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# The Elasticity of Taxable Wealth:
## Evidence from Norway

**Anders H. Bjørkli and Espen N. Arntsen**

**Supervisor: Floris T. Zoutman**

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# Abstract

Using Norwegian administrative household panel data from 2009-16, we investigate the effects of wealth taxation on taxable wealth. The wealth tax reform of 2014-15 offers a compelling quasi-experiment, which enables us to isolate the tax rate variations of the households. We estimate the net-of-tax rate elasticity of taxable wealth to be 2.24 in aggregate, using a Difference-in-Difference framework. Compared to existing studies from various countries, our research indicates that the Norwegian taxation of wealth is not very distortive.

# Preface

This thesis is written as a part of our Master of Science degree in Financial Economics at the Norwegian School of Economics (NHH). We have been fortunate to get the opportunity to research such an important and relevant topic, and we have learned a great deal through this process. We are perhaps most pleased with the opportunity to work empirically with a rich data set and the knowledge that we have acquired about Norwegian tax policies.

We wish to express our deepest gratitude to our supervisor, Associate Professor Floris T. Zoutman. He has provided us with valuable insight, guidance, and positivity throughout the process. We greatly appreciate his expertise and the time he has dedicated. We would also like to express our gratitude to the Norwegian Tax Authorities and the Norwegian Centre for Taxation (NoCET) for the grant opportunity.

Lastly, we would like to thank our families, friends, and fellow students for providing us with an environment where accomplishment and failure are both accepted.

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Espen Nilsskog Arntsen & Anders Hoff Bjørkli

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# 1 Introduction

The distortionary effects of wealth taxation on savings and wealth accumulation is a question that has been receiving growing attention in academia. Quantifying behavioral responses to wealth taxation is crucial in determining optimal taxation policies (Saez and Stantcheva 2018) — especially considering that today's wealth-income ratios appear to return to the high values of the 18th century (Piketty and Zucman 2014). The dispersion in wealth distribution and increases in wealth inequality further fuels the motivation to research the topic. In the 18th century, in countries like France, Great Britain, and Sweden, the top 10-group in the wealth distribution possessed roughly 80-90 percent of all the private assets. This share declined sharply in the 19th century until 1970, where the concentration seemed to increase again (Piketty 2014). Considering that income from capital is not as evenly distributed as labor income, optimal taxation policies are paramount. Governments seek a perfect balance; significant inequality in wealth distribution is undesirable. At the same time, taxes that are too high might motivate individuals to emigrate or evade their wealth into tax havens, leaving the country with less tax revenues. Even though the possibilities are limited for the general population, they are more available for the wealthiest because of an industry designed to help this small group (Alstadsæter, Johannesen and Zucman 2019a).

Over the last three decades, the international trend has been to reduce and remove wealth taxation. In 1990, 12 European countries had an annual wealth tax. In 2019 this number was down to three: Norway, Spain, and Switzerland (Edwards 2019). In Norway, the wealth tax debate became especially prominent when Solberg and Høyre, in the 2013 election campaign, advertised a massive reduction in the wealth tax. The polarization between the left- and right-wing of the political spectrum grew. The left side argues that wealth tax is a fair and effective way to reduce inequality (Arbeiderpartiet 2021; SV 2021; Rødt 2021). The right side argues that the potential distortions of investments and savings are harmful to Norwegian ownership (Høyre 2021; FrP 2021).

There is a long-standing ambiguity of the savings response to rate-of-return shocks induced by wealth taxation. This is due to the countering income and substitution effects (Zoutman 2015; Ring 2020). Taxes reduce the return on savings. If this leads to the individual's required rate of return not being met, they might substitute savings by consumption today; the so-called substitution effect. The income effect works in the opposite direction; individuals

might be motivated to save in order to maintain future consumption since taxes erode future returns (NOU 2018:5). Potential data limitations make it difficult to separate real saving responses from tax evasion. It is also difficult to find compelling variations in the wealth tax that allow estimating causal effects. When assessing the desirability of the wealth tax, policymakers are primarily interested in the long-run effect. Due to the slow-moving nature of wealth, this has shown to be challenging (Ring 2020; Jakobsen, Jakobsen, Kleven, and Zucman 2020).

Studies report that an increase in wealth tax reduces taxable wealth. Their estimates mainly signal changes in reporting behavior, inter-regional migration, or tax avoidance by placing capital in favorable assets (Zoutman 2018; Seim 2017; Brülhart, Gruber, Krapf, and Schmidheiny 2019; Jakobsen et al. 2020; Durán-Cabré, Esteller-Moré, and Mas-Montserrat 2019; Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Foremny, and Martínez-Toledan 2021). However, Ring (2020), when limiting the roles of evasion, finds that households increase savings when subjected to wealth taxation. An indication that the income effect possibly outweighs the substitution effect; households presumably save to meet future tax liabilities. His findings are partially substantiated by Durán-Cabré, Esteller-Moré, and Mas-Montserrat (2019), who find slightly positive saving responses when filtering out tax avoidance and mechanical effects.

In this master thesis, we aim to contribute to the existing empirical research on the elasticity of taxable wealth – especially since there has only been one Norwegian contribution (Ring 2020). The government levies wealth tax on households with wealth net of debt above the threshold of each year's specific tax assessment rules. We have panel data of the Norwegians household's tax returns from 2009-16. The panel structure enables us to control for unobserved heterogeneity between the households. During the first half of this period, the tax authorities increased the wealth tax liability for the wealthy through a higher valuation of assets, especially primary and secondary housing. The second half is characterized by tax rate reliefs and significant increases in the yearly thresholds. The tax reform of 2014-15 provided a great relief in the tax rate and a substantial increase in the threshold. The tax authorities reduced the rate from 1 to 0.85 percent, increased the threshold from 1 to 1.2 mNOK, and some various valuation rules changes. We utilize this reform to isolate the wealth tax variation for the households. The preferred method is a Difference-in-Difference estimator. This enables us to regress households' wealth on their tax rate changes and capture the

potential distortionary effect of wealth taxation. The tax rate changes of the households are heterogeneous, and they constitute a continuous treatment variable, with the run-of-the-mill control group receiving zero change.

The tax rate changes a household could experience in the reform are directly linked to wealth development from 2014 to 2015. Exogenous shocks to the household's wealth might affect whether the household experience a changed tax rate. This would entail a correlation between the unobserved determinant of wealth and the tax rate change. This is a case of reverse causality and would be detrimental in the interference of causal effects. In the spirit of Gruber and Saez (2002), we overcome these potential endogeneity problems by constructing an instrument for the counterfactual wealth tax change. We keep the behavior of the household constant by implementing the 2015 tax system on the 2014 assets, and after that, calculating the tax rate change from 2014 to the 2015 tax system. This is our identification strategy; it relies on the notion that it is solely the changed tax environment that changes the household's tax rate. Because of this strategy, the instrument is plausibly exogenous. The method enables us to estimate the net-of-tax rate elasticity of taxable wealth, which measures how sensitive wealth is to a 1 percentage increase in the net-of-tax rate.[1]

Our research question is as follows:

*What is the elasticity of taxable wealth?*

Based on existing empirical studies, we expect to find a negative relationship between an increase in the tax rate and taxable wealth, i.e., a positive net-of-tax rate elasticity. Moreover, based on the favorable tax policy changes of the 2014-15 reform, we assume that savings and wealth accumulation became more desirable than before.

We divide the thesis into 8 chapters. Chapter 2 presents the theoretical framework and offers reviews of existing studies. Chapter 3 describes the data set used in the analysis. Chapter 4 presents the methodology. Chapter 5 provides a descriptive overview of the data with statistics on the whole data sample and treatment and control groups. Chapter 6 contains the results, discussion, comparison to other studies, and robustness check. Chapter 7 accounts for potential limitations. Chapter 8 contains our conclusion based on the conducted research.

[1] A percentage increase in the net-of-tax rate (1-T), i.e., the share of wealth kept by an individual after tax.

# 2 Theoretical Framework

This chapter intends to give a deeper understanding of the Norwegian taxation of wealth and offers information on the institutional setting. Since we are using the reform of 2014-15 to isolate the wealth tax variation of the households, it is of particular interest to examine the changes made by the policymakers. We will also review recent research on the impact of wealth tax on savings and wealth.

## 2.1 Institutional Setting

Chapter 4. in the Taxation Act regulates the Norwegian wealth tax.[2] The main rule of the act cf. section 4-1 (1) defines how the individual's taxable wealth is determined. The law states that the "taxable wealth shall be calculated as the sales value as of 1 January of the tax assessment year of any assets of the taxpayer that are of economic value, less any debts for which the taxpayer is liable**.**"[3] Section 2-1 (7) states that "the liability to pay net wealth tax is conditional upon the taxpayer being a resident in Norway on 1 January of the year in which the tax is assessed."

The government levies wealth tax only on individuals; limited liability companies and publicly listed companies are exempt from this liability (Olsen and Vigdal 2018). The tax applies to all wealth of the Norwegian resident, regardless of whether the individuals choose to invest in foreign assets. The wealth tax is divided into two parts: one goes to the state, and the other goes to the municipality. The Norwegian Parliament yearly revises the tax rate and the valuation rules, and they determine the maximum rates for the municipalities. The municipality has autonomy over its part of the tax rate. In practice, the municipalities tax wealth with the highest possible rate given the current legislation (Zimmer 2012).

Table 1 below presents the various tax rates, thresholds, and the tax value of different asset classes from 2005-21. For spouses who are assessed jointly on their wealth, the threshold is twice as large.[4] Both single taxpayers and spouses with taxable wealth below the year-specific activation threshold will not face wealth tax obligations. Wealth above the threshold is taxed

[2] We have used an unofficial translation of the Act of 26 March 1999 No. 14 relating to the Taxation of Net Wealth and Income (Taxation Act)**,** made by PwC. Last updated on December 20, 2020. https://min.rettsdata.no/Dokument/gLENG19990326z2D14

[3] The value of debt, like wealth, is calculated as at the value of January 1.

[4] Doubling of the threshold was introduced in 2006.

at the rate of the corresponding year. Assets included in the tax assessment are, for example, primary housing, company shares (listed and unlisted), secondary homes, leisure homes, commercial real estate, cash, and intangible assets. Aggregated debt is deducted on the value of gross wealth, not only debts for which the taxpayer's items are pledged. Meaning, all debt is deductible, including student loans and consumer loans.[5] It is irrelevant whether the debt carries an interest obligation. Wealth and debts located in Norway are mainly third-party reported, while wealth held abroad is primarily self-reported (Skatteetaten(b) n.d.).

The tax value of different assets varies, and the tax system offers significant tax rebates of some assets, especially primary housing. Taxation Act section 4-19 (1) a) states that if an individual has assets subjected to valuation rebate, there shall be a similar rebate in the value of debt associated with these assets. The section excludes primary homes, but it includes secondary homes and commercial real estate. Households with wealth mainly in primary housing will often have low or negative taxable wealth due to the deductibility of debt at market value.

Section 4-12 in the Taxation Act regulates the tax value of shares in listed and non-listed companies. The government offered substantial relief in the tax assessment value of these assets from 2017. The Ministry of Finance (2015) proposed that this would equalize the distortionary effect of asymmetric tax valuation of assets and motivate value-added investments. The market value of listed shares is found on the last trading day prior to the tax assessment year. The tax value of non-listed shares is found on January 1, the year before the tax assessment year. Cf. section 4-12 (2) the tax value of a non-listed share is "the proportion of total tax value of the private limited company as of January 1 the year before the tax assessment year attributable to each share, on the basis of the nominal value of such a share."[6] Investing in non-listed shares is a popular tax avoidance strategy because they are valued at book value, while listed shares are valued at market value. Bjørneby, Markussen, and Røed (2020) show that firm-owners reduce their taxable wealth by investing in human capital because it does not show up on the firm's balance sheet. Globel and Hestdal (2015) estimate

[5] There are some exceptions. See chapter 4. section 4-3 in the Taxation Act.
[6] Cf. Taxation Act section 4-13 (1): if in the year before the tax assessment year a capital increase has been made, or a reduction, then there would be an exception from this rule. As listed companies, one will value the share of ownership in a private limited company at as 1 January in the tax assessment year.

that the average tax valuation rebate for non-listed firms traded at the Norwegian OTC-list is 68.1 percent, making them lucrative as a tax avoidance vehicle.[7]

The wealth tax system has undergone two distinct waves of reforms in the last 16 years. The period 2005-11 brought increases in the wealth tax liability for the wealthy through increases in the valuation of housing, listed/non-listed shares, and business assets. Before the tax reform of 2009-10, housing's tax valuation was based on historical cost, with an annual adjustment from the previous years' tax value (Bjørneby, Markussen, and Røed 2020). This valuation method led to a greater difference between real wealth and taxable wealth, mostly since older housing was undervalued relative to newer housing. This was why the government changed it to estimated sales value based on comparable housing in the same geographical area from 2010 and onward (Ministry of Finance 2009).[8] During 2013-17, the government gave significant reliefs in the tax rate. Furthermore, they reduced the number of households liable to the tax by a more substantial annual increase in the threshold than before. These changes were favorable for households across the wealth distribution.

[7]The OTC-list (over-the-counter) is a marketplace for non-listed shares. Their tax value is according to book value, but they are traded at market value. This enabled Hestdal and Globel (2015) to estimate the average rebate.

[8] The estimated sales value shall be "calculated by multiplying the square meter area of the residence by a square meter rate," cf. Taxation Act 4-10 (4). The square meter rate is set by Statistics Norway.
From the tax year 2021, according to section 4-10 (1), the value of real estate may be put below its estimated sales value. This means that the taxpayer can demand that primary and leisure homes' tax value be reduced to the real market value (documented sales value) instead of estimated sales value. The same applies to the tax value of secondary homes and commercial real estate (Skatteetaten(a) n.d; Taxation Act section 4-10 (2), (3) and (4)).

**Table 1: Wealth Tax Rates, Thresholds, and Valuation Rules**

| | Tax rates and thresholds | | | | Tax valuation of assets<br>PY: % adjustment from previous year's tax assessment value<br>MV: % of assessed market value | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | Tax Rate 1, in % | Threshold NOK | Tax Rate 2, in % | Threshold 2 NOK | Primary home | Leisure home | Secondary Home | Business properties | Listed and unlisted shares |
| 2005 | 0.9 | 151 000 | 1.1 | 540 000 | PY: 0 | PY: 0 | PY: 0 | PY: 0 | MV: 65 |
| 2006 | 0.9 | 200 000 | 1.1 | 540 000 | PY: 25 | PY: 25 | PY: 25 | PY: 25 | MV: 80 |
| 2007 | 0.9 | 220 000 | 1.1 | 540 000 | PY: 10 | PY: 10 | PY: 10 | PY: 10 | MV: 85 |
| 2008 | 0.9 | 350 000 | 1.1 | 540 000 | PY: 10 | PY: 10 | PY: 10 | PY: 10 | MV: 100 |
| 2009 | 1.1 | 470 000 | | | PY: 10 | PY: 10 | PY: 10 | PY:60/MV:40 | MV: 100 |
| 2010 | 1.1 | 700 000 | | | MV: 25 | PY: 10 | MV: 40 | MV: 40 | MV: 100 |
| 2011 | 1.1 | 700 000 | | | MV: 25 | PY: 0 | MV: 40 | MV: 40 | MV: 100 |
| 2012 | 1.1 | 750 000 | | | MV: 25 | PY: 10 | MV: 40 | MV: 40 | MV: 100 |
| 2013 | 1.1 | 870 000 | | | MV: 25 | PY: 0 | MV: 50 | MV: 50 | MV: 100 |
| 2014 | 1 | 1 000 000 | | | MV: 25 | PY: 10 | MV: 60 | MV: 60 | MV: 100 |
| 2015 | 0.85 | 1 200 000 | | | MV: 25 | PY: 0 | MV: 70 | MV: 70 | MV: 100 |
| 2016 | 0.85 | 1 400 000 | | | MV: 25 | PY: 0 | MV: 80 | MV: 80 | MV: 100 |
| 2017 | 0.85 | 1 480 000 | | | MV: 25 | PY: 0 | MV: 90 | MV: 80 | MV: 90 |
| 2018 | 0.85 | 1 480 000 | | | MV: 25 | PY: 0 | MV: 90 | MV: 80 | MV: 80 |
| 2019 | 0.85 | 1 500 000 | | | MV: 25 | PY: 0 | MV: 90 | MV: 75 | MV: 75 |
| 2020 | 0.85 | 1 500 000 | | | MV: 25 | PY: 0 | MV: 90 | MV: 65 | MV: 65 |
| 2021 | 0.85 | 1 500 000 | | | MV: 25 | PY: 0 | MV: 90 | MV: 65 | MV: 55 |

*Source: Bjørneby, Markussen, and Røed (2020) with the extension of the tax assessment year 2021 (Ministry of Finance 2020b). The box highlights the various valuation rules in our sample period.*

### 2.1.1 Wealth Tax Reform of 2014-15

We base our analysis on the tax variation which the reform of 2014-15 offers. The identifying variation in the household's wealth tax rate comes from the tax rate change in the turn of 2014-15. The policymakers changed the tax rate from 1 to 0.85 percent, and they increased the threshold from 1 mNOK to 1.2 mNOK. They further kept the valuation of primary housing unchanged but induced a 10-percentage point increase in the tax valuation of secondary homes and business properties. The Ministry of Finance (2014) proposed the wealth tax changes to the Norwegian Parliament on the basis that it would give stronger incentives to save. They argued that the increased threshold would further motivate households close to the previous thresholds to save. At the same time, offering incentives to households above the threshold by reducing the rate.

While the wealth tax unambiguously reduces the return of the various assets, the reform of 2014-15 offered an increase in the after-tax return due to lesser tax liability. The increase was

applicable for all households above the threshold, including individuals with most of their wealth in secondary housing and business assets. However, their tax relief was not as significant as households with all wealth in primary housing.

The reform of 2014-15 enables us to isolate all the changes made to the wealth tax system, including rates, thresholds, and tax valuation of assets. The reform offers four variations in the household's tax rate; it can be constant at 0, households are below the threshold both in 2014 and 2015. Their rate can be changed from 1 to 0.85 percent if they are above both thresholds. The rate can also be reduced from 1 percent to 0, if they exceed the 2014 but not the 2015 threshold. Lastly, the rate can be increased from 0 to 0.85 percent if they are below the 2014 but exceeds the 2015 threshold. With such a reform, we can separate a treatment and control group. The treatment group receives one of the tax rate changes, while the control group receives zero change, they are below both the 2014 and 2015 threshold.

## 2.2 The Basis of the Wealth Tax Policy

The primary purpose of the Norwegian wealth tax is the redistribution effect and the public sector's need for tax revenue (NOU 2009:10). Wealth is unequally distributed in the population and much more concentrated than income. In a study of inequality, Piketty and Saez (2014) show that in the US, the top 10 percent in the income distribution owned almost 45 percent of the total pre-tax income in 2010. Simultaneously, the top 10 group in the wealth distribution owned around 70 percent of the total wealth. The numbers from Norway are not as extreme. Statistics Norway (2021a) estimate that the top 10 group in the wealth distribution owns approximately 53.5 percent of the total Norwegian wealth.

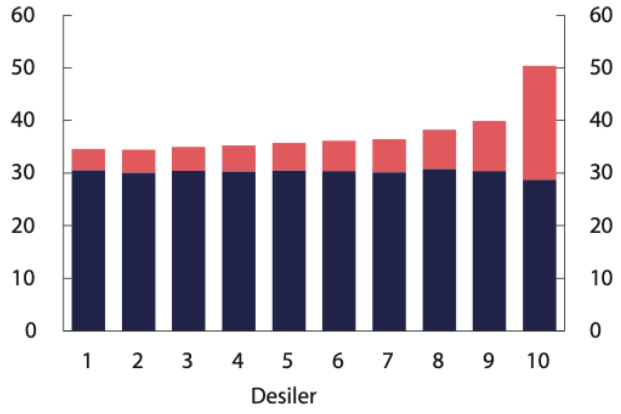
The Norwegian tax system is dual, labor income is taxed progressively, and capital income is taxed at a flat rate, respectively, at 46.4 (maximum) and 22 percent in 2021.[9] In this system, an individual with most of her income coming from capital ownership effectively has a lower average tax than a high-income worker. The wealth tax acts as a mechanism in ensuring that the progressive tax system is maintained. Another justification for the tax is the benefits that a more considerable fortune provides, especially non-economic benefits such as the opportunity

[9] Dividends are multiplied by an adjustment factor of 1.44, as of the tax year 2021; thus, the effective tax rate is 31.68 percent. If one additionally accounts for corporate tax, the effective tax on dividends becomes 46.7 percent (Ministry of Finance 2020b).

to own works of art, social status, power, and influence. An additional function is to provide relief to other taxes and distribute taxes on several bases (NOU 2018:5).

**Tax as the Share of Gross Income. 2018.**

A. *Top 1 percent in the wealth distribution divided into 10 deciles*




B. *Sorted by intervals of income*

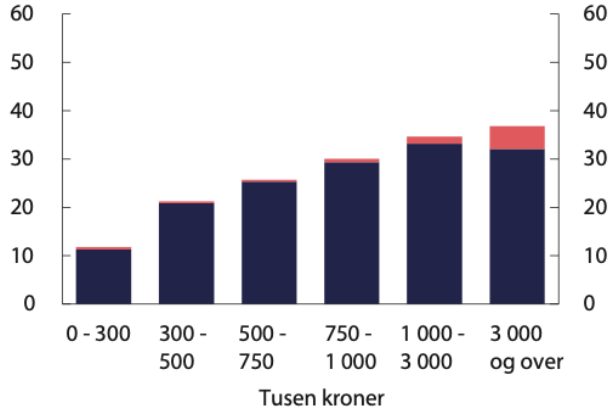



*Figure 1: Tax as the fraction of gross income in 2018. Red: wealth tax. Blue: income tax. Numbers in thousands NOK. Source: The Ministry of Finance (2020a).*

Figure 1B indicates that the tax system becomes regressive without the wealth tax when the individual's income exceeds 3 mNOK. This non-progressivity occurs in particular because of the very definition of income. With income from labor, the tax rate structure ensures progressivity.[10] However, once you include income from capital holdings and sole proprietorships, and add personal deductions, a non-progressiveness emerges at the top income distribution (NOU 2014:13). Moreover, when matching leaked customer lists from tax havens with wealth records in Scandinavia, Alstadsæter, Johannesen, and Zucman (2019a) finds that the wealthiest 0.01 percent approximately evades 25 percent of their taxes. Indicating that the tax system is even more regressive when including tax evasion. As figure 1A suggests, the wealth tax constitutes close to half of the taxes relative to gross income for the tenth decile. Without wealth tax, this group pays the same share of tax to gross income as the group with an income of 750-1000 kNOK (figure 1B). A clear indication that wealth taxation serves to increase the average taxes of wealthy individuals and maintain progressivity.

[10]The bracket tax on income is additional taxes on higher income levels. The maximum effective tax rate of labor income is 46.4 percent in 2021 (Ministry of Finance 2020b).

Aaberge, Modalsli, and Vestad (2020) supplement the official income statistics[11] with company profits not paid as dividends to get a better picture of income inequality. They show that labor income in the top 98 percentile in the income distribution constitutes only 32 percent of the firm owner's total income. Simultaneously, the top 1 percent pays on average 19.8 percent in taxes, a low number comparing to the median, where the average tax is 22.1 percent.[12] These findings substantiate the arguments of having a corrective tax, like the wealth tax, to ensure progressivity. Arguably, to include company profits not paid as dividends as real income of the taxpayer is controversial. However, as Aaberge discusses in an interview in Forskning.no (Amundsen 2020), a wealthy individual may use their companies as personal saving vehicles and withdraw capital when the tax system favors it.[13]

In a recent Norwegian study by Lian, Nesbakken, Jia, Nygård, and Vattø (2019), using data from 2013 to 2019, they investigate how changes in the taxation of wealth and income affect redistribution. Measuring the tax relief relative to disposable income, they find the beneficiaries of a reduction in the wealth tax are primarily individuals with low income and relatively big fortune. This group consists of pensioners, business owners, and workers. An important reason for this pattern is that people might have low income due to losses in the capital market or their businesses. When they rank according to wealth, as opposed to income, the top 1 percent has the most significant relief relative to disposable income. They conclude that there was a slight increase in income inequality from 2013 till 2019. Still, little of this effect is attributable to the reduction in wealth taxation, primarily due to the wealth tax relief for the proportion of wealthy individuals with low income. They make the case that the Norwegian tax system has become moderately less redistributive from 2013 till 2019, and the main reason is the reduction in the base rate of income tax.[14]

As figure 1 suggests, the wealth tax works as a mechanism to maintain the progressive tax structure and raise the average tax for high-income individuals. Figure 2 indicates that the proportion of those levied wealth tax has decreased from 1998 till 2020 due to the continuous

[11] Income stated in the individual tax returns.
[12] 2018 numbers.
[13] Aaberge, Modalsli, and Vestad (2020) showed that the dividend payouts increased rapidly when the dividend tax to be implemented in 2006 was announced in 2004. Right before 2006, the dividend payout decreased sharply.
[14] Base tax rate of income was gradually reduced from 28% to 22% over the sample period. Bracket tax and National Insurance contributions are added to this.

increases in the threshold. Naturally, the average payments increase mechanically because of this. In addition, various rule changes during the period might affect the average payments. The figure shows that the wealth tax liability concentrates on a small proportion of wealthy individuals in the later years, which effectively increases their average tax and ensures progressivity.

**The Proportion Levied Wealth Tax and the Average Payments**

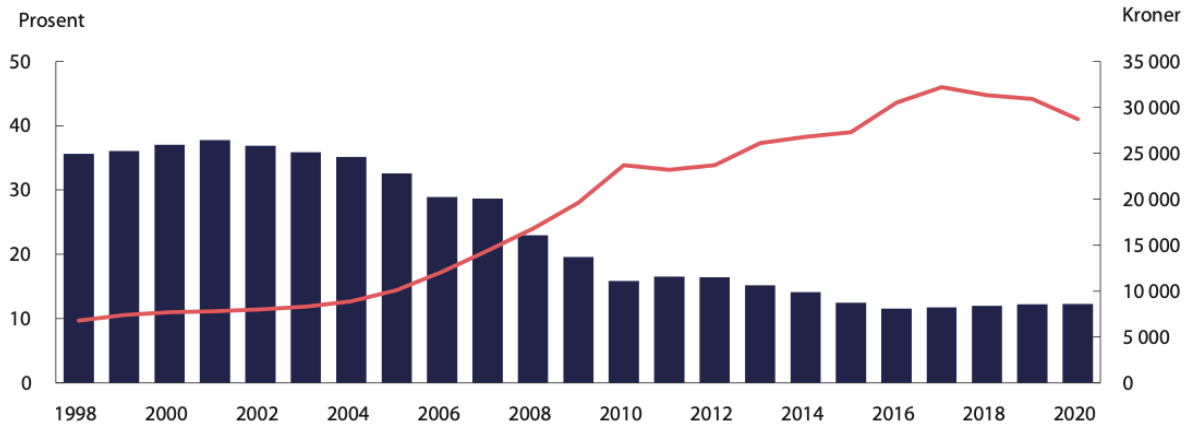



*Figure 2: NOK-2020 prices. Red line: average wealth tax payments. Columns: the proportion of the Norwegian population who pay wealth tax. Source: The Ministry of Finance (2020a) p.58.*

## 2.3 Distortion of Savings and Investments

All personal capital taxes are assumed to affect an individual's saving behavior. However, the impact of an increase in the rate of return on savings is ambiguous because of countering income and substitutions effects (Zoutman 2015; Ring 2020).

The substitution effect ensures that saving increases because the price of future consumption relative to current consumption decreases (Zoutman 2015). An individual chooses to save and delay consumption if the financial gains by saving exceed their required rate of return. If there is a reduction in the wealth tax, then the after-tax return increases, which leads to saving becoming more lucrative. This ensures that individuals choose to delay consumption and save more. However, the substitution effect depends on the behavior adjustments that follow due to changes in an individual's overall spending opportunities. The countering income effect (wealth effect) goes in the opposite direction. If the individual's after-tax return on savings increases, the individual may perceive that they are wealthier. Since future returns increase, individuals do not have to save as much to maintain future consumption (NOU 2018:5). This

might influence whether individuals choose to consume more today. Especially, a perceived increase in wealth may move consumption from normal goods to more lucrative goods, which leads to a reduction in capital available for savings. In the opposite direction, if the after-tax return decreases because of an increasing wealth tax, this can motivate individuals to save to maintain the level of future consumption.

One of the several arguments of why countries have chosen to abolish the wealth tax was that it was assumed to inhibit savings and investment, and therefore, economic growth (Edwards 2019). However, while most studies show that an increase in the wealth tax reduces taxable wealth, the real saving responses are unclear. Mainly because the estimates point towards changes in reporting behavior, tax avoidance, or evasion (Zoutman 2018; Seim 2017; Brülhart et al. 2019; Jakobsen et al. 2020; Durán-Cabré, Esteller-Moré and Mas-Montserrat 2019; Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Foremny and Martínez-Toledan 2021). The empirical evidence of real saving responses shows weak results. Seim (2017) estimates saving responses but finds insignificant effects, and his overall findings primarily point towards changes in reporting behavior. Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019) estimate point towards a reduction in taxable wealth by an increase in the wealth tax, which indicates avoidance with business assets as the primary vehicle. However, when limiting avoidance opportunities and filtering out mechanical effects, they find a slightly positive relationship between savings and the wealth tax. This substantiates the findings of Ring (2020), who shows that in Norway, presumably, the income effects dominate the substitution effect. Households increase their savings when faced with the wealth tax liability, detailed in section 2.4.

These contradicting saving responses directly relate to the research question; we are interested in quantifying the behavioral responses to wealth taxation. However, we cannot separate real saving responses from avoidance or evasion, but we have somewhat limited the scope of evasion. We will further discuss these various findings in more detail in section 2.4.

### 2.3.1 Effective Tax Rates on Returns

The asymmetry of various valuation rules might lead to different after-tax returns of financial assets and distort saving behavior. The OECD economic survey of Norway (2012) presents effective tax calculations from the Norwegian Ministry of Finance. They calculate effective tax rates on real income from different assets using 4 percent return and 2 percent inflation. In

2012 the Norwegian wealth tax was 1.1 percent, and the ordinary tax rate was 28 percent. Primary residence and rental housing had tax advantages with rebates in the market value of 85 and 60 percent, respectively. At the same time, bank deposits and shares had zero rebates.

The calculations with and without wealth tax are shown in table 2. The tax system favors primary residence, with an effective tax rate of 14 percent, compared to shares and interest-bearing accounts, with an effective tax rate of 113 percent. In addition, it is easy to credit-finance housing, and debt is deductible at market value when determining taxable wealth. This provides significant incentives to turn investments towards primary residences. As discussed in NOU (2014:13), the tax system favoring of housing might lead to a loss to society because of housing's financial unproductiveness. Households might accumulate wealth in heavy rebated assets, which they consider a tax system weakness. The OECD (2019) survey of Norway argues that a more uniform valuation method should be considered as differences in asset valuation distort investment decisions.

**Table 2: Effective Tax Rates**

| | Without wealth tax (%) | With wealth tax (%) |
|---|---|---|
| Interest-bearing accounts | 56 | 113 |
| Shares | 56 | 113 |
| Owner-occupied housing | 0 | 14 |
| Rental housing | 56 | 79 |

Effective tax rates on real income from different assets. Example calculation rental housing with a market value of 100 NOK: 4 NOK return → 1.12 (4*28%) ordinary tax + 0.44 (100*40%*1.1%) wealth tax = 1,56 in total tax. When accounting for inflation, the real return is 2 NOK. Effective tax rate is 79% (1.56/2). Source: OECD (2012).

## 2.4 Empirical Evidence of the Effect of Wealth Tax on Taxable Wealth

Our question of interest in this thesis is the causal effect of wealth tax on taxable wealth. There is extensive research on the elasticity of taxable income; however, the elasticity of taxable wealth has not been under the same scrutiny. Nevertheless, quite a bit of recent empirical research has highlighted the question. It is a question of great political interest and importance when determining optimal taxation policies. As mentioned previously, there is an ambiguity about the saving responses to an increase in the after-tax return due to countering income and substitution effects (Zoutman 2015; Ring 2020). This ambiguity further motivates research on the topic to provide policymakers with tangible empirical evidence. Empirical

studies on wealth tax are challenging. Jakobsen et al. (2020) highlight that we are primarily interested in the long-run effect when working empirically with taxation policy. It is easier to work with labor taxes than wealth taxes due to the slow nature of wealth accumulation. Additionally, few countries have data on individuals' wealth, and finding wealth tax variation that allows for estimating causal effects is difficult (Jakobsen et al. 2020). Despite these challenges, researchers are studying the effect of wealth taxation on wealth accumulation in various countries.

Using administrative household data from 1995-04 in the Netherlands, Zoutman (2018) estimates that a 0.1 percentage-point increase in the Dutch wealth tax, which was 1.2 percent, reduces household savings by 1.38 percent after 4 years. He expresses that this response is likely to be changes in reporting behavior. His findings are in the middle of two existing studies. Seim (2017), using administrative records from 2000-06 in Sweden and employing a bunching strategy, estimates the net-of-tax rate elasticity[15] in the range of 0.09-0.27. By the properties of logarithms, we can approximate a reduction in wealth by an 0.1 percentage point increase in the tax rate to be 0.027 percent, using the upper bounds of his estimates.[16] He highlights that about one-third of these elasticity estimates are due to underreported asset values. Further, he discusses that, though small, his estimates point toward evasion rather than real saving responses. According to Seim (2017), one reason is that the Swedish tax system has advantages for different assets and that the system is full of loopholes and insufficient enforcement.

Brülhart et al. (2019), study how households respond to wealth taxation in Switzerland, where the wealth tax is raised at a cantonal[17] level with no federal interference. This enables them to estimate the aggregate responses, considering the rich variation of wealth tax policies and wealth in each canton from 2003-15. They find that a 0.1 percentage point increase in the wealth tax rate reduces reported wealth by 4.11 percent after 4 years. There is, indeed, significant variation between these three findings. However, as Advani and Tarrant (2020) highlight, bunching techniques normally exploit an individuals' motivation to aim their wealth

[15] A percentage increase in the net-of-tax rate (1-T), the share of wealth kept by an individual after tax.
[16] Seim estimates are not entirely comparable to Zoutman, because he estimates the elasticity using the net-of-tax return instead of the gross rate of return. Given that Swedish wealth tax was 1.5 percent, we can approximate for small numbers using the properties of logarithms $log(1 + x) \approx x$ to express in terms of 0.1 percent point increase in the tax rate (Zoutman 2018; Durán-Cabré, Esteller-Moré and Mas-Montserrat 2019).
[17] Member states of the Swiss Confederation.

to go below the threshold, giving elasticities local to the threshold. Since the wealthiest individuals may not simply go below the threshold, the estimations do not pick up their responses. Consequently, the elasticities obtained by bunching techniques are likely to be smaller. Seim (2017) uses a bunching technique, while Zoutman (2018) and Brülhart et al. (2019) both use a difference-in-difference estimation, where behavioral responses of those who do not target the threshold are also included.

Jakobsen et al. (2020) investigate the effects of wealth taxation on wealth accumulation, using administrative wealth records from Denmark. They utilize the reform of 1989, where the wealth tax rate of 2.2 percent started to be greatly reduced, leading to abolishment in 1997. This provides them with a compelling quasi-experiment. Utilizing a Diff-in-Diff regression setup, they find significant effects of wealth taxation on wealth accumulation both in the short and medium run. Their estimates also indicate a smaller effect on the moderately wealthy compared to the very wealthy. When matching their model to the moderately wealthy, they find an effect of 8.9 percent increase in taxable wealth w.r.t a 1 percentage increase in the net-of-tax rate after 8 years. When matching the model with the very wealthy, they obtain a net-of-tax rate elasticity of 11.3 after 8 years.

Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019) study how individuals in Spain (Catalonia) reacted to the reintroduction of the wealth tax. If the average wealth tax rate increases by 0.1 percentage point, their results show a 3.24 percent reduction in taxable wealth after 4 years. Their main finding is that wealth tax does not prevent savings. The decrease in taxable wealth reflects tax avoidance, primarily through changes in the composition of financial holdings, with business assets as the primary vehicle. The estimates of Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019) and Brülhart et al. (2019) indicate that the elasticities are notably higher in Spain and Switzerland, respectively, compared to what Jakobsen et al. (2020) find in Denmark. This might suggest that in countries with mainly little or no third-party reporting, tax evasion and avoidance is particularly prominent, compared to Denmark, where most wealth is third-party reported (Advani and Tarrant 2020).

Ring (2020), studies Norwegian households' response to capital taxation and contributes to the studies mentioned; increases in the wealth tax reduces taxable wealth (Zoutman 2018; Seim 2017; Jakobsen et al. 2020; Durán- Cabré, Esteller-Moré, and Mas-Montserrat 2019;

Brülhart et al. 2019). However, when limiting the role of evasion by only using third-party reported assets and variation below the top 1 percent in the wealth distribution, he finds very different results.[18] He estimates that for each additional NOK affected by a 1 percent wealth tax, the individuals increase their yearly savings by 0.04 NOK. Increases in labor income mainly finance the increases in savings. He points out that capital taxes may incentivize individuals to put in more labor hours to meet these liabilities. His findings suggest that the income effect dominates the substitution effect. Presumably, an increase in the wealth tax motivates households to save to meet the future tax liability.

Like Ring (2020), Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019) initially find a reduction in taxable wealth by the wealth tax. However, by assuming that wealth taxes are primarily paid for by savings and accounting for mechanical effects, they find a positive effect of the wealth tax. An increase of 0.1 percentage point in the 2011 wealth tax rate increases reported wealth by 0.5 percent over 4 years. This translates into a net-of-tax rate elasticity of -0.1 and substantiates to some degree the findings of Ring; the income effect seems to dominate the substitution effect; individuals save to offset the increased tax liability.[19]

Londoño-Vélez and Ávila-Mahecha (2019) study the Colombian wealth tax using administrative data and a government-designed program for voluntary disclosure of wealth.[20] They show that evasion is particularly prominent on the top of the distribution. Two-fifths of the wealthiest 0.01 percent admit to hiding wealth. This group evades about one-third of its assets offshore. Utilizing a bunching strategy, they find that a 1 percentage increase in the net-of-tax rate increases wealth by 2 percent in the short term. Moreover, they find that the marginal bunchers underreport their taxable wealth by 21 percent because of the tax notch.[21] This is done by increasing debt and exploiting that some business assets are not third-party reported, making it easier to underreport.

[18] He motivates this by the findings of Alstadsæter, Johannesen, and Zucman (2019a), who showed that evasion primarily occurs above the 99 percentile in the wealth distribution.
[19] Note: Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019) highlighted this as a potential explanation, while Ring (2020) expresses this with more certainty.
[20] Individuals might have been motivated by the leaked "Panama Papers."
[21] Tax notch: discrete jumps in tax liability at given thresholds of reported wealth (Londoño-Vélez and Ávila-Mahecha 2019).

Agrawal, Foremny, and Martínez-Toledan (2021) study the effect of wealth taxation on the mobility of wealth, using administrative data and tax variation across *Comunidades Autómas*[22] in Spain**.** The wealth tax was abolished in 2008 and again reintroduced in 2011, where regions had autonomy over the wealth tax policies. Madrid chose to set the rate at zero, which created a tax haven. Five years after the reintroduction, the number of wealthy individuals in Madrid had risen by 9 percent. They estimate that the mobility elasticity, with respect to net-of-tax rate, is 7.5 after 5 years, indicating strong mobility effects of wealth taxation. Their elasticity isolates the inter-Spain migration, and they point towards evasion being the dominant mechanism rather than real responses to the wealth tax.

As presented in this section, changes in reporting behavior, internal migration, evasion, or tax avoidance by placing capital in favorable assets explains most of the elasticities in these various studies (Zoutman 2018; Seim 2017; Brülhart et al. 2019; Jakobsen et al. 2020; of Durán- Cabré, Esteller-Moré, and Mas-Montserrat 2019; Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Foremny and Martínez-Toledan 2021). However, Ring (2020), when limiting the roles of evasion, finds a positive saving response when households are subjected to the wealth tax. An indication that the income effect can outweigh the substitution effect. His findings are partially substantiated by Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019), who finds a slightly positive saving response when accounting for mechanical effects.

Considering the studies mentioned above, we expect that an increase in the net-of-tax rate increases taxable wealth. Moreover, since most of the Norwegian assets are third-party reported, and that evasion primarily occurs on the top in the wealth distribution, we expect to find a small elasticity.

[22] Sub-national regions

# 3 Data

## 3.1 Data Set

We base the analysis in this investigation on the Norwegian taxpayer's tax return. We have received access to this data from NoCET (Norwegian Center for Taxation). Individuals are made anonymous; identification numbers replace names. The data set is designed as panel data; we have data from 2009-16 for each individual. If the individual has a spouse, it is identified with a spouse-specific identification number. The advantage of panel data is that we can study each individual over a particular period; it also allows us to control for specific unobserved characteristics of the individuals. Another advantage is that we can build dynamic models that can better indicate the effects of taxation (Wooldridge 2012).

A Norwegian resident is liable to pay tax. Everyone who collects a salary, pension, or disability benefit receives a tax return each year. The tax return contains a summary of wealth, income, debt, and deductions (Skatteetaten(c) n.d.). These categories are primarily third-party reported, e.g., bank deposits, debt, financial holdings, etc. Each individual must check whether the information presented in the tax return is correct, and one may be held accountable for wrongdoings and liable to a non-compliance penalty tax. It makes tax evasion difficult and provides trustworthiness and reliable data for the investigation.

The tax return is quite comprehensive and contains every source of potential taxation and deductions. NoCET has constructed our data set with regard to relevancy for our thesis. We have number-based information of the individual’s taxable income, taxable wealth, tax assessment of housing (primary, secondary, leisure, and foreign housing), deposits, debt, non-listed and listed company shares, cash, movables, bonds, and business assets. Some assets are subject to valuation rebates; the value of the variables presented in the data set is after the rebate, given each tax assessment year's specific rules.

## 3.2 Cleansing

The dataset contains 31 804 386 observations from 2009 to 2016. The panel dimension is somewhat unbalanced; the reason is that not all individuals have data from each year. This might be because of death or emigration. On average, the data set contains 3 975 000 individuals each year. To structure our data set, we start by combining the individual tax returns of spouses, summing the values of their variables. We do this because spouses are

assessed jointly on their taxable wealth, with double the activation threshold as an individual. Individuals without a spouse are still in the data set. We remove all households (singles and couples) who have negative taxable wealth for each year in the period. This counts for 6 526 026 observations. We keep households who go from negative to positive somewhere in the period and vice versa. To be left with relevant observations in the data set, we remove all unstable marriages and individuals without data for each of the eight years – effectively removing spouses who have not been together the entire period and individuals who have died or emigrated during the eight years. This constitutes 5 974 713 observations. After these changes, the panel dimension becomes strongly balanced. This leaves us with 18 438 096 observations, which we use in the first regression model (3), section 4.4. Before running our second regression model (6), section 4.6, we remove households with an average taxable wealth below zero. This counts for 5 200 520 observations, making 13 237 576 left and used in our estimation.

Before receiving access to the data, NoCET removed all individuals with taxable wealth above 5 mNOK or income above 1.5 mNOK. This consideration is because of privacy issues, where one can easier match identification numbers with real names if wealth is big enough. We later discuss some of the implications this removal might have for our estimates.

# 4 Method

This chapter describes the methodology used to study the causal relationship between wealth tax and taxable wealth. We investigate the effects of changes in the wealth tax on the household's taxable wealth. The methodology we use is a static difference-in-difference estimator (Diff-in-Diff). We will present two regression specifications; we base the first on 2014 wealth and the second on average wealth when determining treatment.

## 4.1 Intuition Behind the Empirical Strategy

As mentioned in the introduction, we utilize the tax reform of 2014-15 to isolate the wealth tax variation of the households. The reform of 2014-15 offers a compelling variation in the household's wealth tax and provides two main sources of relief: a reduction in the rate from 1 to 0.85 percent and an increase in the threshold from 1 to 1.2 mNOK. The wealth tax variations for the households are heterogeneous, as described in section 2.1.1. The tax rate changes for the households may arise from two different sources. Exogenously, through the changing tax environment, this allows us to identify causal effects of the impact of the wealth tax on taxable wealth. Endogenously, if households change their financial behavior and by that manner experience a change in the tax rate. In the spirit of Gruber and Saez (2002), we overcome these potential endogeneity problems by constructing an instrument for the counterfactual tax rate change. We expand on these points in section 4.3.

These tax rate variations enable us to categories a treatment and control group, which we use in a quasi-experimental setting. The preferred regression setup is a Difference-in-Difference framework. The treatment group experiences a change in their tax rate, while the control group is unaffected by the tax reform, i.e., they are below the thresholds and do not face wealth tax obligations in either year. This allows us to regress household's wealth on their tax rate change and capture the potentially distortionary effects of wealth taxation by examining wealth development after the reform. Furthermore, it allows us to report the elasticity of taxable wealth (ETW).

In general, our regression and Diff-in-Diff estimations rely on the assumption of parallel trends between the treated and the control group before the reform. We run an event study to test the validity of our models. We motivate our second regression specification by the potential pitfalls of the first specification, which we discuss later on.

## 4.2 Wealth

Let $Y_{it}$ denote our outcome variable taxable wealth for household $i$ in period $t$. Recall, we keep all individuals or couples who go from positive to negative wealth, or vice versa, somewhere in the period. The motivation behind this is not to exclude those subjected to the wealth tax in the reform of 2014-15 if they have negative wealth early in the period. Since we are interested in the elasticity, not levels of taxable wealth, we need to log-transform wealth. A well-known problem in economics is that the natural logarithm of zero or any negative value is undefined. In recent years the inverse hyperbolic sine (arcsinh) transformation has grown popular among applied econometricians because it allows for retaining zero and negative values; it is also similar to the logarithm function (Bellemare and Wichman 2019). Our dependent variable of interest, taxable wealth, is transformed as follows:

$$Y_{it} = \text{arcsinh}\,(Y_{it}) = \ln\,(Y_{it} + \sqrt{{Y_{it}}^2 + 1}) \tag{1}$$

## 4.3 Identification Strategy

As mentioned in the intuition section, the tax rate variation of the households might arise from two sources. Exogenously, through the changing tax environment. Endogenously, through changes in saving behavior or to wealth shocks. The tax rate changes are directly linked to a household's wealth development from 2014 to 2015. Therefore, these unobserved determinants of wealth might influence whether the households experience a changed tax rate. This includes bunching at the kink point of the threshold. Individuals are strongly incentivized to stay below the threshold because of the sharp increase in the wealth tax rate if they exceed it. It is unlikely to be a distorting factor in our estimation because it is challenging to bunch at the threshold by real savings responses, given that asset valuation changes constantly through the year (Jakobsen et al. 2020). However, firm owners might reduce their taxable wealth by placing capital into their private businesses, which are valued at book value (Bjørneby, Markussen, and Røed 2020). Exogenous shocks to wealth, like winning the lottery or inheritance, might also affect whether they experience a changed tax rate; this is a case of reverse causality and would be detrimental in the interference of causal effects. These unobserved determinants of wealth need to be filtered out to isolate the effect of the tax rate change.

We construct an instrument for the counterfactual tax rate change of the households to mitigate the dangers of endogeneity problems as described above. This closely follows Gruber and Saez's (2002) approach. We start by determining the wealth tax rate in a normal situation. Let $t_{2014}$ denote the wealth tax rate household $i$ is subjected to in a normal setting of 2014 assets valued at 2014 tax rules. This is the actual wealth tax rate an individual was liable for in 2014. Meaning, for an individual or couple with taxable wealth above the threshold, their wealth tax rate is 1 percent, 0 else wise. Then we identify what the wealth tax rate for household $i$ would be if i) the wealth tax policies changes from 2014 to 2015 rules and ii) the household does not change its behavior. We determine the household's wealth tax rate valuing 2014 assets with 2015 rules. We correct the 2014 taxable wealth by changing the valuation rebates following 2015 rules for the different assets. After that, we utilize the 2015 threshold. For households with 2014 wealth (valued at 2015 rules) above the 2015 threshold, the wealth tax rate is 0.85 percent; this is denoted by $t_{2014}^{2015}$. This allows us to hold the households' behavior constant, effectively mitigating the danger of unobserved determinants affecting both their wealth and tax rate. Secondly, we calculate the difference between the constructed and the real wealth tax rate as follows:

$$T_i = t_{2014}^{2015} - t_{2014} \tag{2}$$

This instrument is likely to fulfill the condition of independence to the unobserved parameter $\varepsilon$ of wealth. The changes a household may experience in their tax rate are only those induced by the changes in the tax system itself. In that manner, the instrument filters out unobserved changes in the household's financial behavior and exogenous shocks to wealth, which might simultaneously affect both wealth and treatment status. However, announcement effects of the reform might motivate households to align with the expected changes in the tax environment before its entry, leading to a bias in the instrument. Still, it is unlikely because the Ministry of Finance (2014) proposed the reform to the Norwegian Parliament in the latter part of 2014.[23]

$T_i$ measures the wealth tax rate change for household $i$, given that we keep household behavior constant. $T_i$ allows us to capture all changes made to the wealth tax policy, i.e., changes in rates, thresholds, and valuation rebate of assets.

[23] In the election campaign in 2013, Høyre advertised to reduce and remove wealth taxation (Høyre Hovedorganisasjon 2013). Høyre's election victory could have caused some behavioral responses.

### 4.4 Difference-in-difference Estimation

A natural experiment (quasi-experiment) occurs if an exogenous event, for example, the reform of 2014-15, changes the households' environment. Unlike a real experiment where treatment and control groups are randomly selected, a natural experiment arises from the policy change itself. Panel data is beneficial in policy analysis because you follow the same cross-sectional units, allowing us to control unobserved heterogeneity (Wooldridge 2012).

The 2014-15 reform enables us to categorize a treatment and a control group. The treatment group experiences an exogenous treatment, i.e., a change in their tax rate induced by the changed tax environment. The control group does not experience any difference, i.e., they are below both the 2014 and 2015 thresholds. Unlike a binary treatment identification, taking the values of 1 or 0 depending upon the individual receiving treatment or not. Our variable $T_i$ is continuous and may take the values of -1, -0.15, 0, and 0.85 percent. These values represent the four tax rate variation a household can experience in the reform (see section 2.1.1), where 0 represents the control group. This variable is known as a continuous treatment variable or treatment intensity variable. As Angrist and Pischke (2008) describe, the interpretation of continuous treatment variables is essentially the same as the interpretation of binary treatment variables.[24] We extend $T_i$ to all periods to separate the treatment and control group for the entire sample. To capture the effect of the tax policy change on the accumulation of wealth, we generate a time-dummy denoted $After$, taking the value of 1 if the year $\geq$ 2015, 0 else wise. We make an interaction variable between $After$ and $T_i$, which constitute our independent variable of interest, $T_i After_t$. We are now able to run this static difference-in-difference estimation, which comprises our base event for the first regression:

$$Y_{it} = a_i + \eta_t + \beta T_i After_t + \varepsilon_{it,\ t=2009,\ 2010\ldots,T.} \tag{3}$$

Let $Y_{it}$ denote our outcome variable, taxable wealth for household $i$ in period $t$, transformed by the arcsinh function. Let $a_i$ represent household fixed effect, fixed in the sense that it does not have a time subscript. Meaning, we assume that heterogeneity amongst households is time-constant. By including household fixed effect, we can control for the fact that the heterogeneity between the households correlates with both wealth levels and treatment status. This can be, for example, heterogeneity in age, which is fixed to the person. Older individuals

[24] See page 57 of Angrist and Pischke (2008) for more details.

own, on average, more of the total wealth and are therefore more likely to receive treatment status in the reform. If we do not control for this, our estimates might wrongfully include these differences in wealth when estimating the effect of the tax treatment.

Let $\eta_t$ denote time fixed effects. By including $\eta_t$ we are able to control for year-specific unobserved exogenous shocks to wealth which is symmetrical to all households. As economic growth flourishes, technology development accelerates, and prosperity increases, a household's wealth may also grow as a positive consequence. For example, if the stock market or housing prices performs exceptionally well in one particular year, this might affect the household's wealth and potentially their treatment status. If $\eta_t$ is excluded, then we would be unable to separate year-specific exogenous changes in wealth unrelated to the wealth tax from the true impact of the tax treatment. In our regression statistics, the year 2009 is the baseline. Let $\varepsilon_{it}$ denote the unobserved factors affecting the household's wealth in period $t$.

Our main parameter of interest is the coefficient for $T_i After_t$, represented by $\beta$. The coefficient represents the relationship between $T_i After_t$ and $Y_{it}$. $\beta$ captures that for an increase of 1 percentage point in the wealth tax rate, the treatment group experiences a $\beta * 100$ percentage change in wealth. The estimation of $\beta$ allows us to make statements of the elasticity of taxable wealth, which is the main purpose of this study. By definition, the elasticity tells us to what extent wealth responds to a 1 percentage increase in the net-of-tax rate $(1 - T_i)$ (Advani and Tarrant 2020). This is the share of wealth kept after taxation. Given this definition, we present the relationship between our estimated $\beta$ and the elasticity of taxable wealth as follows: [25]

(4) $$ETW = \frac{dlog(Y_{it})}{dlog(1-T_i)} \approx -\frac{dlog(Y_{it})}{dT_i} \approx -\beta * 100$$

Let $\frac{dlog(Y_{it})}{dlog(1-T_i)}$ denote the elasticity of taxable wealth, the percentage change in wealth by a 1 percentage increase in the net-of-tax rate. By the properties of logarithms and derivatives, we can rewrite the elasticity to $-\frac{dlog(Y_{it})}{d\tau}$, where $\frac{dlog(Y_{it})}{d\tau}$ is the estimated coefficient $\beta$, i.e., the

[25] For simplicity, we represent the arcsinh function by log. *d* = difference.

percentage change in wealth to a 1 percentage-point increase in the tax rate.[26] When working with elasticities of wealth/income, the normal convention is to report the net-of-tax rate elasticity, and we will do the same.

### 4.5 Validity of the Diff-in-Diff Regression – Event Study

A crucial assumption of the validity of the difference-in-difference estimator is that the treatment group and control group in the absence of treatment follow the same trend. Moreover, it is the policy change exclusively that induces a change in the trend. If the treatment group and control group deviate from each other before the tax policy change, then causal interpretation will be difficult (Angrist and Pischke 2008).

We assess the validity of our model by examining this assumption. We make the following alteration to our baseline event:

$$Y_{it} = a_i + \eta_t + \theta_t \cdot T_i + \varepsilon_{it}, \; t = 2009, 2010 \ldots . T \tag{5}$$

Where $\theta_t$ is a time dummy for period $t$. We interact the time dummies with $T_i$, and we obtain coefficients for $T_i$ for each year in the period, except the event year 2014, which is normalized to zero. The event year serves as the reference point. $T_i$ still represent the treatment intensity variable. $a_i$ and $\eta_t$ still represent household fixed effects and time-fixed effects, respectively.

We assume that the independent variable $T_i$ before the reform has non-significant estimates, and this is the assumption the Diff-in-Diff regression model (3) relies on. Most households in the treatment group receive a 0.15 percent reduction in their tax rate in the 2014-15 reform, which we extend to all periods. Suppose that in the pre-reform years, the regression picks up an increase in wealth each time it picks up this tax rate reduction, then it will most likely give a significant coefficient for this relationship. This entails that something unobserved happens to the treatment group's wealth, which the control group does not experience, i.e., a deviation in the common trend. Therefore, the model's validity depends on non-significant estimates in the pre-reform years to correctly examine the effect of the 2014-15 reform.

[26] This is mainly applicable for small numbers. Consider a tax rate of 1 percent, 1-T (net-of-tax rate) = (1-0.01), a percentage (1-0.01) * 0.01 = 0.0099. Thereby, a one percentage increase in the net-of-tax rate roughly translates to one percentage-point reduction in the tax rate.

However, based on the treatment and control group specification, there is a possibility that this assumption does not hold. Mainly because it is not certain that those who receive treatment based on our construction of $T_i$ are persistently over the year-specific threshold. This is especially important because for pre-reform behavior to be a sufficient comparison to the post-reform behavior, households should have stable wealth and be subjected to the tax before the reform. Following the intuition of Gruber and Saez (2002), mean reversion can also bias the instrument. Mean reversion entails that wealth might be volatile each year but eventually reverts to its long-run mean. Households with low wealth in one year can have more significant wealth the following year, or vice versa. This can lead to the instrument not being exogeneous, which violates our assumption because mean-reverting can affect both wealth and potentially treatment status. To mitigate these potential problems, we motivate our second regression specification, and we expand on this in the next section.

### 4.6 Second Specification of the Base Event

Following the intuition discussed above, problems of mean reversion and inconsistent treatment status may distort our estimates. This would create issues with the definition of the treatment group. If households in our treatment group are consistently below the yearly thresholds before the reform, they do not constitute the best representation for the treatment group. This might be the case by how we construct our $T_i$, where we allow households to have negative taxable wealth somewhere in the period. Ideally, the treatment group should always be above the threshold. However, this specification does not yield credible results. It might be because not many households wind up in the treatment group if we solely include those above the thresholds for the entire period. This might be due to the bounds of the data set. Recall, NoCET constructed the data set without households with taxable wealth and income above 5 mNOK or 1.5 mNOK, respectively. Intuitively, a treatment group with households in the upper end of the distribution is more stable over the threshold for the entire period, making a separation of treated vs. control better. Nevertheless, as evasion is mainly prominent above the 99 percentile in the wealth distribution (Alstadsæter, Johannesen, and Zucman 2019a), we limit evasion responses by not including this group.

If wealth follows a mean-reverting process, those with high wealth in the year of the reform might have lower wealth in the post-reform year or vice versa. This might bias the instrument, which we base on the 2014 assets. We may hand out treatment status to the households if

taxable wealth bounces over the threshold of 2014 and reverting below the threshold in 2015, unconditional upon the changed tax system. Similarly, a household usually above the thresholds may wind up in the control group, given a reversion of their taxable wealth. Our estimates might include these bounces in wealth since mean reversion can correlate with treatment status. Also, we assume that if wealth follows a mean reversion process, it must be uncorrelated with the changes made in the wealth tax policy (Zoutman 2015; Gruber and Saez 2002).

We augment our baseline event study (3) in two ways to deal with these potential problems. First, instead of valuating 2014 wealth by 2015 tax rules, we compute the average wealth for each household over the sample period. If the average exceeds the 2014 threshold, they would be liable to a wealth tax of 1 percent, represented by $t_{2014}$. Second, we incorporate the 2015 tax system. In this case, the only difference is the increase in the threshold following the 2015 rules. If the average still exceeds the threshold, their rate is changed to 0.85 percent, represented by $t_{2014}^{2015}$. Given our new specification, $T_i$ may take the values -1, -0.15 or 0. Note, the treatment intensity value of 0.85 is now gone; households with average taxable wealth below the 2014 threshold will never go over the 2015 threshold.[27]

We further remove all households with average wealth below zero to make the treatment and control more comparable. Other studies have removed households at higher values. Gruber and Saez (2002) exclude households with income below 10 000 dollars; Weber (2014) does the same and tests with different cutoffs in the robustness check. Using an income/wealth cutoff is common in the literature because of too much mean reversion at the low end of the income distribution. Also, households on the lower level might be a poor comparison group (Weber 2014). Ring (2020) set the wealth cutoff level at zero. We choose to set the cutoff limit at zero because we do not want to restrict our sample more than it already is. Moreover, it is not obvious that mean reversion in the low end of the wealth distribution is as prominent as in the income distribution. Nevertheless, we utilize higher cutoffs in the robustness check in section 6.3.

By implementing this strategy, we mitigate problems of mean reversion and unstable wealth. The new structure of our data set offers some advantages and pitfalls. An individual with a

[27] This strategy is based on advice from our supervisor Floris T. Zoutman.

taxable wealth of 5 mNOK (maximum on our sample) in 2009 and 0 in the other years will not receive treatment in 2014. The average is below the activation threshold. We also exclude outliers of extreme negative wealth. However, this strategy can entail that an individual with a taxable wealth of 5 mNOK in 2009 and 2010, zero else wise, still end up in the treatment group. The same goes for other variations of wealth. By an investigation of our new specification of treatment, this does not seem to be the case. 95 percent of those who are in the treatment group, according to their average taxable wealth, have actual 2014 wealth values over the activation threshold. This percent goes slightly down in 2015 and 2016, most likely because of the increases in the threshold. In the first regression (3) only 78 percent of those in the treatment group, by their 2014 wealth, have average taxable wealth above the threshold. Therefore, we argue that this treatment group is more stable and better represents those affected by wealth taxation.

In our first Diff-in-Diff regression (3), we do not include any control variables. The literature suggests including 10-piece splines of wealth and income to mitigate problems of mean reversion (Gruber and Saez 2002). We choose to include splines in this regression specification. We use this as an index of where in the wealth/income distribution a household belongs. It is constructed on the average value of wealth ($\overline{y_i}$) and income ($\overline{z_i}$) for the household. It is in the range of 1 to 10, where 10 represent those in the top distribution. Also, we include controls for the development of wealth in each age interval over the period.

We have arrived at our second specification of the base event:

$$(6) \quad Y_{it} = a_i + \eta_t + \beta T_i After_t + \sum_{k=1}^{10} \xi_k SPLINE_k(\overline{y_i})t + \sum_{j=1}^{10} \delta_j SPLINE_j(\overline{z_i})t + \sum_{s=1}^{5} \varphi_s AgeDummies_s\, t + \varepsilon_{it},\ t = 2009,\ 2010 \ldots, T.$$

This Diff-in-Diff regression follows the same intuition as (3). The only difference is the inclusion of the controls mentioned above, and the treatment intensity group 0.85 is gone. We base treatment status on average taxable wealth for the household instead of 2014 assets.

Let $\xi_k$ and $\delta_j$ represent the coefficients for the household's wealth/income distribution interacted linearly with time. This allows us to control for the wealth/income development in each group over the sample period. Piketty and Saez (2014) report a growing inequality in

wealth/income in the US. In the period 1970 to 2010, the share of the top decile's wealth/income of the US total increased significantly. Presumably, this is less prominent in Norway. However, suppose we do not include controls for the distributions. In that case, our estimates might wrongfully pick up exogenous changes in wealth of the distributions unrelated to the tax rate change, especially since the top distributions correlate with both wealth and treatment status.

Let $\varphi_s$ denote the coefficients for each of the five equally large intervals of age interacted linearly with time. This allows us to control for the development of wealth in each interval of age over the sample period.[28] The intuition is that we can control for different dynamics in each age group. Young people tend to increase in wealth by reducing their mortgages or save for pensions, while older individuals may start consuming and be on a downward trend in wealth. These potential dynamics might be correlated both with wealth and treatment status; we eliminate the bias from these trends by including this control.

Note, Gruber and Saez (2002) highlight a potential problem if we base the splines on the average income/wealth and include the years after the reform, then the splines become endogenous to the tax reform. However, we argue that because of the slow-moving nature of wealth (Jakobsen et al. 2020) and the fact that we only have two post-reform years, this is unlikely to cause problems.

The baseline event (6) also relies on the common trend assumption and will be tested by setup (5) in section 4.5 with the inclusion of controls.

[28] The interval the household belongs to is determined by their 2015 age.

# 5 Descriptive Statistics

Before investigating the relationships between wealth taxation and taxable wealth, we describe the statistics.

## 5.1 Statistics on the Full Sample

Figure 3 below shows the average taxable wealth development for all individuals in our data set. The average has gone from - 36 000 to 77 000 NOK, increasing approximately 300 percent from 2009 to 2016. Except for 2011, the development of taxable wealth has grown steadily. Calculating the standard deviation from the period, we find that it increases by approximately 33 percent, from 950 000 NOK in 2009 to 1 270 000 NOK in 2016. The standard deviation indicates that wealth does not concentrate around the population average. There are significant differences between the households in the data. To compare, the average taxable income in 2015 was 280 000 NOK, while the standard deviation was 270 000 NOK. This supports the general assumption that wealth is much less evenly distributed than income. Also, growth in both standard deviation and average may signify that wealth distribution has become more prominent. The median might be a more suitable measurement in skewed data distribution instead of the average. The median for taxable wealth has increased from 10 000 NOK to 30 000 NOK in the period. It is lower than the average but an apparent growth. Considering that the wealth tax threshold has increased from 470 000 to 1 200 000 NOK in the same period, the typical household is far from being in a wealth tax position.

**Development in Average Taxable Wealth – Full Sample**

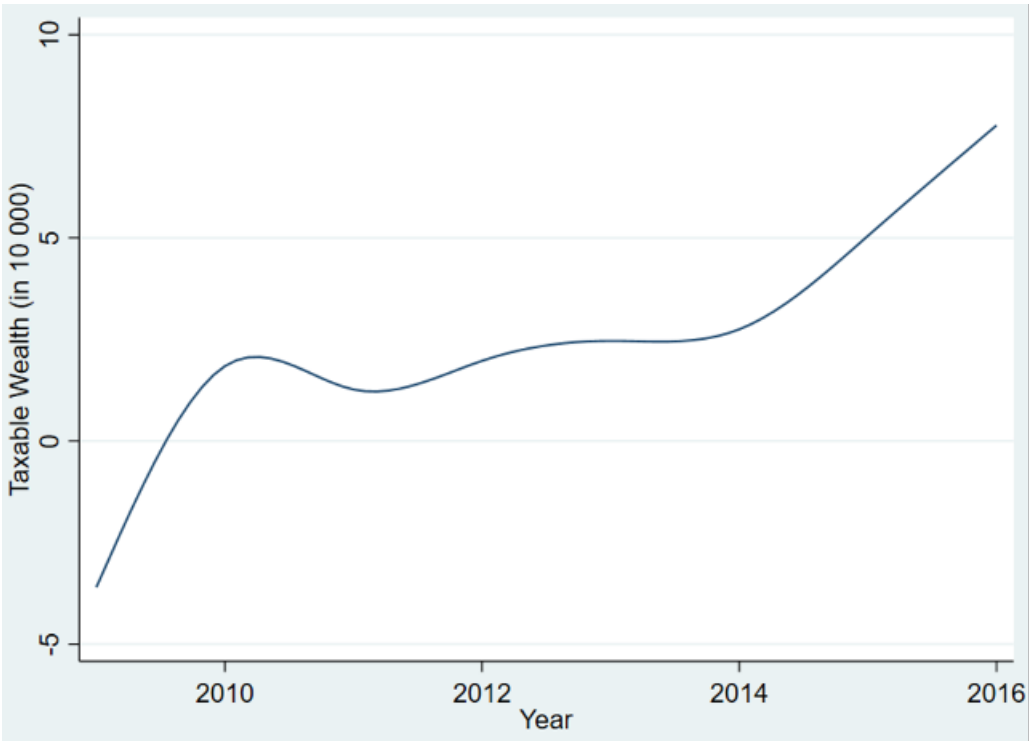



*Figure 3: The graph shows the average taxable wealth from 2009 to 2016 in 10 000 NOK. The graph is collected from the original data set and includes all individuals. Note: We have smoothed the graph using median spline.*

Table 3 below shows the summary statistic of all individuals in our dataset. Not surprisingly, we observe that households invest most of their wealth in housing, with 47.2 percent in 2016. Income presents the individuals' net income, net of any personal deductions. Income in our sample is not representable for the average gross income in Norway, but it gives a good indication of the average taxable income. Numbers from Statistics Norway (2021b) on gross income show that on average, in 2015, Norwegian workers earned 516 000 NOK. Meanwhile, we see that the average taxable income is very much below this number.[29] We also observe a 40 percent growth in debt from 2009 to 2016. On average, increased housing prices have led to increased wealth. We are becoming richer even though the debt is rising. However, high debt levels might still be a risk factor in the economy if exogenous shocks, like Covid-19, disturb the financial balance (Halvorsen 2019).

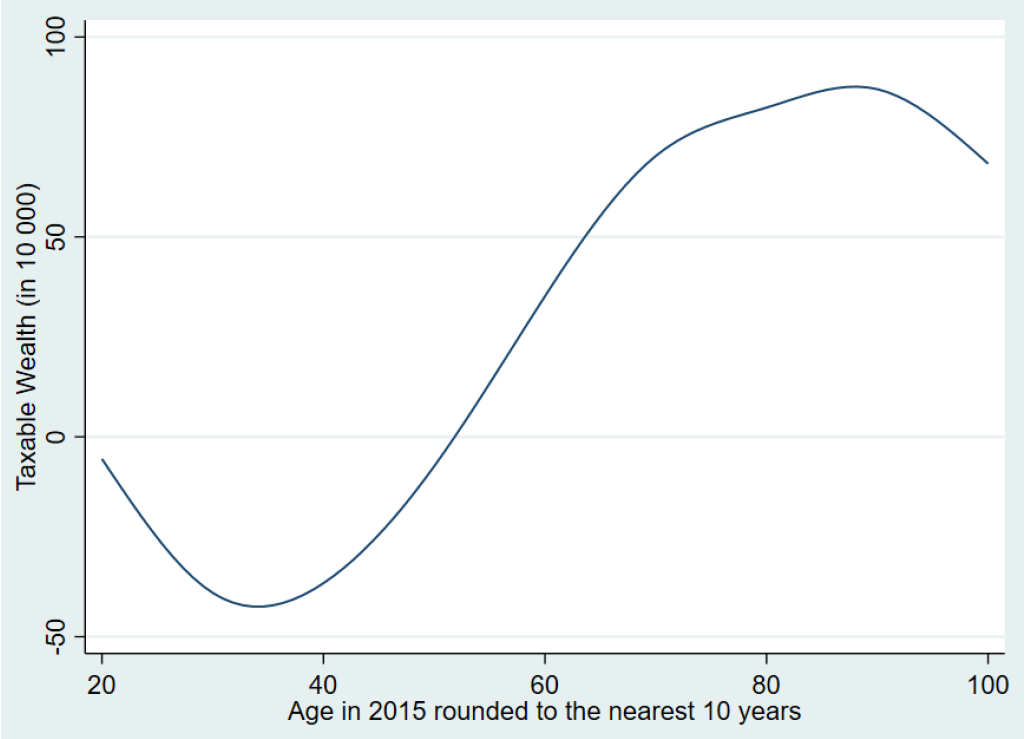
[29] The individuals on the upper end in the income distribution would increase the average if they were included.

**Table 3: Descriptive Statistics on the Full Sample**

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| **Taxable Wealth (mean)** | - 35 900 | 18 400 | 12 700 | 19 700 | 24 700 | 27 500 | 50 500 | 77 700 |
| **Income (mean)** | 230 700 | 238 600 | 249 500 | 259 700 | 268 900 | 276 500 | 284 300 | Missing |
| **Debt (mean)** | 703 800 | 738 200 | 776 700 | 819 400 | 857 200 | 899 200 | 936 400 | 986 100 |
| **Housing % of wealth** | 38,80 | 44,90 | 45,20 | 46,50 | 46,20 | 46,10 | 46,70 | 47,20 |
| **Listed shares % of wealth** | 2,58 | 2,44 | 1,98 | 1,93 | 2,09 | 2,07 | 2,22 | 0,62 |
| **Unlisted shares % of wealth** | 0,97 | 0,90 | 0,89 | 0,89 | 0,92 | 0,97 | 1,02 | 1,12 |
| **Business assets % of wealth** | 4,35 | 3,18 | 3,12 | 2,89 | 2,89 | 2,72 | 2,59 | 2,64 |
| **Men in %** | 49,60 | 49,70 | 49,80 | 50,00 | 50,20 | 50,30 | 50,20 | 50,20 |
| **Age** | 53,85 | 51,75 | 51,68 | 50,64 | 49,60 | 48,65 | 47,80 | 47,64 |
| **Observations** | 3 753 351 | 3 822 537 | 3 892 323 | 3 966 513 | 4 043 608 | 4 110 278 | 4 156 909 | 4 058 867 |

Figure 4 below shows the development of taxable wealth with age. It is judicious to reflect upon the individual's financial life cycle since taxable wealth is our dependent variable of interest. Individuals' tendency to have negative wealth early in life, which becomes positive towards mid-life, is intuitive. Early in life, individuals often consume more than their income dictates. Society expects that younger individuals borrow money for education and primary residence. As life progresses, mortgages are paid off, and income streams are steady. Financial freedom also increases as individuals/couple's offspring leave the nest and start their financial life cycle. When the retirees begin to face their inevitable fate, they sell their assets to the young, who are in the phase of accumulating wealth. This is the classical Modigliani life cycle hypothesis (Deaton 2005). The graph substantiates this hypothesizes and shows that negative wealth early in life reverts at mid-age and peaks at retirement. Moreover, it is evident, that on average, it is the older individuals who pay wealth taxation in Norway.

**Relationship Between Age and Taxable Wealth in 2015**




F*igure 4: Average taxable wealth (in NOK) in relation to age from the original data set. Based on 2015 wealth. Note: We have smoothed the graph using median spline.*

Figure 5 below presents the number of individuals liable to wealth taxation each year, and it shows the average wealth tax payments. We created the graph after combining couples. We observe that the number of households liable to wealth tax has decreased over the period, while the average wealth tax payments have increased. Average payments have gone significantly up, starting with 6 700 NOK in 2009, peaking in 2013 with 8 500 NOK, and ending in 2016 with 8 250 NOK; that is a total increase of 23 percent. Knowing that the yearly activation thresholds have increased over the period, the increase in average payments are most likely coming from growth in wealth. As fewer people are liable to wealth taxation, the remaining wealthy taxpayers drag the average upwards. The number of individuals paying wealth tax has gone drastically down; in 2009, it was approximately 650 000 individuals, constituting 17 percent of the individuals in the data set. In 2016, the number was down to about 400 000, just around 10 percent. The wealth tax liability is more concentrated around the wealthiest individuals in 2016 compared to 2009.

Figure 5 is quite similar to the one made by Statistics Norway and The Ministry of Finance (Figure 2), presented in section 2.2. With the exception that our data do not contain the top 1.5 percent of the wealthiest individuals. Comparing the two graphs, we observe the same

trend and almost the same percentage of individuals paying wealth tax. The difference lies in the average wealth tax payments. In figure 2, average payments are roughly 25 000 NOK in 2009, ending in over 30 000 NOK in 2016. Almost four times the average payments individuals in our data set paid.

**Average Wealth Tax Paid and Numbers of Individuals Paying Wealth Tax in 2009-16**

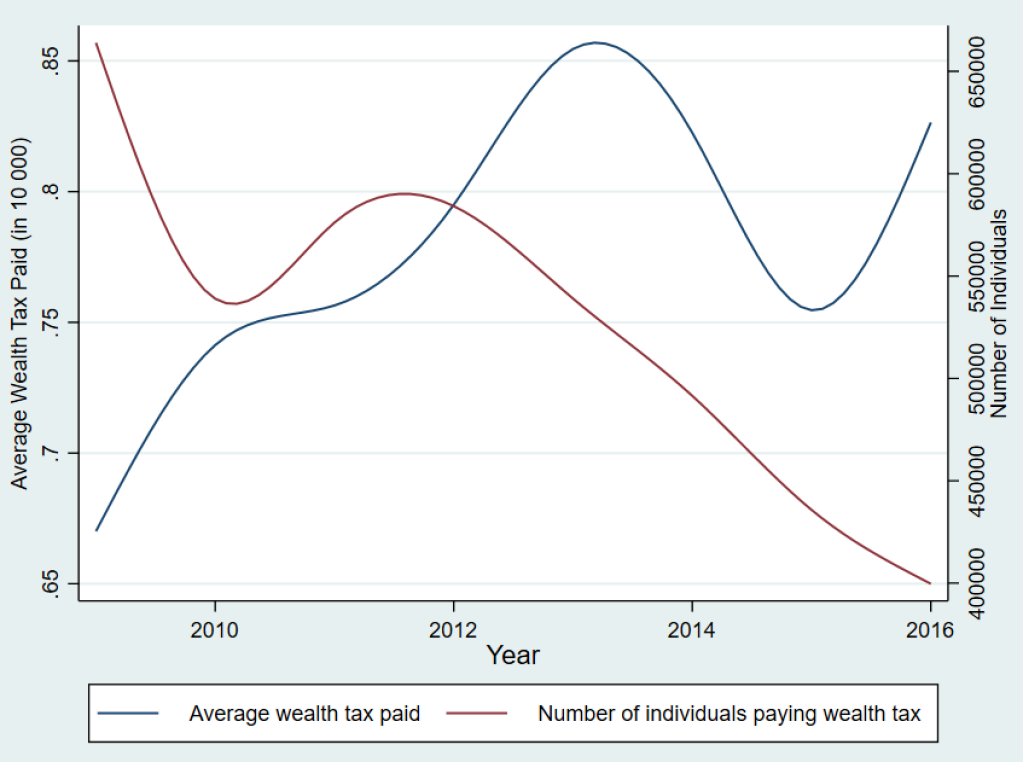



*Figure 5: Average wealth tax paid (left y-axis) and the number of individuals paying wealth tax (right y-axis) from 2009 to 2016. The graph is collected from the original data set and includes only individuals who pay wealth tax. Average wealth tax paid is in NOK. Note: We have smoothed the graph using median spline.*

## 5.2 Treatment and Control Group – First Regression Specification (3)

Figure 6 and Table 4 below show the development and statistics for the treatment and control group we use in the first Diff-in-Diff regression (3). We exclude households with negative taxable wealth through the whole period and households without observations in all years.

From the graph, we observe that the treatment group had, on average, a 117 percent total increase in taxable wealth over the period; increasing from nearly 900 000 NOK to over 1 950 000 NOK in the eight years. The control group, on the other hand, had a more volatile development. Starting in the neighborhood of 30 000 NOK in 2009, ending in 2016 with approximately 181 000 NOK; a 500 percent total growth. The control group is quite more

unstable than the treatment group. As previously discussed, this might induce some problems with identification. The households in the treatment group are, on average, older than the households in the control group, which the life-cycle graph of taxable wealth also reflects. In addition, households in the treatment group have more weight of the financial composition in listed shares and business assets than the control group.

**Development in Average Taxable Wealth for the Treatment and Control Group**

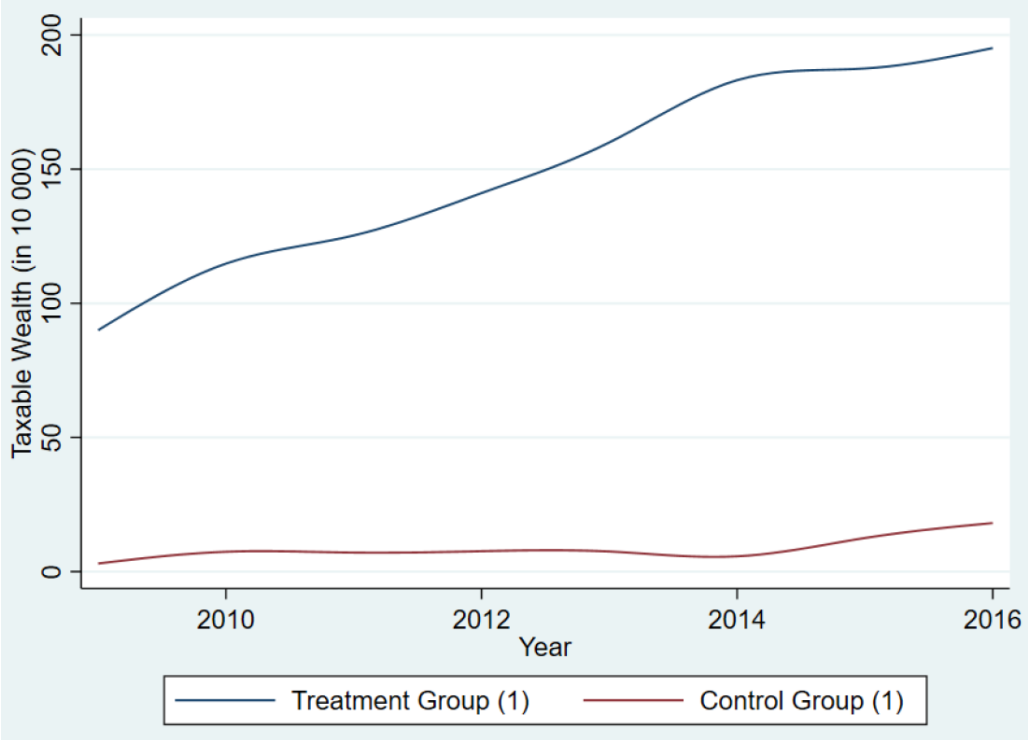



*Figure 6: The development of average taxable wealth in NOK for the treatment and control group used in the first Diff-in-Diff regression (3), from 2009 to 2016. Note: We have smoothed the graph using median spline.*

**Table 4: Treatment and Control Group Used in the First Diff-in-Diff Regression (3)**

**Treatment Group**

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| **Taxable Wealth (mean)** | 898 900 | 1 148 100 | 1 252 900 | 1 410 900 | 1 599 700 | 1 831 800 | 1 875 200 | 1 951 500 |
| **Income (mean)** | 332 000 | 345 700 | 365 000 | 382 700 | 394 100 | 411 610 | 408 500 | Missing |
| **Debt (mean)** | 317 500 | 312 500 | 305 700 | 395 700 | 275 190 | 227 600 | 271 450 | 298 600 |
| **Housing % of wealth** | 30,20 | 38,07 | 38,59 | 38,81 | 38,26 | 37,21 | 38,10 | 38,82 |
| **Listed shares % of wealth** | 6,07 | 5,70 | 4,75 | 4,70 | 5,13 | 5,12 | 5,58 | 1,90 |
| **Unlisted shares % of wealth** | 2,50 | 2,40 | 2,43 | 2,45 | 2,54 | 2,74 | 2,66 | 2,65 |
| **Business assets % of wealth** | 7,79 | 5,91 | 5,81 | 5,52 | 5,33 | 4,99 | 4,74 | 4,56 |
| **Men in %** | 51,05 | 51,1 | 51,03 | 51,15 | 51,15 | 51,1 | 51,15 | 51,15 |
| **Age (mean)** | 65,86 | 65,88 | 65,89 | 65,89 | 65,89 | 65,89 | 65,89 | 65,88 |
| **Observations** | 459 048 | 459 048 | 459 048 | 459 048 | 459 048 | 459 048 | 459 048 | 459 048 |

**Control Group**

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| Taxable Wealth (mean) | 30 316 | 74 320 | 71 070 | 76 310 | 75 115 | 57 060 | 125 485 | 181 360 |
| Income (mean) | 217 190 | 230 170 | 244 980 | 258 270 | 270 500 | 280 585 | 291 910 | Missing |
| Debt (mean) | 334 000 | 362 600 | 398 200 | 439 600 | 481 000 | 529 400 | 540 900 | 564 600 |
| Housing % of wealth | 31,44 | 36,21 | 37,15 | 39,07 | 39,76 | 40,51 | 41,61 | 42,48 |
| Listed Shares % of wealth | 3,60 | 3,40 | 2,80 | 2,68 | 2,80 | 2,80 | 2,90 | 0,70 |
| Unlisted shares % of wealth | 0,98 | 0,93 | 0,92 | 0,92 | 0,95 | 0,98 | 1,07 | 1,17 |
| Business assets % of wealth | 4,46 | 3,52 | 3,52 | 3,35 | 3,40 | 3,28 | 3,19 | 3,18 |
| Men in % | 50,60 | 50,70 | 49,90 | 50,02 | 50,20 | 50,02 | 50,60 | 50,20 |
| Age (mean) | 53,46 | 53,49 | 53,51 | 53,52 | 53,52 | 53,51 | 53,51 | 53,50 |
| Observations | 1 845 714 | 1 845 714 | 1 845 714 | 1 845 714 | 1 845 714 | 1 845 714 | 1 845 714 | 1 845 714 |

## 5.3 Treatment and Control Group – Second Regression Specification (6)

The statistics and development for the treatment and control group we use in the second Diff-in-Diff regression are shown in Figure 7 and Table 5 below. We define the treatment group almost in the same manner as in the first Diff-in-Diff regression. Instead of basing treatment status on the 2014 assets, we base it on average taxable wealth for the household over the sample period. We restrict the control group to only include positive wealth values to make the groups more comparable and stable. By determining treatment status on average taxable wealth, we are more confident that the households are persistently in a wealth tax position.[30]

We see a steady growth of average wealth in both groups. On average, the treatment group has seen their wealth increased by 90 percent, from 1 145 000 NOK in 2009 to 2 185 000 NOK in 2016. The control group has had a steeper growth with 180 percent in the same period, from 187 000 NOK to 531 000 NOK. Table 5 also shows some financial composition differences between the groups. The treatment group has a lesser percentage of wealth in housing and more in listed shares and business assets than the control group. This is in line with the notion that business owners and active investors are financially better off than individuals with income streams primarily from labor. A US survey conducted by Federal Reserve Bulletin (2020) indicates that 31 percent of households in the bottom half of the income distribution invest in stocks. In the top decile, as much as 90 percent hold stocks. We

[30] Ninety-five percent of those with average taxable wealth above the 2014 threshold had actual 2014 taxable wealth above the threshold.

see the same tendencies. This might indicate that households with higher income have more financial freedom and want to save in order to consume more later in life.

In comparison with the first treatment and control group, we experience some differences. First of all, visually, it looks like these groups are more comparable. Second, the first control group contains over 560 000 more households than the second. Third, the second control group is less volatile and does not increase as much during the period as the first. And fourth, the second treatment and control group contain individuals who have a more considerable taxable wealth. This makes the households in the control group more similar in behavior with the treatment group because the wealth gap is smaller than in the first treatment and control group.

**Development in Average Taxable Wealth for the Treatment and Control Group**

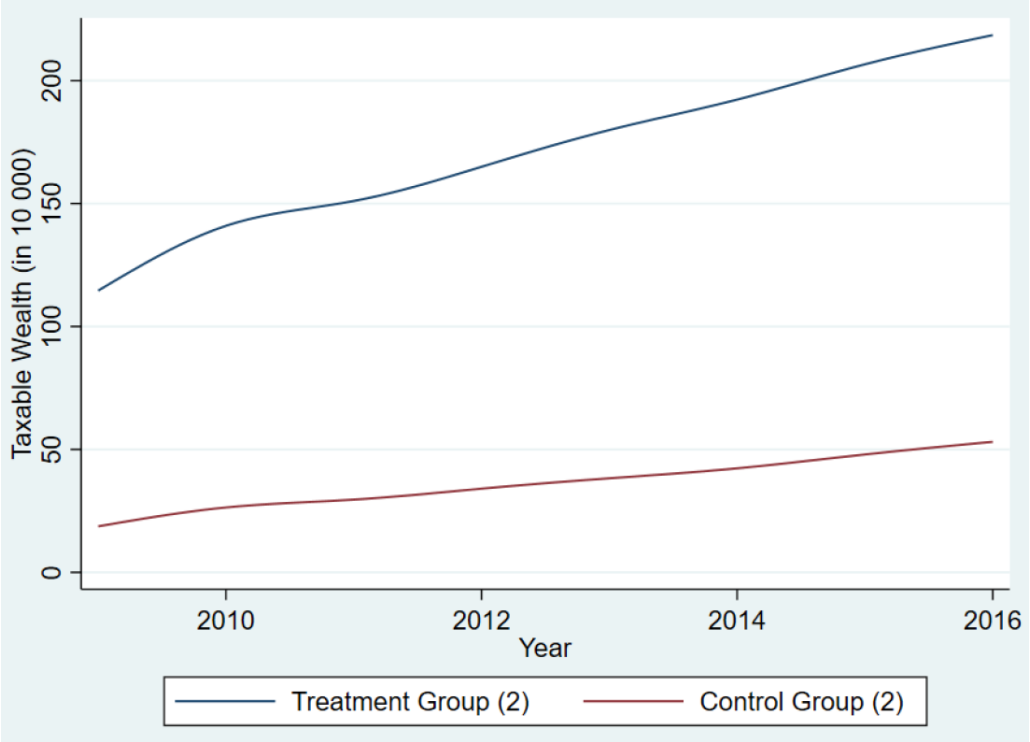



*Figure 7: Development in average taxable wealth (in NOK) in the treatment and control group used in the second Diff-in-Diff regression, from 2009 to 2016. Note: We have smoothed the graph using median spline.*

## Table 5: Treatment and Control Group Used in Second Diff-in-Diff Regression (6)

**Treatment Group**

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| Taxable Wealth (mean) | 1 145 800 | 1 409 600 | 1 510 400 | 1 649 700 | 1 799 200 | 1 922 900 | 2 066 500 | 2 185 300 |
| Income (mean) | 311 310 | 354 210 | 372 600 | 388 590 | 401 800 | 410 740 | 408 400 | Missing |
| Debt (mean) | 236 100 | 226 100 | 220 800 | 220 400 | 219 500 | 217 200 | 217 100 | 227 300 |
| Housing % of wealth | 28,02 | 35,41 | 35,93 | 36,07 | 35,94 | 35,64 | 36,15 | 36,78 |
| Listed shares % of wealth | 6,37 | 6,12 | 5,04 | 5,01 | 5,48 | 5,52 | 5,93 | 2,14 |
| Unlisted shares % of wealth | 2,83 | 2,71 | 2,71 | 2,76 | 2,70 | 2,77 | 2,78 | 2,79 |
| Business assets % of wealth | 8,00 | 6,26 | 6,14 | 5,85 | 5,59 | 5,18 | 4,89 | 4,68 |
| Men in % | 51,32 | 51,32 | 51,32 | 51,32 | 51,32 | 51,32 | 51,32 | 51,32 |
| Age (mean) | 67,38 | 67,40 | 67,41 | 67,41 | 67,41 | 67,41 | 67,41 | 67,40 |
| Observations | 371 542 | 371 542 | 371 542 | 371 542 | 371 542 | 371 542 | 371 542 | 371 542 |

**Control Group**

| | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 |
|---|---|---|---|---|---|---|---|---|
| Taxable Wealth (mean) | 187 460 | 264 230 | 296 110 | 340 690 | 382 830 | 423 350 | 479 740 | 531 090 |
| Income (mean) | 219 600 | 230 160 | 243 700 | 255 700 | 266 700 | 275 490 | 282 260 | Missing |
| Debt (mean) | 213 500 | 213 400 | 214 600 | 218 800 | 223 300 | 227 800 | 233 400 | 247 900 |
| Housing % of wealth | 32,8 | 37,35 | 37,89 | 38,88 | 39,02 | 38,93 | 39,51 | 40,07 |
| Listed Shares % of wealth | 3,73 | 3,67 | 3,01 | 2,90 | 3,10 | 3,08 | 3,20 | 0,76 |
| Unlisted shares % of wealth | 0,92 | 0,88 | 0,87 | 0,88 | 0,91 | 0,95 | 0,99 | 1,04 |
| Business assets % of wealth | 4,57 | 3,65 | 3,60 | 3,46 | 3,44 | 3,32 | 3,22 | 3,20 |
| Men in % | 37,21 | 37,21 | 37,21 | 37,21 | 37,21 | 37,21 | 37,21 | 37,21 |
| Age (mean) | 58,46 | 58,48 | 58,49 | 58,49 | 58,49 | 58,49 | 58,49 | 58,48 |
| Observations | 1 283 155 | 1 283 155 | 1 283 155 | 1 283 155 | 1 283 155 | 1 283 155 | 1 283 155 | 1 283 155 |

# 6 Results and Discussion

This chapter presents the results from the Diff-in-Diff regression (3) in section 4.4 and the extended and altered Diff-in-Diff regression (6) in section 4.6. Also, we present the results from the event study of both the regression specifications, including the long-run elasticity of taxable wealth. We base our analysis, discussion, and robustness check on the second regression specification (6) due to the instability of our first specification (3).

## 6.1 The First Regression Specification (3)

**Table 6: First Regression Specification (3)**

| | (1)<br>Main |
|---|---|
| $T_i After_t$ | -0.174***<br>(0.007) |
| 10-piece spline for Wealth | NO |
| 10-piece spline for Income | NO |
| Age splines | NO |
| Household/Time fixed effect | YES |
| R-Squared | 0.014 |
| Nr of Observations | 18 438 096 |

Note: This is the first regression specification (3) from section 4.4. Taxable wealth is transformed by the arcsinh function. We exclude households with strictly negative wealth values for the whole period, allowing individuals to have negative taxable wealth for some years. The parameter of interest is $T_i After_t$, representing a (100* $T_i After_t$) percentage change in wealth by an increase of 1 percentage point in $T_i$ (tax rate). Fixed effects estimation is used in STATA. Standard errors are clustered at the individual/household level. Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01

**Event Study of the First Regression Specification (3)**

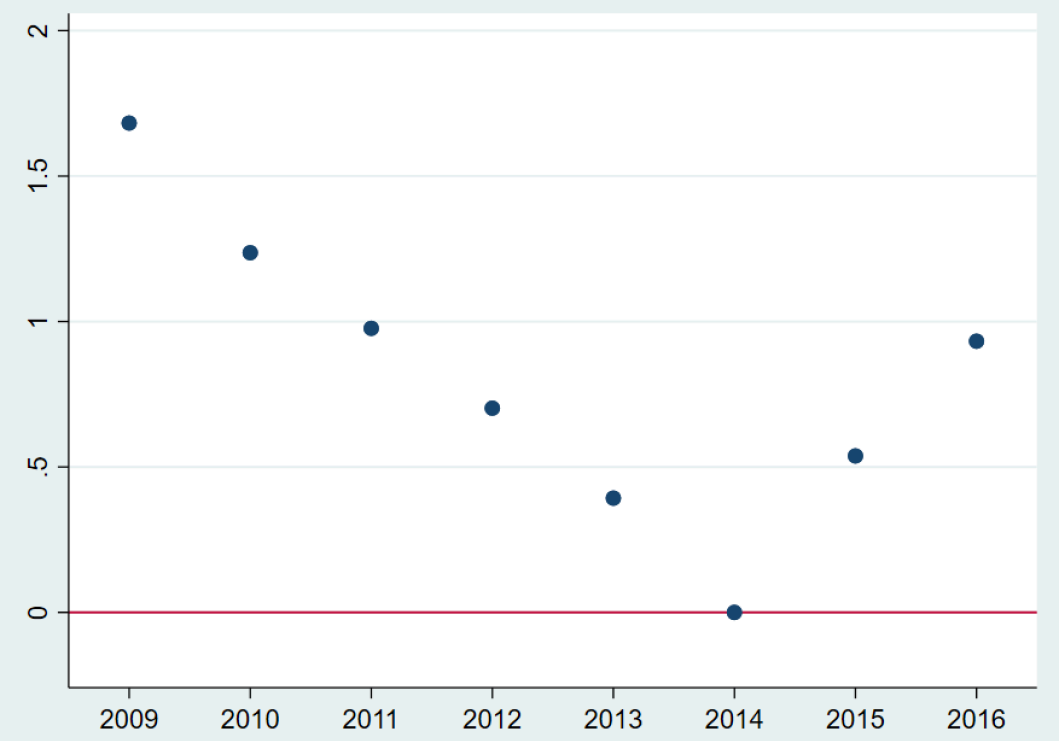



*Figure 8: The event study is based on the estimation (5) in section 4.5. Note: in table 6, $T_i After_t$ is an interaction between $T_i$ and the two years after 2014. In the event study, $T_i$ interacts with each year in the period, where 2014 is normalized to zero. 2014 is the wealth we chose to incorporate the 2015 tax rules on, i.e., the tax policy entrance point. Standard errors are clustered at the individual/household level. Confidence intervals are at the 95 percent significance level.*

Following the intuition from the Diff-in-Diff regression (3): $T_i After_t$ constitutes our instrument for the wealth tax rate interacted with a time dummy ≥2015. By including a time dummy, we observe the effect for the treated household $i$, after the reform. The coefficient represents that for a 1 percentage point increase in the wealth tax rate, the treatment group experiences a $(T_i After_t * 100)$ percentage change in wealth.

Our main results (1) identify that for a 1 percentage point increase in the wealth tax rate, the treated experience a reduction in wealth of 17.4 percent aggregate in the two years. The coefficient is significant at the 1 percent level. The effect is relatively large, but the sign of the coefficient is reassuring and in line with our expectations. Given the relationship between the net-of-tax rate elasticity and our estimated coefficient, a 1 percentage increase in the net-of-tax $(1 - T_i)$ leads to a 17.4 percent increase in wealth. The reduction in wealth by an increase in the tax rate, is in line with findings of other studies (Zoutman 2018; Seim 2017; Brülhart et al. 2019; Jakobsen et al. 2020; of Durán- Cabré, Esteller-Moré, and Mas-Montserrat (2019); Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Formny and Martínez-Toledan 2021).

### 6.1.1 Event Study

However, as discussed in section 4.5, the validity of our regression estimates depends on the common trend assumption. Looking at the event study in figure 8, this assumption evidently fails. We observe a strong V-shape of the variable $T_i$ interacted with yearly dummies, where 2014 is normalized to zero. The premise relies on non-significant estimates in the pre-reform years, which means that the treatment status in the model-implied construction of $T_i$ should not offer significant predictions in the pre-reform years. It does, highly significant estimates (CI's are included, just not visible). As discussed in the method chapter, especially sections 4.5 and 4.6, these problems might arise in the case of mean reversion in wealth. Recall, we choose to include households with negative taxable wealth somewhere in the period, but not the entire period. Using an income/wealth cutoff is common in the literature because of too much mean reversion at the low end of the distribution. Also, households on the lower level might be a poor comparison group (Weber 2014). Eighteen percent of those who receive treatment status have negative taxable wealth somewhere in the period. Indeed, the argument put forward by Weber (2014) might very well be the reason our event study is not valid. These households with negative wealth must be very volatile in order to be over the 2014 threshold. Problems of mean reversion in our two groups are not unlikely. Since the validity of our Diff-in-Diff estimations is not anchored in the event study, we proceed to the next model, where we successfully overcome these problems.

## 6.2 The Second Regression Specification (6) – Main Results

**Table 7: Second Regression Specification (6) – Main Results**

| | (1) Main Result | (2) No Age Spline | (3) Only Wealth Splines | (4) No Controls |
|---|---|---|---|---|
| *Panel A: Main Results* | | | | |
| $T_i After_t$ | -0.0224*** (0.005) | -0.0211** (0.005) | -0.0116*** (0.005) | 0.266*** (0.005) |
| 10-piece spline for Wealth | YES | YES | YES | NO |
| 10-piece spline for Income | YES | YES | NO | NO |
| Age splines | YES | NO | NO | NO |
| Household/Time fixed effects | YES | YES | YES | YES |
| R-Squared | 0.089 | 0.084 | 0.074 | 0.069 |
| Nr of Observations | 13 237 576 | 13 237 576 | 13 237 576 | 13 237 576 |
| *Panel B: Long-Run Results* | | | | |
| 2015 # $T_i$ | -0.0171*** (0.0038) | -0.0167*** (0.0038) | -0.0138*** (0.0038) | 0.0539*** (0.0037) |
| 2016 # $T_i$ | -0.0651*** (0.0051) | -0.0644*** (0.0051) | -0.0585*** (0.0051) | 0.0767*** (0.0049) |
| 10-piece spline for Wealth | YES | YES | YES | NO |
| 10-piece spline for Income | YES | YES | NO | NO |
| Age splines | YES | NO | NO | NO |
| Household/Time fixed effects | YES | YES | YES | YES |
| R-Squared | 0.088 | 0.084 | 0.074 | 0.069 |
| Nr of Observations | 13 237 576 | 13 237 576 | 13 237 576 | 13 237 576 |

Note: The outcome variable wealth is transformed by the arcsinh function. A household may have negative taxable wealth in one or more years, even if the average taxable wealth is positive. Panel A presents the main results of the second Diff-in-Diff regression (6), section 4.6. The estimation of $T_i After_t$ is based on the two subsequent years after the entry point of the 2014 reform. Fixed effects estimation is used in STATA. Splines for wealth/income are controls for the development in each group over the period. Age splines are controls for wealth development within five equally larger intervals of age over the period. Other studies have used the age of the primary earner. We chose to sum and divide couples' age by two to determine which group they belong to. Panel B: debunks the two subsequent years after the reform into a short-run and "long-run" estimate of the elasticity of taxable wealth. Standard errors are clustered at the individual/household level. Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

**Event Study of the Second Regression Specification (6) – Main Result (1)**

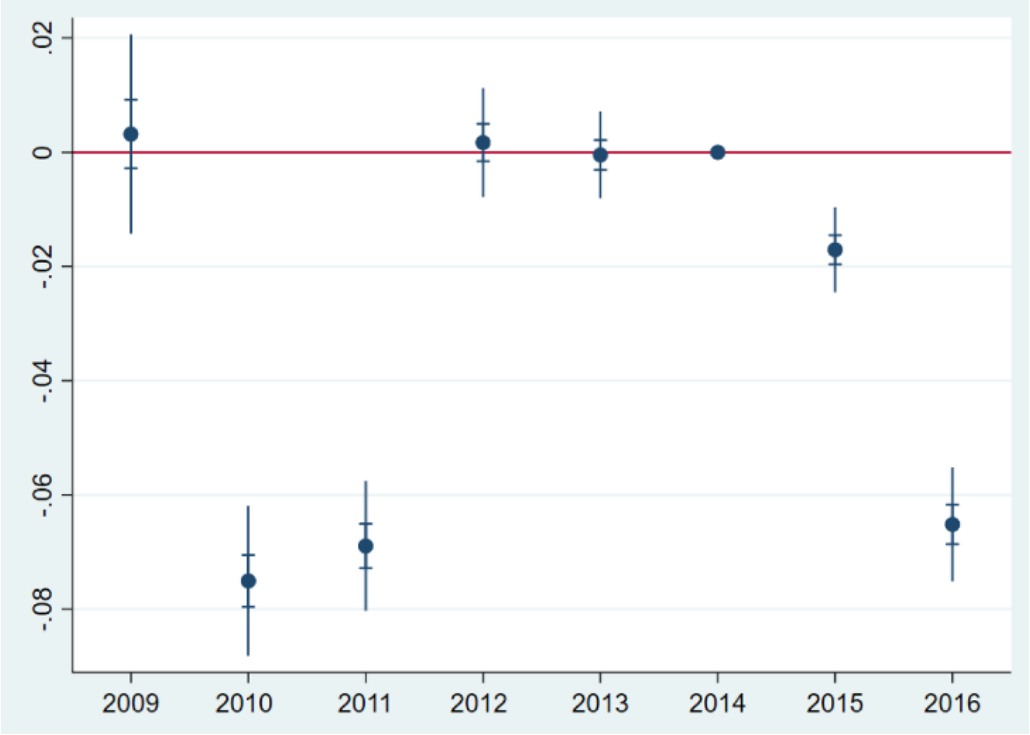



*Figure 9: Event study based on the second regression Specification (6), section 4.6 – Main Result (1). Note: in Table 7 Panel A, $T_i After_t$ is an interaction between $T_i$ and the two years after 2014. In the event study, $T_i$ interacts with each year in the period, where 2014 is normalized to zero. 2014 is the entrance point of the tax reform. Standard errors are clustered at the individual/household level. Confidence intervals are on the 95 percent significance level.*

Following the intuition of the second regression specification (6). The interpretation of $T_i After_t$ is the same as in the first regression specification. We base the treatment and control group in this Diff-in-Diff regression on their average wealth instead of the 2014 assets as in the first regression. We remove all households with average taxable wealth below zero. The descriptive statistics (section 5.3) show that the treatment and control groups are more comparable than the two groups in the first regression. They are more similar in terms of wealth levels. The development of the control group's taxable wealth is more stable than the first control group. Also, comparing the two figures of the two different sets of groups (section 5.2 and 5.3), the second seems to capture a more similar trend.

Panel A of regression Table 7 presents the main results of the second regression specification. We include four alterations of this regression specification, including one additional control variable at the time, leading up to the main results (1). We observe that the model is sensitive to control variables. Regression (4) gives a positive coefficient for tax rate, a large and significant effect at the 1 percent level. When we include controls for the household's wealth distribution in regression (3), the sign of the coefficient switches, which is not unique for our

model as the same happened in Gruber and Saez (2002). We obtain a net-of-tax rate elasticity of 1.16. The coefficient is significant at the 1 percent level. In regression (2), we include controls for the income distribution of the household as well. Reassuring, the coefficient is still negative, and we obtain a larger effect, significant at the 5 percent level.

### 6.2.1 Main Results (1)

In addition to wealth/income distribution controls, we added controls for the development of wealth in each age interval over the period. We observe that the effect has enlarged. A 1 percentage point increase in the tax rate reduces wealth by 2.24 percentage in aggregate, significant at the 1 percent level. This translates to a net-of-tax rate elasticity of 2.24, which is our main finding in this thesis. We debunk the two post-reform years in panel B and obtain a more sizeable coefficient for 2016. A 1 percentage increase in the net-of-tax rate increases wealth by 6.5 percent. This indicates a sluggish response to taxation and is in line with most of the other studies. Our findings substantiate the existing literature, an increase in the tax rate reduces taxable wealth (Zoutman 2018; Seim 2017; Brülhart et al. 2019; Jakobsen et al. 2020; Durán- Cabré, Esteller-Moré, and Mas-Montserrat 2019; Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Formny and Martínez-Toledan 2021). Moreover, it contradicts the findings of Ring (2020), which is the only other Norwegian study on this topic.

### 6.2.2 Compared to Scandinavian Studies

Seim (2017), using a bunching technique, estimates the net-of-tax rate elasticity in Sweden to be 0.09-0.27. By an approximation, a 0.1 percentage point increase in the tax rate reduces wealth by 0.027 percent using the upper bounds of his estimates.[31] In our estimates, a 0.1 percentage point increase in the tax rate reduces wealth by 0.224 percent. As Advani and Tarrant (2020) highlight, bunching techniques normally exploit an individuals' motivation to aim their wealth to go below the threshold, giving elasticities local to the threshold. Since the wealthiest individuals may not simply go below the threshold, the estimations do not pick up their responses. Consequently, the elasticities obtained by bunching techniques are likely to be smaller. Our estimates are somewhat larger than Seim's, but we utilize a diff-in-diff

[31] Seim's (2017) estimates are not entirely comparable because he estimates the elasticity w.r.t the net-of-tax rate while we estimate in terms of increases in the tax rate. Given that the Swedish wealth tax was 1.5 percent, we can approximate for small numbers using the properties of logarithms $\log(1+x) \approx x$ to express in terms of 0.1 percentage point increase in the tax rate (Zoutman 2018; Durán-Cabré, Esteller-Moré and Mas-Montserrat 2019).

identification, where we include not only those who target the threshold. Hence, our elasticity is reasonably larger. However, the evidence for larger elasticities of the wealthiest is not entirely apparent but suggestive (Jakobsen et al. 2020; Zoutman 2018).

Jakobsen et al. (2020) estimate the 8-year net-of-tax rate elasticity, using Danish wealth data in a Diff-in-Diff framework, to be 8.9 for the moderately wealthy and 11.3 for the very wealthy. Considering that this is the 8-year aggregate response, they are more similar in terms of our estimates. We have data only for two post-reform years; looking at panel B, the second-year estimates come close to Jakobsen et al.'s (2020) findings of the moderately wealthy. If we had more than two post-reform years, our estimates of aggregate responses would likely become similar to the Danish findings on the moderately wealthy.[32]

Ring (2020), in the only other Norwegian study on the elasticity of taxable wealth, finds very different results than us. He estimates that for each NOK subjected to the 1 percent wealth tax, households increase savings by 0.04 NOK, primarily financed by labor earnings. Notably, he initially finds that the wealth tax reduces the amount of taxable wealth which households report. However, he arrives at this positive effect after limiting evasion opportunities. Suggesting that the income effect dominates the substitution effect, individuals presumably save to maintain future consumption or to meet the tax liability. He removes the wealthiest 1 percent (NoCET removed the top 1.5 percent in our data); he motivates this by the findings of Alstadsæter, Johannesen, and Zucman (2019a), evasion primarily occurs above the 99 percentile of the wealth distribution. By his argument, we also limit the scopes of evasion. Further, he keeps only individuals with wealth above zero. Our choice of cutoff limits coincides with his. Nevertheless, we arrive at different effects of Norwegian wealth taxation. Most of the assets in the data set are third-party reported, leaving us with minor concerns about tax fraud or evasion. However, some assets like foreign housing and deposits are primarily self-reported.[33] Ring (2020) excludes these assets in his estimation; we do not. This

[32] The moderately wealthy constitutes the households between the 97.6th and the 99.3rd percentile in wealth distribution.

[33] Norwegian tax authorities cooperate with 109 countries regarding the exchange of information of the individuals' financial standings. The Norwegian tax authorities receive information on 350 000 Norwegian residents with foreign capital holdings (NTB 2021; Skatteetaten(b) n.d.). However, this does not necessarily limit individuals from underreporting foreign holdings in non-collaborative countries. The Norwegian tax authorities must rely on the individual to report correctly.

might entail that our elasticities pick up evasion if households, for example, move capital to offshore accounts or underreport the value of foreign assets.

The findings of Seim (2017) suggest that evasion is greatly restricted when self-reporting is limited. Additionally, the findings of Alstadsæter, Johannesen, and Zucman (2019b) indicate that when the Norwegian government boosted the enforcement effort, many wealthy individuals disclosed previously hidden assets.[34] Notably, the disclosure of wealthy individuals evaded assets did not increase avoidance by investing in tax-favored assets.

These findings lead us to the conclusion that evasion is unlikely a distorting factor in our estimation. Because our data set does not contain the top 1.5 percent, the efficient enforcement by the Norwegian government makes evasion difficult, and the fact that self-reporting is limited also reduces these responses. However, there are several popular tax avoidance schemes, which our estimates might pick up. Bjørneby, Markussen, and Røed (2020), using Norwegian data, find that business owners invest in human capital to reduce their taxable wealth due to the favorable tax valuation of private companies.[35] Importantly, we must distinguish our estimates of those by Ring (2020). Our estimations point in the direction of a reduction in taxable wealth by an increase in the wealth tax rate. Evasion responses are somewhat limited. Nevertheless, we do not take the same measures as Ring (2020) to verify that these effects are limited. We cannot be entirely sure whether the effects we are seeing are real saving responses to wealth taxation or if it signals evasion or avoidance. Regardless, wealth taxation reduces the household's taxable wealth. The implication of this may be in particular interest of the policymaker's because it may signal inefficient resource allocation when households are faced with an increased tax liability—for example, placing a considerable amount in primary residences instead of value-added investments. In addition, households might place more capital into non-listed companies to reduce their taxable wealth and tax liability.

[34] The Norwegian tax administration operates a tax amnesty program. 1,500 taxpayers disclosed previously hidden assets during 2008-2016.
[35] Glodal and Hestdal (2015) estimate that the average tax valuation rebate for non-listed firms traded at the Norwegian OTC-list is 68.1 percent, making them lucrative as a tax avoidance vehicle.

### 6.2.3 Compared to Studies Outside Scandinavia

Zoutman (2018), using data from the Netherlands, estimates that a 1 percentage point increase in the tax rate reduces wealth by 11.6 percent after 1 year and 13.8 percent after 4 years. His estimates show a slight difference between the short-run and long-run elasticity, indicating that most of the adjustment seems to come immediately. He expresses that this is unlikely to be changes in real behavior, changes in reporting behavior are more plausible. These estimates are quite larger than our Norwegian estimates. Because wealth levels are more concentrated in the Netherlands than Norway[36] and studies suggest larger elasticities in the upper end of the distribution,[37] these higher elasticities are arguably reasonable.

Our estimates are quite smaller than those found in Switzerland and Spain. Durán-Cabré, Esteller-Moré, and Mas-Montserrat (2019), using Spanish data, estimate that a 1 percentage point increase in the tax rate reduces taxable wealth by 32.4 percent after 4 years. They conclude that savings are unaffected and that their estimates mainly signal avoidance, with business assets as the primary vehicle. Note, they estimate how individuals respond to the reintroduction of the wealth tax in Spain. The wealth tax jumped from zero to 2.5 percent at most.[38] This significant increase in the wealth tax liability might give stronger behavior responses than our Norwegian study, where there has only been a marginal reduction in the tax rate. While in Switzerland, Brülhart et al. (2019) estimate that a 1 percentage point increase in the tax rate reduces wealth by 41.1 percent in aggregate after 4 years. These relatively high elasticities indicate that evasion and avoidance are particularly prominent in countries with little to no third-party reporting. While in the Scandinavian countries, where most assets are third-party reported, the elasticities (ours included) are much smaller (Ring 2020; Jakobsen et al. 2020; Seim 2017).

In Norway, the wealth tax is centralized, while in Switzerland, there is inter-cantonal variation, and in Spain, there is variation between the *Comunidades Autómas.*[39] Agrawal, Foremny, and Martinez-Toledano (2020) estimate the mobility elasticity w.r.t. net-of-tax rate to be 7.5 after 5 years in Spain. They isolate the effects of inter-regional migration, leaving

[36] The Netherlands had a wealth gini-coefficient of 0.902 in 2018 compared to Norway which had 0.798 (Credit Suisse 2019).
[37] Estimates of Zoutman (2018) and Jakobsen et al. (2020)
[38] The wealth tax was progressive - ranging from 0.2 to 2.5 percent, with regional differences in both thresholds and tax rates (Durán-Cabré, Esteller-Moré, and Mas-Montserrat 2019).
[39] Sub-national regions

out other responses. This response is smaller than what Durán-Cabré, Esteller-Moré, and Mas- Montserrat (2019) find in Spain. They measure the overall elasticity but exclude migration responses, giving them much larger elasticities. Brülhart et al. (2019) estimate that inter-cantonal migration explains about 24 percent of their elasticities. These responses indicate that in countries with inter-regional variation in tax policies, the elasticities are more significant than in countries with a uniform tax policy. Our estimates are reasonably smaller by this argument because there is no variation between the Norwegian municipalities, even though they have autonomy over their part of the wealth tax.

Londoño-Vélez and Ávila-Mahecha (2019), using Colombian data, estimate that a 1 percentage increase in the net-of-tax rate increases wealth by 2 percent in the short-term. These findings are, arguably, the closest to our elasticity. However, as they utilize a bunching strategy, it is reasonable to assume that they would have been larger using a Diff-in-Diff framework.

### 6.2.4 Summary

Changes in reporting behavior, inter-regional migration, or tax avoidance by placing capital in favorable assets explains most of the elasticities in these various studies (Zoutman 2018; Durán- Cabré, Esteller-Moré, and Mas-Montserrat 2019; Londoño-Vélez and Ávila-Mahecha 2019; Agrawal, Formny, and Martínez-Toledan 2021). We are, however, unable to separate these responses from real saving behavior in our study. By the more significant response in the second year, it seems reasonable to think Norwegian households are primarily passive savers,[40] as in Denmark (Chetty et al. 2014). They might be surprised by their new after-tax return after the reform. Their response to this reform is not apparent until they have had the time to assess the new rate of return (Zoutman 2015). In the second post-reform year (2016), households had the time to evaluate their new after-tax rate of return and align with the new tax environment. The response may capture both real saving behavior as well as avoidance. The short-run elasticity may reflect active savers' early adjustment, while the long-run estimates include the sluggish response of passive savers.

[40] Chetty et al. (2014) describe passive savers as those who are inattentive to tax subsidies and are heavily influenced by contributions made on their behalf. While the active savers respond to tax subsidies by shifting assets across accounts.

### 6.2.5 Event Study

This model also relies on the common trend assumption proposed in section 4.5. When comparing the event study of the second regression specification (6) (figure 9) to the first regression specification (3) (figure 8), we see a remarkable improvement. We observe non-significant predictions of the instrument in the years: 2009, 2012, and 2013. Where 2014 is the base year and normalized to zero. This shows that our second regression specification (6) has successfully mitigated some of the problems in our first regression specification. By basing treatment status on average taxable wealth instead of 2014 assets, we mitigate implications of mean reversion. This model is more stable and representable of those continuously subjected to the wealth tax liability. Furthermore, the control group has been made more comparable.

However, we see large significant effects of the instrument in the years 2010 and 2011. These estimates are coherent with the wave of increases in the valuation of assets from 2008-11. Before the tax reform of 2009-10, housing's tax valuation was based on historical cost, with an annual adjustment from previous years' tax value (Bjørneby, Markussen, and Røed 2020). The policymakers changed it to estimated sales value based on comparable housing in the same geographical area from 2010 and onward (Ministry of Finance 2009).[41] These policy changes inevitably increased many households' taxable wealth due to the great difference between market value and historical cost, especially since older housing was undervalued relative to newer housing. At the same time, business assets went from an annual adjustment of historical cost to market value, a significant increase for some wealthy individuals.

Those who received treatment in our model by their average wealth most likely experienced these rather significant increases in taxable wealth. Most households in the treatment group receive a reduction of 0.15 percent in their tax rate in the 2014-15 reform. This reduction is represented by $T_i$ which we extended to all years. Meaning, when the regression picks up this reduction and that wealth substantially increases due to the tax policy changes during 2009-11, we obtain these significant elasticities. They are most likely entirely driven by the mechanical increases in wealth due to the policy change. However, they might also pick up

[41] Due to simplification reasons, the lowest wealth tax rate (0.9%) was removed in 2009, leaving all taxable wealth taxed at 1.1%. Regardless, this had minor effects on what people paid in wealth tax because of the increased threshold (Ministry of Finance 2008).

behavioral responses. Households might be motivated to scheme their taxable wealth when faced with this significant increase in wealth and potential tax liability.
While most of the adjustments come simultaneously with the changes in tax valuation of housing (2010), we see a smaller effect in 2011. Given that the 2010 threshold is low compared to the following years, many households experienced an increased tax liability, also shown in Figure 5. Fortunately, after the effects of the tax policy changes during 2008-11 have died down. Our instrument is stable with non-significant estimates in the two pre-reform years, 2012-13. The policymakers did not make extensive changes in those two years, with only a slight increase in the threshold, valuation of secondary housing, and business properties.

Following the above discussion, we will argue for the validity of our second regression specification (6). Even though our model picks up two pre-reform estimates, we keep in mind that our treatment group (most likely) experienced these exogenous increases in wealth by the changed housing valuation. This does not, arguably, invalidate our findings; it only shows that it is hard to isolate responses to one particular reform in a dynamic tax environment.

## 6.3 Robustness Check

**Table 8: Robustness Check of the Second Specification (6)**

| | (1) Two Groups | (2) Mean Income > 100k | (3) Mean Wealth > 50k | (4) Mean Wealth > 100k |
|---|---|---|---|---|
| $T_i After_t$ | -0.0224*** | -0.0328*** | -0.0472*** | -0.0816*** |
| | (0.00449) | (0.0049) | (0.0048) | (0.0051) |
| 10-piece spline for Wealth | YES | YES | YES | YES |
| 10-piece spline for Income | YES | YES | YES | YES |
| Age splines | YES | YES | YES | YES |
| Household/Time fixed effects | YES | YES | YES | YES |
| R-Squared | 0.0878 | 0.0923 | 0.0985 | 0.104 |
| Nr of Observations | 13 237 576 | 11 514 696 | 11 646 352 | 10 772 552 |

Standard errors in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Table 8 present the robustness check of the main result from Table 7 Panel A. In (1), we choose to only focus on two treatment intensity groups instead of three. Those with taxable wealth above the 2014 threshold and below the 2015 thresholds receive a -1 percentage point change in their wealth tax rate. Those individuals constitute 5.49 percent of the observations. Instead of removing those individuals, we place them in the control group. The intuition is to have a more stable treatment group, those above both 2014 and 2015 thresholds, and subject to the wealth tax in both years. Placing them in the control group makes little impact on our estimate; it coincides with our main results. Because this group consists of few households, our estimates are no surprise.

In the spirit of Weber (2014), we choose to increase the cutoffs of inclusion. From regression (2), we see that a cutoff based on average income does not change our findings to a large degree. We see a slightly larger effect than our main results, significant at the 1 percent level. We still exclude those with average taxable wealth below zero. The motivation by setting an income cutoff is to exclude those who do not have the opportunity, arguably, to change their behavioral responses to wealth taxation by their income. These might be financially passive individuals, people who have inherited wealth or are widower/widows. We obtain a slightly larger effect when excluding this group, an elasticity w.r.t net-of-tax rate of 3.28 ($p<0.01$).

In (3) and (4), we choose to set the wealth cutoff higher. In both cases, we obtain larger estimates, significant at the 1 percent level. The motivation is to observe the sensitivity of our model to other cutoff limits. While regression (3) is still in line with our main findings, regression (4) suggests a more significant effect than initially obtained. This might indicate that our main results underestimate the effects. When we compare a treatment and control group even more similar in terms of wealth, the effect increases. However, this specification does not yield as credible results as our main findings in an event study. By setting the cutoff higher, those in the treatment group are still the same. Nevertheless, the control group might be more unstable. They are closer to the yearly thresholds early in the period. Meaning, they might have been subjected to wealth tax when the threshold was lower than in 2014-15, making them unfit to be a control group. We argue that this cutoff is too high, given that we use average taxable wealth. Moreover, when including those with taxable wealth above zero and below 100 kNOK, the responses of those with taxable wealth above 100 kNOK and below the 2014-15 thresholds seem to stabilize.

# 7 Limitations

After working empirically with the effects of wealth tax policies, we especially learned that it is hard to define the correct regression specification. Even though the literature offers concrete guidelines on dealing with the various issues that might arise, the transferability is not always obvious. In the first Diff-in-Diff estimation (3), we decided to include individuals who might have had negative taxable wealth in one or more years, but not all. In other studies, the cutoff was usually set around 10 000 euros or 10 000 dollars. In our case, this would have been more feasible if we worked with a shorter period. We argued that removing individuals who received treatment in the tax reform of 2014-2015 because of negative taxable wealth in prior years would go against its purpose. However, based on Diff-in-Diff regression (6) and the robustness check, we see that the model is sensitive to other cut-offs. Also, the persistence of taxable wealth above the yearly thresholds.

We recognize that our research has more limitations that might affect our estimates. We do not include the top 1.5 percent of the wealthiest part of the population. This was a choice made by NoCET and is due to privacy concerns regarding identifying individuals by their financial composition. Our households paid on average 6700 NOK in wealth tax in 2009; this increased to 8250 NOK in 2016. The official numbers from the Ministry of Finance (2020a) are approximately 25 000 NOK in 2009 and 30 000 NOK in 2016. Our findings point to a small but significant effect of an increase in the wealth tax rate. This may be because the households, on average, do not pay a considerable amount of tax in our sample. The wealthiest households might have better access to lawyers and accountants, helping them scheming their taxable wealth. We are convinced that if we had this group in our data set, then the elasticities would be more extensive, as suggested by the findings of Zoutman (2018) and Jakobsen et al. (2020). Nevertheless, we would still be unable to separate real behavior from avoidance and evasion.

Our empirical strategy does not take into consideration those individuals in the pursuit of making a fortune. We do not identify the effect on entrepreneurs and those in liquidity restrained situations. We neither include those who have emigrated because of the wealth tax. Ernst Ravnaas investigated capital flight by wealthy Norwegians, and wealth taxation was decisive as to why some chose to emigrate (Fasting 2016).

In our long-run panel, we only have two subsequent years after the reform. Preferably, since policymakers are mainly interested in the long-run effect of taxation policy, it would have been beneficial with more than two years post-reform. Because of the slow-moving nature of wealth accumulation and the time it takes for households to adjust to the new tax environment, we think the long-run effects would be more considerable. These tendencies are evident in various countries (Jakobsen et al. 2020; Brülhart et al. 2019; Zoutman 2018; Durán-Cabré, Esteller- Moré, and Mas- Montserrat 2019; Agrawal, Foremny and Martinez-Toledano 2020).

# 8 Conclusion

The aim of this thesis was to investigate the causal relationship between wealth taxes on wealth accumulation and savings. We were interested in making a contribution to this nascent field within the distortionary effects of wealth taxation. There have been various studies from foreign countries, but only one focused on this particular issue in Norway. Quantifying these behavioral responses to taxation is vital for policymakers to determine optimal taxation policies that lead to minimal efficiency loss. In most countries, an increase in the wealth tax rate reduces reported taxable wealth. Either by changed reporting behavior or by avoidance through investing in favorable assets. There is also evidence for strong migration responses within countries with inter-regional tax variation. Ring's (2020) study of behavioral responses to the Norwegian wealth tax indicated a positive relationship. Households save when faced with a wealth tax liability. We were surprised by this finding, considering the estimates from other countries, and it motivated us to make an additional contribution from Norway.

Our findings contradict the findings of Ring (2020). Similarly, as in foreign studies, we find a negative relationship between an increase in the wealth tax rate and savings/reported taxable wealth. However, as there are significant differences between Ring's (2020) methodology and ours, we cannot separate real behavior from avoidance. Still, our findings are of particular interest to the Norwegian policymakers because they might signal a shifting of wealth towards tax-favorable assets, as well as reduced savings.

We find small but significant distortive effects of wealth taxation. Notably, the Norwegian wealth tax is less distortive compared to wealth taxes in countries outside Scandinavia. The findings may indicate that in Scandinavian countries with mostly third-party reporting, avoidance and evasion opportunities are greatly limited.

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