

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.

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Keywords: Intertemporal labor supply, Frisch elasticity, Labor supply, 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 elas-

ticities 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.² 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

¹The 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.

²Examples of such frictions are adjustment costs (Cogan, 1981; Ham, 1982), hours constraints (Altonji and Paxson, 1988; Dickens and Lundberg, 1993), costs of changing jobs (Altonji and Paxson, 1992), inattentiveness (Chetty, Looney, and Kroft, 2009) and inertia (Jones, 2010).

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, 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 non-market 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.³ 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.⁴ First, Bianchi, Gudmundsson, and Zoega (2001) consider labor supply during the same tax-free year, but only among a small random sample of workers.⁵ 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

³Models 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).

⁴In Section 7 I provide a detailed summary and meta-analysis of previous work.

⁵Other 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).

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 pre-trends and macroeconomic shocks.

Second, in a contemporaneous study, [Martinez, Saez, and Siegenthaler \(2021\)](#) analyze the labor supply responses to a two-year tax holiday in Switzerland. This tax holiday resulted from the transition from an income tax system where current taxes depended on income in the previous two years to an annual pay-as-you-earn system. They estimate an average intensive margin elasticity of 0.025 and identify no extensive margin responses. My results suggest that all of the employment responses and a disproportionate proportion of the hours and earnings response to temporary tax cuts arise from young first-time workers and those close to retirement. Both groups are excluded from the analysis in [Martinez, Saez, and Siegenthaler \(2021\)](#). The remaining differences likely arise from differences in labor market flexibility. Flexibility in working hours, as measured by the cyclicalities 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.

In [Sigurdsson \(2021a\)](#) I present new 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

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. 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. It is by harnessing the differences that I identify the intensive margin of the elasticity of the labor supply. 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 In addition, taxes were only cut temporally for a single year, thereby allowing the study of labor supply responses during that year. A one-year change in incentives is the relevant frequency for a business cycle analysis of employment fluctuations. Because of all these features, the tax-free year comes close to being an ideal

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

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

⁸In 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.).

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

natural experiment to estimate the Frisch elasticities.

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

¹⁰For 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*.

¹¹As 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).

the flexibility to account for local conditions is greater. Therefore, despite this centralization, real wages are very flexible in Iceland when compared with many other European countries.

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

¹²Summary statistics are reported in Appendix Table A.12.

separately) and 52 weeks in total.

The reason why employers (and self-employed workers) were required to report the working time of their employees was twofold. First, the calculation of a worker's accident insurance fees depended on the number of weeks an employee worked during the year. This insurance covered accidents leading to a worker's injury or death that occurred on the job or on the way to or from work. The insurance fee, which was updated every year on January 1 and paid by the employer, varied from job to job and differed by occupation and the risk of injury and accidents in a given job.¹³ 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. However, in contrast to the insurance fee, this tax was independent of occupation and sector.¹⁴ Therefore, the number of weeks registered for workers on their pay slip 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 employment and labor input by sector and occupation for which official statistics are constructed, which places 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.¹⁵ 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

¹³As an example, in 1987, the insurance payment for a blue-collar factory worker per week worked amounted to about 0.14% of the worker's average monthly earnings, which was more than three times the weekly fee for office clerks.

¹⁴In 1987, this tax per week worked equaled 0.31% of the average weekly earnings of blue-collar workers.

¹⁵As 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. This method predicts actual tax liabilities with great precision. 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.

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 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%.¹⁶ 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.1](#). 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

¹⁶In 1986, the municipal tax rate ranged between 5% and 11.5%.

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.¹⁷ 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 Figure A.5), 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

$$y_{it} = \text{bracket}_{i,t-1} + \delta_t + \eta \cdot B_{i,t-1} \times \delta_{t=1987} + \mathbf{X}'_{it}\gamma + \mu_{it} \quad (1)$$

where y_{it} is the outcome of interest of individual i in year t , $\text{bracket}_{i,t-1}$ 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 \mathbf{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

$$y_{it} = \text{bracket}_{i,t-1} + \delta_t + \varepsilon \cdot \log(1 - \tau_{it}) + \mathbf{X}'_{it}\gamma + \nu_{it} \quad (2)$$

¹⁷Similar 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.

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.¹⁸ The results for both labor earnings and weeks worked are presented in Figure 4. The set 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.¹⁹ 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

¹⁸Appendix Figures A.2, A.3 and A.4 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 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.

¹⁹Figure 4 demonstrates how this identification strategy is useful in dealing with possible effects of macroeconomic shocks. As shown in Appendix Figure A.5, a negative macroeconomic shock in 1983 hit the Icelandic economy, related to a resource shock in the fishing sector and resulting in a drop in the real exchange rate. As Figure 4 illustrates, this macroeconomic shock did not differentially affect outcomes across the tax brackets in a statistically significant way.

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.

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).²⁰

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.²¹ 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

²⁰As reported in Appendix Table A.2, this implied elasticity is similar to that obtained from a specification for logarithms of weeks worked.

²¹About 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.

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 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.

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%.²²

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

²²These 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).

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 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.6 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 1987.²³

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.1), 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 C, 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.5 and A.6 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).²⁴ While the relative earnings response

²³In Figure A.6 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.

²⁴In 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

(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 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.

While the transition to a withholding-based tax system generated only a temporary incentive in 1987, the tax system also saw several permanent changes in 1988, announced in the spring of 1987. These changes may therefore have already influenced labor supply in 1987. In Appendix D I discuss whether and how the permanent reform in 1988 may affect the estimates and perform a range of tests to evaluate the robustness of the results, which all broadly support the main results reported earlier. Based on this and other analysis, I argue that the 1988 changes are unlikely to affect the results for two reasons. 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. In line with this, Appendix Figures A.2, A.3 and A.4 document that while the marginal tax rates changed between 1986 and 1988 for the top and bottom tax brackets, average tax rates across all brackets were broadly similar before and after.²⁵ Second, in addition to having been more complicated than the tax-free year to work out, these changes also appear to have been much less salient (Figure 2).

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. 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

A.7, this results in elasticity estimates between 0.232 and 0.289, similar to the bracket average in Table A.5. Studying the elasticity of weeks worked, as reported in Table A.8 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.

²⁵As explained in (Olgeirsson, 2013), this is in line with the intention of the government, which aimed for no change in average taxes and distribution of the tax burden.

counterfactual.²⁶

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.²⁷ 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.²⁸

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”.

²⁶Estimation of labor supply elasticities using grouping of individuals on similar life-cycle trends was pioneered by Ashenfelter (1984) and later applied by Angrist (1991) in a grouping instrumental variables approach. The method used in this section differs from this earlier work in that it combines cohort grouping and a natural experiment, where the former generates comparable groups on similar life-cycle trends and the latter provides the identifying variation.

²⁷This 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.

²⁸Cases 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.

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 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:

$$y_{ik} = \alpha_{ig} + \delta_k + \eta \cdot D_{gk} + \mathbf{X}'_{ik}\gamma + \mu_{ik} \quad (3)$$

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 \mathbf{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:

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

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.

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.²⁹ 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 0.12.³⁰

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

²⁹I 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.

³⁰Appendix E 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.5). 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.9 perform 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.7 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 as actual hours cannot be easily adjusted to a new desired level in the event of a wage change (Chetty, 2012). This implies that for many questions for which intertemporal labor supply and the size of the Frisch elasticity plays an important role—such as for understanding business cycle fluctuations in employment and evaluating the impact of a reform to public policies—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 and their macroeconomic impact.

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.

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.

While it is challenging to measure temporal flexibility and the prevalence of hours constraints, previous studies have found that workers value highly the flexibility of adjusting their work schedule (Chen et al., 2019, 2020) and have indirectly inferred that hours constraints often seem binding, as hours appear more flexible for job changers than stayers (Altonji and Paxson, 1988, 1992; Martinez-Granado, 2005). Ideally, one would like to observe individuals' employment contracts and compare the responses of workers who have the flexibility of deciding their hours to those locked into a particular wage–hours bundle. As such information is not available, I specify two proxies for temporal flexibility and hours constraints.

Motivated by the examples above, 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:

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

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.

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 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.10 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.³¹

³¹I have studied the heterogeneity in responses across firms, sectors, and occupations for numerous other factors that may influence workers' ability to adjust hours of work. Three results are worth mentioning. First, I find substantially larger elasticities among workers in smaller firms. This is consistent with the notion that workers are complements in production, and adjusting hours requires coordination ([Labanca and Pozzoli, 2018](#); [Battisti et al., 2016](#)), a process that is easier in smaller firms. Second, sector-level responses are larger in capital-intensive sectors and sectors where capital-labor complementarity is high, although the differences are not very pronounced. Third, workers in high routine task intensive (RTI) jobs ([Autor et al., 2003, 2006](#)) have larger elasticities than those in low RTI jobs, but the differences are much smaller than according to my measures of flexibility.

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

³²I 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.

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.9 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 be written in terms of its subcomponents as

$$\begin{aligned} E_T &= E_p + E_s \\ E_T &= E_p^{\text{Cont}} + \gamma \cdot (E_p^{\text{New}} - E_p^{\text{Cont}}) + E_s \end{aligned} \quad (6)$$

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

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

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 F](#) 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

³³Several 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.

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.

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

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

³⁵Cross-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.11 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.

³⁶The 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.

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 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

³⁷In line with this, Appendix Table A.11 reports that the cross-elasticities are negative and large for men who are constrained in their primary job, but not statistically significant for others.

³⁸See Appendix Table A.17 for details of the studies included in Figure 9. The figure attempts to provide an informative comparison rather than an exhaustive survey. Earlier, and in some cases more extensive, surveys of the literature on labor supply elasticities include Killingsworth (1983), Pencavel (1986), Blundell and MaCurdy (1999), Meghir and Phillips (2010), Keane (2011), Chetty (2012) and Chetty et al. (2013).

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.

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.7).

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 (see, e.g. [Heckman, 1993](#); [French, 2004](#); [Barrett and Hamermesh, 2017](#)).

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 messen-

³⁹Specifically, 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.

gers 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 employment in Alaska during an oil pipeline boom in the 1970s.⁴⁰ 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 between Iceland and Switzerland, such as the salience of the reform and the framing of its announcement, I argue on the basis of other 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.

Importantly, 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

⁴⁰It is interesting to note that the median age in the Alaskan population in 1970 was just 22.9 years (Carrington (1996), Table 1). Therefore, the estimate in Carrington (1996) is perhaps more comparable to that for young cohorts in Iceland.

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 G I collect several measures of labor market flexibility for OECD countries. Whether measured as worker flows (being the “fluidity” of the labor market), cyclical importance of hours per worker, or as wage flexibility or the flexibility of institutions, all measures are highly correlated (see Figure A.11). 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 cyclical importance of hours per worker, measuring the flexibility of hours, is more than twice as high in Iceland than in Switzerland, while flow rates in and out of unemployment in Iceland are three times higher.⁴¹

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 cyclical importance 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.

⁴¹Both the cyclical importance 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.”

Using economic theory and prior estimates, I can provide a prediction of the Hicksian elasticity implied by my estimate of the Frisch elasticity.⁴² 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):

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

where ρ is the intertemporal substitution in consumption (EIS), mpe is the marginal propensity to earn (MPE) out of unearned income, i.e. the income effect, and $\frac{A}{wh}$ is the ratio of wealth to labor income.⁴³ Appendix Figure A.12 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, 2019). In our calculations, we use an MPE of 0.11 implied by the estimates in Imbens, Rubin, and Sacerdote (2001).⁴⁴ 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.12. 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

⁴²Standard 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.

⁴³Similarly, via the Slutsky equation, the model yields the following relation between the Marshallian and the Frisch elasticities: $\varepsilon_{\text{Marshallian}} = \varepsilon_{\text{Frisch}} + \frac{A}{wh} \cdot mpe(1 - \rho \cdot mpe)$.

⁴⁴While 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, 2019).

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

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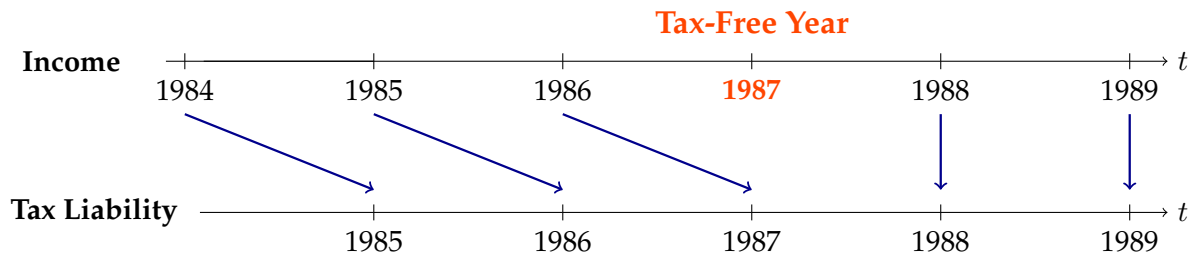


Figure 1: Income tax system before and after the tax reform

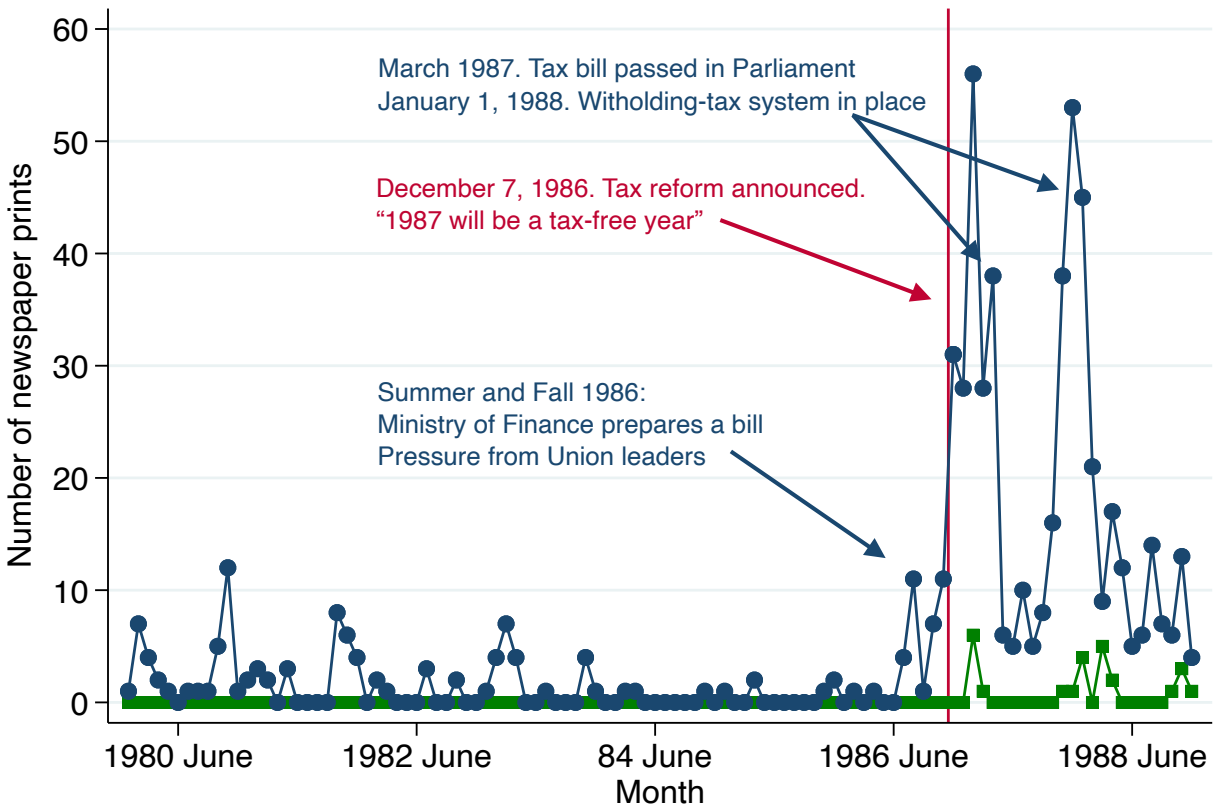
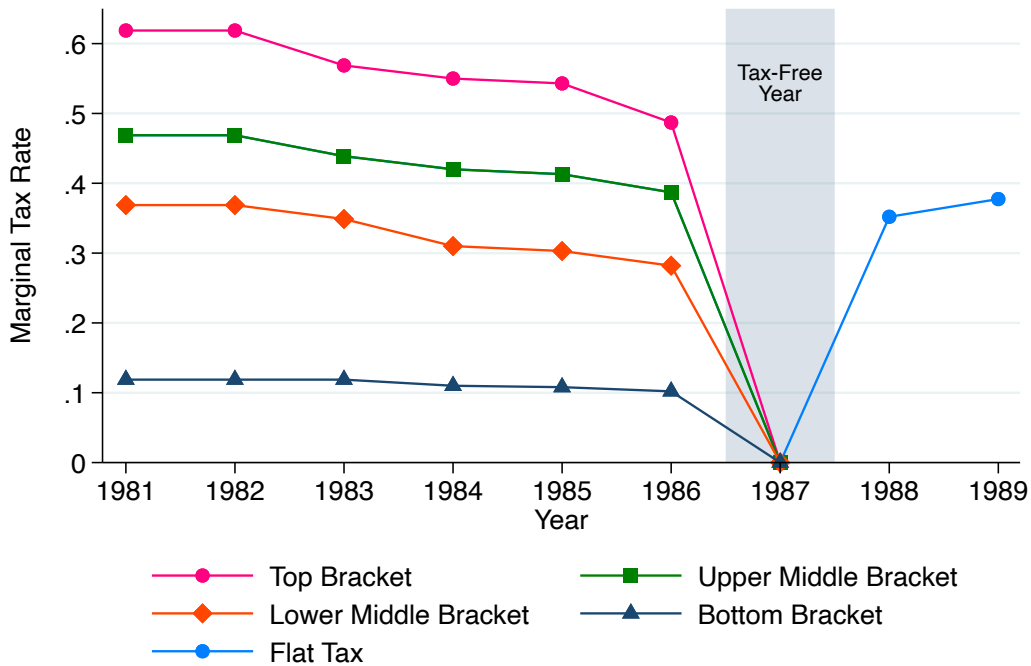
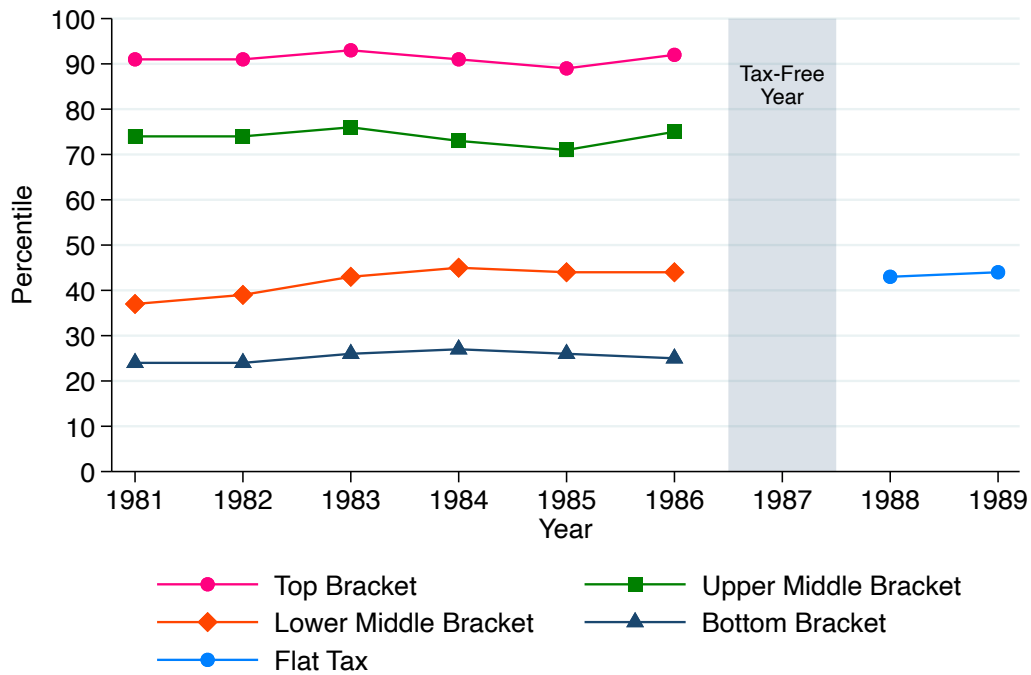


Figure 2: Number of printed newspapers mentioning withholding tax

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 skattlutfall", "eitt skattþrep" and "flatur skattur". The count is based on searches in the Icelandic newspaper database [Tímarit.is](http://timarit.is) for the six main newspapers (*Alþýðublaðið*, *Dagblaðið Visir (DV)*, *Dagur*, *Morgunblaðið*, *Tíminn*, *Þjóðviljinn*). The total number of printed newspapers per month is about 145 on average.



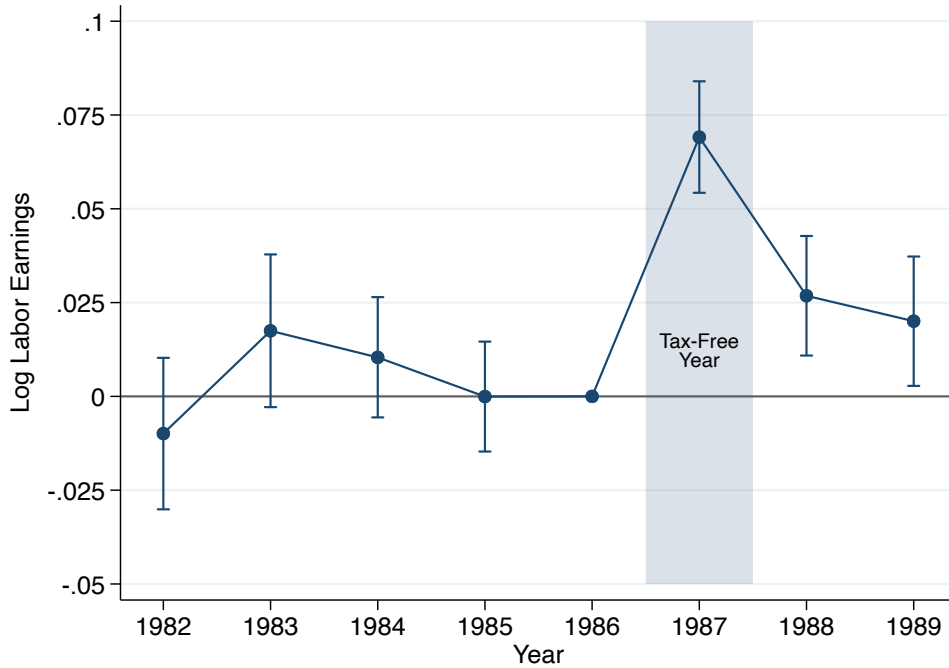
(a) Marginal tax rate by tax bracket



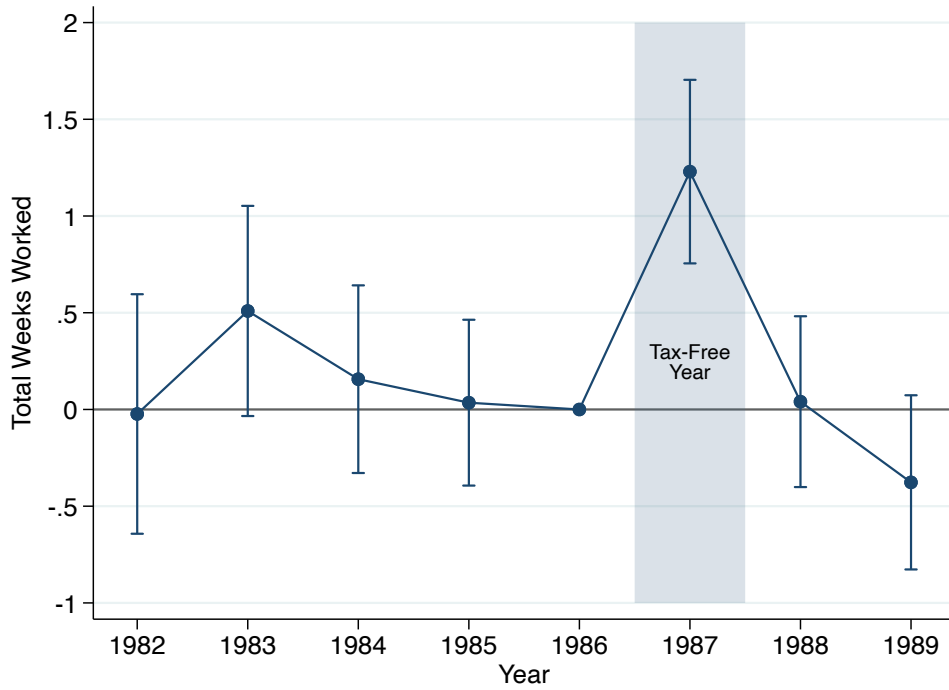
(b) Tax bracket thresholds in percentiles of income

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

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

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.2, A.3 and A.4 provide a graphical presentation of the reduced-form evidence and the first stage.

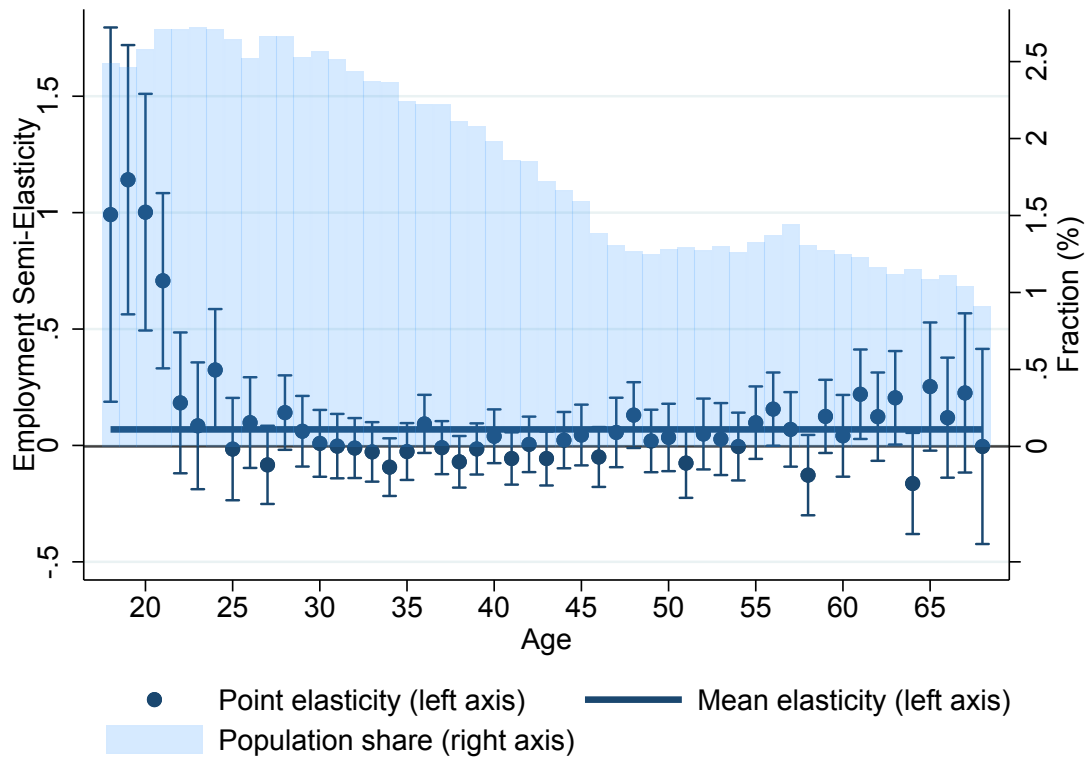
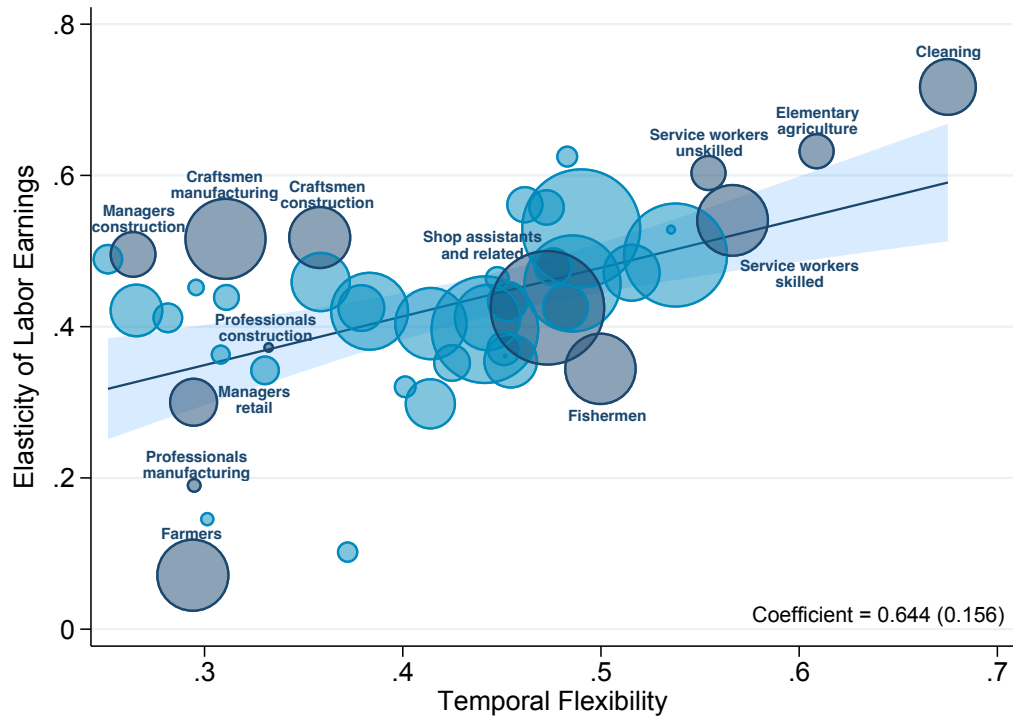
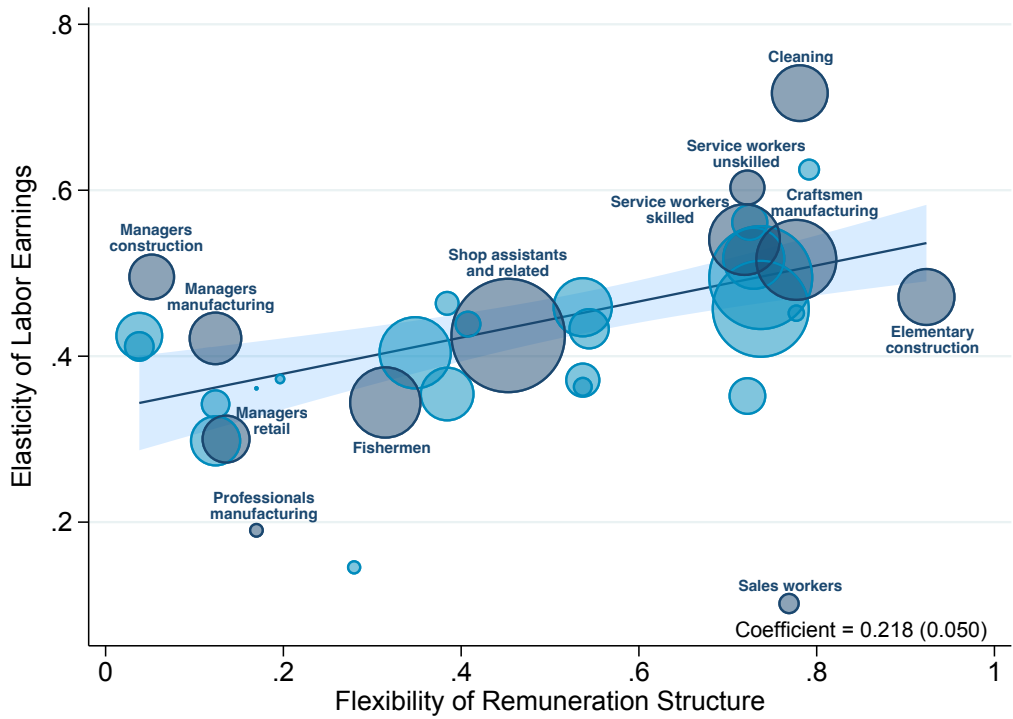


Figure 5: Employment semi-elasticity (extensive margin) by age

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.16. 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.

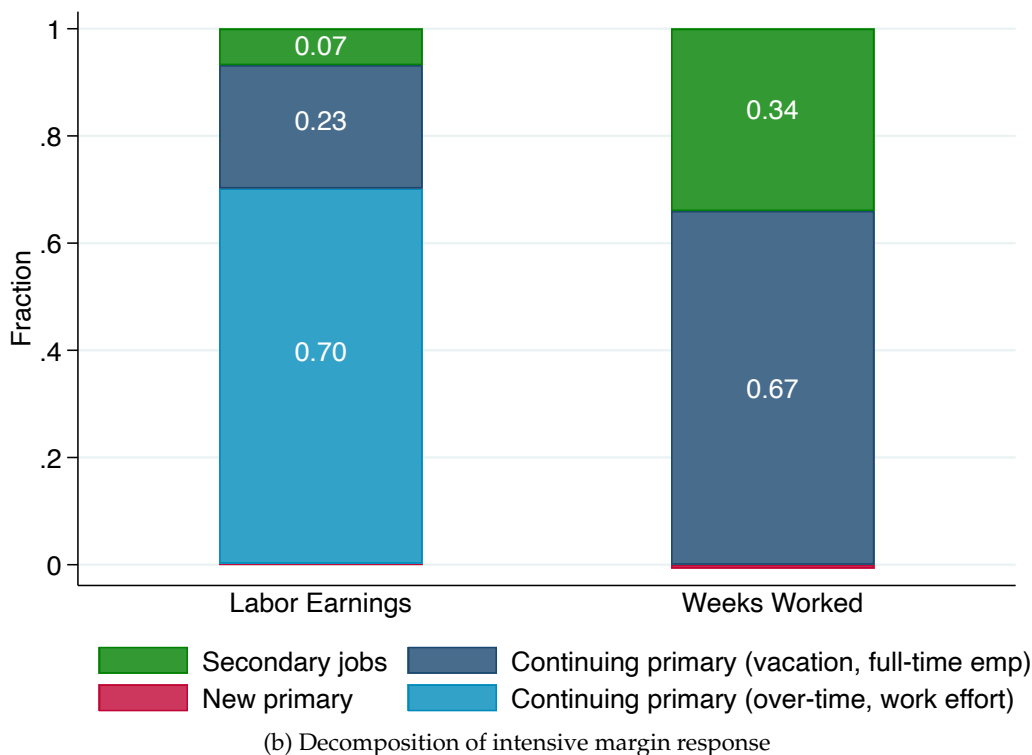
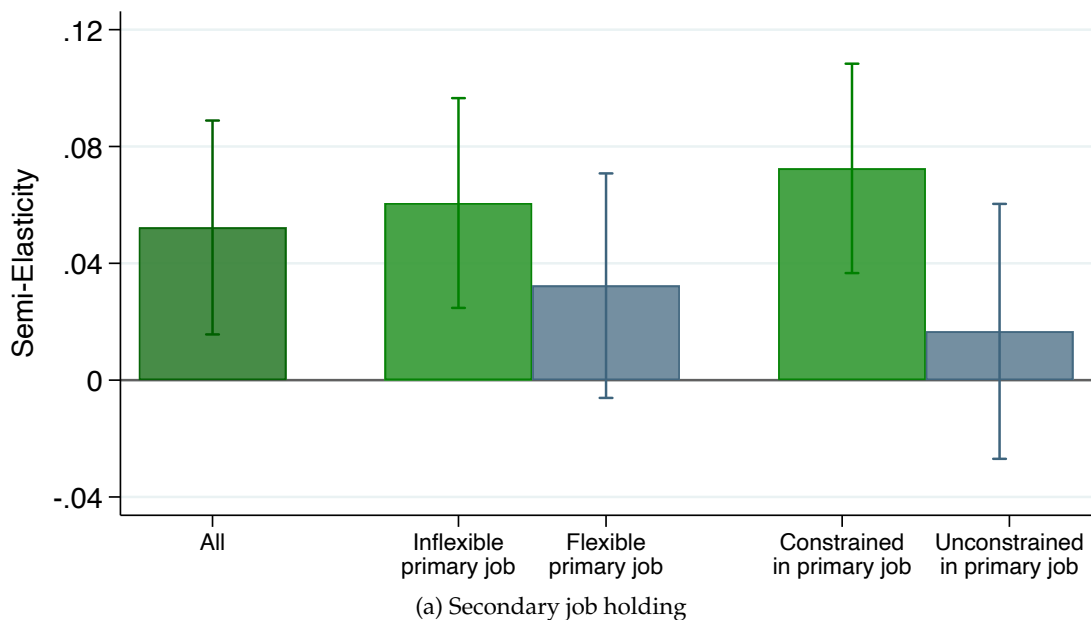


Figure 7: Take-up of secondary jobs and decomposition of intensive margin responses

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.

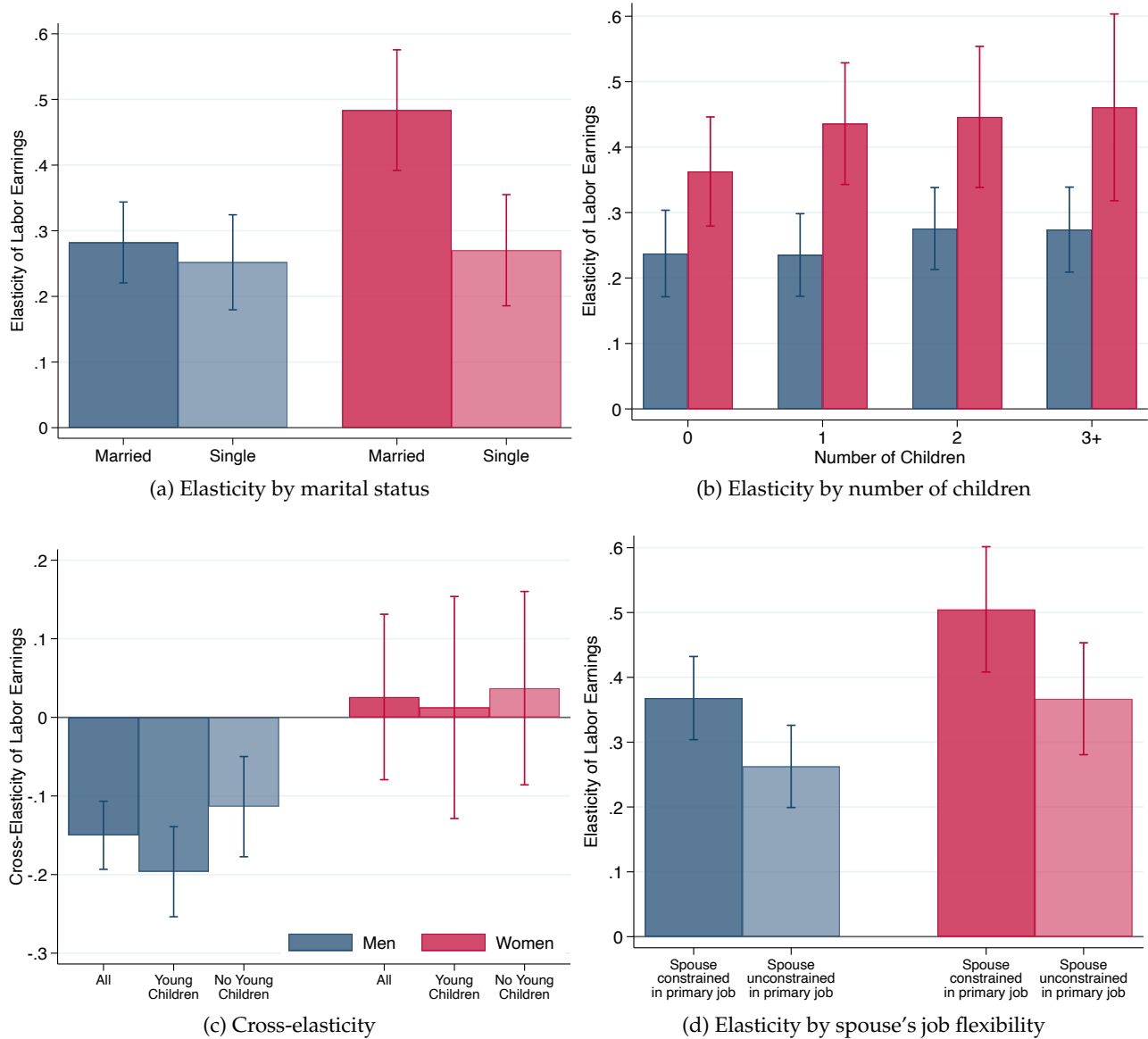


Figure 8: Elasticity of labor earnings of men and women by family status

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):

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

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 $\varepsilon^{\text{cross}}$ identifies the cross-elasticity. “Young children” refer to children aged 0–6 years. Appendix Table A.11 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.10 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.

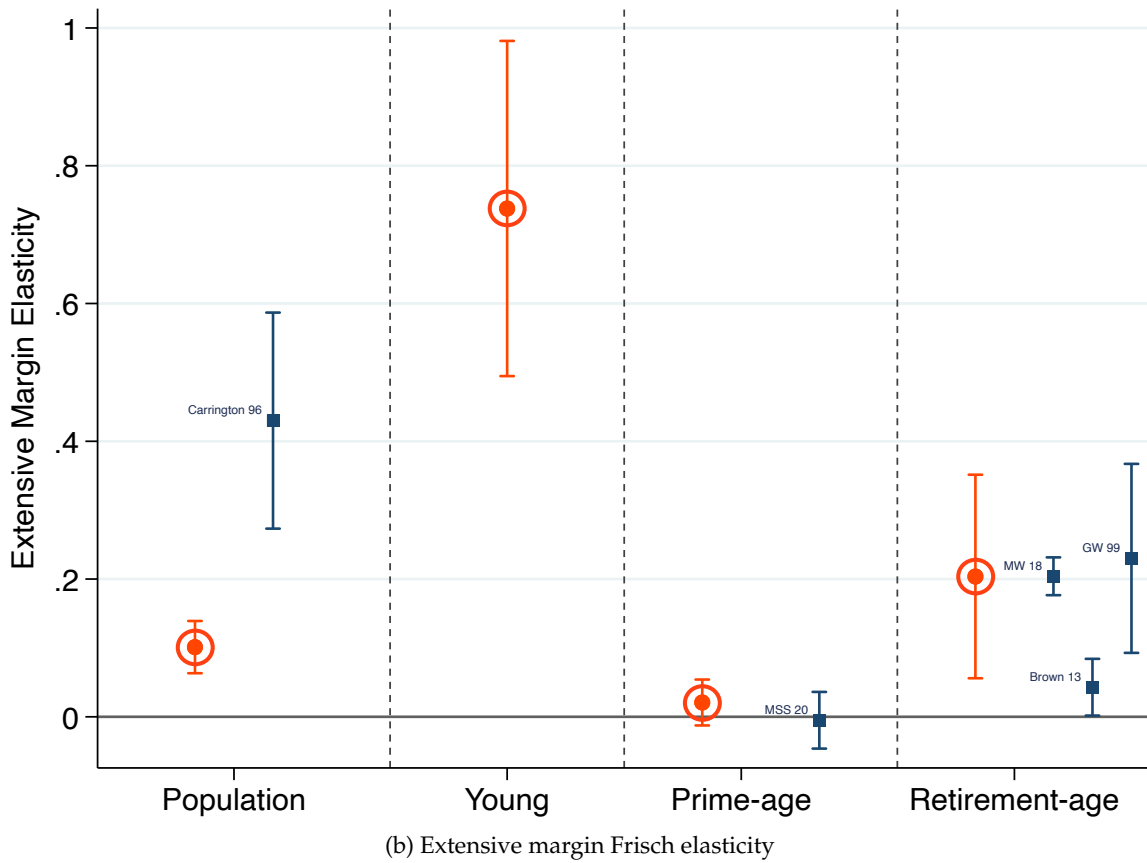
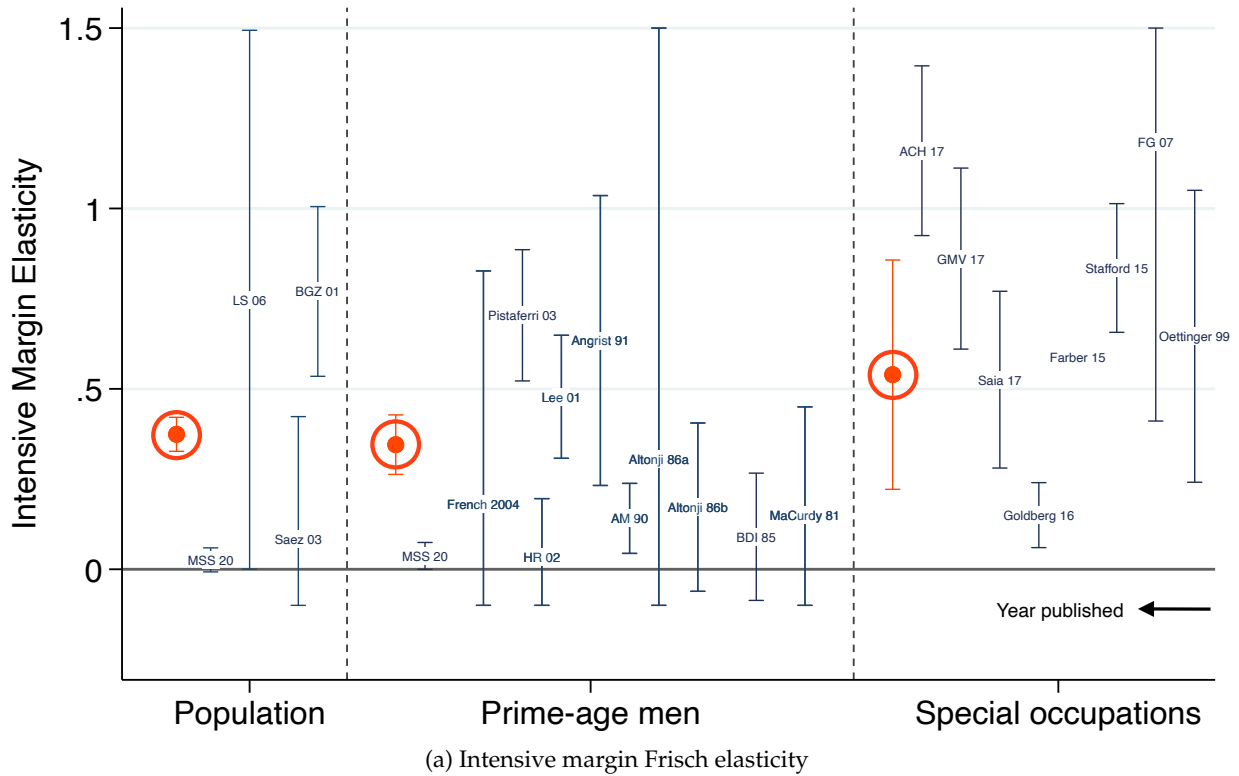


Figure 9: Summary of Frisch elasticity estimates

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.17.

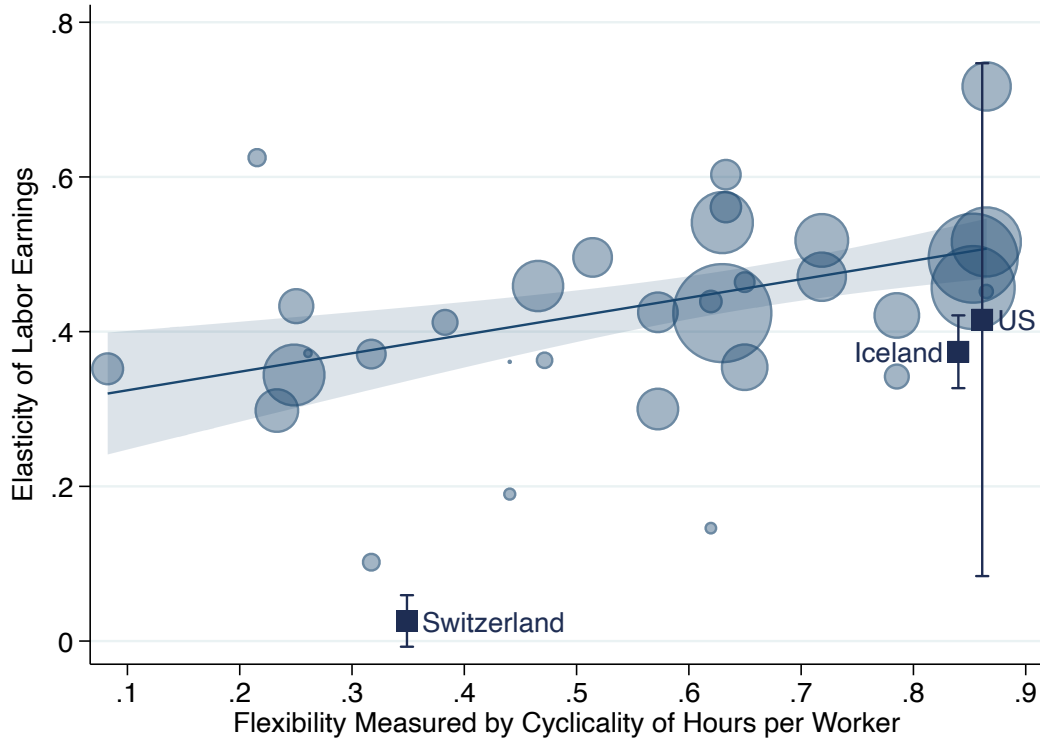


Figure 10: Intensive margin Frisch elasticity across countries and occupation by flexibility in hours

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 cyclical correlation 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 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*** (0.024)	0.401*** (0.032)	4.926*** (0.784)	6.549*** (1.074)	-0.033 (0.024)	0.030 (0.030)
Reduced form (dy)	0.077*** (0.005)	0.077*** (0.006)	1.023*** (0.162)	1.267*** (0.207)	-0.004 (0.003)	0.004 (0.002)
First stage ($d \log(1 - \tau)$)	0.207*** (0.001)	0.193*** (0.001)	0.207*** (0.001)	0.193*** (0.001)	0.127*** (0.001)	0.119*** (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*** (0.014)	0.058*** (0.014)	0.415*** (0.069)	0.396*** (0.073)	0.015 (0.012)	0.015 (0.012)	0.115*** (0.043)	0.025 (0.040)
Reduced form (dy)	0.008*** (0.001)	0.006*** (0.001)	0.024*** (0.004)	0.023*** (0.004)	0.002 (0.002)	0.002 (0.002)	0.010*** (0.004)	0.002 (0.004)
First stage ($d \log(1 - \tau)$)	0.110*** (0.001)	0.110*** (0.001)	0.058*** (0.003)	0.059*** (0.003)	0.130*** (0.001)	0.130*** (0.001)	0.087*** (0.003)	0.088*** (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$