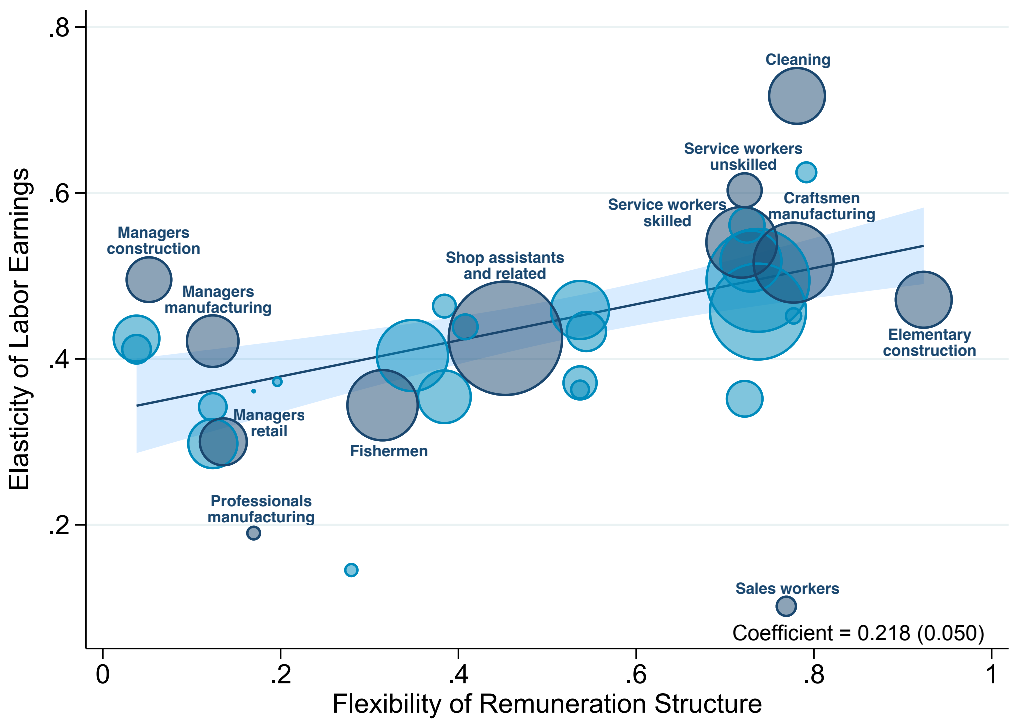


Figure 5: Employment semi-elasticity (extensive margin) by age

Notes: The figure plots the employment semi-elasticity for each cohort estimated in separate regressions according to equation (4), where the dependent variable is an employment indicator. The vertical bars plot the 95% confidence intervals. The horizontal line plots the average elasticity, as reported in Table A.28. The shaded area (bars) is the population distribution, where each bar corresponds to the share of the working age population (in %).



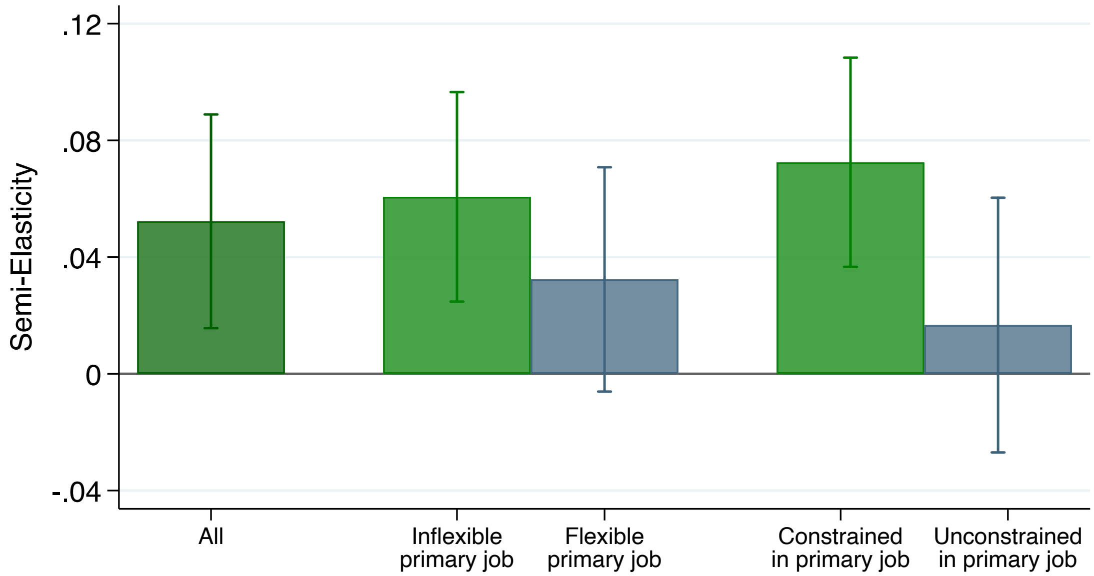
(a) Earnings elasticity by temporal flexibility



(b) Earnings elasticity by flexibility of remuneration structure

Figure 6: Labor supply elasticities by job flexibility

Notes: Each panel plots labor earnings elasticity estimates by group against a measure of adjustment frictions. In panel (a), "*temporal flexibility*" is measured using the coefficient of variation in weeks worked, i.e. the occupation-level dispersion in working time; see main text for details. In panel (b), "*Flexibility of Remuneration Structure*" is the share of workers within an occupation who work and are paid by the marginal hour; see main text for details. Occupation-level elasticities are estimated using the tax-bracket research design, described in Section 4, where I interact the treatment indicator with occupation indicator and control for occupation fixed effects. I match on pre-reform characteristics to generate similar treatment and control groups. The size of the dots in each graphs is proportional to the number of workers in each occupation.

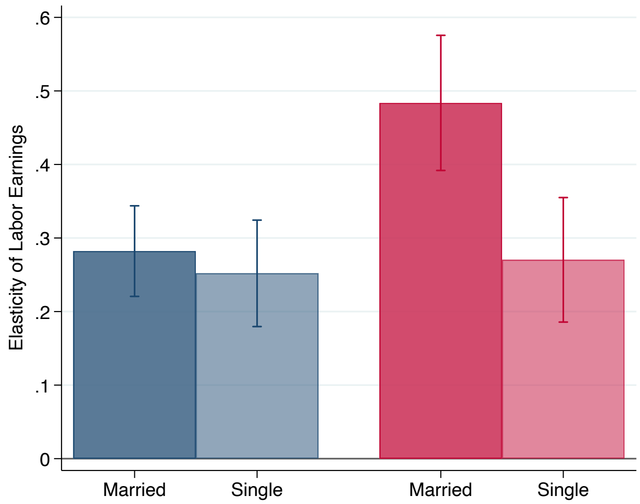


(a) Secondary job holding

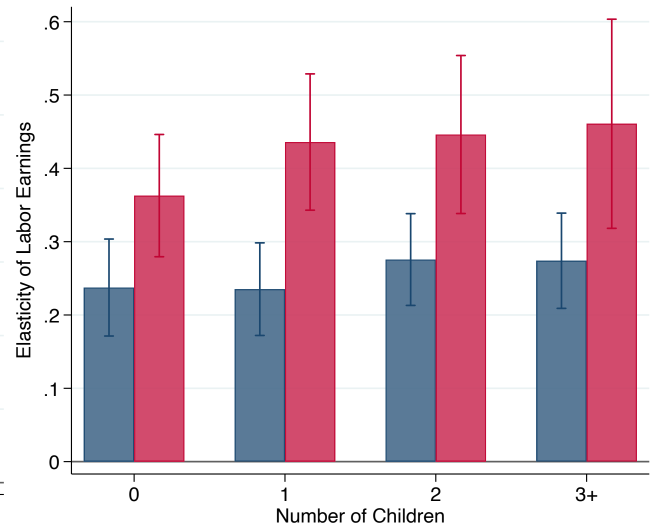
(b) Decomposition of intensive margin response

Figure 7: Take-up of secondary jobs and decomposition of intensive margin responses

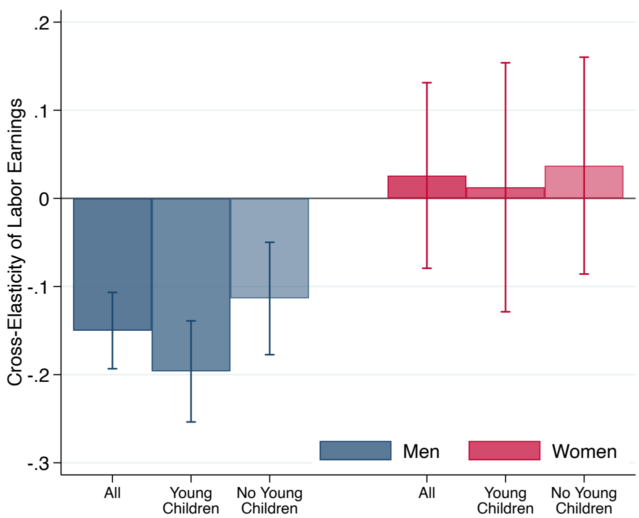
Notes: Panel (a) presents the estimated effect on secondary job holding. The figure presents results from a 2SLS estimation of equation (2), where the dependent variable is an indicator of holding a secondary job, measured by working at least one week on a job other than the primary job within the year. The pre-reform mean of this dependent variable is 0.297. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. The figure shows 95% confidence intervals based on robust standard errors clustered by individual. *"Inflexible primary job"* is an indicator for holding a primary job in an occupation with below-median "temporal flexibility", as measured in Section 6.1, but zero otherwise. *"Constrained in primary job"* is an indicator for working 52 weeks in the primary job in the prior year, but zero otherwise. Estimates by subgroups obtained by interacting group indicators with the log of the net-of-tax rate of the individual and their spouse as well as the respective instrumental variables. Regressions control for gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. The figure shows 95% confidence intervals based on clustered robust standard errors. Panel (b) presents a decomposition of the intensive margin response in labor earnings and weeks worked into subcomponents, as described by equation (7). Calculations are based on estimates of equation (2) in levels of each outcome and the numbers presented are the contribution of each component to the total effect.



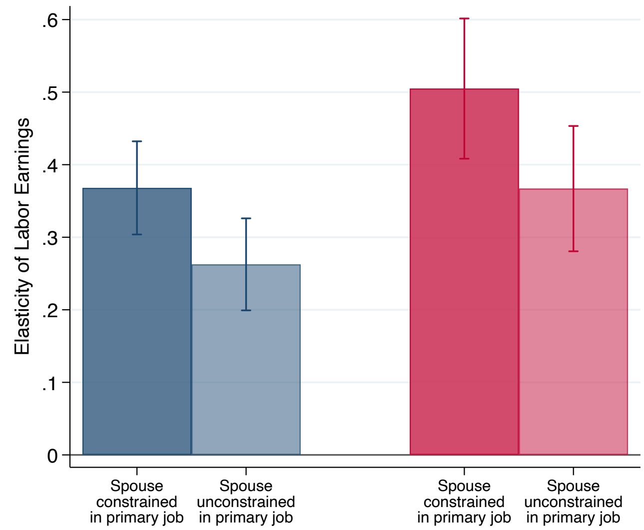
(a) Elasticity by marital status



(b) Elasticity by number of children



(c) Cross-elasticity



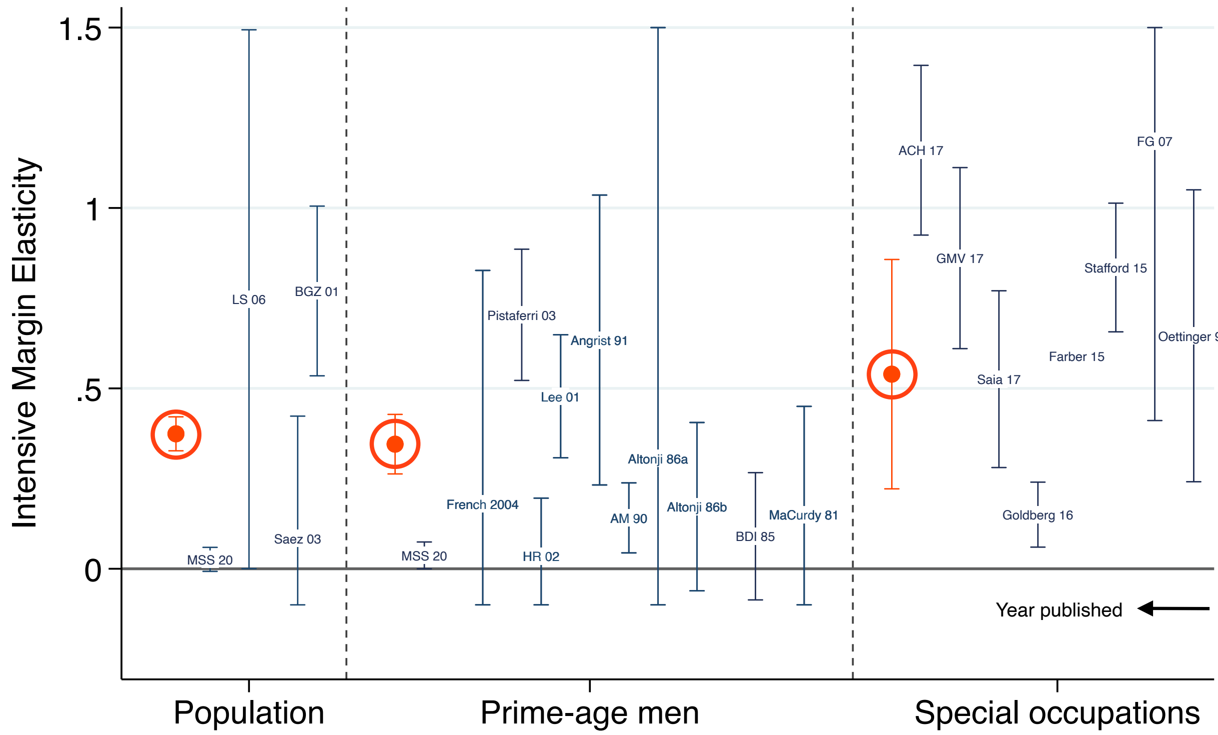
(d) Elasticity by spouse's job flexibility

Figure 8: Elasticity of labor earnings of men and women by family status

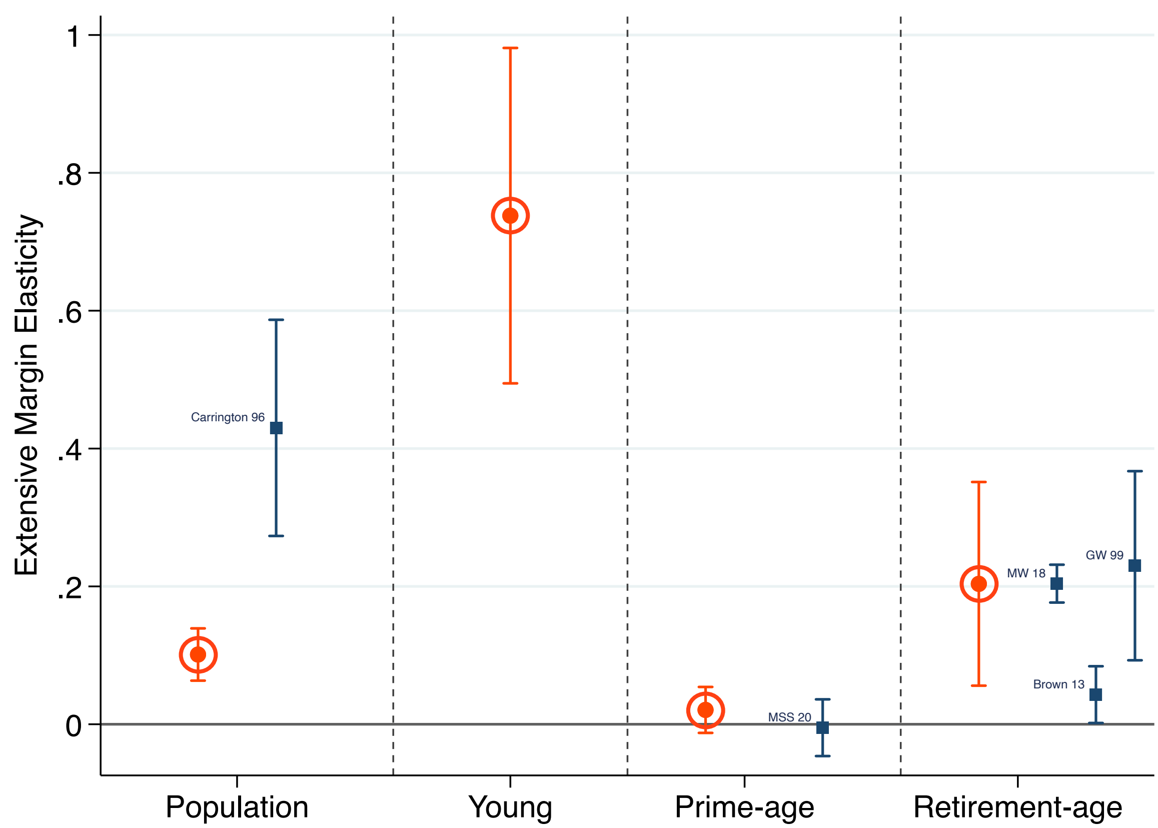
Notes: The figure presents estimates of labor earnings elasticities for men and women by family status. Family status, such as marital status and the number of children, defined as of the previous year. Estimates by subgroups are obtained using regression equation (2) by interacting group indicators with the log of the net-of-tax rate of the individual and his spouse as well as the respective instrumental variables. Panel (a) presents estimates separately for men and women by marital status. "Married" refers to the legal status of being married or registered as cohabiting. Panel (b) presents estimates separately for men and women by number of children. Panel (c) presents cross-elasticities for married men and women, which are estimated using the following modification of equation (2):

$$y_{it} = bracket_{i,t-1} + \delta_t + \varepsilon^{own} \cdot \log(1 - \tau_{it}) + bracket^{spouse}_{i,t-1} + \varepsilon^{cross} \cdot \log(1 - \tau^{spouse}_{it}) + \boldsymbol{X}'_{it}\gamma + \nu_{it}$$

where the dependent variable is the logarithm of the individual's labor earnings, and the two endogenous variables, the individual's log net-of-tax rate and their spouse's log net-of-tax rate, are instrumented with an interaction between indicators of treatment status and tax-free year for the individual and his spouse separately. The coefficient ε^{cross} identifies the cross-elasticity. "Young children" refer to children aged 0–6 years. Appendix Table A.12 presents estimates including spouse's income as an addition regressor to allow for income effects. Panel (d) estimates elasticities for men and women depending on whether their spouse is constrained in their primary job, indicating whether they were working a full 52 weeks in the primary job in the previous year. For robustness, Appendix Figure A.18 splits the sample by whether a worker holds an inflexible primary job or not, defined as occupations with below median temporal flexibility according to equation (5). All regressions control for age, number of children, education, and whether living in the capital area or not. The figure shows 95% confidence intervals based on clustered robust standard errors.



(a) Intensive margin Frisch elasticity



(b) Extensive margin Frisch elasticity

Figure 9: Summary of Frisch elasticity estimates

Notes: The figure provides a summary of the intensive margin (panel a) and extensive margin (panel b) Frisch elasticity estimates. Estimates are organized by subgroup or population studied. My estimates are circled and in orange. For the subgroup of special occupations, my elasticity estimate is for the subsample of taxi and transportation drivers. The point estimates refer to the authors' main, representative or preferred specification. The 95% confidence intervals are either based on reported standard errors or computed using the delta method. For details, see Appendix Table A.29.

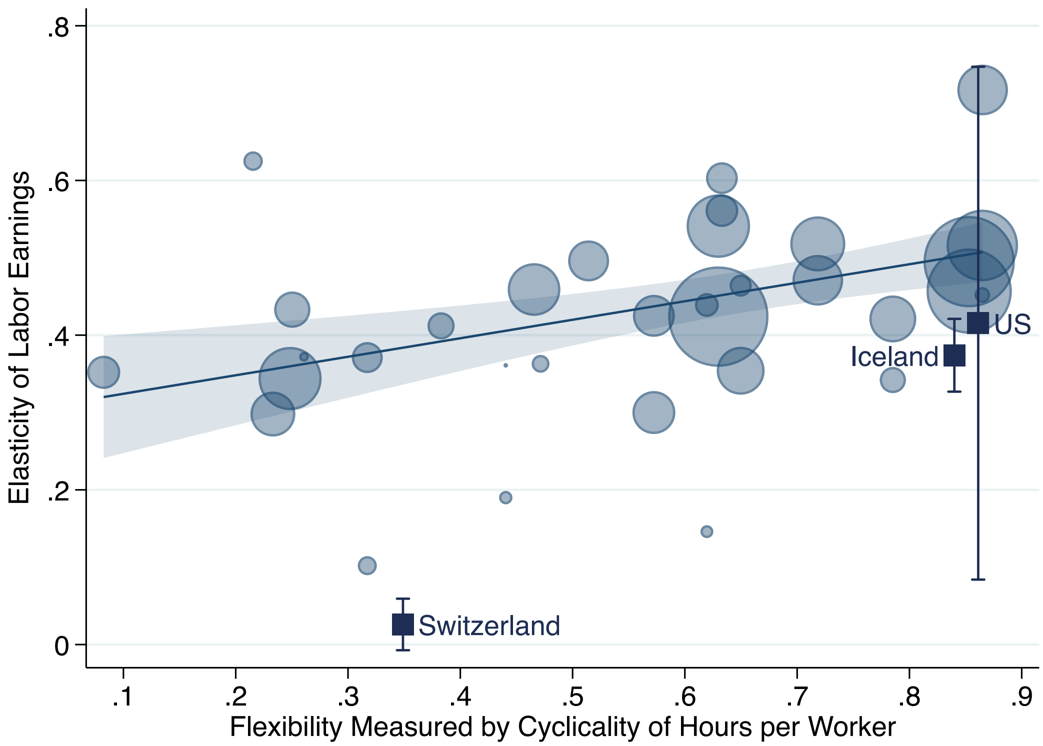


Figure 10: Intensive margin Frisch elasticity across countries and occupation by flexibility in hours

Notes: The figure plots intensive margin Frisch elasticity estimates by country and occupation against the correlation between total hours and hours per worker. The solid squares reflect country-level elasticity estimates, which are from the "Population" panel in Figure 9. For the US the dot 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). Total hours worked, th, are defined (in logarithmic terms) as $th = h + n$, where h is the average number of hours worked per worker, and n is the number of people employed (both divided by the size of the labor force). The time series are detrended using the Hodrick–Prescott (HP) filter so that th, h and n reflect the cyclical components. Measures of cyclicality are from Sigurdsson (2011) and Rogerson and Shimer (2011). the transparent circles are estimates of intensive margin Frisch elasticity by occupation against the occupation-level correlation between total hours and hours per worker. Occupation-level elasticities are estimated using the tax-bracket research design, described in Section 4, where I interact the treatment indicator with occupation indicator and control for occupation fixed effects. I match on pre-reform characteristics to generate similar treatment and control groups. The size of the dots is proportional to the number of workers in each occupation. Occupation-level correlation between total hours and hours per worker are constructed using administrative microdata from the *Icelandic Survey on Wages, Earnings and Labor Costs (ISWEL)* containing information on working hours and employment in the private sector (see e.g. Sigurdsson and Sigurdardottir, 2016, for details). I compute the occupation-level correlations in the in the same way as I do for countries, as described above.

Table 1: Effects of the Tax-Free Year on Intensive Margin Labor Supply

	Log labor earnings		Weeks worked		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.374***	0.401***	4.926***	6.549***	-0.033	0.030
	(0.024)	(0.032)	(0.784)	(1.074)	(0.024)	(0.030)
Reduced form (dy)	0.077***	0.077***	1.023***	1.267***	-0.004	0.004
	(0.005)	(0.006)	(0.162)	(0.207)	(0.003)	(0.002)
First stage ($d\log(1-\tau)$)	0.207***	0.193***	0.207***	0.193***	0.127***	0.119***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean of outcome variable	—	—	48.43	48.43	0.914	0.914
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matching	No	Yes	No	Yes	No	Yes
Observations	526,955	526,458	520,438	519,941	530,900	530,397

Notes: The table presents the results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable (y) is defined in the top panel and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate in columns (1)–(4) and one minus the average tax rate in columns (5)–(6). Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors are clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Effects of the Tax-Free Year on Extensive Margin

	All		Young		Prime age		Old	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.069***	0.058***	0.415***	0.396***	0.015	0.015	0.115***	0.025
	(0.014)	(0.014)	(0.069)	(0.073)	(0.012)	(0.012)	(0.043)	(0.040)
Reduced form (dy)	0.008***	0.006***	0.024***	0.023***	0.002	0.002	0.010***	0.002
	(0.001)	(0.001)	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)
First stage ($d\log(1-\tau)$)	0.110***	0.110***	0.058***	0.059***	0.130***	0.130***	0.087***	0.088***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)
Mean dependent variable	0.672	0.672	0.551	0.551	0.728	0.728	0.566	0.566
Match-strata fixed effects	Yes	No	Yes	No	Yes	No	Yes	No
Individual fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Number of matched observations	576,571	576,571	130,097	130,097	399,493	399,493	62,248	62,248

Notes: The table presents results from life-cycle difference regressions, where each row and column entry corresponds to one regression estimate. "*Young*" are individuals younger than 25 years old in 1987, "*Prime age*" are individuals between 25 and 59 years old, and "*Old*" are individuals 60 years and older. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is employment and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents reduced form estimates based on equation (3). The bottom row presents first-stage regression estimates based on equation (3), where the outcome variable is the logarithm of one minus the average tax rate in columns. "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, the number of children, education, location indicator and percentile of income. The number of matched observations corresponds to observations for the treatment group. Robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Online Appendix of:

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

Jósef Sigurdsson
June 14, 2022

A Overview of the Icelandic Income Tax System

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

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

Income taxes in Iceland are levied at two levels: a national tax and a local municipal tax. As described in Section 2, during 1987, all taxes on labor income at both levels were set to zero. The tax schedule prior to the reform consisted of three national-level brackets and a municipal tax. In addition, there were a few small and lump-sum income taxes, such as the health insurance contribution, cemetery charge, church tax and contribution to the construction fund for the elderly. All taxable income—both labor and capital income—was taxed equally and in the same way at the national and municipal levels.[46] Before arriving at the tax base, multiple deductions could be made. As these deductions differed substantially between the national and municipal levels, the tax base for the two levels was different. The components that were deductible at both levels included fringe benefits; travel allowances; purchases of tools, machines and instruments; mandatory savings; child support; and education-related costs. At the national level there were various other deductions such as a special fisher's deduction, deductions for each day spent at sea, special deductions for the costs of starting a family ("wedding deduction"), interest expenses, pension savings, union membership fees, charitable gifts, etc. Moreover, in exchange for a subset of these options for deduction, the tax law offered taxpayers the option to instead deduct a fixed 10% from the national-level tax base, an option many exploited. While including both labor and capital income as the national-level tax base, pension and social security benefits were not part of the municipal tax base but were included in the national-level tax base. To summarize, the tax base at the municipal level tended to be higher than that at the national level. Because of those features, the progressive income tax schedule consisted of four brackets, consisting of three national-level brackets and a municipal tax. In addition, each worker had a personal tax allowance, both at the municipal and national levels, deducted from the computed tax payments. At the national level, this amount was fixed and was the same for everyone, but the municipal allowance depended on marital status and the number of children. The allowance at both levels was deducted from the outstanding tax liability.

Since 1978, Iceland has had an individual tax system, such that married and cohabiting individuals have been taxed as single units, not jointly. Therefore, each spouse files his/her own tax return, and has a separate tax allowance and deductions. However, the tax system has some joint aspects that were incorporated into the tax system with the aim of lowering the tax burden of two-adult households with a single earner and households with low-income secondary earners. First, married and cohabiting individuals were allowed to transfer to their spouses both their personal tax allowance and tax deductions that remained unaccounted for after their own income taxes had been paid in full.[47] Second, married or cohabiting workers whose spouses were out of the labor force or with a very low income could increase the amount taxed in the first bracket by up to half of what remained after their spouses' income was fully accounted for.

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

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

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

Due to the reform, many of the deductions that were an integral part of the old tax system were abolished. These included a deduction for newly married couples, mandatory pension savings, union membership fees, interest payments on loans and mortgages, various work-related deductions and a 10% fixed deduction. Deductions from the municipal tax were abolished, but the tax rates were lowered such that the municipal tax revenue was almost unaffected. As a result, the tax base at the national and municipal levels became the same after the reform. In addition, other adjustments were made to the tax system, such as replacing the interest payment deduction with an interest allowance and a housing allowance for first housing purchases, paying out child benefits directly instead of being integrated into the tax system, and incorporating minor fees such as fees to the church and cemeteries into the main income tax, all of which simplified the tax system and made it easier to manage for the authorities. In exchange for the deductions in the old system, the personal tax allowance was increased by half and now served as a single source of tax deduction, with the aim of keeping the tax burden the same in the new and simplified system.[48]

B The Tax Reform and the Timeline of Events

On January 1, 1988, Iceland took up a withholding-based pay-as-you-earn income tax system. Prior to the reform, income taxes were collected with a one-year lag. That is, as depicted in Figure 1, the tax liability and tax payments due every month in year t were computed based on income in year $t-1$. This system was similar to that in place in most Western countries prior to adopting the modern pay-as-you-earn tax systems.[49] When the tax reform was announced on December 6, 1986, it was also announced that during the transition year of 1987, labor income would not be taxed. As Figure 1 depicts, this implies that while people were paying taxes every year, including in 1987 when they paid taxes based on their income earned the year before, all income earned in 1987 was tax free. Therefore, the reform did not influence the government's budget, as the tax revenue flows were uninterrupted, and nor did it generate a cash-flow effect on workers.[50] However, as all marginal income earned in 1987 was tax free, the reform generated a strong incentive for intertemporal substitution: work more during the tax-free year and less in other years.

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

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

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

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

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

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

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

On December 6, 1986, new collective agreements were signed and the Finance Minister announced the tax reform, which was the government's input to a settlement. The pay-as-you-earn tax system was scheduled to be implemented on January 1, 1988. The Finance Minister ordered the aforementioned tax-reform committee to prioritize proposing simplifying changes to the income tax system that would be necessary for an implementation of a withholding-based tax system. To avoid a heavy tax burden and "double taxation" during the transition to the new system, i.e. that workers would pay taxes on both income earned in 1986 and 1987 using their 1987 income, it was decided that all labor income earned in 1987 would be exempt from taxes.[51] Naturally, the reform received much media attention in the following days and weeks. Newspapers printed headlines such as "*A Tax-Free Year*" and "*Pay-as-you-earn tax system in 1988 – all income in 1987 tax-free*". Politicians and union leaders emphasized the opportunity that this reform provided, and in an interview, the chairman of one of the largest labor unions was quoted as saying "*Now it is time for everyone outside the labor market to enter, and for all workers to earn tax-free income. There is work for everyone that wants to work.*"[52]

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

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

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

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

In practice, workers and firms were to collect information as usual and file taxes at the beginning of 1988 as in earlier years. The tax authorities sent out advertisements emphasizing that the requirement for enjoying a tax-free year was to file taxes as usual, and they produced flyers explaining the new tax system and that income earned in 1987 was tax free (see Appendix Figures A.3 and A.4). For those who would not file their taxes, their income would be approximated based on their income in the year before and they would be taxed as in a normal year. Reporting information as usual was also important because other taxes, such as on capital income and wealth, and benefits were unchanged in 1987; the only change in that year was that income taxes were set to zero.[53]

While the general rule was that all labor income in 1987 should be exempt from taxes, some attempts were made to prevent an abuse of the reform. The documents and explanations associated with the law explicitly expressed a very positive view and encouragement of the legislature towards workers, exploiting the opportunity that the reform provided to increase their disposable income in 1987 by increasing their labor supply by any or all means. However, a clear aim was that any abuse of the reform by entrepreneurs or firm owners should be prevented. The law therefore specified two exceptions to the general rule. First, increased earnings in 1987 that were not due to more work or changes in employment arrangements, such as promotion, but rather reflecting transfers of income from other years should be taxed as usual. Second, inflation-adjusted increases in earnings of self-employed workers and business owners exceeding 25% should be taxed as usual. Studying the records, however, I find that these measures seem to have played only a limited de facto role.[54]

C Data and Measurement

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

C.1 Tax Calculator

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

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

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

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

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

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

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

C.2 Summary Statistics

Table A.13 presents summary statistics in 1986 for the population of 16–70-year-olds as a whole for all wage earners and for self-employed individuals. The average age in the population is 38 years and 45% of the population are women. About 36% have a junior college degree (post-compulsory schooling) and 10% have a university degree. Among those with nonzero labor earnings, the average weeks worked is 41. The average marginal tax rate was 19% and the average tax rate—computed as the average tax payments divided by the tax base—was roughly 11%.

C.3 Occupation and Sector Classification

Pay slips include information about occupation according to a two-digit classification. There are 74 separate occupation classes recorded. The occupation classification is based on the International Labor Organization's (ILO) International Standard Classification of Occupations (ISCO), version ISCO-88. More details on the classification are provided in documentation on ILO's website. Table A.14 documents the structure of the classification and lists the broader occupation groups.

The pay slips also record the sector for each firm. In total there are 189 separate sector classes recorded. The sector classification is based on the United Nations' International Standard Industrial Classification of All Economic Activities (ISIC). Details about the classification are provided in documentation on UN's website. Table A.15 documents the structure of the sector classification.

C.4 Education Classification

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

D Tax Bracket DD: Bracket Persistence and Mean Reversion

There is an extensive literature estimating the elasticity of taxable income (see e.g. Saez et al., 2012, for a recent survey). In particular, dating back to a seminal study by Feldstein (1995), much work has been carried out studying tax reforms in the US in the 1980s and 1990s.

A particular feature of these reforms is that they generated decreases in tax rates at the top of the income distribution. The fact that much of the variation exploited is centered at the top of the income distribution has spurred much discussion on possible consequences for the estimated elasticities (Saez et al., 2012). Three problems have been highlighted. First, as highlighted in Gruber and Saez (2002), if the income distribution is continually widening, e.g. due to factors such as skill-biased technical change and globalization, it may be difficult to disentangle the long-term effects of tax changes from these trends, particularly at the top of the income distribution. Second, as income is often the main driver of marginal tax rates, and income has both permanent and transitory components, a positive transitory income shock in the pre-reform year will tend to result in lower income in the years following, therefore biasing elasticity estimates downward. Third, studies using tax return data, particularly from the US, often have little information about taxpayers other than that about their income and taxes, which makes it difficult to control for differences in the characteristics of taxpayers at the top vs. those at lower levels in the income distribution.

Compared with this literature, the natural experiment provided by the tax-free year has several advantages that allow me to overcome these biases. First, the tax-free year affected taxes across the entire income distribution. Furthermore, most of the analysis is concerned with short-term responses to a temporary tax cut. Therefore, this alleviates the concerns related to long-term trends such as the evolution of inequality. Second, the variation generated by the tax-free year is not as closely linked to levels of labor earnings as the variation exploited in the aforementioned studies. Owing to multiple tax deductions and tax credits, there was a substantial overlap in the earnings distributions across tax brackets. Third, my data have very detailed information about taxpayer characteristics, as well as their earnings, deductions and tax payments, allowing me to control for a rich set of covariates in the regressions.

Even with these advantages, a potential bias might potentially arise because of temporary mean-reverting income shocks. For example, some people in a high tax bracket in the previous year are there because of an income shock that reverts to the mean in the current year, generating a downward bias in the earnings elasticity. Although I find that individual tax bracket positions tend to be persistent, as documented in Figure A.5, and our analysis of the pre-reform years finds no evidence of false positives, as documented in Figure 4, I have performed additional analysis along several dimensions to assess the robustness of the results to these concerns.

I now document the results from two informative exercises. First, I perform a prediction exercise, where I predict workers' tax brackets (treatment status) using a rich set of individual characteristics, with the aim of constructing more stable treatment and control groups. 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. This is an out-of-sample prediction, in the sense that the outcomes for the year predicted are excluded but information from all other pre-reform years is included. The set of right-hand-side predictors includes indicator variables for tax brackets in the past three years and individual characteristics including dummies for age, gender, marital status and the number of children, and a dummy for living in the capital area, which are also included in interaction with the tax brackets. The model also includes a full set of dummies for the previous year's percentile in the income distribution. As documented in Figure 3b, the tax bracket thresholds correspond to roughly similar quantiles of the income distribution over time. Including dummies for the previous year's percentile in the income distribution in the model proxies for, e.g. distance from the tax bracket thresholds, across which temporary shocks might push individual workers. In every year, I assign workers to tax brackets based on the predicted probabilities from this estimation, provided that the bracket position is predicted with at least 50% probability.[55] 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. Second, I have also performed an estimation where we define workers' treatment status based only on those who stay in the same bracket for the three consecutive years prior to 1987, while excluding others.

Figure A.22 plots coefficient estimates from a dynamic reduced-form estimation where the treatment status is based on the predicted tax bracket. Similar to the main specification (Figure 4), the pre-reform coefficients are not statistically significant, implying parallel trends. Table A.17 presents estimates of the elasticity of labor earnings, where the treatment status is assigned using the same procedure. The elasticity estimates, as well as the reduced-form estimates, are roughly similar to those estimated under the main specification and, if anything, only marginally larger. Similarly, the estimates of the elasticity of weeks worked, reported in Table A.18, are very similar to those under the main specification. These results are also robust to using more or fewer lags of the tax bracket position in the prediction exercise. Tables A.19 and A.20 report the effects on labor earnings and weeks worked, respectively, using a specification where treatment status is based only on those who remain

[55]The results are robust to requiring higher levels of prediction accuracy.

in the same bracket for the three consecutive years prior to 1987. The results are broadly similar to the main specification.

E Permanent Tax Changes, Expectations and Long-Term Effects

The tax-free year generated a temporary incentive to exchange leisure time for working time in 1987, possibly at the expense of less work in the years that followed. This major reform that, as documented in the current paper, induced strong labor supply responses, may also have had some positive effects on labor supply extending beyond 1987, such as through the forces of habit and learning. However, the tax system also saw several permanent changes in 1988, which themselves may have generated effects on labor supply. During the first few months of 1987, when technical and legal aspects of the new withholding-based tax system were being worked out by the government and the tax authorities, the aim was to simplify the tax system in order to ease the transition (Olgeirsson, 2013). As a result, many of the pre-existing tax deductions were abolished and the progressive tax schedule was replaced with a flat tax rate, corresponding to the rate in the upper-middle tax bracket. While fewer tax deductions were compensated for by substantially increasing the personal tax allowance, the reform posed permanent effects on average and marginal tax rates.

An important question to ask is whether the permanent reform in 1988 affects my estimates of the responses to the tax-free year in 1987. If workers were responding to a tax reform in 1987 that they perceived to be permanent rather than temporary, the estimates of the Frisch elasticity in this paper will be confounded by an income effect arising from the permanent change in taxes that is likely to be non-negligible. There are two arguments for why there may be limited effects of the permanent reform spilling over to my estimates. First, while the tax-free year was announced in December 1986, which resulted from a change in tax collection, no announcement was made on changes to the tax schedule under the new tax collection system. As described in Section 2, that process went on during the first few months of 1987 and the bill spelling out the new tax law was passed by Parliament in late March 1987. By then, workers had been aware of the much-advertised tax-free year for several months. Second, relative to the simple and salient nature of the tax-free year, many of the implications of the new tax code for marginal tax rates were much less clear. In particular, an important part of the tax reform was the removal of tax deductions, which affected the tax base and therefore the marginal tax rates. For most taxpayers, assessing how changes in tax deductions and allowances would affect their marginal tax rates was likely to have been a complicated task.

To statistically evaluate this question, I perform several robustness tests. First, I evaluate the sensitivity of the estimates to controlling for the change in taxes between 1986 and 1988 to capture the income effect. I measure the change in taxes in three ways: (1) change in the logarithm of the net-of-marginal tax rate, (2) change in inverse hyperbolic sine transformation of income tax payments (to capture zero tax payment before or after), and (3) change in net-of-average tax rate, where the average tax rate is computed as the ratio of income tax payments and the income tax base. As Tables A.21 and A.22 document, the elasticity estimates are broadly robust to these controls, resulting in either slightly larger but statistically indistinguishable estimates compared to the main specification (0.40), or when controlling for change in marginal tax rates a somewhat lower elasticity estimate (0.28). Next, I restrict the focus to the upper- and lower-middle brackets. As workers in these brackets saw limited changes in their marginal tax rates between 1986 and 1988, with the 1986 upper-middle bracket tax rate corresponding to the flat rate in 1988, they should be minimally influenced by the permanent reform. As reported in Table A.23, this yields an earnings elasticity estimate of between 0.325 and 0.386, which is similar to what is reported for the upper-middle bracket in Table A.6, as well as being broadly consistent with the main estimates. In Section 5, I develop a new research design where, as to be described in more detail, one of the advantages is that the control group experiences neither treatment nor the possible anticipation of a permanent reform. In addition, I can apply this research design to a sample restricted to only the two brackets for which the marginal tax rates were similar between 1986 and 1988. The results, reported in Tables A.24 and A.25, are consistent with my main estimates and the results presented in Section 5.

The primary focus of the current paper is short-run responses to the tax-free year with the aim of estimating the Frisch elasticities. However, for completeness, I report a small set of informative results in the appendix on responses that are more permanent. Permanent effects are obtained by estimating equation (2) for the outcome period 1988–1990, but excluding 1987 from the sample. The results in Table A.27 indicate large permanent effects. In order to understand these relatively large permanent effects, studying the responses of men and women separately provides an important insight. While the earnings elasticity is economically very small and statistically indistinguishable from zero for men, it is large for women and highly significant. A plausible explanation for these gender differences is (i) more persistent effects of strong responses of women in 1987, or (ii) responses to the changes in the transferability of tax deductions and allowances between spouses in 1988, which may have influenced the marginal tax rates for women more than for men.

F Life-Cycle Labor Supply Model and Identification Strategy

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

F.1 Model

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

$$\max_{\{C_{it},L_{it}\}} \sum_{t=1}^{T} \frac{1}{(1+\rho)^{t-1}} U_{it}(C_{it}, L_{it}), \quad N_{it} = \bar{L} - L_{it} \tag{9}$$

subject to

$$A_{it} = (1 + r_t)A_{it-1} + w_{it}N_{it} - C_{it} \tag{10}$$

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

$$U_{it}(C_{it}, L_{it}) = \gamma_{C_it} C_{it}^{\alpha_C} - \gamma_{N_it} N_{it}^{\alpha_N}, \quad N_{it} = \bar{L} - L_{it} \tag{11}$$

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

$$\log \gamma_{N_{it}} = \sigma_i + \mu_{it} \tag{12}$$

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

$$\log N_{it} = \frac{1}{\alpha_N - 1} (\log \lambda_{it} - \log \alpha_N + \log w_{it} - \sigma_i + \mu_{it}) \tag{13}$$

The Frisch consumption demand function can be written in a similar fashion. In (13), λ_{it} is the Lagrange multiplier on wealth. From the envelope theorem, we have that

$$\lambda_{it} = \frac{1 + r_{t+1}}{1 + \rho} \lambda_{it+1} \tag{14}$$

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

$$\log \lambda_{it} \approx r_{t+1} - \rho + \lambda_{it+1} \tag{15}$$

Using the above approximation, the labor supply equation (13) can be written as follows

$$\log N_{it} = F_i + bt - \varepsilon R_t + \varepsilon \log w_{it} + u_{it} \tag{16}$$

where

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

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

$$F_i = Z_i\theta + \sum_{t=1}^{T} \gamma_t \log w_{it} + A_{i0}\theta + \alpha_i \tag{17}$$

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

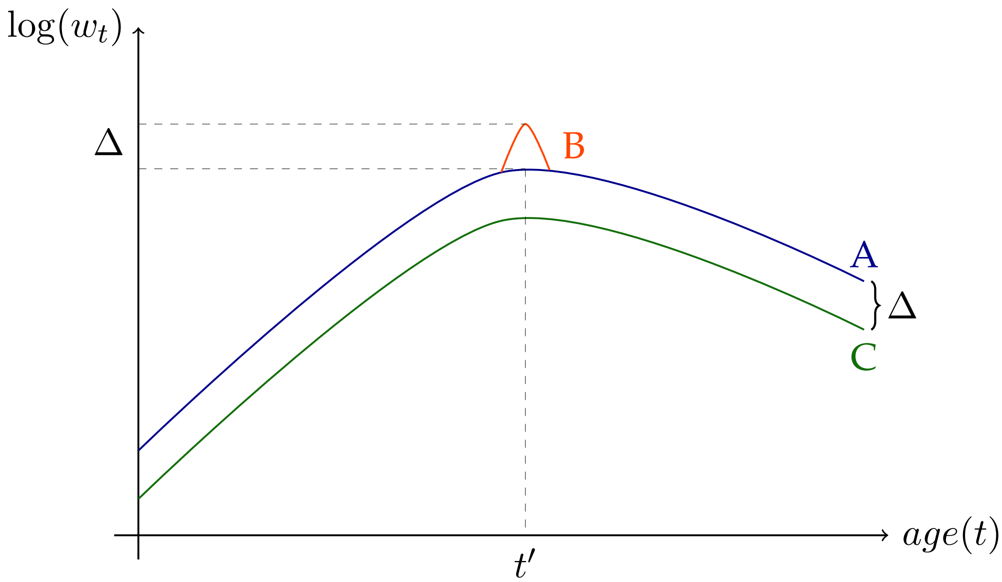


Figure A.1: Evolutionary and transitory wage changes over the life cycle

$$w_{it} = \pi_{0i} + \pi_{1i}t + \pi_{2i}t^2 + \nu_{it} \tag{18}$$

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

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

with M_i being a vector of determinants of wages that are exogenous and constant over the lifetime, such as education, g_j are vectors of parameters, and ν_{it} is an error term. Substituting (18) into (19) yields

$$F_i = Z_i\theta + \pi_{0i}\bar{\gamma}_0 + \pi_{1i}\bar{\gamma}_1 + \pi_{2i}\bar{\gamma}_2 + A_{i0}\theta + \xi_i \tag{19}$$

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

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

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

Figure A.1 plots wage paths over the life cycle, according to the process in (18). Consider an individual whose wage path can be described by path A. As he becomes older, individual A's wages increase, to which the individual responds by adjusting hours. Such evolutionary wage changes are known to the individual as the wage path, and therefore generate a substitution effect and no income effect. The parameter governing these responses is ε, which is the intertemporal (λ-constant) *Frisch* elasticity of substitution. While this is an elasticity that determines responses to an evolutionary change in wages, it can also be interpreted as determining responses to a particular type of parametric change, i.e. one associated with a wage increase at time t' but holding the marginal utility of wealth constant.

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

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

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

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

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

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

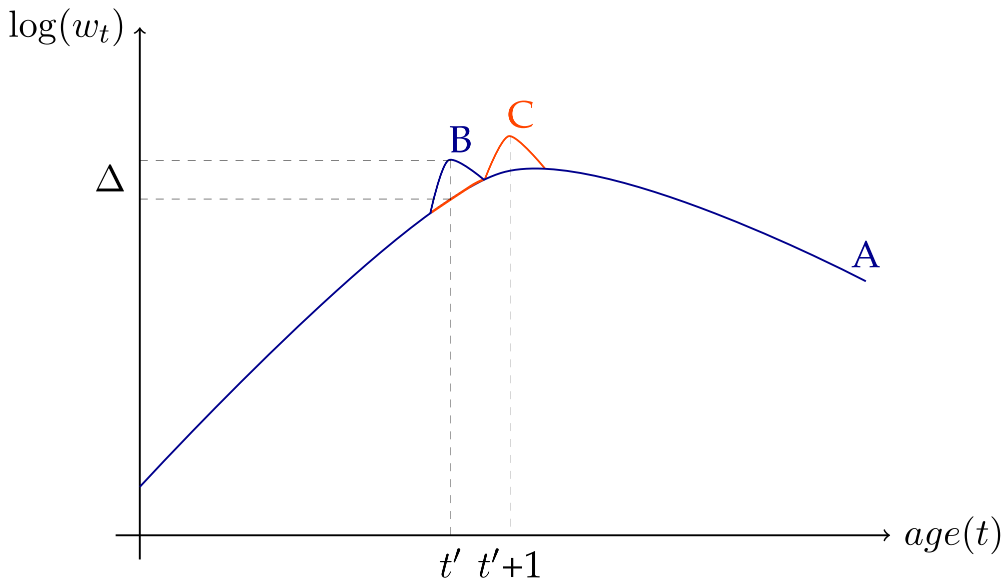


Figure A.2: Tax-free year at different periods over the life cycle

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

G Life-Cycle Research Design: Aggregate Earnings Responses

The life-cycle research design described in Section 5 complements the tax-bracket research design described in Section 4 by allowing for identifying extensive margin labor supply responses. More generally, by including the extensive margin, the life-cycle research design allows for estimating the labor supply responses of the whole population. In this section I use this method to estimate aggregate labor supply responses, including both the intensive and extensive margin.

Results. As described in the main text, the research design builds on pairwise cohort-by-cohort differences. It therefore naturally produces separate elasticity estimates by cohort. Figure A.15 plots the elasticity of labor earnings for each cohort by their age in 1987. Across the prime ages, the elasticity is stable at levels between 0.4 and 0.5, which is slightly larger but broadly consistent with the earnings elasticity estimates presented in Section 4. For the older cohorts—those around or at retirement age—the figure displays slightly larger elasticities.[56] The groups that stand out are the youngest

[56]While workers receive pensions and are eligible for old-age benefits from age 67 years, it is common to retire later and some choose to retire earlier, e.g. at the time when their spouse reaches the statutory retirement age.

cohorts—between the ages of 18 and 30 years—who have the largest elasticities, as high as 2 among the youngest cohorts. Although the elasticity is largest only for the few youngest cohorts, this has an important implication for the aggregate elasticity. The population aged 18–30 years corresponds to about 22% of the population, thus pulling up the average elasticity. Furthermore, as documented in Section 5, this difference in earnings elasticity by age is reflecting to a large extent differences in extensive margin responses, which are driven by young cohorts and cohorts close to retirement age.

Table A.28 presents estimates of the average labor supply elasticity in the population using this method. The table is organized such that the bottom row reports the first-stage estimates, the middle row reports the reduced-form estimates and the top row reports the final elasticity estimates. The top row of column (1) reports an elasticity estimate of 0.654, which is highly significant at the 1% level. In column (2) the regressions include individual fixed effects to capture any time-invariant cross-sectional heterogeneity, which only marginally affects the estimates. Columns (3) and (4) report the average responses in terms of weeks worked, implying a semi-elasticity of about three additional weeks.

Triple-difference design. To evaluate further the robustness of the main results, I can combine the two empirical strategies in a triple-difference design. In this, I augment differences across adjacent birth cohorts with within-birth-cohort differences across tax brackets. The benefit of this design is that, in addition to comparing similar individuals expected to be on similar life-cycle labor supply paths, it differences out all possible common time effects, reducing the identifying variation to cross-sectional variation alone. The resulting earnings elasticity, reported in Appendix Table A.26, is 0.431, which is similar to the estimates reported in Section 4.

Comparison across estimates and methods. When interpreting these estimates and comparing them with those presented earlier in Section 4, it is important to keep in mind their differences. Two main factors separate the estimates from the two designs. First, as the tax bracket DD identifies elasticities from a cross-sectional variation in tax rates, the estimates are restricted, by construction, to the employed population. Indeed, the positive employment response documented above contributes to the aggregate earnings elasticity of about 0.65. In order to arrive at comparable estimates based on the two methods, I apply the life-cycle differences method on the same sample as used in the tax bracket DD estimation, yielding an earnings elasticity estimate of 0.529 (SE 0.010), which is somewhat larger than the intensive margin elasticity reported in Section 4.

Second, as the life-cycle differences 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 what these effects contribute, on net, 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 DD compared with the tax bracket DD if these effects are common across tax brackets. Survey data on paid hourly wages lends little support for a reduction in wage rates in 1987 and shows, somewhat importantly, similar movements across occupations.[57] On the other hand, workers receiving large tax cuts may spend less time on leisure but also on home production, such as home cleaning, cooking and childcare. This may then generate demand for labor inputs in those sectors 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.[58] Moreover, if these workers disproportionately fall into lower brackets, the elasticities estimated using the tax bracket DD will be biased downwards relative to those estimated using the life-cycle differences method. More generally, while the tax bracket DD method is able to "difference out" all aggregate time effects, it will be biased downwardly if workers in the upper tax brackets are less responsive or face greater adjustment constraints than those in the lower brackets.

H Collective Labor Supply Model with Home Production

Consider a family consisting of a married couple, where m indexes the husband and f the wife and their children (if any). Adults allocate their working time between two activities. First, they can sell their labor on the market and earn a fixed wage, w. Labor income is then used to buy a market consumption good, c. Second, they allocate time to producing goods and services at home, such as taking care of their children or making food only consumed by the family. The latter incorporates the insight from Becker (1965) that a significant proportion of the time spent away from work is home production.

The preferences of each spouse $i \in \{m, f\}$ are described with a quasi-linear utility function in consumption and working time:

$$u_i = c_i + y_i - \frac{\eta}{1+\eta}(n_i + h_i)^{\frac{1+\eta}{\eta}} \tag{20}$$

where c_i is spouse i's consumption of the market good, y_i is spouse i's consumption of the home-produced good, n_i is spouse i's market hours, and h_i are hours allocated to home duties. The parameter η governs the curvature of the disutility of work.

The spouse i's budget constraint is:

$$c_i \leqslant (1 - \tau_i)w_i n_i + z_i + s_i \tag{21}$$

where τ_i is spouse i's marginal tax rate, z_i is spouse i's unearned income, and s_i are the net transfers received by spouse i.

The couple engages collectively in home production, where home-produced goods and services are assumed a public good within the household. The domestic good is produced according to Cobb–Douglas production technology

[57] See Appendix Figure A.10, which plots hourly wage rates by occupation through the 1980s. The figure uses data collected by the Icelandic Wage Research Committee. The wage increase that is visible at the end of 1986 and beginning of 1987 likely results from national-level collective wage negotiations that took place during 1986 and new agreements signed that year.

[58] In addition, during a tax-free year, there may be incentive for firms to frontload investment in order to increase their activity in a period when labor is in greater supply. Appendix Figure A.11 documents increased growth in the capital stock—primarily machines, equipment and buildings (plant)—in 1987 compared with the years before and after.

$$Y(h_m, h_f) = (\kappa_m h_m)^{\alpha_m} (\kappa_f h_f)^{\alpha_f}, \qquad \alpha_m + \alpha_f \leqslant 1 \tag{22}$$

where $\kappa_i h_i$ is the effective labor input of each spouse. I assume that the home-produced good is a public good within the household. Therefore:

$$y_m = y_f = Y(h_m, h_f) \tag{23}$$

Following the literature on collective labor supply (see, e.g. Chiappori, 1988; Apps and Rees, 1988), I assume that family decisions lead to Pareto optimal allocations. Each spouse has his/her individual preferences and maximization problem, but the couples agree to maximize a collective family utility function, which is the weighted sum of the individual utility functions.[59] Furthermore, I assume full commitment, so that married couples stay married, and the weighting parameter μ in the family welfare function is exogenous and constant. The family's decision problem is to maximize the following collective family utility function:

$$u(c_m, c_f, y_m, y_f, n_m, n_f, h_m, h_f) = \mu u_m(c_m, y_m, n_m, h_m) + (1-\mu) u_f(c_f, y_f, n_f, h_f) \tag{24}$$

subject to (22), (23) and the family's budget constraint.

The solution to the model provides a labor supply function for husbands and wives:

$$n_i = ((1-\tau_i)w_i)^{\eta} - \frac{\kappa_i h_i}{\kappa_j} \left(\frac{\alpha}{\mu(1-\tau_i)w_i} \right)^{\frac{1}{1-\alpha}}, \quad i, j \in \{m, f\}, j \neq i \tag{25}$$

H.1 Own-Wage and Cross-Wage Labor Supply Elasticities

Using this simple framework, I ask two questions and obtain predictions from the model which I then explore using the data. First, how do husbands and wives respond to changes in their wage rate or, equivalently, their marginal tax rate? Computing own-wage elasticity of labor supply, $\varepsilon_{n_i, w_i} = \frac{\partial n_i}{\partial w_i} \frac{w_i}{n_i}$, yields

$$\varepsilon_{n_i, w_i} = \eta + \left(\eta + \frac{1}{1-\alpha_i} \right) \frac{h_i}{n_i} \tag{26}$$

The elasticity consists of two components. First, in an individualistic model without home production, the labor supply elasticity corresponds to the constant preference parameter η. Second, given home production, the labor supply elasticity has a second component. As an increase in the market wage (or a decrease in taxes) increases the opportunity cost of home production, workers will substitute hours from home production to market work. Equation (26) provides the first prediction of the model: for couples engaging in home production, individuals' own-wage elasticity is stronger the

[59] This simple framework only illustrates the spousal cross-response arising from substitutability in home production, but not that from complementarities in leisure time. Allowing for such complementarities would generate an opposing force, and the overall cross-response would be the combination of the two. Given my results imply negative cross-elasticities in most cases, the results can be interpreted as the force of substitutability in home production dominating the complementarity of leisure.

more important is their labor input for home production and the more specialized they are in home production. This explains why labor supply elasticities may differ across couples. If women engage in relatively more home production, e.g. due to a comparative advantage or bargaining power in the household, they will have a larger labor supply elasticity due to substitutability between time spent on home production and market work.

The second question is how husbands and wives respond to changes in their spouse's wage, or the marginal tax rate. Computing the cross-wage elasticity of labor supply, $\varepsilon_{n_i,w_{-i}} = \frac{\partial n_i}{\partial w_{-i}} \frac{w_{-i}}{n_i}$, yields

$$\varepsilon_{n_i,w_{-i}} = -\frac{1}{\alpha_i} \frac{h_i}{n_i} \tag{27}$$

The cross-elasticity is negative and depends on relative hours allocated to home vs. market work and the output elasticity in home production. From the perspective of the individual, if the spouse's wage increases, the spouse's opportunity cost of time allocated to home production, relative to market work, also increases. As the members of the couple are perfect substitutes in home production, a change in the spouse's wage induces a change in the couple's relative opportunity costs of market work. Therefore, in response to an increase in their spouse's wage, individuals will allocate more time to home production and less to market work. Equation (26) provides the second prediction of the model: within couples engaging in home production, the cross-wage elasticity is larger (in absolute value) the more time is spent on home production but the lower the elasticity of their input in home production.

Evidence based on time-use surveys indicates that women allocate more time than men to chores within the household (Aguiar et al., 2013). It is also reasonable to assume, at least in households with small children, that females' output elasticity in home production is larger than that for men.[60] Based on that, the model implies that households with more children, where both spouses take part in home production but women play the leading role, married women will have a larger own-wage elasticity than their husbands. However, the cross-elasticity may be stronger (more negative) for married men than for married women if relatively more time input is needed from them to substitute for their wives' time.

I What Features Shape the Labor Supply Responses?

There are multiple margins along which heterogeneity in labor supply may arise and a vast literature provides a range of standing theories. In Section 6, I organize my exploration and discussion around three main characteristics of individuals and their employment arrangements that have the common feature of generating frictions in individuals' abilities to adjust their labor supply and we consider how they can adjust.

As a validation of this choice, this section takes a systematic machine-learning approach to revealing the key dimensions of heterogeneity. The methodology involves three steps. First, I estimate

[60] Bredemeier and Juessen (2013) construct a model of family labor supply with a Cobb–Douglas home production function. When calibrating their model, they set the female output elasticity in home production to 0.7 and the elasticity for men to 0.3.

the labor supply elasticity at the individual level using the life-cycle DD method used in Section 5.1, matching each individual to a counterfactual constructed from a group of individuals with the exact same characteristics. Next, I use the random forest algorithm, developed by Breiman (2001), to predict labor supply elasticity using a broad set of characteristics.[61] Finally, I exploit the comparative advantage of the random forest algorithm relative to other machine-learning methods, allowing the ranking of characteristics by their importance.

Figure A.23 plots the relative importance of the characteristics in predicting labor supply elasticity, measured using the gain achieved by splitting along the dimensions of a given characteristic. The characteristics in the random forest prediction are broadly categorized into two groups: characteristics of the individual, such as gender and age, and characteristics of the individual's job and employment arrangements, such as occupation and working time. The figure presents the results from three models. First, and as presented in the first bar, a model based only on individual characteristics highlights age to be an important feature, followed by whether and how many children individuals have. In the second bar, we present a model based only on employment and job characteristics, all defined in pre-reform values. This model highlights the importance of *weeks*, which bundles the importance of weeks worked in three pre-reform years, as well as labor earnings and net wealth. The third bar plots the results from the full model incorporating both individual and employment characteristics, as well as the characteristics of the spouses of married individuals. This reveals that working time in the years prior to the reform is the single most important feature, followed by earnings, age, wealth, measures of spousal labor market activity, and finally, job characteristics such as sector, occupation and firm size.

Figure A.23 illustrates the importance of three themes. The first theme is labor-market attachment, highlighted by the importance of working time, age, earnings and wealth. The second theme is the importance of flexibility in employment arrangements, emphasized by the weight of weeks worked and job characteristics. The third and final theme is family status and spousal coordination underscored by the importance of spousal labor market activity and children. These themes appropriately align with the analysis presented in Section 6.

J Measures of Labor Market Flexibility Across Countries

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

Following this categorization, I collected several measures of labor market flexibility for a set of OECD countries, including Iceland, Switzerland and the US. Figure A.20 presents four subfigures

[61] Athey and Imbens (2016) and Wager and Athey (2017) develop a methodology that uses random forests to estimate heterogeneous treatment effects. This methodology relies on random assignment and can therefore be readily applied to RCTs. In contrast, my research design builds on difference-in-differences. Therefore, the current research design first obtains causal effects at the individual level and then uses the random forest algorithm to characterize the heterogeneity in the effects. I then proceed to a more thorough analysis guided by the patterns revealed.

that display the general pattern in this international comparison. As shown, Iceland has a flexible labor market, much more so than Switzerland and other countries in continental Europe, and one that is closer to the US labor market. In addition, Figure A.20 demonstrates that these measures, while different, are correlated.

First, Figure A.20a depicts monthly flow probabilities into and out of unemployment. According to this "fluidity" measure of labor market flexibility, the US stands out as having the most fluid labor market, followed by Iceland. In fact, as shown, worker flows in Iceland are two to three times larger than in Switzerland. Hobijn and Sahin (2009) document similar differences for job flows. In addition, the monthly job-finding rate in Iceland is 30.5% compared with 56.3% in the US and 13.4% in Switzerland.

Second, Figure A.20b presents statistics on the cyclicality of hours per worker and their relative contribution to the cyclical variation in total hours. If workers have the flexibility to adjust their hours and the intensive margin is operative, we would expect hours per worker to move with the business cycle and to explain a significant share of changes in total hours. As Figure A.20b reveals, this is true in Iceland and in the US, but to a much lesser extent in Switzerland. In Iceland and the US, the cyclical components of hours per worker are highly correlated with the cyclical component of total hours, with correlations of 0.86 and 0.84, respectively. Similarly, in Iceland, the ratio of the standard deviation in hours to the standard deviation in employment is 0.83. This implies that hours per worker explain about 45% of the cyclical variation in total hours, which is more than twice as much as in Switzerland. Indeed, Rogerson and Shimer (2011) note that "An extreme example is Switzerland, where [...] most of the cyclical movement in total hours is accounted for by movements between non-participation and employment at a fixed number of hours per worker."

Third, Figure A.20c details wage flexibility. The figure plots the coefficient on the unemployment rate gap from a regression of the growth of real labor compensation on a constant, the unemployment rate gap (the difference between unemployment and NAIRU), a long moving average of labor productivity growth, and lagged real labor compensation growth. According to this measure, among the OECD countries, real wage flexibility is highest in Iceland.

Fourth, Figure A.20d plots two different measures of institutional flexibility. On the y-axis, it plots the replacement rate of unemployment benefits of workers' previous earnings in the first year of unemployment. On the x-axis, it plots the average of the indices in the *OECD Indicators of Employment Protection*, where a higher index implies stricter employment protection. The replacement rate in Iceland is around the country average, while employment protection in Iceland is less than in most other European countries. Unsurprisingly, the US stands out on both dimensions as having a more flexible institutional framework.

K Supplementary Figures

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

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

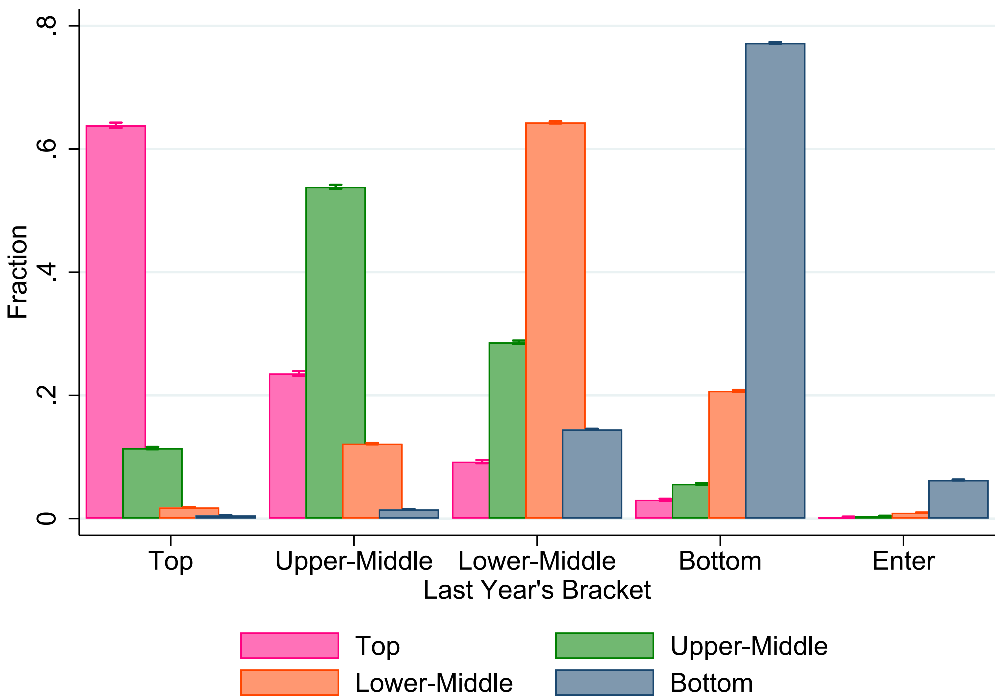
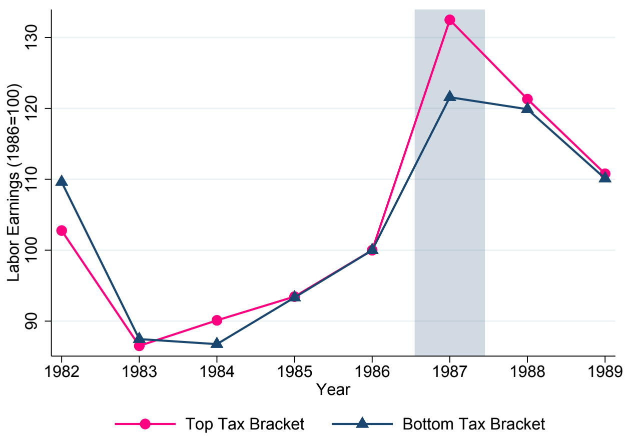
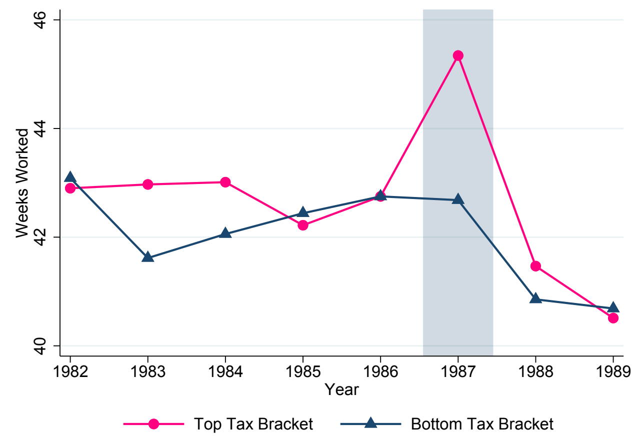


Figure A.5: Transitions between tax brackets, 1982–1986

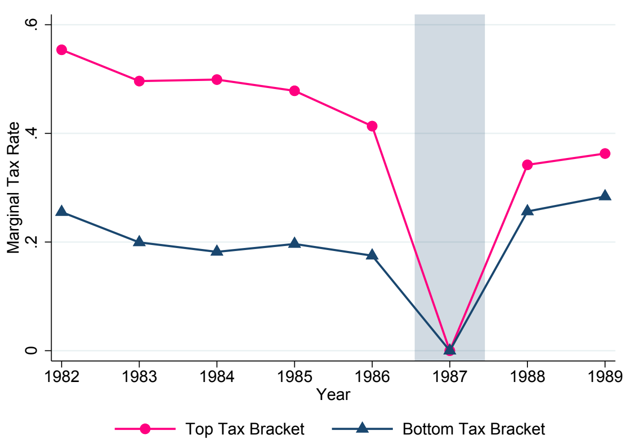
Notes: The figure plots the average transition rate between tax bracket during the pre-reform period, 1982-1986. That is, every year I compute the rate of transition from a given tax bracket to all other brackets and the rate of stays within the same bracket. I then compute averages of the resulting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



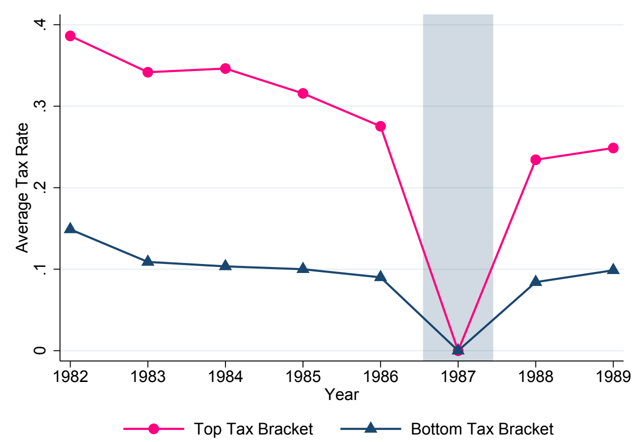
(a) Labor earnings (reduced form)



(b) Weeks worked (reduced form)



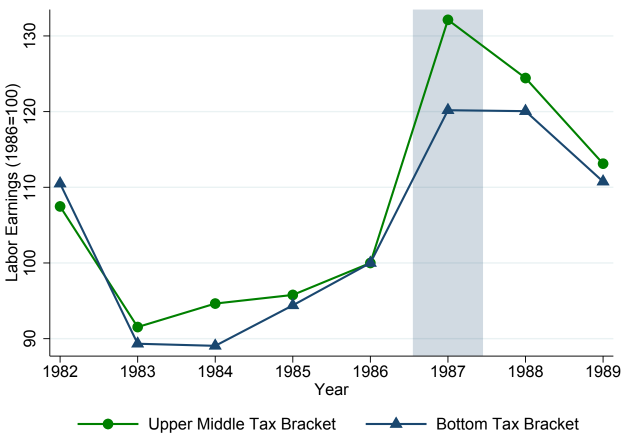
(c) Marginal tax rates (first stage)



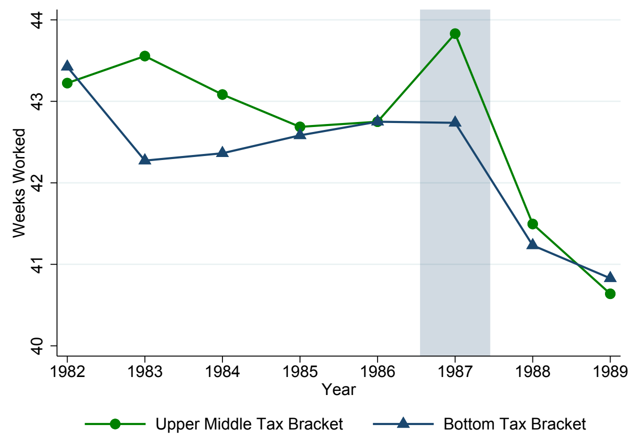
(d) Average tax rates

Figure A.6: Graphical evidence: Top tax bracket

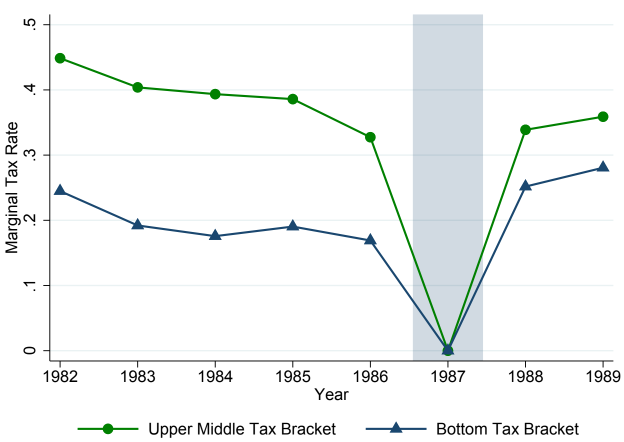
Notes: The figure provides the evolution of average (a) labor earnings, (b) weeks worked, (c) marginal tax rates, and (d) average tax rates by tax bracket, where the tax bracket status in year t is defined according to the tax bracket in $t-1$. Labor earnings are in real terms, normalized to 100 in 1986. Weeks worked are the averages of total weeks worked by individuals, i.e. in all jobs, normalized to the bottom-bracket average in 1986. In each graph, using the method of DiNardo et al. (1996), I nonparametrically reweigh the distribution of age (partitioned into 10-year bins) and pretreatment characteristics (marital status, number of children, three-level education) of individuals in the bottom tax bracket group to match the distribution of individuals in the top tax bracket. In each panel, the difference between the slopes of the two series in 1987 gives a difference-in-differences estimate, while a comparison in other years provides placebo tests of the natural experiment. The graphs for labor earnings and weeks worked imply the reduced-form effects of the tax-free year on these measures of labor supply. Correspondingly, the difference in a series of marginal tax rates provides an estimate of the first stage.



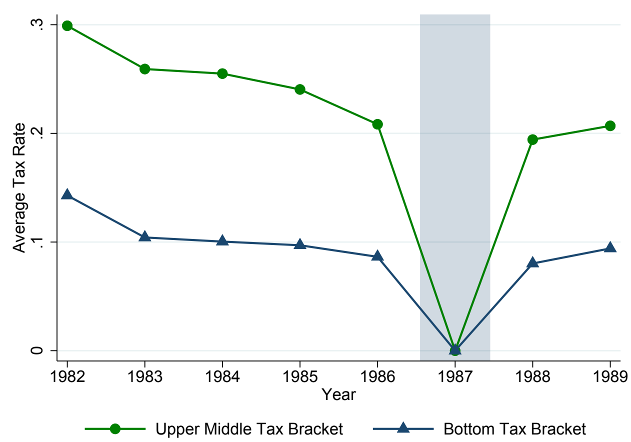
(a) Labor earnings (reduced form)



(b) Weeks worked (reduced form)



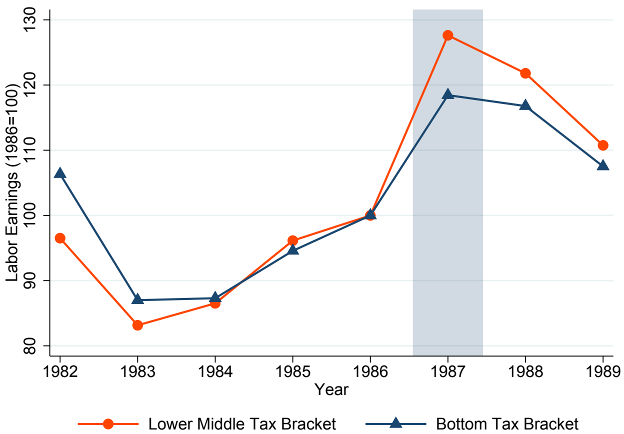
(c) Marginal tax rates (first stage)



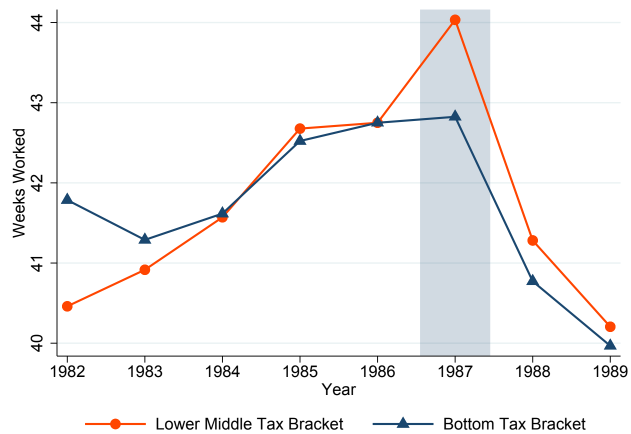
(d) Average tax rates

Figure A.7: Graphical evidence: Upper-middle tax bracket

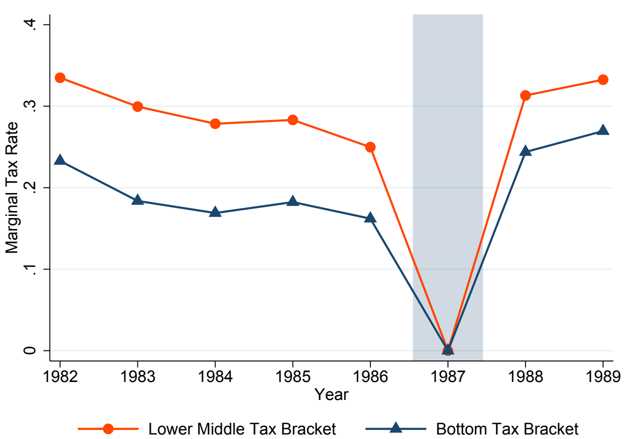
Notes: The figure shows the evolution of average (a) labor earnings, (b) weeks worked, (c) marginal tax rates, and (d) average tax rates by tax bracket, where the tax bracket status in year t is defined according to the tax bracket in $t-1$. Labor earnings are in real terms, normalized to 100 in 1986. Weeks worked are averages of total weeks worked by individual, i.e. in all jobs, normalized to the bottom-bracket average in 1986. In each graph, using the method of DiNardo et al. (1996), I nonparametrically reweigh the distribution of age (partitioned into 10-year bins) and pretreatment characteristics (marital status, number of children, three-level education) of individuals in the bottom tax bracket group to match the distribution of individuals in the upper-middle tax bracket. In each panel, the difference between the slopes of the two series in 1987 gives a difference-in-differences estimate, while a comparison in other years provides placebo tests of the natural experiment. The graphs for labor earnings and weeks worked imply the reduced-form effects of the tax-free year on these measures of labor supply. Correspondingly, the difference in a series of marginal tax rates provides an estimate of the first stage.



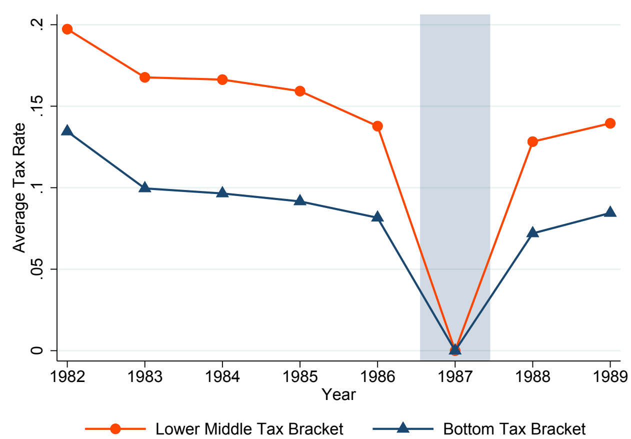
(a) Labor earnings (reduced form)



(b) Weeks worked (reduced form)



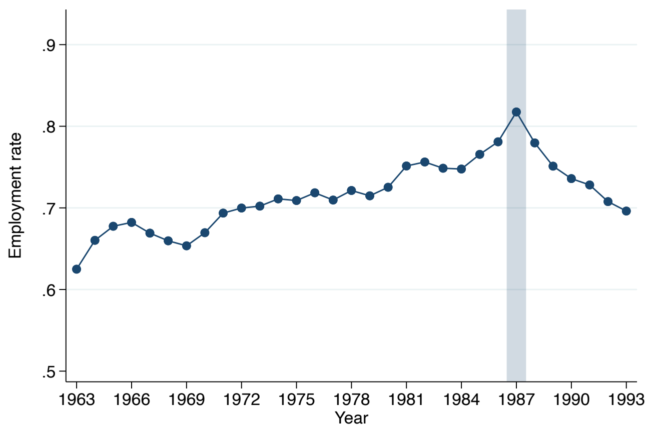
(c) Marginal tax rates (first stage)



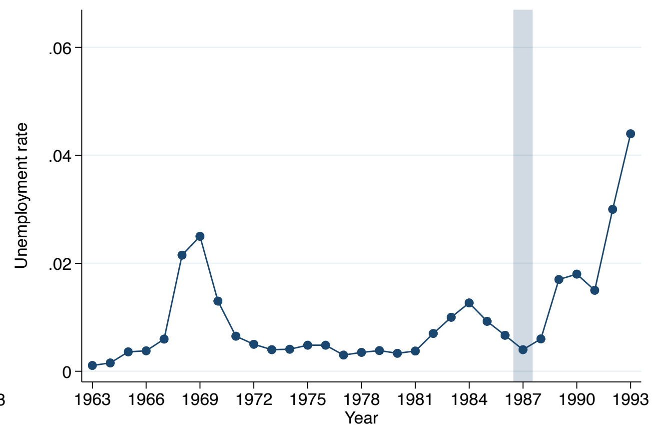
(d) Average tax rates

Figure A.8: Graphical Evidence: Lower-Middle Tax Bracket

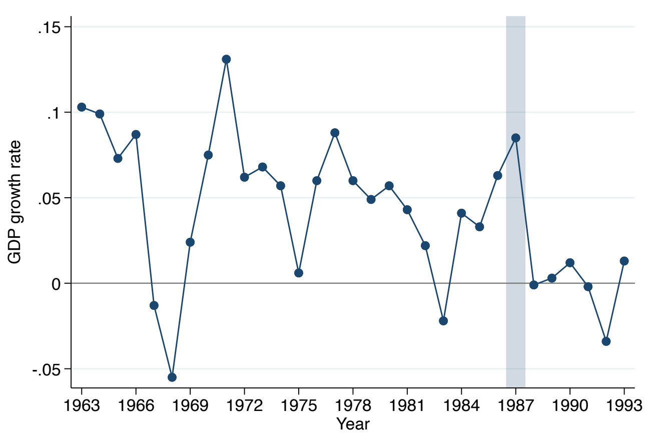
Notes: The figure shows the evolution of average (a) labor earnings, (b) weeks worked, (c) marginal tax rates, and (d) average tax rates by tax bracket, where the tax bracket status in year t is defined according to the tax bracket in $t-1$. Labor earnings are in real terms, normalized to 100 in 1986. Weeks worked are averages of total weeks worked by individual, i.e. in all jobs, normalized to the bottom-bracket average in 1986. In each graph, using the method of DiNardo et al. (1996), we nonparametrically reweigh the distribution of age (partitioned into 10-year bins) and pretreatment characteristics (marital status, number of children, three-level education) of individuals in the bottom tax bracket group to match the distribution of individuals in the lower-middle tax bracket. In each panel, the difference between the slopes of the two series in 1987 gives a difference-in-differences estimate, while the comparison in other years provides placebo tests of the natural experiment. The graphs for labor earnings and weeks worked imply the reduced-form effects of the tax-free year on these measures of labor supply. Correspondingly, the difference in a series of marginal tax rates provides an estimate of the first stage.



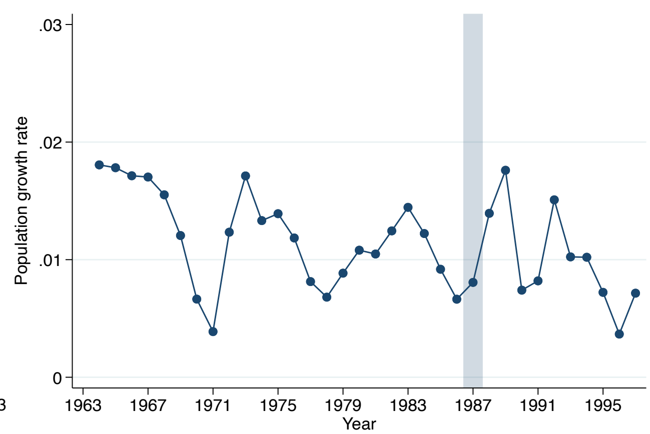
(a) Employment



(b) Unemployment



(c) GDP growth



(d) Population growth

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

Notes: Panel (a) plots the employment rate, measured by Statistics Iceland as the ratio of total man-years (full-time equivalent workers) to the working age population. Panel (b) plots the unemployment rate, as registered at the Directorate of Labor. Panel (c) plots the yearly growth rate in real GDP, measured by Statistics Iceland. Panel (d) plots the yearly population growth rate.

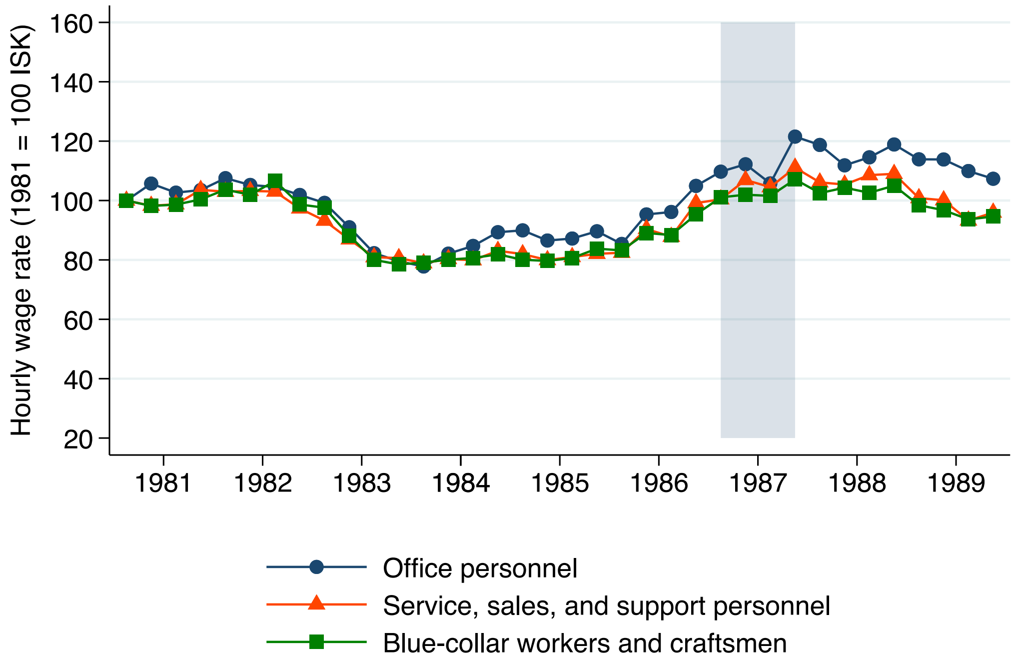


Figure A.10: Hourly wage rates by occupation

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

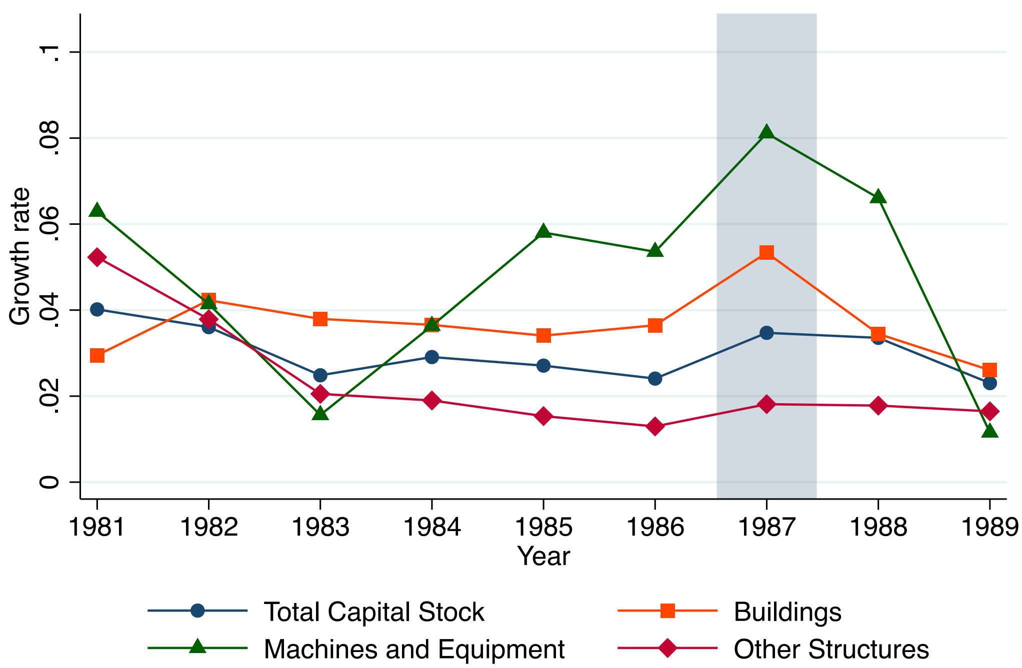
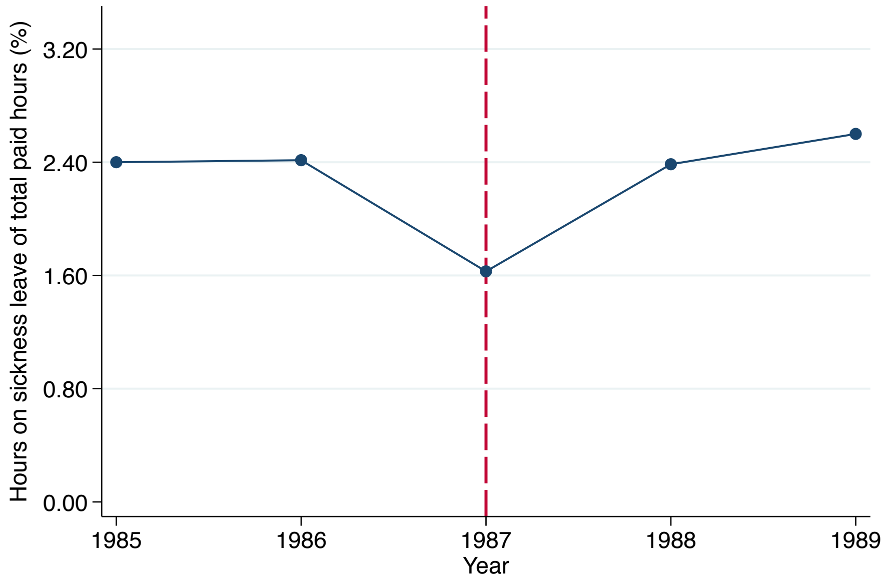
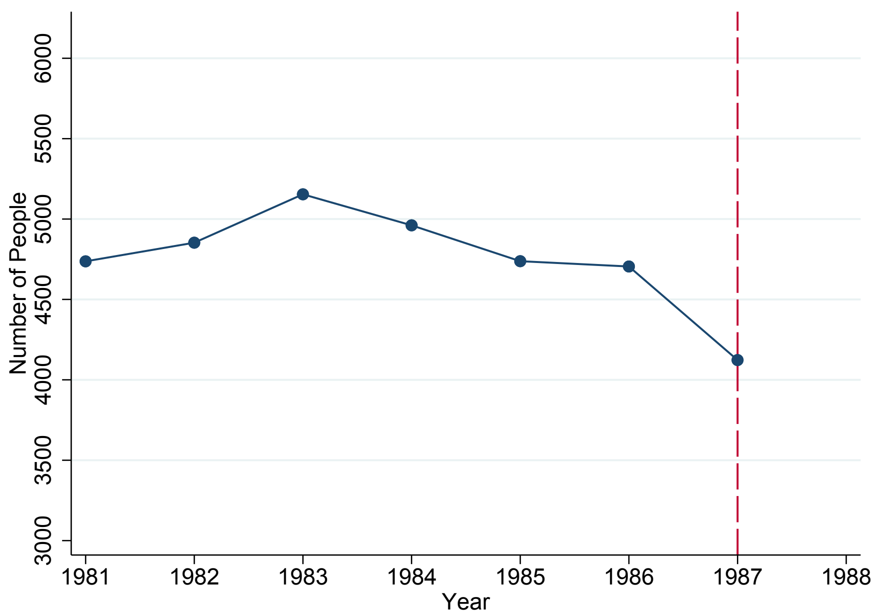


Figure A.11: Growth rate of the capital stock

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



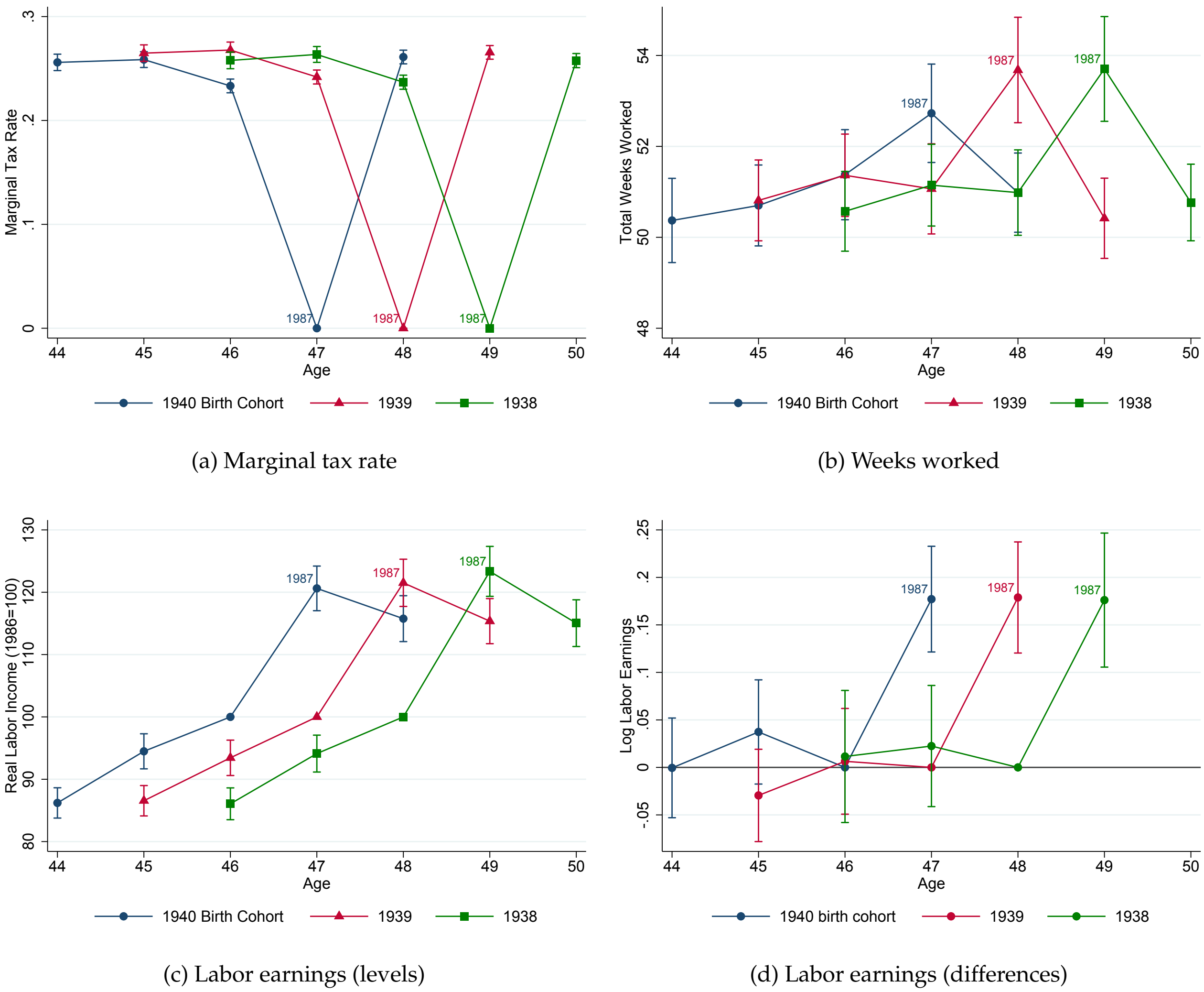
(a) Sick leave, in hours of work



(b) Recipients of sickness benefits

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

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



(a) Marginal tax rate

(b) Weeks worked

(c) Labor earnings (levels)

(d) Labor earnings (differences)

Figure A.13: Graphical example illustrating the life-cycle research design

Notes: The figure provides a graphical example illustrating the life-cycle research design by showing the evolution of marginal tax rates, weeks worked and labor earnings for a sample of three birth cohorts, born in 1940, 1939 and 1938 in the data. Each graph is based on a matched sample based on the procedure described in the main text in Section 5.1. The vertical bars plot the 95% confidence intervals. Panel (a) plots the marginal tax rates, illustrating the staggering of when the birth cohorts experience the tax-free year over their lifetime. In panel (b), I plot the average weeks worked, documenting that these cohorts work on average about 51 weeks in normal years, but increase their working time to roughly 54 weeks in 1987. Panel (c) plots the evolution of real labor earnings, normalizing the averages to 100 in 1986. This figure depicts similar trends among the three cohorts in the years prior to 1987 but a clear temporary divergence from that trend in the tax-free year. I make that point clearer in panel (d), which plots the difference in earnings for each cohort relative to the cohort born one year earlier. This removes the (common) life-cycle trends visible in panel (c) and better illustrates the change in earnings. The figure lends support to the key identifying assumption that adjacent cohorts follow similar life-cycle trends in labor supply.

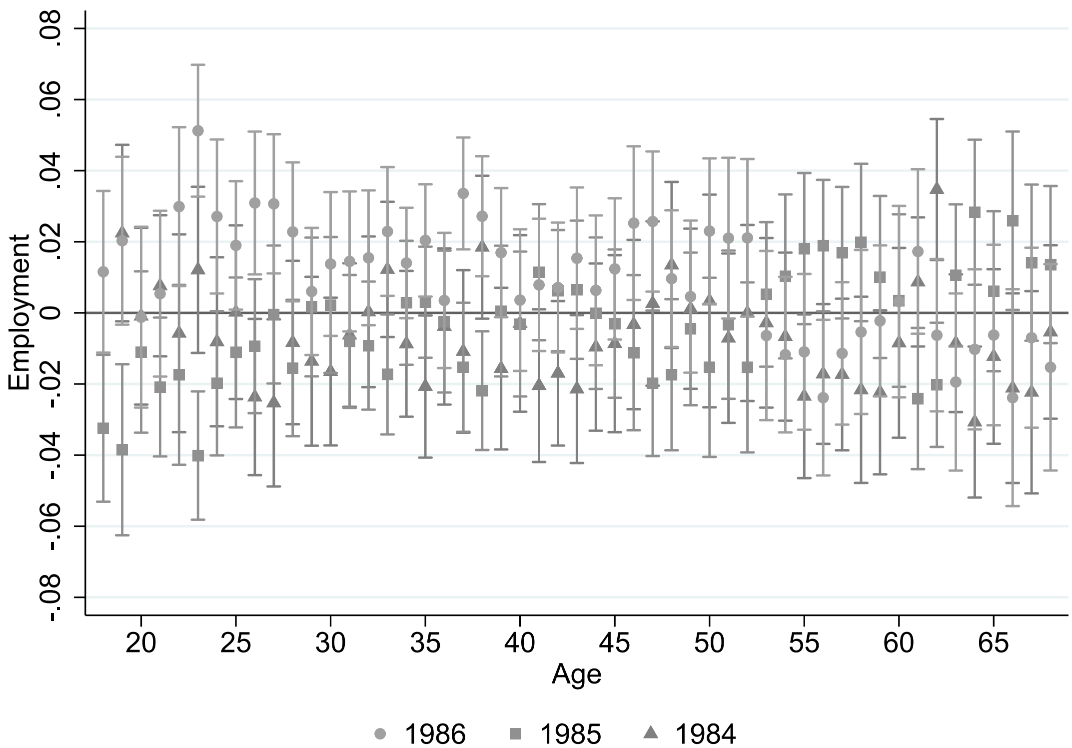


Figure A.14: Employment semi-elasticity (extensive margin) by age — Placebo tests

Notes: The figure plots estimates of equation (3), i.e. a reduced-form estimate using the life-cycle DD, by cohort where the outcome variable is employment. The figure plots estimates for three placebo tax-free years: 1986, 1985, and 1984.

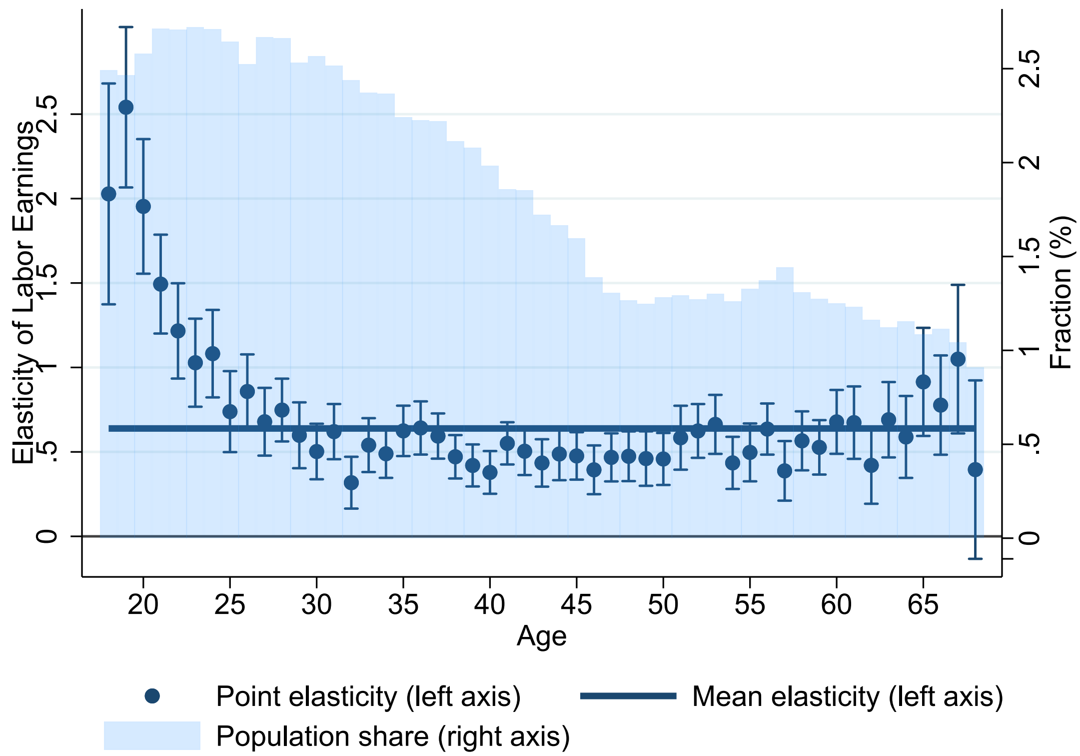


Figure A.15: Elasticity of labor earnings by age

Notes: The figure plots the elasticity of labor earnings for each cohort of age 18–68 years in 1987. Each point on the graph is a separate estimate from equation (4), where the dependent variable is the logarithm of labor earnings and the treatment group is the age denoted on the x-axis in 1987. The vertical bars plot the 95% confidence intervals. The horizontal line plots the average elasticity, as reported in Table A.28. The shaded area (bars) is the population distribution, where each bar corresponds to the share of the working age population (in %).

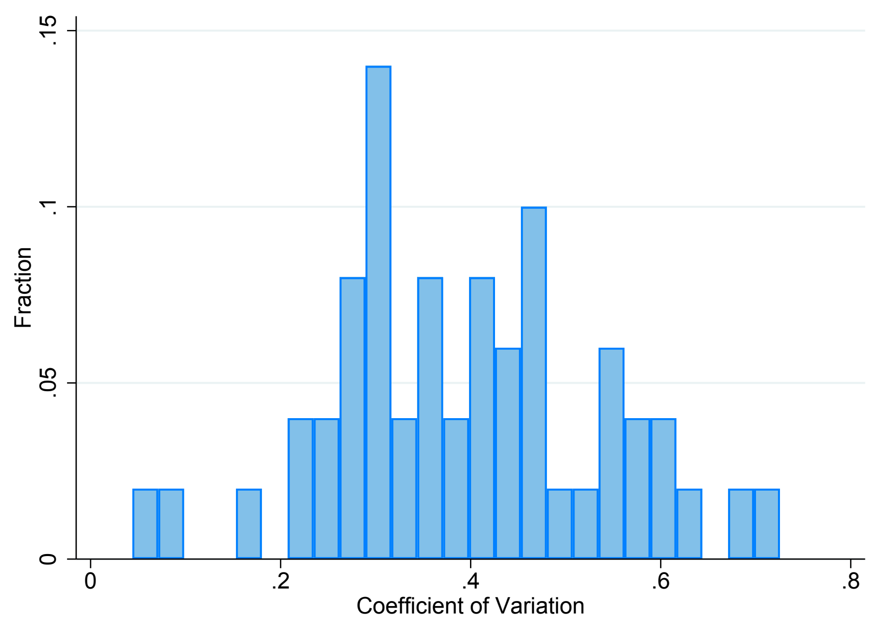


Figure A.16: Relative variability in weeks worked by occupation

Notes: The figure plots the histogram of the coefficient of variation of weeks worked by occupation, measured using equation (5).

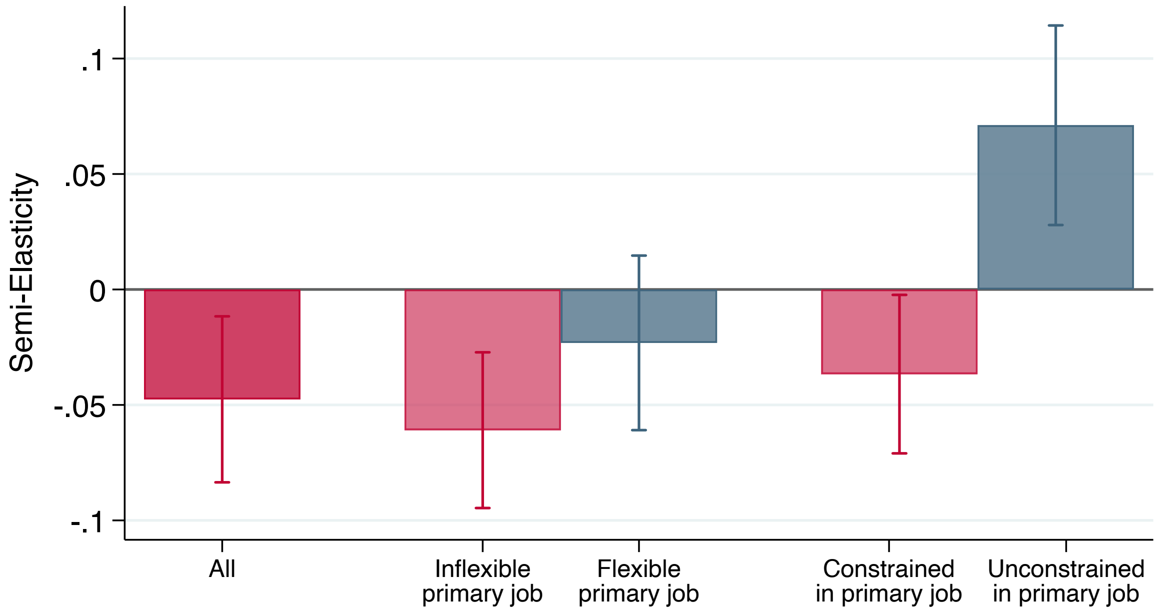


Figure A.17: Primary-job change

Notes: The figure presents the estimated effect on primary-job change. The figure presents results from a 2SLS estimation of equation (2), where the dependent variable is an indicator that equals one if the primary job is different from the primary job in the previous year, and zero otherwise. The pre-reform mean of this dependent variable is 0.232. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. The figure shows 95% confidence intervals based on robust standard errors clustered by individual. *"Inflexible primary job"* is an indicator of holding a primary job in an occupation with below-median "temporal flexibility", as measured in Section 6.1, otherwise zero. *"Constrained in primary job"* is an indicator for working 52 weeks in the primary job in the prior year, otherwise zero.

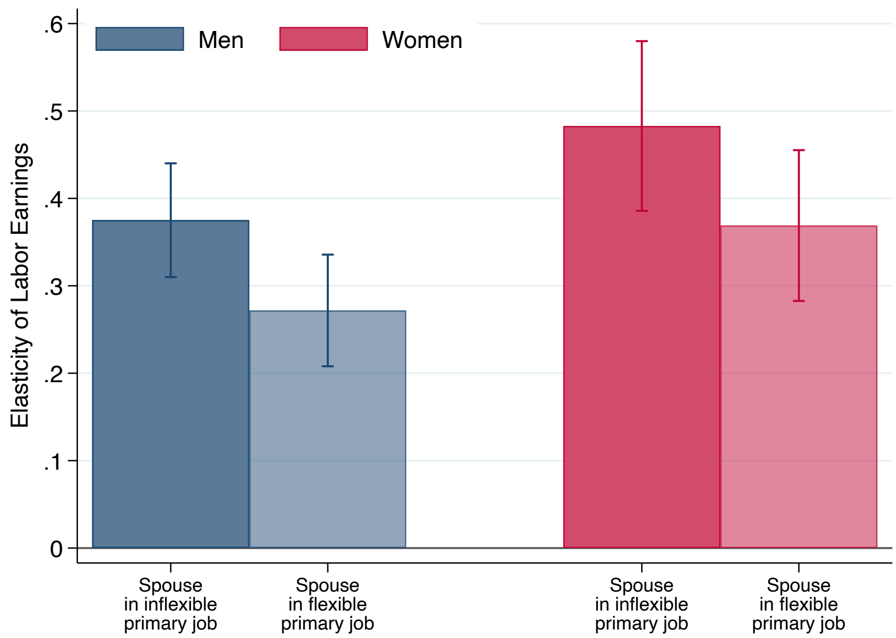


Figure A.18: Elasticity by spouse's job flexibility

Notes: The figure presents estimates of labor earnings elasticities for men and women by their spouse's job flexibility. More precisely, the figure splits the sample by whether a worker holds an inflexible primary job, defined as occupations with below-median temporal flexibility according to equation (5). The regression controls for age, number of children, education, and whether living in the capital area or not. The figure shows 95% confidence intervals based on clustered robust standard errors.

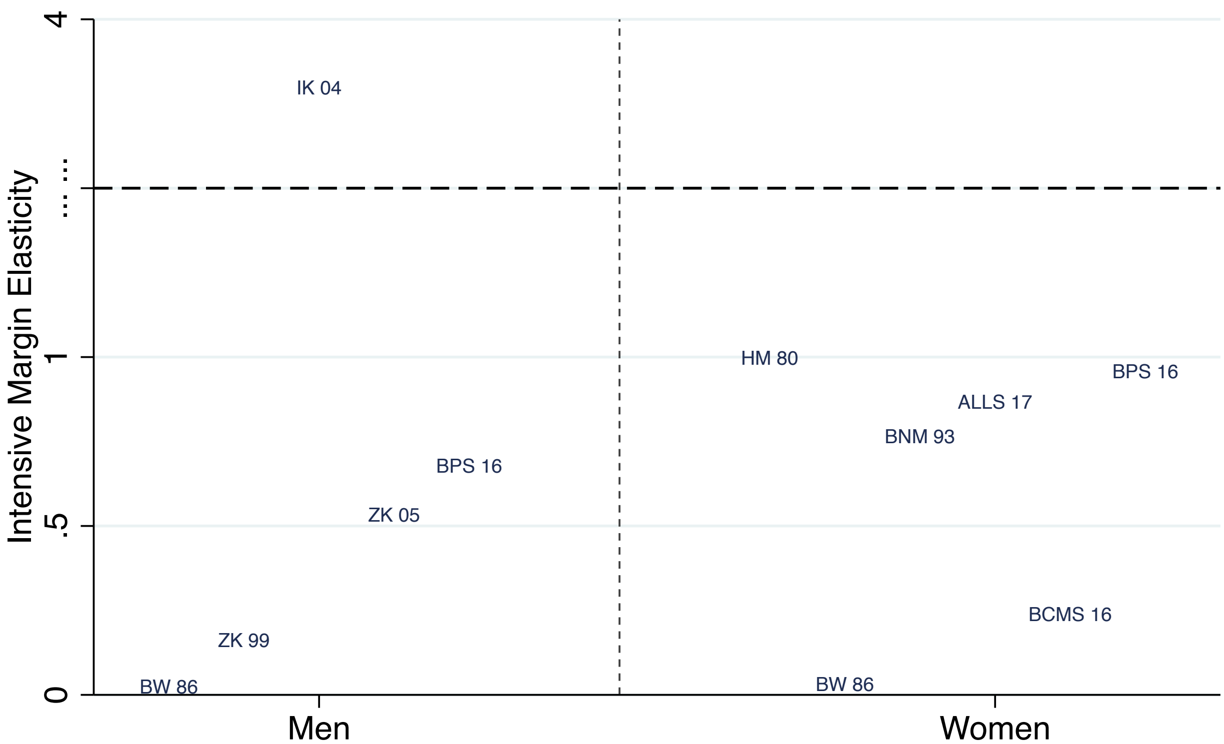
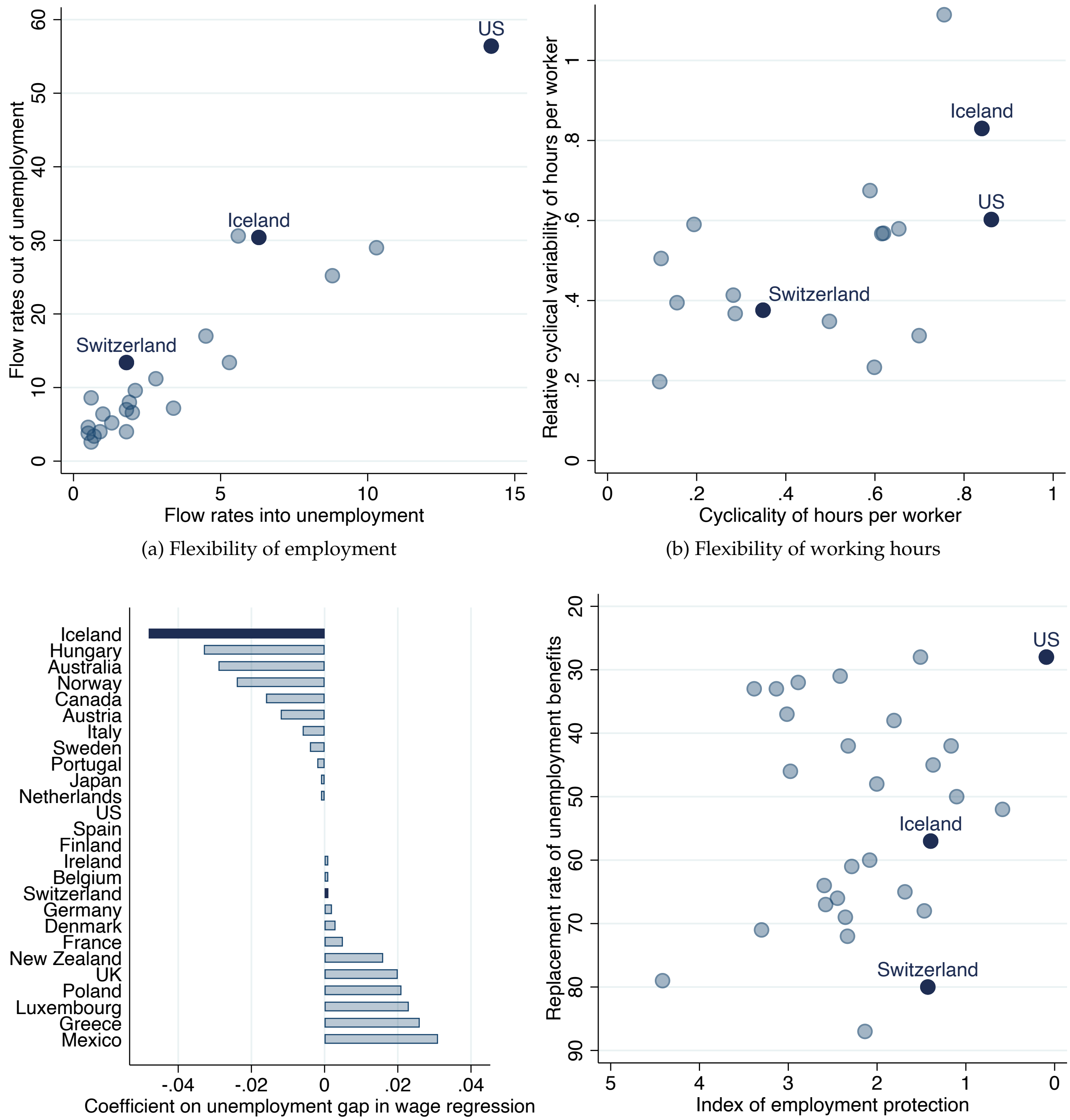


Figure A.19: Summary of structural estimates of intensive margin elasticities

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



(a) Flexibility of employment

(b) Flexibility of working hours

(c) Wage flexibility

(d) Flexibility of institutions

Figure A.20: Measures of labor market flexibility across OECD countries

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

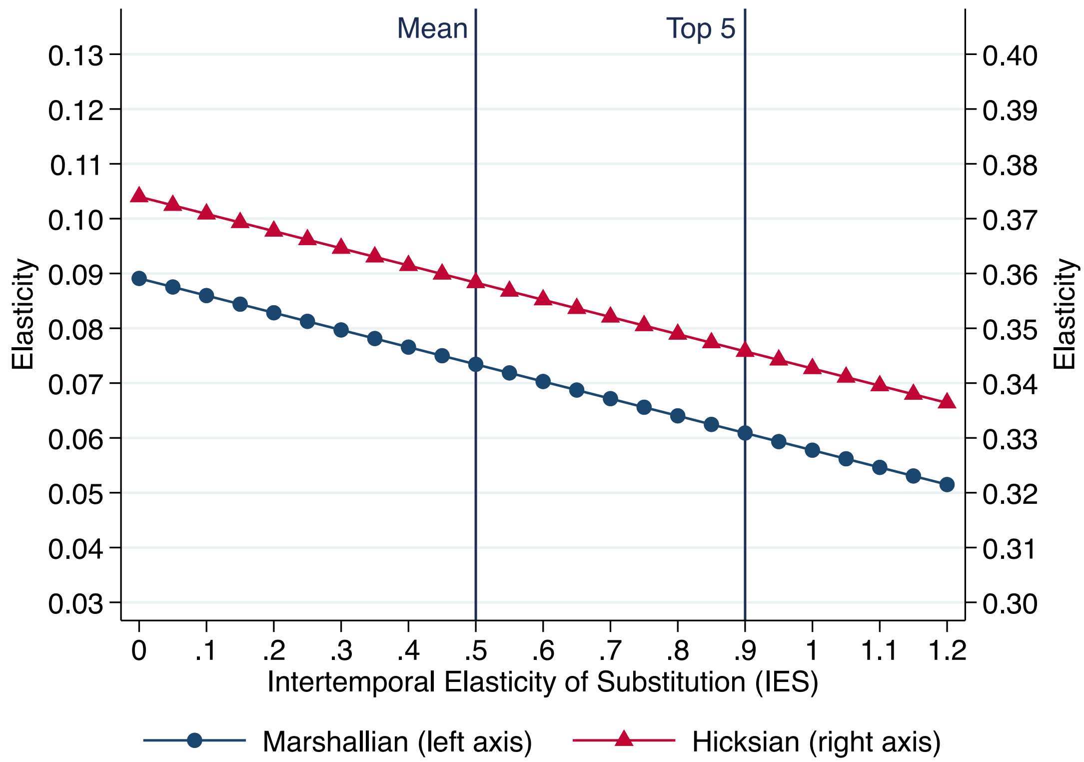
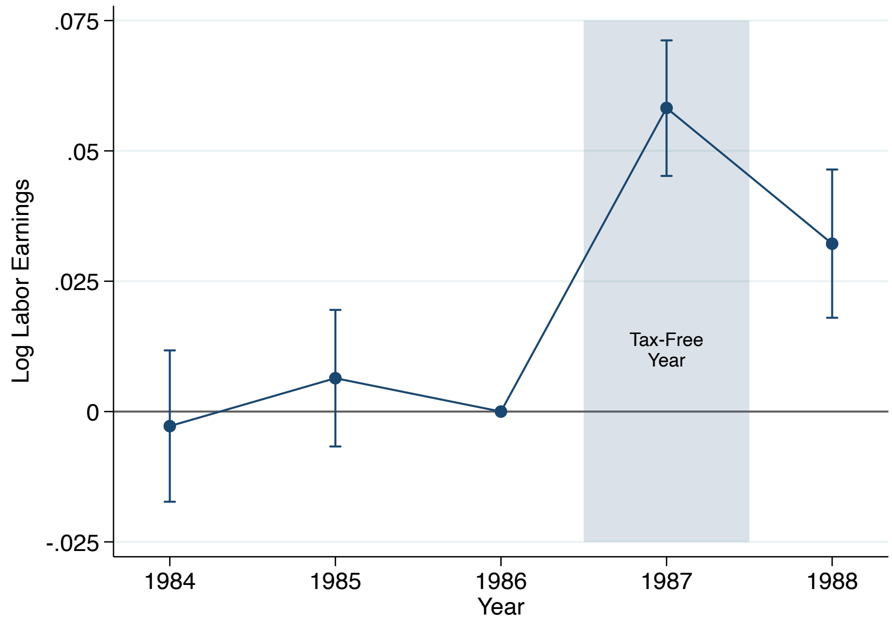


Figure A.21: Implied Hicksian-, Marshallian- and intertemporal substitution elasticity (IES)

Notes: The figure reports values of the Hicksian elasticity, Marshallian elasticity and intertemporal elasticity of substitution consistent with my estimate of intensive margin Frisch elasticity of 0.374. The calculations assume the marginal propensity to earn (MPE) out of wealth, ε_{A}, is 0.11, which is based on estimates from Imbens et al. (2001) for lottery winners (see the main text for a discussion). The ratio of wealth to labor income, $\frac{A}{wh}$, of 2.59 is the median ratio in 1986, calculated using individual tax records. The vertical line "*Mean*" denotes the average of 2,735 estimates of the EIS reported in 169 empirical studies summarized in the meta-analysis in Havránek (2015). Vertical line "*Top 5*" marks the average estimate across 33 studies published in the top-five general interest journals.



(a) Reduced form: Log labor earnings

(b) Reduced form: Total weeks worked

Figure A.22: Predicted tax brackets: Dynamic difference-in-difference

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

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

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

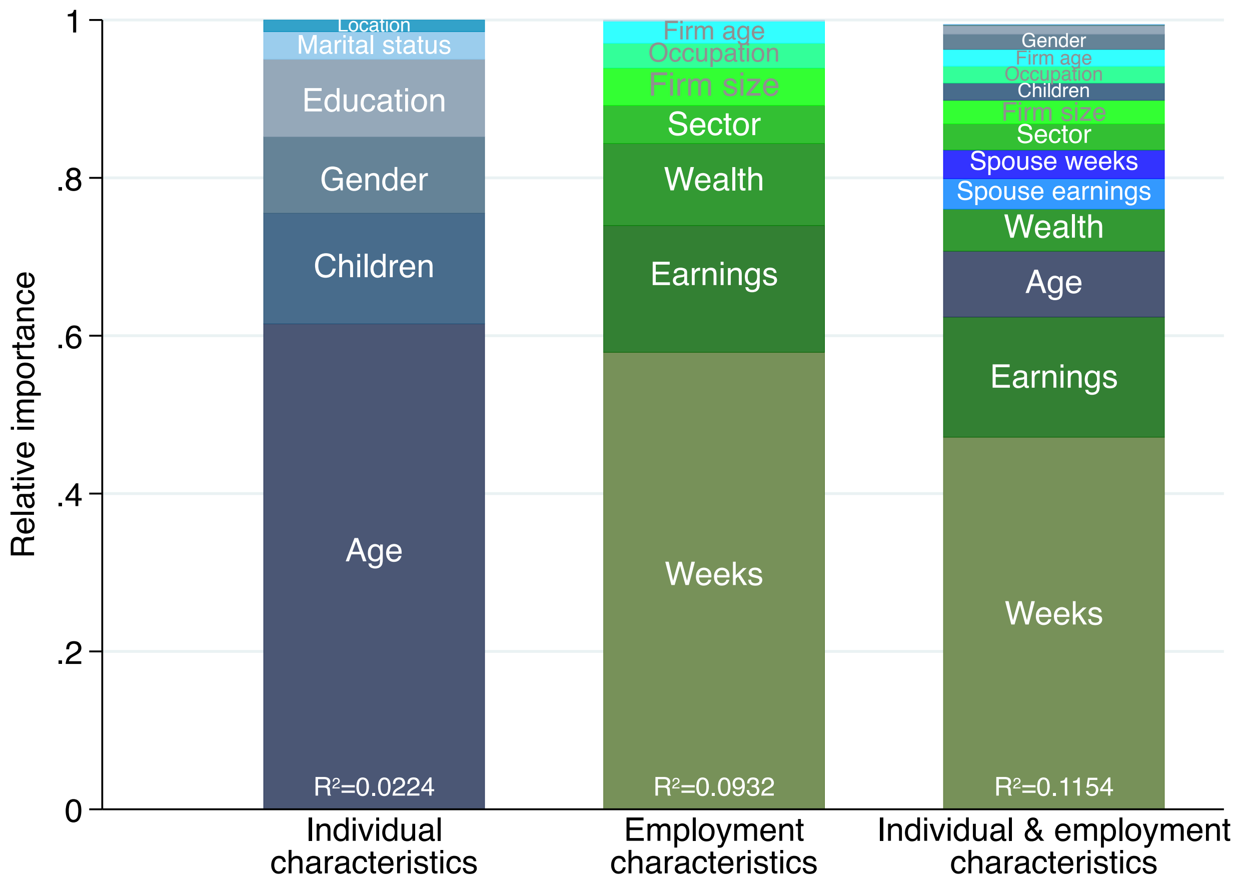


Figure A.23: Feature importance in explaining variation in elasticities

Notes: The figure plots the relative contribution of each feature in predicting labor supply elasticity. This is measured by first estimating labor supply elasticity at the individual level using the life-cycle DD design, matching each individual to a counterfactual constructed from all individuals with exactly the same set of characteristics. I then predict labor supply elasticity using the available set of characteristics and the random forest algorithm. The importance of each feature is then the gain in prediction achieved over all trees through splits using a given feature. The total gain is normalized to one, giving the relative importance of each characteristic in each model. R^2 is calculated through cross-validation, where model predictions using the training data are compared with actual values. All employment and job characteristics are pre-reform values as of 1986, except *weeks* which bundles the prediction gain using weeks worked in the three pre-reform years. This measure (*weeks*) serves as my measure of labor market attachment.

L Supplementary Tables

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

	Log labor earnings				Weeks worked			
	Wage earners		Self-employed		Wage earners		Self-employed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.373***	0.406***	0.484***	0.521***	2.337***	5.563***	10.127***	8.700***
	(0.027)	(0.036)	(0.057)	(0.074)	(0.787)	(1.076)	(2.180)	(2.623)
Reduced form (dy)	0.076***	0.078***	0.103***	0.106***	0.480***	1.062***	2.161***	1.772***
	(0.005)	(0.007)	(0.012)	(0.015)	(0.161)	(0.204)	(0.464)	(0.532)
First stage ($d\log(1-\tau)$)	0.205***	0.191***	0.213***	0.204***	0.205***	0.191***	0.213***	0.204***
	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
Mean of outcome variable	—	—	—	—	46.62	46.62	58.61	58.61
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Matching	No	Yes	No	Yes	No	Yes	No	Yes
Observations	448,592	448,232	78,363	78,226	441,961	441,602	78,477	78,339

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. Columns (1)–(2) and (5)–(6) report estimates for wage earners and columns (3)–(4) and (7)–(8) report estimates for the sample of business owners and workers with income from self-employment. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable (y) is defined in the top panel and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Elasticity of Total Weeks Worked

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.093***	0.090***	0.168***
	(0.026)	(0.026)	(0.035)
Reduced form ($d\log y$)	0.019***	0.019***	0.032***
	(0.005)	(0.005)	(0.007)
First stage ($d\log(1-\tau)$)	0.207***	0.208***	0.193***
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	515,232	515,232	514,737

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of total number of weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of total number of weeks worked. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Effect on Earnings and Employment-Related Income

Wages and salaries	93.7%
Fringe benefits, travel allowances, etc.	2.6%
Drivers' payments	0.7%
Gifts from employer	0.1%
Pension payment from employer	0.3%
Bonuses, sales commission, etc.	0.7%
Board remuneration	2.0%
Sum	100%

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

Table A.4: Effect of Tax-Free Year on Capital Income

	(1)	(2)	(3)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	310***	291***	272**
	(118)	(109)	(131)
Reduced form (dy)	64***	61***	53**
	(24)	(23)	(25)
First stage ($d\log(1-\tau)$)	0.207***	0.208***	0.193***
	(0.001)	(0.001)	(0.001)
Mean of outcome variable	72.34	72.34	72.34
Share of treatment effect on labor earnings	0.021	0.021	0.018
Controls	No	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	530,900	530,900	530,900

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is real taxable capital income in 1981$ and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is real taxable capital income in 1981$. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. "Share of treatment effect on labor earnings" refers to the ratio of the top row to a similar estimate of real labor earnings in 1981$. Robust standard errors clustered by individual are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.5: Effect of Tax-Free Year on Self-Employment

	(1)	(2)	(3)
2SLS DD ($\frac{dP}{d\log(1-\tau)}$)	0.104***	0.102***	0.155***
	(0.014)	(0.013)	(0.019)
Reduced form (dP)	0.021***	0.021***	0.030***
	(0.003)	(0.002)	(0.003)
First stage ($d\log(1-\tau)$)	0.207***	0.208***	0.193***
	(0.001)	(0.001)	(0.001)
Mean of outcome variable	0.149	0.149	0.149
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	530,900	530,900	530,397

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is an indicator for having income from self-employment and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is an indicator for having income from self-employment. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Effect of Tax-Free Year on Labor Earnings by Tax Brackets

	Lower-Middle		Upper-Middle		Top	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.484***	0.539***	0.286***	0.304***	0.236***	0.200***
	(0.037)	(0.042)	(0.020)	(0.029)	(0.016)	(0.033)
Reduced form ($d\log y$)	0.069***	0.072***	0.083***	0.084***	0.111***	0.087***
	(0.005)	(0.005)	(0.006)	(0.008)	(0.007)	(0.014)
First stage ($d\log(1-\tau)$)	0.142***	0.133***	0.293***	0.272***	0.467***	0.434***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matching	No	Yes	No	Yes	No	Yes
Observations	368,645	368,402	202,600	202,030	146,702	143,676

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Effect of Tax-Free Year on Weeks Worked by Tax Brackets

	Lower-Middle		Upper-Middle		Top	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	6.973***	9.437***	0.693	1.671	4.932***	5.571***
	(1.208)	(1.678)	(0.720)	(0.886)	(0.644)	(0.725)
Reduced form (dy)	0.987***	1.203***	0.203	0.465	2.301***	2.513***
	(0.170)	(0.213)	(0.211)	(0.247)	(0.300)	(0.326)
First stage ($d\log(1-\tau)$)	0.142***	0.133***	0.293***	0.272***	0.467***	0.434***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Mean dependent variable	45.99	45.99	47.85	47.85	47.09	47.09
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Matching	No	Yes	No	Yes	No	Yes
Observations	363,770	363,542	200,099	199,943	145,205	145,028

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is total number of weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.8: Tax Bracket DD: Labor Earnings – Top and Upper-Middle vs. Lower-Middle Brackets

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.232***	0.289***	0.233***
	(0.029)	(0.029)	(0.034)
Reduced form ($d\log y$)	0.037***	0.046***	0.034***
	(0.005)	(0.004)	(0.005)
First stage ($d\log(1-\tau)$)	0.160***	0.158***	0.147***
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	431,459	431,459	430,911

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.9: Tax Bracket DD: Weeks Worked, Top and Upper-Middle vs. Lower-Middle Brackets

	(1)	(2)	(3)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	3.100***	4.246***	3.268***
	(1.137)	(1.133)	(1.410)
Reduced form (dy)	0.497***	0.675***	0.482***
	(0.182)	(0.180)	(0.208)
First stage ($d\log(1-\tau)$)	0.160***	0.158***	0.147***
	(0.001)	(0.001)	(0.001)
Mean of outcome variable	49.79	49.79	49.79
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	520,438	520,438	425,579

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is total number of weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the total number of weeks worked. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.10: Effects of the Tax-Free Year on Extensive Margin — Robustness

	All		No Fishing Sector		No Tradable Sector	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.069***	0.058***	0.072***	0.060***	0.096***	0.066***
	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
Reduced form (dy)	0.008***	0.006***	0.008***	0.006***	0.010***	0.008***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
First stage ($d\log(1-\tau)$)	0.110***	0.110***	0.105***	0.106***	0.111***	0.115***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Mean dependent variable	0.672	0.672	0.659	0.659	0.707	0.707
Match-strata fixed effects	Yes	No	Yes	No	Yes	No
Individual fixed effects	No	Yes	No	Yes	No	Yes
Number of matched observations	576,571	576,571	548,347	548,347	447,559	447,559

Notes: The table presents results from life-cycle difference regressions, where each row and column entry corresponds to one regression estimate. "*No Fishing Sector*" excludes all firms and workers employed in the fishing sector, including both fishing and fish-processing. "*No Tradable Sector*" excludes all firms and workers employed in the tradable sector. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is employment and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents reduced form estimates based on equation (3). The bottom row presents first-stage regression estimates based on equation (3), where the outcome variable is the logarithm of one minus the average tax rate in columns. "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, the number of children, education, location indicator and percentile of income. The number of matched observations corresponds to observations for the treatment group. Robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.11: Heterogeneous Labor Supply Responses by Flexibility of Employment Arrangement

	Temporal flexibility		Constrained in in primary job		Hours flexibility	
	Low (1)	High (2)	Yes (3)	No (4)	Low (5)	High (6)
	A. Labor Earnings					
2SLS DD estimate	0.371***	0.451***	0.315***	0.520***	0.412***	0.500***
	(0.031)	(0.036)	(0.031)	(0.037)	(0.037)	(0.044)
Observations	526,458		526,458		526,458	
	B. Weeks Worked					
2SLS DD estimate	6.198***	5.191***	2.511***	7.744***	6.243***	8.580***
	(1.016)	(1.214)	(1.007)	(1.234)	(1.240)	(1.474)
Mean weeks pre-reform	51.64	45.09	53.29	44.25	48.26	47.72
Observations	519,941		519,941		519,941	

Notes: The table presents results from a 2SLS estimation of equation (2), where each row and column entry corresponds to one regression estimate. The dependent variable is indicated above each panel. Estimates by subgroups are obtained by interacting group indicators with the log of net-of-tax rate and the instrument in regression (2). *Temporal flexibility* splits the sample by a measure of relative variability in weeks worked within an occupation; see the main text for details. "Low" flexibility refers to workers below median of the distribution over the job flexibility measure and "High" refers those above median. *"Constrained in primary job"* is an indicator that equals one (*"Yes"*) if working 52 weeks in the primary job prior to the tax-free year, and zero (*"No"*) for those working 51 weeks or less. *Hours flexibility* splits the sample by occupations based on the share of workers with fixed-salary contracts, where "Low" share refers to occupation with a fixed-salary share below median of the distribution and "High" share refers to occupations above median. All regressions are weighted after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.12: Cross-Elasticities of Earned Income: Husbands and Wives

	All		Children age 0–6		Age (years)		Constrained in primary job	
	(1)	(2)	0 (3)	⩾ 1 (4)	60 < (5)	⩾ 60 (6)	Yes (7)	No (8)
	Husbands							
Cross-elasticity	-0.172***	-0.150***	-0.121***	-0.206***	-0.193***	-0.044	-0.199***	-0.088*
	(0.022)	(0.022)	(0.037)	(0.028)	(0.024)	(0.051)	(0.053)	(0.045)
IHS(spouse income)	–	-0.015***	–	–	–	–	–	–
	–	(0.001)	–	–	–	–	–	–
Observations	223,919	223,919	223,919		223,919		223,919	
	Wives							
Cross-elasticity	0.025	0.014	0.042	0.006	0.014	0.082	-0.184	0.208*
	(0.054)	(0.053)	(0.080)	(0.065)	(0.059)	(0.103)	(0.112)	(0.109)
IHS(spouse income)	–	0.032***	–	–	–	–	–	–
	–	(0.009)	–	–	–	–	–	–
Observations	102,283	102,283	102,283		102,283		102,283	

Notes: The table presents estimates of the earnings responses of married and cohabiting individuals to their spouse's net-of-tax rate. These cross-elasticities are estimated using the 2SLS estimation of the following modification of equation (2):

$$y_{it} = bracket_{i,t-1} + \delta_t + \varepsilon^{own} \cdot \log(1-\tau_{it}) + bracket^{spouse}_{i,t-1} + \varepsilon^{cross} \cdot \log(1-\tau^{spouse}_{it}) + \boldsymbol{X}'_{it}\gamma + \nu_{it}$$

where the dependent variable is the logarithm of the individual's labor earnings and the two endogenous variables, the individual's log net-of-tax rate and his spouse's log net-of-tax rate, are instrumented with an interaction between indicators of treatment status and the tax-free year for the individual and his spouse separately. The coefficient ε^{cross} identifies the cross-elasticity. Estimates by subgroups are obtained by interacting group indicators with the log of the net-of-tax rate of the individual and spouse as well as the respective instrumental variables. *"Constrained in primary job"* is an indicator that equals one (*"Yes"*) if working 52 weeks in a primary job pre-reform. All regressions control for age, education, whether living in the capital area or not, and the number of children aged 0–18 years. Column (2) includes the inverse hyperbolic sine (IHS) function of spouse's income, instead of in logs, to account for the possibility of the spouse's income being zero. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.13: Summary Statistics for the Icelandic Working-Age Population and Subsamples

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

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

Table A.14: Occupation Classification

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

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

Table A.15: Sector Classification

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

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

Table A.16: Education Classification According to Statistics Iceland's Education Register

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

Table A.17: Effect of Tax-Free Year on Labor Earnings: Predicted Tax Bracket

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.397***	0.401***	0.393***
	(0.027)	(0.027)	(0.026)
Reduced form ($d\log y$)	0.081***	0.081***	0.078***
	(0.005)	(0.005)	(0.006)
First stage ($d\log(1-\tau)$)	0.206***	0.205***	0.203***
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	311,736	310,982	311,673

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. Treatment status is assigned based on the predicted tax bracket in a given year; see the text for details. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.18: Effect of Tax-Free Year on Total Weeks Worked: Predicted Tax Bracket

	(1)	(2)	(3)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	6.710***	6.023***	6.467***
	(0.887)	(0.828)	(1.019)
Reduced form (dy)	1.367***	1.224***	1.292***
	(0.179)	(0.167)	(0.203)
First stage ($d\log(1-\tau)$)	0.206***	0.205***	0.203***
	(0.001)	(0.001)	(0.001)
Mean dependent variable	48.64	48.64	48.64
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	307,108	304,465	307,045

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is total number of weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the total number of weeks worked. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.19: Effect of Tax-Free Year on Labor Earnings: Continuous Bracket Position

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.331***	0.273***	0.356***
	(0.025)	(0.025)	(0.029)
Reduced form ($d\log y$)	0.112***	0.093***	0.117***
	(0.008)	(0.008)	(0.009)
First stage ($d\log(1-\tau)$)	0.338***	0.341***	0.329***
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	115,997	115,997	115,870

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. Treatment status is assigned to workers who remain in the same tax bracket for the three consecutive years prior to 1987, while excluding others. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.20: Effect of Tax-Free Year on Weeks Worked: Continuous Bracket Position

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	6.370***	5.596***	7.470***
	(0.748)	(0.737)	(0.988)
Reduced form ($d\log y$)	2.161***	1.916***	2.462***
	(0.253)	(0.251)	(0.324)
First stage ($d\log(1-\tau)$)	0.338***	0.341***	0.329***
	(0.001)	(0.001)	(0.001)
Mean dependent variable	49.01	49.01	49.01
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	114,117	114,117	113,990

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. Treatment status is assigned to workers who remain in the same tax bracket for the three consecutive years prior to 1987, while excluding others. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is total number of weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.21: Tax Bracket DD: Labor Earnings, Controls for 1988 Tax Rates

	(1)	(2)	(3)	(4)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.374***	0.404***	0.399***	0.279***
	(0.024)	(0.022)	(0.023)	(0.022)
Δ MTR$_{86,88}$	No	Yes	No	No
Δ Taxes$_{86,88}$	No	Yes	No	No
Δ ATR$_{86,88}$	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	526,955	526,955	526,955	526,955

Notes: The table presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. Δ MTR$_{86,88}$ is the log difference in the net-of-marginal tax rate between 1986 and 1988. Δ Taxes$_{86,88}$ is the inverse hyperbolic sine transformation of income tax payments between 1986 and 1988. Δ ATR$_{86,88}$ is the log difference in the net-of-average tax rate between 1986 and 1988, where the average tax rate is computed as the ratio of income tax payments and the income tax base. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.22: Tax Bracket DD: Weeks Worked, Controls for 1988 Tax Rates

	(1)	(2)	(3)	(4)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	4.926***	7.966***	5.334***	3.993***
	(0.784)	(0.723)	(0.736)	(0.742)
Δ MTR$_{86,88}$	No	Yes	No	No
Δ Taxes$_{86,88}$	No	Yes	No	No
Δ ATR$_{86,88}$	No	No	No	Yes
Controls	Yes	Yes	Yes	Yes
Mean of outcome variable	48.43	48.43	48.43	48.43
Observations	520,438	520,438	520,438	520,438

Notes: The table presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. Δ MTR$_{86,88}$ is the log difference in the net-of-marginal tax rate between 1986 and 1988. Δ Taxes$_{86,88}$ is the inverse hyperbolic sine transformation of income tax payments between 1986 and 1988. Δ ATR$_{86,88}$ is the log difference in the net-of-average tax rate between 1986 and 1988, where the average tax rate is computed as the ratio of income tax payments and the income tax base. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.23: Tax Bracket DD: Labor Earnings – Upper-Middle vs. Lower-Middle Brackets

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.325***	0.386***	0.337***
	(0.048)	(0.048)	(0.058)
Reduced form ($d\log y$)	0.036***	0.042***	0.033***
	(0.005)	(0.005)	(0.006)
First stage ($d\log(1-\tau)$)	0.111***	0.110***	0.099***
	(0.001)	(0.001)	(0.001)
Controls	Yes	Yes	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Matching	No	No	Yes
Observations	380,253	380,253	379,783

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Occupation and sector fixed effects are group dummies for occupation and sector groups. "Matching" refers to weighted regressions after coarsened exact matching on age and pretreatment marital status, the number of children, and education. Robust standard errors clustered by individual are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.24: Life-Cycle DD: Labor Earnings, Upper-Middle and Lower-Middle Brackets

	(1)	(2)	(3)
2SLS DD ($\frac{d\log y}{d\log(1-\tau)}$)	0.493***	0.490***	0.426***
	(0.001)	(0.001)	(0.001)
Reduced form ($d\log y$)	0.150***	0.149***	0.136***
	(0.003)	(0.003)	(0.003)
First stage ($d\log(1-\tau)$)	0.303***	0.303***	0.317***
	(0.001)	(0.001)	(0.001)
Match-strata fixed effects	Yes	Yes	No
Individual fixed effects	No	No	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Number of observations	250,762	250,762	232,264

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (3), where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of equation (3), where the outcome variable is the logarithm of one minus the marginal tax rate. "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, the number of children, education, location indicator, and percentile of income. Occupation and sector fixed effects are group dummies for occupation and sector groups. The number of observations corresponds to observations for the treatment group. Robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.25: Life-Cycle DD: Weeks Worked, Upper-Middle and Lower-Middle Brackets

	(1)	(2)	(3)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	2.210***	2.259***	1.024***
	(0.353)	(0.344)	(0.334)
Reduced form (dy)	0.673***	0.689***	0.326***
	(0.107)	(0.105)	(0.106)
First stage $(d\log(1-\tau))$	0.303***	0.303***	0.317***
	(0.001)	(0.001)	(0.001)
Mean dependent variable	48.15	48.15	48.15
Match-strata fixed effects	Yes	Yes	No
Individual fixed effects	No	No	Yes
Occupation fixed effects	No	Yes	No
Sector fixed effects	No	Yes	No
Number of observations	248,850	248,850	229,894

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable is total weeks worked and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (3), where the outcome variable is total weeks worked. The bottom row presents results from a first-stage DD estimation of equation (3), where the outcome variable is the logarithm of one minus the marginal tax rate. "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, number of children, education, location indicator, and percentile of income. Occupation and sector fixed effects are group dummies for occupation and sector groups. The number of observations corresponds to observations for the treatment group. Robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.26: Triple-Differences Estimation: Earnings, Weeks and Employment

	Earnings	Weeks	Employment
	(1)	(2)	(3)
2SLS DD	0.431***	2.439***	-0.007
	(0.008)	(0.291)	(0.004)
Reduced form	0.144***	0.816***	-0.002
	(0.003)	(0.098)	(0.001)
First stage	0.335***	0.335***	0.335***
	(0.002)	((0.002))	((0.002))
Mean dependent variable	–	48.85	0.917
Match-strata fixed effects	Yes	Yes	Yes
Number of matched observations	398,033	390,959	401,491

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of the following equation:

$$y_{ik} = \alpha_{ig} + \delta_k + bracket_{i,k-1} + \alpha_{ig} \times bracket_{i,k-1} + \beta_D D_{gk} + \beta_B B_{i,k-1} + \eta \cdot D_{gk} \times B_{i,k-1} + \boldsymbol{X}'_{ik}\gamma + \nu_{ik}$$

The coefficient of interest is η, which identifies the triple difference. This captures the variation in labor supply specific to the treated birth cohorts (relative to the control birth cohorts), for the workers in high-tax brackets (relative to those in low-tax brackets), during the tax-free year (relative to the year before). As before, the elasticity of labor supply is identified by estimating a version of the equation above that includes the logarithm of the net-of-tax rate $\log(1-\tau_{ik})$, which is then instrumented with the triple-difference interaction term $D_{gk} \times B_{i,k-1}$. The dependent variable is the logarithm of labor earnings and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation, where the outcome variable is the logarithm of labor earnings. The bottom row presents results from a first-stage DD estimation of the equation, where the outcome variable is the logarithm of one minus the marginal tax rate. "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, the number of children, education, location indicator, and percentile of income. The robust standard errors clustered at the match-strata level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A.27: Effect of Permanent Reform

	All		Men		Women	
	Earnings (1)	Weeks (2)	Earnings (3)	Weeks (4)	Earnings (5)	Weeks (6)
2SLS DD	0.424***	4.681***	0.038	-2.371*	0.606***	4.624
	(0.050)	(1.349)	(0.045)	(1.349)	(0.158)	(4.082)
Reduced form	0.046***	0.487***	0.006	-0.345*	0.032***	0.233
	(0.005)	(0.137)	(0.007)	(0.137)	(0.007)	(0.201)
First stage	0.103***	0.103***	0.145***	0.145***	0.050***	0.050***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Mean dependent variable	—	45.62	—	48.17	—	41.34
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	675,673	676,253	437,486	436,232	238,187	240,021

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The post-reform period is 1988–1990 and 1987 is dropped from the sample. The top row presents results from a 2SLS estimation of equation (2), where the dependent variable is defined in the top panel above each column and the net-of-tax rate is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (1). The bottom row presents results from a first-stage DD estimation of equation (1), where the outcome variable is the logarithm of one minus the marginal tax rate. Controls are gender, age, education, marital status, whether living in the capital area or not, and the number of children aged 0–18 years. Robust standard errors clustered by individual are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.28: Effects of the Tax-Free Year on Extensive Margin and Aggregate Labor Supply

	Log labor earnings		Weeks worked		Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
2SLS DD ($\frac{dy}{d\log(1-\tau)}$)	0.658***	0.639***	3.036***	2.469***	0.068***	0.058***
	(0.017)	(0.016)	(0.346)	(0.325)	(0.013)	(0.014)
Reduced form (dy)	0.150***	0.143***	0.672***	0.555***	0.008***	0.006***
	(0.003)	(0.003)	(0.077)	(0.073)	(0.001)	(0.001)
First stage ($d\log(1-\tau)$)	0.208***	0.209***	0.221***	0.224***	0.110***	0.110***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)
Mean dependent variable	—	—	38.37	38.37	0.672	0.672
Match-strata fixed effects	Yes	No	Yes	No	Yes	No
Individual fixed effects	No	Yes	No	Yes	Yes	No
Number of matched observations	537,240	537,240	532,664	532,664	587,332	586,321

Notes: The table presents results from difference-in-differences (DD) regressions, where each row and column entry corresponds to one regression estimate. The top row presents results from a 2SLS estimation of equation (4), where the dependent variable (y) is defined in the top panel and the net-of-tax rate ($\log(1-\tau)$) is instrumented with an interaction between indicators of treatment status and tax-free year. The middle row presents results from a reduced-form DD estimation of equation (3), where the outcome variable is defined in the top panel. The bottom row presents results from a first-stage DD estimation of equation (3), where the outcome variable is the logarithm of one minus the marginal tax rate in columns (1)–(4) and one minus the average tax rate in columns (5)–(6). "Match-strata fixed effects" refers to group fixed effects, where each group is a cell used in coarsened exact matching on age, gender and pretreatment marital status, the number of children, education, location indicator and percentile of income. The number of matched observations corresponds to observations for the treatment group. Robust standard errors clustered at the match-strata level are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.29: Details and Sources for Figure 9

Study	Label	Group	Variation	Notes
Intensive margin – Figure 9a				
Martinez, Saez, and Siegenthaler (2021)	MMS 20	Population	Taxes	Table 2, column (2)
Looney and Singhal (2006)	LS 06	Population	Taxes	Table 5, column (3). SIPP and NBER tax panel.
Saez (2003)	Saez 03	Population	Taxes	Table 5, column (3). Elasticity of wage income.
Bianchi, Gudmundsson, and Zoega (2001)	BGZ 01	Population	Taxes	Based on Table 6 and own calculations. See footnote in Section 7.1 for details.
Martinez, Saez, and Siegenthaler (2021)	MMS 20	Prime-age men	Taxes	Appendix Table A2, column (2).
French (2004b)	French 04	Prime-age men	Wages	Table 3 (median of estimates). PSID, Men.
Pistaferri (2003)	Pistaferri 03	Prime-age men	Wages	Table 2. Men aged 26–59.
Ham and Reilly (2002)	HR 02	Prime-age men	Wages	Table 1, column (4). PSID, men of age 23–60.
Lee (2001)	Lee 01	Prime-age men	Wages	Table 2. PSID, men aged 25–60.
Angrist (1991)	Angrist 01	Prime-age men	Wages	Table 2. PSID, men of age 21–64.
Altug and Miller (1990)	AM 90	Prime-age men	Wages	See Keane (2011) for calculation of elasticity. PSID, Household-heads of age 25–46.
Altonji (1986)	Altonji 86a	Prime-age men	Wages	Table 2, column (7). PSID, men aged 25–60.
Altonji (1986)	Altonji 86b	Prime-age men	Wages	Table 4, column (3). PSID, men aged 25–60.
Browning, Deaton, and Irish (1985)	BDI 85	Prime-age men	Wages	See Keane (2011) for calculation of elasticity.
MaCurdy (1981)	MaCurdy 81	Prime-age men	Wages	Table 1, column (1). PSID, men of age 25–46.
Angrist, Caldwell, and Hall (2020)	ACH 17	Uber drivers	Wages	Table 5, column (1).
Giné et al. (2017)	GMV 17	Boat owners	Wages	Table 6, column (3).
Saia (2017)	Saia 17	Pizza deliverers	Wages	Table A1.
Goldberg (2016)	Goldberg 16	Agricultural workers	Wages	Table 4, column (1). Standard errors calculated as elasticity is calculated by author.
Farber (2015)	Farber 15	Taxi drivers	Wages	Table 6.
Stafford (2015)	Stafford 15	Lobster hunters	Wages	Table 2.
Fehr and Goette (2007)	FG 07	Bicycle messengers	Wages	Table 3 and text. Average of two estimates.
Oettinger (1999)	Oettinger 99	Baseball stadium vendors	Wages	Table 6, column (5).
Extensive margin – Figure 9b				
Martinez, Saez, and Siegenthaler (2021)	MMS 18	Population	Taxes	Table 2, column (1).
Carrington (1996)	Carrington 96	Population	Wages	Calculated based on estimates in Table 2. See Chetty et al. (2013) for details.
Manoli and Weber (2016)	MW 16	Retirement-age	Pension	Table 3. Average across estimates within 12 months from threshold.
Brown (2013)	Brown 13	Retirement-age	Pension	Table 4, column (4).
Gruber and Wise (1999)	GW 99	Retirement-age	Taxes	Calculated using data reported in Table 1. See Chetty et al. (2013) for details.

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