Wealth Taxation and Household Saving: Evidence from Assessment Discontinuities in Norway

Marius A. K. Ring*

Please click here for most recent version

May 27, 2020

Abstract

I use a quasi-experiment in Norway to examine how households respond to capital taxation. The introduction of a new wealth assessment methodology in 2010 led to geographic discontinuities in household exposure to wealth taxes, along both the extensive and intensive margins. I exploit this novel variation using rich administrative data and a Boundary Discontinuity approach. In contrast to existing work, I find that exposure to wealth taxes has a positive effect on saving as well as a positive effect on labor earnings. For each additional NOK subject to a 1 percent wealth tax, households increase their yearly financial saving by 0.04 NOK. This increase in saving is largely financed by increased labor earnings. My results imply that income effects may dominate substitution effects in household responses to (net-of-tax) rate-of-return shocks, which has important implications for both optimal capital taxation and macroeconomic modeling.

*Kellogg School of Management, Northwestern University and Statistics Norway. I thank my advisors, Dimitris Papanikolaou, Anthony DeFusco and Matthew Notowidigdo for helpful comments and advice. I owe considerable gratitude to Andreas Fagereng and Thor O. Thoresen for their help in initiating this research project, as well as their continued support and guidance. I thank Scott Baker, Liam Brunt, Jim Poterba, Martin B. Holm, Matthias Doepke, Krisztina Molnár, Manudeep Bhuller, Evelina Gavrilova, Enrichetta Ravina, SeHyoun Ahn, Gernot Doppelhofer, Jacopo Ponticelli, and Floris Zoutman for helpful discussions, and the participants at the Kellogg Finance bag-lunch seminars, Skatteforum, NHH Norwegian School of Economics, London Business School, BI Norwegian Business School, Drexel, Yale, NYU, UT Dallas, WUSTL, Stanford, UCLA, MIT, Wisconsin-Madison, UT Austin, and the Center for Retirement Research at Boston College for helpful comments and questions. Support from the Research Council of Norway is gratefully acknowledged.

1 Introduction

How households respond to changes in the net-of-tax rate-of-return is crucial to both optimal capital taxation and macroeconomic modeling. In optimal capital taxation, it determines the extent of distortionary effects on saving behavior and labor supply. Quantifying these distortions is necessary for determining the optimal tax policy (Atkinson and Sandmo 1980, Straub and Werning 2019, Saez and Stantcheva 2018). In macroeconomics, it determines the ability of standard representative agent models to explain the aggregate effects of monetary policy (Kaplan, Moll, and Violante, 2018) and informs the importance of new transmission channels (Auclert 2019, Wong 2019). More generally, empirical responses to rate-of-return shocks inform the Elasticity of Intertemporal Substitution (EIS). The EIS is a key parameter in virtually all economic models involving intertemporal decision-making, but there is no consensus on what it should be.

Despite this broad importance, there is a dearth of applicable empirical evidence, reflecting challenges related to both identification and measurement. Exogenous shocks to the interest rate may have general equilibrium effects inhibiting the identification of the pure rate-of-return effect needed to inform micro-founded models. A potential solution is to exploit variation in capital taxation caused by peculiarities in the tax-code to identify partial-equilibrium effects. However, this strategy typically offers two problems. First, one must often compare households who differ on tax-relevant characteristics, such as wealth or gross income, that are also determinants of saving behavior. Second, even if capital taxation were randomly assigned, data limitations may preclude researchers from distinguishing between real saving responses or tax evasion. This is problematic, since evasion responses are uninformative of responses to other rate-of-return shocks, such as interest rate changes or capital taxation when evasion opportunities are restrained.

These empirical challenges are complemented by a long-standing theoretical ambiguity about even the *sign* of saving responses to rate-of-return shocks.[1] This ambiguity is due to countering income and substitution effects from increasing both the absolute and relative price of future consumption. Which effect dominates depends crucially on the EIS, for which the existing range of empirical estimates spans 0 to 2.[2] This is an "enormous range in terms of its implications for intertemporal behavior and policy" (Best, Cloyne, Ilzetzki, and Kleven, 2018)

[1] Boskin (1978) indirectly refers to the theoretical ambiguity in his seminal empirical paper: "In brief summary, there is very little empirical evidence upon which to infer a positive relationship (substitution effect outweighing income effect) between saving and the real net rate-of-return to capital. Surprisingly little attention has been paid to this issue – particularly in light of its key role in answering many important policy questions – and those studies which do attempt to deal with it can be improved substantially."

[2] In a standard life-cycle model without (non-capital) incomes, the income effect dominates whenever the EIS < 1. Including (endogenous) labor income lowers this cut-off to around 0.3 in the model in section 7, which is calibrated to the empirical setting of this paper.

and includes strikingly different household responses to economic news (Schmidt and Toda, 2019).

In this paper, I use a quasi-experimental setting in Norway that allows me to address the identification and measurement challenges discussed above. I exploit variation in household exposure to wealth taxes to identify the effect of adverse shocks to the (net-of-tax) rate-of-return on saving behavior and labor supply. While there is no distinction between taxing the stock or taxing the flow of capital in most models, wealth taxation differs from capital income taxation by requiring regular assessments of the stock of capital.[3] The steps that the Norwegian government has taken to make such assessments provides promising quasi-experimental variation in the net-of-tax rate-of-return.

In Norway, wealth taxes are levied annually as a 1 percent tax on taxable wealth exceeding a given threshold. The relatively low threshold subjects 12 to 15 percent of tax-payers to the wealth tax.[4] The two key components of the tax base are financial wealth and housing wealth. While financial wealth may assessed at third-party reported market values (which limits scope for evasion through misreporting), housing wealth must be determined by the tax authorities. In 2010, the tax authorities implemented a new model to assess the housing wealth component. This hedonic pricing model contained municipal fixed effects, which imposed geographic discontinuities in assessed housing wealth even in the absence of any true discontinuities in house prices. These discontinuities provide substantial identifying variation in taxable wealth and thereby (i) whether or not households pay a wealth tax and (ii) the amount of wealth taxes they pay. This provides variation in both the marginal and average net-of-tax rate-of-return. I use data on structure-level ownership and location as well as the exact parameters of the hedonic pricing model to implement this identifying variation in a Boundary Discontinuity Design (BDD) approach.

I first consider the effect on yearly financial saving. My estimates imply that for each additional NOK pushed above the tax threshold, and thereby subject to the wealth tax, households *increase* their yearly financial saving by NOK 0.04. These estimates adjust for the mechanical wealth-reducing effects of increased taxation and constitute evidence of behavioral responses to capital taxation that go in the opposite direction of what is typically assumed.[5] This adjusted saving propensity is four times larger than what is necessary to maintain the same level of wealth after taxes are collected, consistent with households increasing savings to offset future

[3]This includes Chamley (1986) and Judd (1985). The equivalence comes from assuming homogenous returns. As an example, a tax on capital income of τ_{CI} of a fixed rate-of-return r, is equivalent to a wealth tax of $\tau_{CI} \cdot r/(1+r)$. For further discussions of this equivalence, see e.g., Bastani and Waldenström (2018) and Guvenen, Kambourov, Kuruscu, Ocampo, and Chen (2019).

[4]This refers to the years 2010–2015, which is the time period that I study.

[5]References in the popular press to the potential disincentive effects of wealth or capital taxation are abundant. In economic modeling, the typical assumption is that capital taxation reduces saving, see e.g., Saez and Stantcheva (2018)

wealth tax payments.

I find that a majority of the increase in saving is financed by increased labor income. For each additional NOK subject to a wealth tax, households increase total taxable labor income by NOK 0.01. I further find that the effect on labor income is driven entirely by households initially above the wealth tax threshold. These are the households positioned to experience larger income effects. To obtain a better proxy for concurrent labor supply, I isolate salary and self-employment earnings from other sources of labor income, such as pensions. This yields a starker response: For each additional NOK subject to a wealth tax, households increase labor earnings by NOK 0.017. These findings constitute novel evidence of a non-trivial cross-elasticity between the return on capital and the supply of labor. By cumulating the saving and labor income responses over a 5 year treatment period, I find that 50% to 84% of the cumulative saving effect is financed by increased labor incomes.

I continue by examining the effect on portfolio allocation. First, I consider the effect on the share of financial wealth invested in the stock market. The perhaps dominant hypothesis is that risk-averse agents will respond to a wealth-tax induced drop in life-time consumption by allocating less of their wealth to risky assets. I find no evidence of any effect on the risky share and can rule out any economically large effects. I further examine the effect on the interest rates households achieve on their deposits and debts. I hypothesize that the adverse income effect of increased taxation may induce households to enjoy less financial leisure, in the sense that they exert greater effort towards financially optimizing the returns they receive or pay on their deposits or debt. To explore this hypothesis, I use itemized data on capital incomes that allow me to calculate realized interest rates. I firmly reject the above hypothesis and can rule out any economically meaningful effects.

Given the non-linear nature of rate-of-return shocks caused by taxing only wealth in excess of a threshold, it is useful to consider the underlying structural parameters governing these responses. Hence, in the last part of this paper, I use a simple multi-period life-cycle model to explore which values of the EIS are consistent my estimates. I find that in order to replicate treatment effects on savings and labor income growth within the 95% (90%) confidence interval around my empirical results, an EIS below 0.5 (0.3) is needed. This illustrates how my empirical findings can provide a new upper bound for the EIS, which is in the lower range of existing estimates. This finding is robust in the sense that (i) it can be derived from either my savings *or* labor earnings results, and (ii) it is largely insensitive to the value of the Frisch elasticity that governs the elasticity of labor supply.

I use these bounds to simulate savings and labor supply responses to linear rate-of-return shocks to infer the implied elasticities. At the cut-off of 0.5 (0.3), the simulated 5-year *uncompensated* elasticity of savings to the net-of-tax rate-of-return is 0.11 (-0.01). For labor supply it is 0.13 (-0.31). In other words, at the 10% level, my empirical findings are inconsistent with parameterizations of a life-cycle model that would produce positive elasticities of saving and labor supply to the rate-of-return.

This paper contributes to three literatures. I contribute to the nascent but growing literature examining household responses to wealth taxation. This literature has found that wealth taxes reduce the amount of taxable wealth that households report (Seim 2017, Londoño-Vélez and Ávila-Mahecha 2018, Zoutman 2018, Jakobsen, Jakobsen, Kleven, and Zucman 2019, María Durán-Cabré, Esteller-Moré, and Mas-Montserrat 2019, Brülhart, Gruber, Krapf, and Schmidheiny 2019).[6] However, these findings do not necessarily imply that wealth taxes cause households to save less, as evasion responses may dominate (real) saving responses. Consistent with this ambiguity, I find strikingly different effects when limiting the role for evasion.I limit the role for evasion by (i) focusing on savings in the form of financial wealth, which is primarily third-party reported in Norway, and by (ii) obtaining identifying variation in wealth tax exposure from below the top 1%, where evasion is less prominent.[7]

By speaking to real saving responses to (net-of-tax) rate-of-return shocks, I also contribute to the empirical literature aimed at estimating the Interest Elasticity of Saving (See, e.g., Boskin 1978 and Beznoska and Ochmann 2013). This literature has seen few recent contributions and thereby a "paucity of empirical estimates" (Saez and Stantcheva, 2018). Finally, since the outcomes I consider are tightly connected to the Elasticity of Intertemporal Substitution, I contribute to the large empirical literature aimed at estimating it (See, e.g., Best, Cloyne, Ilzetzki, and Kleven 2018, Gruber 2013, Vissing-Jørgensen 2002, and Bonaparte and Fabozzi 2017).

Relative to this combined body of work, I make three main contributions. My first contribution is to provide micro-level evidence while comparing households who are similar on socio-economic observables. While assessed tax assessments change discontinuously at geographic boundaries, these changes are not predictive of changes in other pre-period observables, such as housing transaction prices, wealth, labor income or education in my preferred BDD specifications. This contrasts with micro-econometric studies that obtain identifying variation in after-tax returns by using differential tax treatment arising from differences in characteristics such as wealth, income and asset shares.

My second contribution is to also study how (net-of-tax) rate-of-return shocks affect labor earnings, which is crucial to optimal taxation (see e.g., Atkinson and Sandmo 1980 and Saez

[6]The existing literature has either focused completely on evasion (Seim (2017) and Londoño-Vélez and Ávila-Mahecha (2018)), or has not distinguished between real and reporting (i.e., evasion) responses to wealth taxation. This fact is stressed by both Zoutman (2018) and Jakobsen et al. (2019) .

[7]Wealth taxes are levied at a relatively low threshold in Norway, and the treatment at hand, namely increased tax assessment of housing, is particularly well-suited for identifying responses for the moderately wealthy, where housing wealth accounts for a large share of taxable net wealth. (Fagereng, Guiso, Malacrino, and Pistaferri, 2018). Alstadsæter, Johannesen, and Zucman (2019) show that wealth tax evasion primarily occurs above the 99th percentile of the wealth distribution.

and Stantcheva 2018) and may inform the parametrization of life-cycle models with endogenous labor supply.

My third contribution is to also study the effects on portfolio allocation and realized returns. This has seen little empirical attention, despite its importance for economic modeling. By showing that (i) the risky share of financial wealth and (ii) the realized returns on risk-free assets are unaffected by rate-of-return shocks, I provide justification for treating rates-of-return as exogenous in partial equilibrium.

This paper also provides new evidence of (a lack of) wealth tax evasion by studying responses to wealth taxation in the form of bunching at tax thresholds. The visual evidence does not favor bunching, which greatly contrasts previous findings from Denmark, Sweden, Colombia and Switzerland.[8] The findings in Seim (2017) suggested that tax evasion may be greatly restricted if self-reporting is limited. My findings are consistent with this, suggesting that evasion is addressable through limiting the extent of self-reporting, as argued by Saez and Zucman (2019).

This paper has implications for the growing literature studying the effects of household heterogeneity on monetary policy transmission. The importance of this literature relies partially on the premise that standard intertemporal substitution effects are unable to explain the aggregate effects of monetary policy. This premise is validated by my finding that income effects dominate substitution effects in household responses to rate-of-return shocks and that a low EIS is necessary to explain my results.

Finally, this paper contributes to the literature employing BDD frameworks.[9] My empirical setting features heterogenous border areas that differ significantly in terms of residential density and treatment discontinuities, which presents some interesting empirical challenges that I explore in detail. My key contribution is to design a simple semi-parametric approach that is successful at explaining observable geographic variation in house prices and household characteristics and facilitates graphical presentation of regression estimates.

The paper proceeds as follows. Section 2 discusses the institutional features and assessment model for housing wealth. Section 3 presents a simple two-period theoretical framework that relates tax assessment shocks to rate-of-return shocks, and highlights the theoretical ambiguity of the effects on saving. Section 4 introduces the data, the identification strategy, and the empirical specifications. Section 5 the empirical findings. Section 6 explores bunching behavior. Section 7 considers the implied structural parameters in light of a life-cycle model. Section 8 concludes.

[8] See Jakobsen et al. (2019), Seim (2017), Londoño-Vélez and Ávila-Mahecha (2018) and Brülhart et al. (2019), respectively.

[9] See, e.g., Black (1999) and Bayer et al. (2007) who also employ BDD designs to incorporate treatment discontinuities that vary across boundary areas.

2 Institutional Details

2.1 Wealth Taxation in Norway

In Norway, wealth taxes are assessed according to the following formula:

$$\text{wtax}_{i,t} = \tau_t(TNW_{i,t} - Threshold_t)\mathbb{1}[TNW_{i,t} > Threshold_t], \tag{1}$$

where $wtax_{i,t}$ is the amount of wealth taxes incurred during year, t, and is due the following year. τ_t is the tax rate applied to any Taxable Net Wealth (TNW) in excess of a time-varying threshold. The tax rate, τ, was 1.1% during 2009–2013, 1% in 2014, and 0.85% in 2015.[10] Taxable Net Wealth (TNW) is the sum of taxable assets minus liabilities, where housing wealth is assessed at a discounted fraction of estimated market value (25% for owner-occupied housing).[11] The tax is assessed on individuals, but married couples are free to reshuffle assets and liabilities between them, effectively taxing married households on the sum of their taxable net wealth in excess of two times the wealth tax threshold.

The market value of all financial assets (debt) held through (borrowed from) domestically registered financial institutions are third-party reported each year. Private-equity wealth, i.e., the value of unlisted stocks, is reported by the stock issuer as a part of their financial reporting to the tax authorities.[12]

In this paper, I identify effects from quasi-random variation in $TNW_{i,t}$, arising due to the implementation of a new methodology to assess the housing wealth component. This identifying variation in $TNW_{i,t}$ affects the marginal rate-of-return that households face to the extent that it switches on $\mathbb{1}[TNW_{i,t} > Threshold_t]$ in equation 1, and thereby lowers the marginal after-tax return by τ_t. It affects the average rate-of-return to the extent that it lowers the ratio of net-of-tax capital incomes (Pre-Tax Capital Incomes - $wtax_{i,t}$) to $TNW_{i,t}$ by increasing $wtax_{i,t}$. This effect on the average return is a combination of intensive and extensive margin effects, while the effect on the marginal return is driven only by extensive margin variation in wealth tax exposure.

In the next subsection, I describe the model used to assess the housing wealth component in more detail.

[10] Prior to 2009, there were two thresholds. All wealth above the highest threshold was taxed at 1.1%, while the intermediate levels of wealth were taxed at 0.9%. During 2009–2015, the single threshold was gradually increased from NOK 470,000 to 1,250,000 (USD 78,000 to 208,000, using the 2010 USD/NOK exchange rate of around 6.)

[11] Prior to 2008 some other assets were taxable at a discount as well. For example, stocks only entered with 85% of their market value in 2007. During 2008–2015, the only asset class taxed at a discount was real-estate. While primary housing (owner-occupied) was taxed at 25%, secondary housing was assessed a tax value of 40%–60% of the estimated market value.

[12] These reports are audited for all firms with revenue > 5 MNOK, > Employees or assets > 10 MNOK. The value of financial assets/debts are also reported directly to the tax authorities from financial institutions.

2.1.1 Introduction of a Hedonic House Price model

In 2010, the Norwegian Tax Authorities implemented a major change to how they assess the housing wealth component of Taxable Net Wealth (TNW). Prior to 2010, assessed housing wealth was set to an inflated multiple of the initial tax assessment, typically corresponding to 30% of construction cost.[13] As some regions experienced larger house price growth than others, this lead to regional disparities in the ratio of assessed housing wealth to observed transaction prices. To rectify this, the tax authorities began assessing housing wealth using a hedonic real-estate pricing model that included geographic fixed effects. This was primarily communicated to home-owners through a letter sent out in August 2010. I describe this communcation in more detail in section B.7.

Utilizing a large national dataset on property transactions during 2004–2009,[14] house prices were estimated according to equation 2. This estimation took place separately for each of thee three structure types, $s \in \{\text{Detached housing, non-detached housing, condominiums}\}$, and for each region, R. The regions were mainly defined as one of the twenty Norwegian counties or one of the four largest cities (Oslo, Bergen, Stavanger, Trondheim). For non-detached housing and condominiums, of which there were fewer transactions, some counties were combined, presumably to increase sample size in each regression.

$$\begin{aligned} \log(Price/size)_i &= \alpha_{R,s} + \gamma_{Z,s} + \zeta^{size}_{R,s} \log(size_i) + \zeta^{Dense}_{R,s} \text{DenseArea}_i \\ &+ \zeta^{Age}_{1,R,s} \mathbb{1}\{Age_i \in [10,19]\} + \zeta^{Age}_{2,R,s} \mathbb{1}\{Age_i \in [20,34]\} \\ &+ \zeta^{Age}_{3,R,s} \mathbb{1}\{Age_i \geq 35\} + \varepsilon_i \end{aligned} \tag{2}$$

Municipalities, or within-city districts for the largest 4 cities, were assigned to within-region price zones, Z, separately for each structure type-region. While I can observe the assignments of Z and R in the appendices of the reports, the underlying process is only briefly described: Municipalities were assigned to price zones depending on "analyses of past price levels",[15] and non-transacting municipalities were grouped in with low price level municipalities. Consistent with this, I observe that members of the same price zone tend to have similar past-price levels, and smaller municipalities are more likely to be grouped in with multiple other municipalities

[13] The tax value of a house would first enter as it's construction cost. Then each year the tax value is changed by some percentage, e.g., -5%, 0%, 10%. The existing practice of using initial construction cost is described in the government budget of 2010: https://www.statsbudsjettet.no/Statsbudsjettet-2010/Dokumenter/html/Prop-1-L-Skatte--og-avgiftsopplegget-2010-mv---lovendringer/3-Nytt-system-for-formuesverdsetting-av-bolig

[14] The housing price model used to assess house values at year t would include transactions during $t-5, ..., t-1$. When households were given preliminary estimates of their assessed values during 2010, only 2004–2008 data was used. When actual tax values were assigned, 2009 data was included.

[15] My translation of a comment in the 2009 report.

within that region, regardless of geographic proximity.[16] All the estimated coefficients for a total of 44 regressions are provided in regression output form. I provide an example of these regression outputs in Figure A.5

These coefficients were then provided to the Tax Authorities, who applied the estimated coefficients to data from real-estate registers, and home-owner verified data on housing characteristics. These assessments were done largely out-of-sample, as most houses present in 2010 were not transacted during 2004–2009. The following formula was used to assess the tax value of housing:

$$\widehat{TaxVal}_i = 0.25\text{size}_i \cdot \exp(\overline{\log(Price/size)}_i) \cdot \exp(0.5\hat{\sigma}^2_{R,s}), \tag{3}$$

where $\exp(0.5\hat{\sigma}^2_R)$ is the concavity adjustment term, with $\hat{\sigma}^2_{R,s}$ being the Mean Squared Error (MSE) of the regression for structure type s in region R.

Thus we can write $\log(\widehat{TaxVal}_i)$ as

$$\begin{aligned}
\log(\widehat{TaxVal}_i) &= \log(0.25) + \hat{\alpha}_{R,s} + \hat{\gamma}_{R,Z,s} + (1 + \hat{\zeta}^{size}_{R,s}) \log(size_i) + \hat{\zeta}^{Dense}_{R,s}\text{DenseArea}_i \qquad (4) \\
&+ \hat{\zeta}^{Age}_{1,R,s}\mathbb{1}\{Age_i \in [10,19]\} + \hat{\zeta}^{Age}_{2,R,s}\mathbb{1}\{Age_i \in [20,34]\} + \hat{\zeta}^{Age}_{3,R,s}\mathbb{1}\{Age_i \geq 35\} \\
&+ \log(0.5\hat{\sigma}^2_{R,s}).
\end{aligned}$$

I collect all the data necessary to replicate the assessed house values from Statistics Norway's reports. In Figure A.4 in the Appendix, I show how utilizing these coefficients and the real-estate registers allows me to accurately predict assessed tax values, as observed in household tax returns.

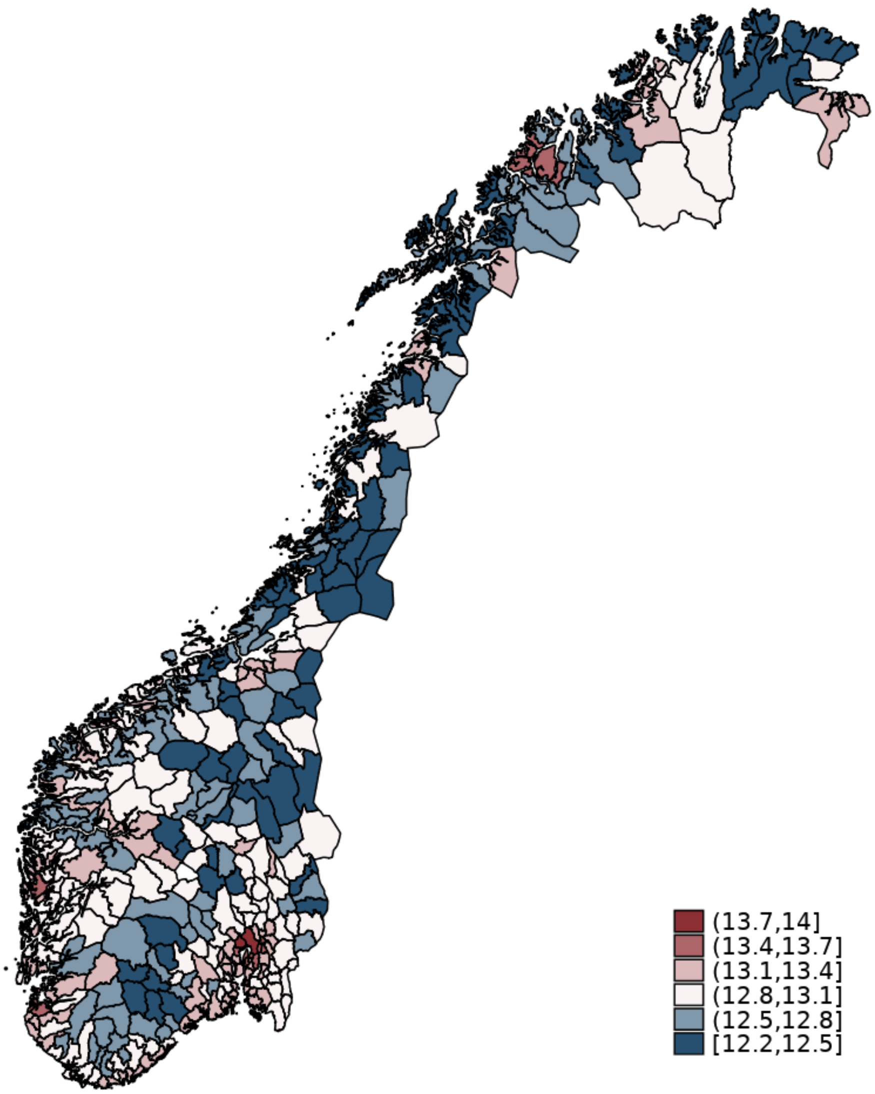
The actual tax values may differ from predicted tax values for a few key reasons. First, the coefficients I utilize are based on estimating equation 2 on 2004–2008 data. These are, to the best of my knowledge, the same coefficients that were use in the tax value calculators available during 2010. When assessing tax values after the end of the tax year, the coefficients were re-estimated on a dataset that also included 2009 data. Thus the inclusion of more data would impact the coefficients and the assessed tax values. Second, housing wealth as observed in the tax returns, $TaxVal$ also include the value of secondary homes, while I estimate model-predicted tax values, $\widehat{TaxVal}$, only considering primary residences. This leads to a few cases where $TaxVal > \widehat{TaxVal}$. Third, households may have moved during 2010. And finally, they may filed a complaint regarding the tax assessment. While assessed tax values are meant to equal 0.25× market value, households who can document that their assessment exceeds 0.30×

[16] This essentially precludes the use of border areas contained within one price zone to be used for placebo testing. The most intuitive definition of a placebo treatment variable would be the differences in past average transaction prices, but given the assignment rule, there would be very little identifying variation.

market value may have the assessment lowered to 0.30× market value, but not to 0.25×. In other words, even if the assessment is 20% too high, there are no incentives to complain.

Figure 1: Model-Implied Geographic Variation in Tax Assessment for a Standard House

This figure shows the logarithm of the 2010 assessed tax value of an identical single family home assessed as if it were located inside one of the municipalities below, predicted using the coefficients in the Tax Authorities' hedonic pricing model. Each color shade correspond to a bin of $\widehat{TaxVal}$ with a width of 0.3 log-points. I assume a house size of 130 m^2, an age of 25–34 years, and a location in an area defined as densely populated area. The assessed log tax value has a mean of 13.30 and a standard deviation of 0.37, across (equal-weighted) municipalities. For municipalities with within-city districts making up separate price zones, I assign the unweighted average tax assessments for the purpose of this illustration.



3 Conceptual framework

The purpose of this section is to present a simple two-period life-cycle model of consumption to relate saving responses to tax assessment shocks to responses to changes in the net-of-tax rate-of-return, and show how these responses relate to structural parameters.[17] I then show how comparing the effects of assessment shocks on the *marginal* versus *average* net-of-tax rates-of-return can help us assess whether tax assessment shocks can be used as instruments for (linear) rate-of-return shocks. Later, in subsection 5.2.1, I combine the main insights from this model with empirical data to discuss expected heterogenous responses.

Consider the following simple two-period model, where households chose how much to consume in each period (C_1, C_2) , and may save S. I focus on saving responses and assume perfect foresight to keep the model simple. Households have an initial endowment of Y_1, which can be thought of as initial wealth plus first-period exogenous income, and face exogenous income of Y_2 in period 2. At the end of this section, I discuss the impact of introducing endogenous labor supply. The tax authorities impose a tax τ on taxable net wealth, $W = SR + A$, in excess of a threshold $\bar{W}$. A is some premium that the tax authorities add to a household's wealth, analogous to the empirical variation in tax assessments for the housing component of net wealth.

Baseline optimization problem.

$$\begin{aligned}
\max_{C_1,C_2,S} \quad U(C_1, C_2, S) &= \frac{1}{1-\gamma}C_1^{1-\gamma} + \beta\frac{1}{1-\gamma}C_2^{1-\gamma} && (5)\\
\text{s.t.} \quad C_1 + S &= Y_1 && (6)\\
\text{and} \quad C_2 &= Y_2 + RS - \tau\mathbb{1}[SR + A - \bar{W} > 0](SR + A - \bar{W}) && (7)
\end{aligned}$$

We can rewrite the constraint for period 2 as:

$$C_2 = Y_2 + SR(1 - \tau\mathbb{1}[SR + A - \bar{W} > 0](SR + A - \bar{W})) + \tau\mathbb{1}[SR + A - \bar{W} > 0](\bar{W} - A)$$

Where the last term is virtual income (in period 2), which compensates for the fact that τ is not applied to all savings due to the tax threshold.

Wealth taxes offer a slightly complicated optimization problem, with agents potentially bunching such that $SR + A - \bar{W} = 0$. Since bunching is not an empirically important phenomenon in my setting, and a few key insights are obtainable with a few simplifying approximations, I take the following simpler route. First define $\tilde{R} = R(1 - \tau\mathbb{1}[SR + A - \bar{W} > 0])$ and $\tilde{V} = \tau\mathbb{1}[SR + A - \bar{W} > 0](\bar{W} - A)$. We can then rewrite the budget constraint for period 2 as $C_2 = Y_2 + \tilde{R}S + \tilde{V}$. Then I assume that agents take $\tilde{R}$ and $\tilde{V}$ as given when they optimize, which can be thought of as a linearization of the budget constraint around the empirical means.

[17] In the last section, where I calibrate a life-cycle model to infer which EIS my results I can rule out, I use a multi-period model with endogenous labor supply. However, the key intuition can be found in this simpler two-period model.

The problem then becomes:

Simplified optimization problem.

$$
\begin{aligned}
\max_{C_1,C_2,S} \quad U(C_1,C_2,S) &= \frac{1}{1-\gamma}C_1^{1-\gamma} + \beta\frac{1}{1-\gamma}C_2^{1-\gamma} && (8)\\
\text{s.t.} \qquad C_1 + S &= Y_1 && (9)\\
\text{and} \qquad C_2 &= Y_2 + \tilde{R}S + \tilde{V} && (10)
\end{aligned}
$$

Assuming that constraints bind, imposing the first-order condition on S, and reorganizing then leads to the following expression for S:

$$S = \frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}Y_1}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}} - \frac{Y_2 + \tilde{V}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}} \tag{11}$$

Suppose $\tilde{R}$ and $\tilde{V}$ are differentiable with respect to the tax assessment variable, A. Now I assume that agents optimally change their behavior when A affects $\tilde{R}$ and $\tilde{V}$. Their response can be decomposed, using the chain rule, as the sum of a rate-of-return effect and a virtual income effect:

$$\frac{dS}{dA} = \underbrace{\frac{dS}{d\tilde{R}}\frac{d\tilde{R}}{dA}}_{\text{Rate-of-return Effect}} + \underbrace{\frac{dS}{d\tilde{V}}\frac{d\tilde{V}}{dA}}_{\text{Virtual Income Effect}} \tag{12}$$

Rate-of-return effect. By reducing the marginal rate-of-return, $\tilde{R}$, increases in tax assessment, A, cause a "traditional" rate-of-return effect, which I write out below.

$$\frac{dS}{d\tilde{R}} = Y_1\left(\frac{1}{\gamma} - 1\right)\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2} + (Y_2 + \tilde{V})\frac{1 + \frac{\beta}{\gamma}[\beta\tilde{R}]^{\frac{1}{\gamma}-1}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2}. \tag{13}$$

The first term in equation 13 gives rise to the theoretical ambiguity in household responses to rate-of-return shocks. Its sign depends on the Elasticity of Intertemporal Substitution, $\frac{1}{\gamma}$. The second term is the "human wealth effect", where an increase in $\tilde{R}$ lowers the net present value of future incomes, $Y_2 + \tilde{V}$, which induces more saving.

We can rewrite the expression for $\frac{dS}{dR}$ above, using the formula for S, to see the over-all ambiguity more clearly in the presence of a human wealth effect.

$$\frac{dS}{d\tilde{R}} = \frac{1}{\gamma}\frac{[\beta R]^{\frac{1}{\gamma}}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2}\left(Y_1 + \frac{Y_2 + \tilde{V}}{\tilde{R}}\right) - \frac{S}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}. \tag{14}$$

We see now that if savings are sufficiently positive, $S > 0$, and the EIS, $\frac{1}{\gamma}$, is sufficiently small, then this expression is negative.[18]

Virtual Income Effect. By affecting $\tilde{V}$, shocks to tax assessment cause an additional (virtual) income effect:

$$\frac{dS}{d\tilde{V}} = -\frac{1}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}. \tag{15}$$

The magnitude and sign of this channel depends critically on how A affects $\tilde{V}$. If we define the average rate of return as $\tilde{R}^{avg}$, such that $S\tilde{R}^{avg} = S\tilde{R} + \tilde{V}$, then we can rewrite $\tilde{V}$ as $\tilde{V} = S(\tilde{R}^{avg} - \tilde{R})$, which is simply savings multiplied by the difference between the average and marginal rates-of-return. We may therefore write the effect of tax assessment shocks on virtual income as the following:[19]

$$\frac{d\tilde{V}}{dA} \;\; = \;\; S\left(\frac{d\tilde{R}^{avg}}{dA} - \frac{d\tilde{R}}{dA}\right). \tag{16}$$

While these derivatives are not well-defined analytically due to the presence of indicator functions, their empirical counterparts can be estimated empirically by considering differential effects of tax assessment shocks on the marginal versus average after-tax rates-of-return. This will be a useful exercise, because the differential effects dictate how my tax assessment shocks yield a treatment comparable to linear rate-of-return shocks.

Relative importance of income and virtual income effects. To understand the relative importance of these two effects, I rewrite the (decomposed) effect of tax assessment shocks on saving from equation 12 when the EIS is zero to isolate income effects. I use the expression for $dS/d\tilde{R}$ from equation 14, and substitute in for $d\tilde{V}/dA$, and reorganize, to get:

[18] As an example, consider the case when $\beta\tilde{R} = 1$. Further assume that $\tilde{R} = 1.5$, and, without loss of generality (since it will be divided through), that $Y_2 + \tilde{V} = 1$. $S > 0$ then implies $Y_1 > 1$. In this case, $dS/d\tilde{R} \leq 0$ whenever $\frac{1}{\gamma} \leq 2.5 - \frac{2.5+1/1.5}{Y_1}$. Setting $Y_1 = 1.5$, in other words that current income and wealth exceeds future (nominal) income and wealth by $\tilde{R} - 1 = 50\%$, yields $1/\gamma \leq 0.38$.

[19] This assumes that S is not also affected by A in any way that affects V. This is related to the assumption that agents take $\tilde{R}$ and $\tilde{V}$ as given when choosing the optimal amount of S.

$$\frac{dS}{dA} = -\frac{S}{\tilde{R}+1}\left[\underbrace{\left(\frac{d\tilde{R}}{dA}\right)}_{\text{Income Effect}} - \underbrace{\left(\frac{d\tilde{R}}{dA} - \frac{d\tilde{R}^{avg}}{dA}\right)}_{\text{Virtual Income Effect}}\right]. \tag{17}$$

The term denoted income effect represents the effect of tax assessment on saving through changing a linear rate-of-return. The second term indicates the effect through changing virtual income. This equation suggests that we can evaluate the relative impact of these two channels by comparing the effects of tax assessment shocks on the marginal versus average rates-of-return. If I find $\mathbb{E}[d\tilde{R}/dA]$ to be twice as large as $\mathbb{E}[d\tilde{R}^{avg}/dA]$, then this suggests that half the income effects are offset by opposing virtual income effects. If I find $\mathbb{E}d[\tilde{R}/dA]$ to be only half that of $\mathbb{E}[d\tilde{R}^{avg}/dA]$, then this suggests that income effects are amplified by a factor of two.

Endogenous labor supply. In section B.9 in the Appendix, I modify the existing optimization problem by introducing endogenous labor supply in period 1. The preferences feature a constant Frisch elasticity, and additive separability in the (dis)preferences for consumption and labor supply. This added complexity has no effect on the qualitative conclusions in the previous section, but shows that the labor earnings response will be of a same sign as, but smaller in magnitude than, the savings response. The savings response takes the same form, but is scaled up in magnitude. This added responsiveness will depend on the parameters governing labor supply.

4 Empirical

4.1 Identification

I identify household responses to an increased exposure to wealth taxation caused by higher tax assessments on housing. Since this tax assessment is the result of a model that aims to predict housing wealth, more treated households will, by construction, own more expensive homes on average. This may be an important violation of the exclusion restriction, given that housing wealth is known to be an important determinant of household saving behavior, and is likely highly correlated with other important determinants such as income or wealth.

To address this issue, I employ a Boundary Discontinuity Design (BDD) approach. The purpose of this empirical design is to utilize the fact that treatment varies discontinuously at geographic boundaries, thus allowing me to remove the effects of potential confounders that vary smoothly.

Given the structure of the house price model (introduced in section 2.1.1), tax assessments will be geographically discontinuous even if past transaction prices are truly smooth. This implies that that two identical houses, on different sides of a geographic boundary, may have very different assessments due only to cross-price zone differences in average past transaction prices.

The success of the BDD approach in isolating treatment effects from tax assessment hinges on the following: Potential confounders must vary smoothly at the geographic treatment boundaries, and my parametrization of the BDD regression equations must be able to capture this smooth variation to the extent that it can also affect the outcome variables of interest. I describe my BDD approach in detail in the next subsection.

The BDD approach cannot address the presence of other discontinuities that may be correlated with saving behavior. This is a problem to the extent that these discontinuities are correlated with assessment discontinuities. The prime candidate for this is actual house prices. If differences in mean prices across boundaries tend to go hand in hand with actual geographic discontinuities, then this is a potential problem.[20] I perform numerous tests to address these concerns.

An attractive feature of my empirical setting is that the treatment discontinuities were only recently introduced. This offers two key advantages relative to a setting identifying effects from a non-time varying treatment discontinuity. First, it allows me to focus on households who made their residential location choices prior to having any knowledge of the impending wealth assessment discontinuities.[21] This addresses the concern that households may have self-selected into lower or higher tax assessment. In addition, it allows me to speak to the parallel-trends assumption by examining the effect of higher tax assessment on pre-period outcome variables.

Another concern when studying the effects of geographically confined increases in taxation is that households may be affected through the effect on local government finances. In section B.6 in the Appendix, I argue that this is unlikely to play a meaningful role in my empirical setting since wealth taxes are primarily paid by the very wealthiest households, who were not disproportionately affected by this reform. The impact on municipal finances would thus be too small to trigger a meaningful response.

Finally, it is worth noting that this setting will identify partial-equilibrium effects. Since

[20] Although log-differencing the main outcomes (thereby taking out fixed effects) reduces the identifying assumption to that of a parallel trends assumption, the validity of the research design may be improved by verifying that there are no true discontinuities in house prices.

[21] (1) Per my investigations, the fact that geographic assessment discontinuities exist is still a little-known fact even at the time of writing. (2) This wouldn't be a benefit to research examining house price capitalization, since a key driver of house prices is selection.

bordering municipalities are typically tightly economically linked, there is little scope for geographic discontinuities in terms of general equilibrium treatment effects. For example, there is little reason to suspect that wages are affected by this treatment, since employers face no frictions in choosing employees from either side of one of the treatment boundaries.

4.2 Empirical Specification

Distance and Boundary Areas. I define the key geographic measure, d_i, as the signed distance, in kilometers, to the closest municipal boundary, where households on the low assessment side of the borders receive a negative distance, and households on the high assessment side receive a positive distance.[22]

I will refer boundary areas, b, as sets of households assigned to the same municipal boundary. Within a boundary area, b, households are defined to be on the high assessment side if the average household within that boundary would see a higher tax assessment on that side.[23] Geographic variables, such as d_i, b, and geographic location, $\mathbf{c}_i$, are all measured in 2009.

Identifying variation. I define Δ_i as the log increase in tax assessment that arises for household i if it were assessed on the high instead of the low assessment side of the border. This variable is a border-area and structure-type specific (linear) function of $\mathbf{H}_i = \{log(Size)_i,\ \text{DenseArea}_i,\ \{\mathbb{1}[\text{Age}_i \geq a], a = 0, 10, 20, 35\}\}$ and isolates the identifying variation in model-implied tax assessment, $\log(\widehat{TaxVal}_{i,t})$, to come from cross-border (but within border area) differences in pricing model coefficients, and allows this effect to vary with $\mathbf{H}_i$, measured as of 2009.

$$\Delta_i \equiv \log(\widehat{TaxVal}_i)\Big|_{d_i>0} - \log(\widehat{TaxVal}_i)\Big|_{d_i<0} \tag{18}$$

Main reduced-form regression specification. The following regression equation yields the estimator, $\hat{\beta}$, for the reduced-form effect of increased tax assessment on some outcome variable, $y_{i,t}$, measured at year t.

$$y_{i,t} = \beta \log(\widehat{TaxVal}_i) + g_b(\mathbf{c}_i)\Delta_i + \delta'_{b,s}\mathbf{H}_i + \gamma'_t\mathbf{X}_i + \varepsilon_{i,t}, \tag{19}$$

[22] I calculate d_i by minimizing the distance to the nearest residence in a different municipality (or within-city district). This has the benefit of not assigning households as being close to a border that is vacant on the other side.

[23] Within a boundary area, a municipality is defined to be on the high assessment side of the average detached house (by far the largest group in my sample) in the border area would receive a higher assessment in that area. If there is no differences for single family homes, i.e., they are in the same price region and price zone, I do the same exercise for non-detached houses, and if necessary for condominiums.

The inclusion of border area and structure-type specific linear controls in housing characteristics, $\mathbf{H}_i$, isolates the identifying variation in $\log(\widehat{TaxVal}_{i,t})$ to $\mathbb{1}[d_i > 0]\Delta_i$. While this term identifies the effect of a *discontinuous loading* on Δ_i, the term $g_b(\mathbf{c}_i)\Delta_i$ is meant to capture the effect of anything that loads continuously on Δ_i. I describe this term in more detail below.

To increase precision, and to alleviate concerns that relevant observed heterogeneity is not appropriately controlled for, I include a number of household-level controls. $\mathbf{X}_i$ is a vector of 2009-valued household characteristics: A single dummy, a single dummy interacted with a male dummy, a third-order polynomial in the average age of household adults, log(Labor Income), log(Gross Financial Wealth [GFW]) College [Dummy for whether any of the adults have a college degree], A debt dummy, log(Debt), the share of GFW invested in the stock market [SMW], the log of the tax-return observed assessed tax value of housing, a dummy for whether the tax returns indicate ownership of other real estate, and the log of the assessed value, and finally a dummy for whether the household is reported to own non-listed stocks [PE Dummy]. $\mathbf{X}_i$ is not included as a vector of controls when I examine whether the identifying variation is correlated with pre-treatment household characteristics.

In most specifications, observations are pooled by treatment period, where the pre-period is 2004–2009 and the post-period is 2010–2015. Equation 19 is then estimated separately for these periods, allowing the slopes without a t subscript to vary by treatment period. My main specification imposes equal weights on all observations and clusters at the census tract level (*grunnkrets*). I provide results using triangular (distance-based) weights for my main results in Table A.3 in the Appendix. Results using different levels of clustering (household and municipality) are reported in Table A.4. Neither standard errors nor estimates are sensitive to these specifications.

Addressing continuous geographic loading on Δ_i. My method to capture potential confounding heterogeneity is to introduce the term $g_b(\mathbf{c}_i)\Delta_i$. $g_b(\mathbf{c}_i)$ is a border area-specific function of household i's location, and is meant to capture geographically heterogenous loading on Δ_i. Similar to Dell (2010), I test multiple such specifications. The baseline specification involves controlling for (signed) border distance in kilometers:

$$g_b(\mathbf{c}_i) = \gamma^- \mathbb{1}[d_i < 0]d_i + \gamma^+ \mathbb{1}[d_i > 0]d_i, \tag{20}$$

where γ^- and γ^+ are to be estimated. However, there is considerable heterogeneity in residential density across the border areas in my sample. The extent to which confounding variables change more rapidly, in a geographic sense, in denser urban areas is problematic. I provide a fuller discussion of this issue, and the approaches to addressing it in Appendix A. I highlight the main aspects of the approaches below.

My preferred specifications address this issue by allowing the slope on border distance to vary parametrically with measures of residential density. The main preferred measure, *Scaled Border Distance*, simply scales border distance (in kilometers) by a measure of average distances in a border area.[24] The second preferred measure, *Relative Location*, maps all households onto [-1,1], where households at 0 are equidistant to the low- and high-side centers.[25] As a robustness, I also allow the slope on border distance to vary by border area. Since this involves the estimation of many slopes, this limits precision and inhibits visual verification.

Two Stage Least Squares Specification. I implement a fuzzy BDD approach to provide IV estimates of how changes in tax assessment affect a given outcome, $y_{i,t}$. The expectation is that the first-stage coefficient, $\hat{\beta}^{FS}$ is close to one. The coefficient of interest is β^{IV}. Given the inclusion of the term $\delta_{b,s}^{FS\prime}\mathbf{H}_i$, the identifying variation in $\log(\widehat{TaxVal_i})$ is equal to $\mathbb{1}[d_i > 0]\Delta_i$; the discontinuous loading on the high-side assessment premium.

$$\log(TaxVal_{i,t}) = \beta^{FS}\log(\widehat{TaxVal_i}) + g_b^{FS}(\mathbf{c}_i)\Delta_i + \delta_{b,s}^{FS\prime}\mathbf{H}_i + \gamma_t^{FS\prime}\mathbf{X}_i + \varepsilon_{i,t}^{FS} \tag{21}$$

$$y_{i,t} = \beta^{IV}\log(TaxVal_{i,t}) + g_b^{IV}(\mathbf{c}_i)\Delta_i + \delta_{b,s}^{IV\prime}\mathbf{H}_i + \gamma_t^{IV\prime}\mathbf{X}_i + \varepsilon_{i,t}^{IV} \tag{22}$$

Specification to test differences on observables. When testing whether my identifying variation is correlated with pre-treatment observables, I estimate the following equation, which removes socio-economic controls from the main specification in equation 19. The coefficient of interest is β.

$$y_i = \beta\log(\widehat{TaxVal_i}) + g_b(\mathbf{c}_i)\Delta_i + \delta_{b,s}'\mathbf{H}_i + \varepsilon_i \tag{23}$$

4.2.1 Empirical specification relative to BDD literature

The similarity between my empirical specification and that of the existing BDD literature (e.g., Black (1999) and Bayer et al. (2007)) that incorporates cross-boundary area variation in treatment intensity can be seen by acknowledging that $\mathbb{1}[d_i > 0]\Delta_i$ in equation 19 may be replaced with $\log(\widehat{TaxVal})$ and I would still obtain the exact same estimator $\hat{\beta}$. However, writing out the identifying variation as a discontinuous loading, $\mathbb{1}[d_i > 0]\Delta_i$, facilitates a

[24] Specifically, the distance between the two centroids of the two municipalities (or within-city districts) whose residents occupy a given border area, b. This centroid-distance measure is thus b-specific.

[25] Households at $RelLoc \in [-1, 1]$ must travel (as the crow flies) $RelLoc \cdot X$ km further to get to the high side, than they would to get to the low side, where X is the distance between the centroids of the left and high sides.

graphical RDD representation of estimates.[26]

Beyond this graphical contribution, my approach differs from the existing approach in how it deals with potentially confounding geographic heterogeneity. First of all, my approach, as highlighted from the main reduced-form specification in equation 19, differs by addressing the fact that the relevant confounders covary with $\mathbb{1}[d_i > 0]\Delta_i$, and not just $\mathbb{1}[d_i > 0]$. The traditional approach is to utilize a specification similar to the baseline regression specification in equation 19, but without controlling directly for geographically smooth heterogeneity. Instead, the approach would be to *uniformly* reduce the cut offs (bands) for which i would be included, based on d_i alone. The motivation for this seems to arise from the motivation that relevant confounders covary with $\mathbb{1}[d_i > 0]$.

The purpose of introducing these cut-offs is to compare households as similar as possible (in terms of, e.g., neighborhood characteristics). However, in many cases, one would expect that confounders vary more rapidly in settings where treatment discontinuities are larger. If this is the case, then imposing uniform cut-offs implies that the boundary areas that offer the most identifying variation will also have the worst control group. My approach directly addresses this concern.

Second, my approach takes seriously geographic heterogeneity in residential density. This is important whenever potential confounders change more rapidly, in a geographic sense, in denser areas. My solutions towards addressing this is a useful contribution, since it may be applied to settings where there are many boundary areas that differ significantly, without having to reduce the sample size, by dropping boundary areas, to achieve homogeneity. In Appendix A, I provide examples of how geographic heterogeneity in residential density may invite the false detection of discontinuities in observable characteristics.

4.3 Data

I combine a wide range of administrative registers maintained by Statistics Norway. These registers contain primarily third-party reported data, and are all linkable through unique de-identified person and property identification numbers. A detailed description of the financial data sources can be found in Fagereng et al. (2018).

Financial data. Data on household financials come from household tax returns. These include breakdowns of household assets, such as housing wealth, deposits, bonds, mutual funds, listed stocks, and private equity holdings. It also includes the sum of household liabilities. I can further distinguish between third-party reported domestic wealth holdings (e.g., domestic

[26]Existing papers, e.g., Black (1999) and Bayer et al. (2007), do not provide RDD-style figures to illustrate their main empirical findings. Bayer et al. (2007) provide graphical evidence only when using a binary treatment cut-off, but their main estimation strategy leverages the full identifying variation, allowing treatment discontinuities to vary across border areas.

deposits), and self-reported foreign holdings of real-estate, deposits and other securities, separately. The tax data includes a breakdown of household income, such as self-employment income, wage earnings, pensions, UI income, and the sum of government transfers. It also contains a detailed breakdown of capital income, such as interest income from domestic or foreign deposits, and realized gains or realized losses. This data is spans 1993 to 2015.

Real-estate data. The real-estate ownership registers provides end-of-year data of the ownership of each plot of land in Norway. Using de-identified property ID numbers, I can populate each property with the buildings it contains. Then, using structure ID numbers, I can populate each structure with the housing units that it contains (e.g., multiple apartments, attached homes, or a single detached house). I can combine this with data on housing unit characteristics, such as size. An attractive feature of the administrative data is that it location data based on the geographic coordinates at the structure-level, instead of district- or census block-level (for examples see Dell (2010) and Bayer et al. (2007), respectively). These data sources cover 2004 to 2016.

Real-estate transaction data. I also use data on real-estate transactions to examine past and future transaction prices. This dataset is comparable to the CoreLogic dataset often used in real-estate research in the U.S., but can be linked to the other data sources through de-identified property and buyer/seller identification numbers (for both private individuals and corporate entitites). I collapse the dataset on the property-ID level, keeping information on most recent transaction prior to 2009 and earliest transaction during or after 2010. I restrict the data to transactions noted as being conducted on the open market, thus excluding other events such as bequests or expropriations. This dataset spans 1993 to 2016.

Other data sources. I also use data on demographics from the National Population Register. This contains data on birth year, gender, and marital links. I also obtain data on educational attainment as of 2010 from the National Education Database.

4.3.1 Variable definitions

Gross Financial Wealth (GFW) is the sum of domestic deposits, foreign deposits, bonds held domestically, listed domestic stocks, domestically held mutual funds, non-listed domestic stocks (e.g., private equity holdings), foreign financial assets (stocks, bonds and other securities), and outstanding claims.[27]

Labor Income is the sum of salaries, max(self-employment income,0) and transfers (incl., labor-related pensions and unemployment benefits). I also examine effects when only considering salaries and self-employment income.

Stock Market Share (SMS) is the ratio of listed domestic stocks and domestically held

[27] Foreign deposits and foreign financial assets are self-reported. Outstanding claims are primarily self-reported. The third-party reported components include claims on unpaid wages.

mutual funds to GFW.

I measure saving as one-year log-differences of financial savings (GFW). Log-differencing wealth variables is standard in the wealth tax literature.[28]

I further follow Jakobsen et al. (2019) in adjusting for the "mechanical effects" of increased wealth tax exposure. Absent any behavioral responses, higher wealth tax exposure mechanically reduces wealth by lowering the net-of-tax rate-of-return. To address this, I add wealth taxes incurred during $t-1$, and thus payable during period t, to savings at time t, for all households:

$$\begin{aligned} \text{Adjusted } \Delta \log(GFW_{i,t}) &\equiv \log(GFW_{i,t} + wtax_{i,t-1}) - \log(GFW_{i,t-1}) \\ &\approx \frac{\Delta GFW_{i,t}}{GFW_{i,t-1}} + \frac{wtax_{i,t-1}}{GFW_{i,t-1}} \end{aligned} \tag{24}$$

The definition of saving above is important to consider for anyone who uses the estimated effects for calibration or inputs into optimal taxation models. Specifically, the implied elasticities arising from regressions defining saving as in 24 need to be adjusted in order to serve as a target to calibrate measures of savings growth that include mechanical effects. Alternatively, implied elasticities that incorporate such mechanical effects by not making this adjustment will be provided in the results section.

The majority of my variables will be measured in natural log-points. To accommodate zeros within specific components of financial wealth (e.g., self-reported) or for debt, and to limit the influence of large log-changes caused by small level differences, I shift levels by an inflation-adjusted NOK 10,000 (USD 1,700).[29]

4.3.2 Sample selection

I only keep households who lived in the same building during 2007–2009, owned at least 90% of their primary residence and had a positive assessed tax value of their house in 2009. In addition, I require that their residence is registered to be larger than 50 square meters (approx. 540 square feet). This is to limit the probability that size is mis-measured, or that

[28] Zoutman (2018) considers 1–3 year log differences, Brülhart et al. (2019) considers 3 year log differences. Jakobsen et al. (2019) consider log-values, but incorporates household fixed effects to produce estimated effects on 1–8 year log-differenced wealth.

[29] This implies that a reduction in debt from NOK 138,000 (the 50th percentile) to 0 (the 25th percentile) appears as a log-difference of -2.695 rather than -11.835 when using a $\log(1+x)$ specification, which is considerably closer to the true percentage change of -100%. A similarly large magnitude would appear when using the asymptotic sine transformation (asinh), which is employed by Londoño-Vélez and Ávila-Mahecha (2018). There are only negligible differences for similar changes in the main outcome variables. For example, a change in gross financial wealth (GFW) from the 50th to the 25th percentile yields a log-difference of -0.925 compared to a log-difference of -0.951 when using the $\log(1+x)$ specification.

this is not their intended long-term residence. I drop households whose tax records indicate ownership in building coops in 2009, due to the lack of data on housing unit assignment within coops. I further restrict the sample to only include households with an income above NOK 150,000 (approx. USD 25,000) in 2009, which is well below the poverty limit in Norway. Such households are thus unlikely to the relevant sample for this study. I further exclude households where the average age of adults is less than 25 years. I then only keep households with a taxable net wealth (per adult) in 2009 strictly above NOK 0 and below NOK 6,000,000. NOK 6,000,000 corresponds to the 99th percentile of taxable net wealth per adult among the remaining households in 2009. Restricting to positive TNW households is standard in the wealth tax literature, and in my setting implies that the sample is fairly balanced with respect to whether or not households paid wealth taxes. This restriction leads to 66% of the sample having paid wealth taxes in 2009, and 60% end up paying wealth taxes during 2010–2015. I further trim the sample by removing households with labor incomes above NOK 4,300,000, which corresponds to the 99.95th percentile of the labor income distribution in 2009.

The primary reason for incorporating these upper bounds, is that ultra-high income and high net worth households have more complicated balance-sheets and likely have access to more evasion technologies. A secondary reason is to increase the ability of income to explain unobserved variation in outcome variables by removing a handful of extreme outliers from the income distribution. In addition, treatment effects will be fairly small relative to existing wealth or incomes for such households as housing wealth makes up a rather small proportion of total wealth for very wealthy households. This means that I can remove these households without eliminating much identifying variation in e.g., fraction of wealth subjected to a wealth tax. Excluding such households has an important additional benefit: Excluding the ultra wealthy allows the sample means to provide more relevant benchmarks for approximating level changes as the product of sample means and estimated log-differences.

An immediate consequence of selecting only households with initial positive taxable net wealth is that the resulting sample has a fairly high average age of 62, and thus fairly close to retirement. This is close to the average age of 61 in Jakobsen et al. (2019).[30] From a theoretical perspective, this suggests that these households are not highly influenced by the human wealth effect in their saving responses to rate-of-return shocks, which is consistent with my empirical results. I would argue that this is not necessarily a concern from an external validity point of view since savings tend to be concentrated among older households.[31] A recent report from Statistics Norway shows that the average age of wealth tax payers was 63 years in 2015, and

[30] Given population aging, my sample is likely younger. Their sample statistics are based on average pre-period (1982–1985) ages, while my statistics are based on 2009. This is 24 years later on average.

[31] See for example the Federal Reserve Bulletin 09/2017 Vol 103, No. 3, which shows that median net worth is the highest for household whose head is 65-74 years of age. Their median net worth is 5 times larger than households aged 35-44.

that individuals above 65 years of age account for 48% of wealth tax payers.[32]

I also impose some geographic cut-offs. When considering border distance in kilometers, I only consider households within 10km of the border, which accounts for around 80% of my sample. When using scaled border distance, I consider households within $[-0.6, 0.6]$ (the distance to the border is at most 60% of the distance between the two municipal centroids). This cut-off similarly keeps approximately 80% of the sample. The main purpose of this is to allow for the estimation of lower-order polynomials in these distance measures, without giving too much weight to geographic outliers. In Figure A.3 in the appendix, I show how households are distributed according to the different distance measures. In Tables A.6, A.7 and A.8, I provide results when varying these cut-offs.

5 Results

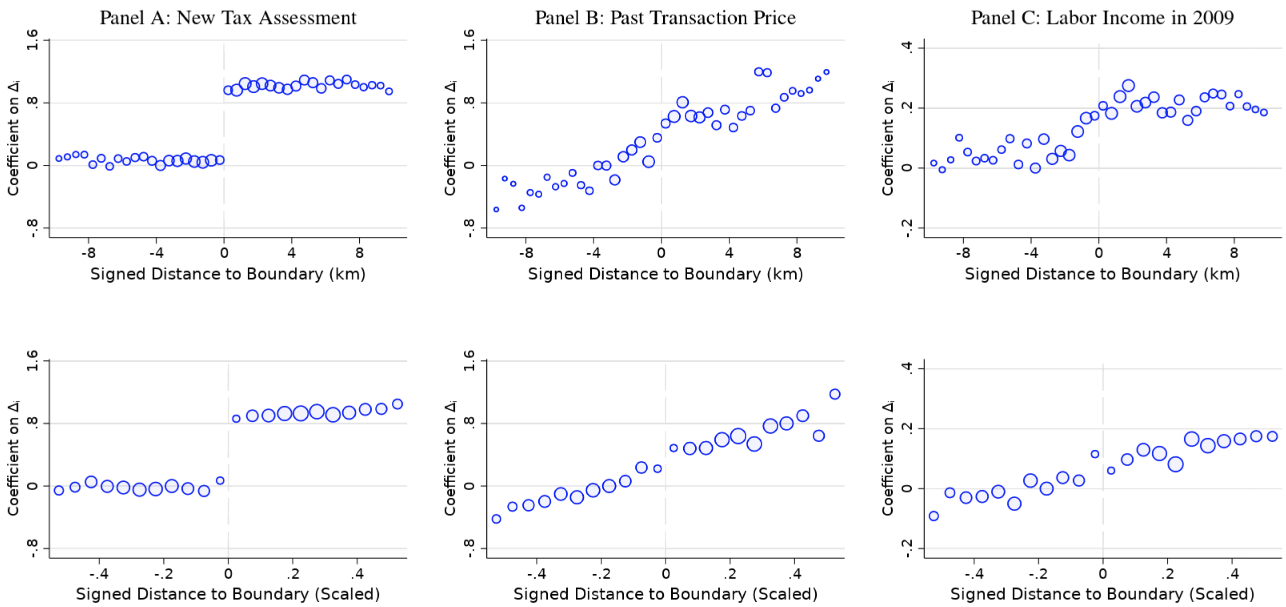
5.1 A Graphical Overview

In this section, I show graphically how tax values are discontinuous at municipal borders, while past transaction prices and labor incomes appear to behave continuously. These results are presented in Figure 2 below.

[32] https://www.ssb.no/inntekt-og-forbruk/artikler-og-publikasjoner/naer-hver-tredje-over-65-ar-betaler-formuesskatt

Figure 2: Assessed Tax Values and Observable Characteristics

The graphs below show the effect on actual tax assessment (as observed in tax returns), past transaction prices and pre-treatment incomes of living in a boundary region where hedonic pricing model coefficients imply a one log-point assessment premium on the high assessment side. Panel (A) considers log tax assessment in 2010 to verify the treatment discontinuity. Panel (B) considers the smoothness of observed past log transaction prices (2000–2009). Panel (C) considers log labor incomes in 2009. The effects are estimated separately for geographic bins, according to the different location measures. The top row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. Bins with less than 1% of observations are not plotted. The size of the circles corresponds approximately to the relative size of that bin in the estimation sample. Estimated coefficients stem from estimating a coefficient on Δ_i in equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal}_i)$ and $g_b(c_i)\Delta_i$. This specification includes a vector of housing controls, but no household characteristics. Panel (B) includes all home-owners present during 2009 who purchased their home during 2000-2009, and is not subject to the main sample restrictions. A robustness exercise is performed in Table 10.



Panel A shows that for a given model-implied treatment discontinuity, Δ_i, assessed housing wealth does indeed rise by close to Δ_i log-points. As expected, the coefficient does not vary significantly other than at the discontinuity, given that only observable characteristics are used for tax assessment.[33] Formal tests of the presence of any discontinuities are performed in subsection 5.2.

Panels B and C show how past transaction prices (using transactions during 2000-2009) vary geographically. Regardless of which geographic measure is used, these variables do not appear to behave discontinuously at the treatment boundary. I perform formal tests of the presence of discontinuities in Tables 10 and 11, respectively, in subsection 5.9. From Table 10 column (2), we see that if all within-boundary variation in observed transaction prices were attributable to the treatment discontinuities, rather than smooth geographic variation, the jump should be approximately 0.8. However, these plots reveal that we are closer to the ideal scenario of a

[33]The slight positive coefficient may be explained by the fact that tax-return observed $TaxVal$ also includes the value of secondary homes. Some fraction of households will thus have $TaxVal = \widehat{TaxVal} +$ Value of Secondary Home. The propensity to own a secondary is higher for wealthier households, and as we move towards the right on the geographic axis, households get richer.

true jump of 0. Similarly for labor incomes, in Table 11, column (2), we see that if the entire correlation between model-implied tax assessments and incomes were driven by a discontinuity, the observed jump should be approximately 0.15.

The key take-away from these plots is that past transaction prices and labor income change non-linearly relative to border distance measured in kilometers, but linearly relative to scaled border distance. While visual inspection does not suggest a discontinuity in labor incomes, a formal test in Table 11, column 3, where slopes on border distance (in km) are estimated with side-specific second-order polynomials, shows a discontinuity of 0.1, significant at the 1% level. Thus in order for regression-estimates to agree with our visual inspection, we either need polynomials of an even higher order, or further limit the sample, both of which would have adverse effects on precision.

Formal tests of a discontinuity using scaled border distance (linear slope on each side) in Table 11, column 4, finds no evidence of a discontinuity, and a point-estimate of 0.002. Comparisons of formal tests of discontinuities on past transaction prices provide similar results. My parametric approach for accounting for cross-border heterogeneity in density thus appears to be an attractive way to avoid the detection of non-existing discontinuities without estimating higher-order polynomials in border-distance or losing power by focusing on narrow bands around the borders.

5.2 First Stage Effects on Wealth Tax Outcomes

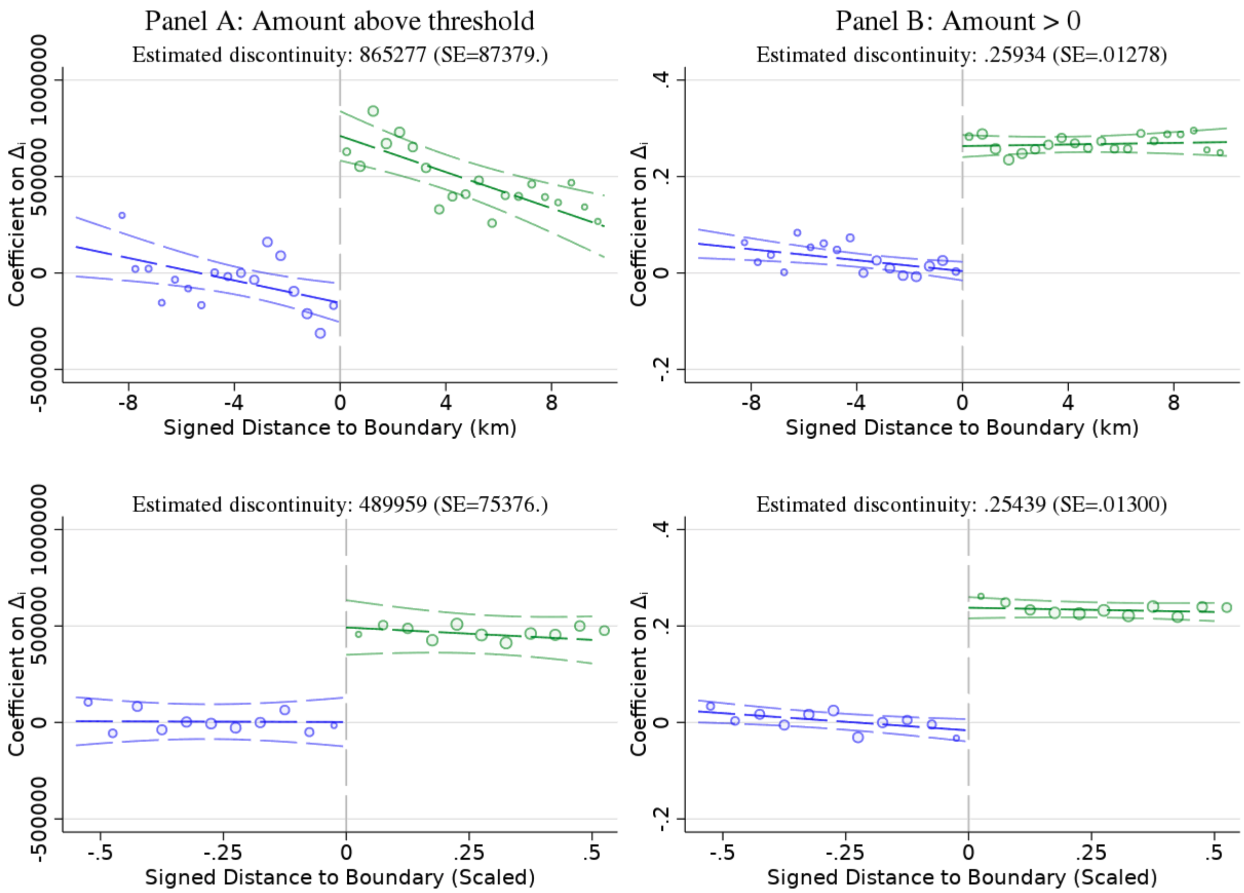
My main specifications will estimate the effect of increased tax assessment on household outcomes. In order to relate these estimates to more generalizable quantities, I perform "first stage regressions" to inform the wealth tax effects of increased tax assessment. Specifically, I provide reduced-form estimates of how changes in (model-implied) tax assessment affects the extensive margin propensity to pay a wealth tax and the amount subject to a wealth tax. I show these results graphically in Figure 3. The pre-period (placebo) version of these plots can be found in Figure A.7 in the Appendix.

I also calculate the impact on the marginal and average after-tax rates-of-return. The estimates isolate the effect coming through wealth taxation, thereby excluding potential behavioral responses affecting the rate-of-return these households achieve. These estimates, and the exact methodologies, are provided in Table 1, which uses the scaled border distance specification. Table A.9 in the Appendix provides the version using the unscaled border distance (in km) specification.

In Figure 3, I show clear evidence of a discontinuous treatment effect in terms of the amount subject to a wealth tax (Panel A, rows 1 and 2) and the probability of facing a positive marginal wealth tax rate (Panel B, rows 1 and 2). A 1-log point increase in model-implied tax assessment increases the amount of taxed wealth by between 0.5 to 0.9 MNOK, and increases the propensity to pay a wealth tax by about 25 percentage points, or roughly 42% relative to a mean of 60%.

FIGURE 3: GRAPHICAL PRESENTATION OF THE REDUCED-FORM EFFECTS ON WEALTH TAX EXPOSURE

This graph shows the reduced-form effects of increased tax assessment on (A) how much of household savings is subject to a wealth tax, i.e., the amount of wealth above the tax threshold, and (B) whether or not a household pays a wealth tax. These outcomes are measured during 2010–2015. The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider post-period saving outcomes for the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.



Row 1 uses distance in kilometers as the running variable, and shows larger effects in terms of the amount subject to a wealth tax in Panel A. The reason is likely that households located near the boundary *in a kilometer sense* tend to be drawn from more urban areas (See Figure A.2 in the Appendix.) House prices in more urban areas are higher, and thus the same percent increase in tax assessments may, in these areas, lead to higher level differences, thus leading to larger intensive margin effects on wealth taxation.

In Table 1, I also include the first stage effect of increasing model-implied tax assessment on actual (as observed in tax returns) tax assessment, $\log(TaxVal)$. This coefficient is 0.8194 in the full sample, and thus fairly close to 1. A coefficient of 1 would be expected in the absence of moving and ownership of secondary homes. In column (2) I provide the estimates corresponding to Panel B, row 2, in Figure 3. Column (3) shows the effect on the marginal rate-of-return, which is roughly the coefficient in column(1) multiplied by the average wealth tax rate of 1.04%. Column (4) shows the effect on the amount of wealth above the threshold, corresponding to Panel A, row 2, in Figure 3. Column (5) provides the effect on the average rate-of-return. I discuss the differences between the marginal and average rates-of-return at the end of this subsection.

Table 1: First Stage Effects on Wealth Tax Outcomes

This table provides the reduced-form using scaled border distance as the geographic measure in equation 19. Column (1) considers the tax value of housing, as observed in tax returns. Column (2) considers the effect on being above the wealth tax threshold. Column (3) considers the effect on the marginal rate-of-return, by isolating extensive margin effects from wealth taxation. This is done by defining the dependent variable to be $-\tau_t \mathbb{1}[TNW_{i,t} > Threshold_t]$. Column (4) examines the effect on the amount above the wealth tax threshold, $\mathbb{1}[TNW_{i,t} > Threshold_t](TNW_{i,t} - Threshold)$. Column (5) considers isolates the effect of increased wealth taxation on the average rate-of-return. This is done by defining the dependent variable as $-\tau_t \mathbb{1}[TNW_{i,t} > Threshold_t](TNW_{i,t} - Threshold)/TNW_{i,t}$, which is evaluated as 0 if $TNW_{i,t} \leq 0$. pp is short for percentage points, and indicates that coefficients (SEs) are multiplied by 100. Standard errors, provided in parenthesis, are clustered at the census tract level.

		Extensive Margin		Extensive and intensive margin	
	$\log(TaxVal)$	$\mathbb{1}[TNW > Threshold]$	$r^{marginal}$ (pp.)	AmountAbove	$r^{average}$ (pp.)
	(1)	(2)	(3)	(4)	(5)
			Full sample		
$\log(\widehat{TaxVal})$	0.8194***	0.2545***	-0.2675***	489959***	-0.1544***
	(0.0316)	(0.0130)	(0.0136)	(75376)	(0.0083)
$F(\hat{\beta} = 0)$	672	383	388	42	347
			Households above tax threshold in 2009		
$\log(\widehat{TaxVal})$	0.8450***	0.2047***	-0.2159***	740511***	-0.1713***
	(0.0440)	(0.015926)	(0.0166)	(120820)	(0.0111)
$F(\hat{\beta} = 0)$	369	165	168	38	239
			Households below tax threshold in 2009		
$\log(\widehat{TaxVal})$	0.7994***	0.2957***	-0.3103***	237618***	-0.1312***
	(0.0414)	(0.0196)	(0.0205)	(48691)	(0.0103)
$F(\hat{\beta} = 0)$	373	227	230	24	162
Scaled Border Distance	Yes	Yes	Yes	Yes	Yes

In the latter sections, I will provide IV estimates of the effect of increased tax assessment $\log(TaxVal)$ on household outcomes, where $\log(TaxVal)$ is instrumented for using model-implied variation in tax assessment. This captures the main source of first-stage uncertainty. Once this is accounted for, the relationship between $TaxVal$ and the wealth tax outcomes provides in columns (2)–(5) in Table 1, such as the amount subject to a wealth tax, is driven by the mechanical relationship dictated by the wealth tax formula in equation 1. Any estimation error in this relationship will therefore not be accounted for when I provide implied elasticities and propensities, e.g., the amount saved per NOK subject to a wealth tax.

An important finding here is that the effect on the marginal rate-of-return is larger than the effect on the average rate-of-return. In light of my conceptual framework introduced in section 3, this suggests that the income effects associated with changing a linear rate-of-return are generally muted. This implies that any finding that saving increases due to higher tax assessment would be indicative of a low (intertemporal) substitution effect, and in general that my reduced-form findings provide a lower bound of the saving effect of changing a linear rate-of-return (where average and marginal rates are affected equally).

In general, the quasi first stage estimates, such as those in Table 1, should be interpreted with some caution, since they may be affected by behavioral responses. For example, if household Taxable Net Wealth is extremely elastic with respect to wealth taxation, increased tax assessment may cause households to lower TNW sufficiently to avoid having to pay a wealth tax. Such behavior would push the first stage estimates towards zero. However, as I will show, behavioral responses are modest. I therefore do not believe that this is a first order concern, and that this framework thus provides useful quantities with which to compare the subsequent estimates of how increases in tax assessment affect household behavior.

5.2.1 Heterogeneous responses

The conceptual framework in Section 3 suggests that income effects are amplified (muted) to the extent that the effect on the average rate-of-return is larger (smaller) than the effect on the marginal rate-of-return. Table 1 above suggests that income effects are muted in the full sample of households, as the effect on the marginal rate-of-return is 0.2675/0.1544-1=73% larger. This underlies some significant heterogeneity: In the above sample, the effect on the marginal rate-of-return is only 19.5% larger than the effect on the average rate-of-return, while in the below sample, it is 125% larger. I would therefore expect to see more (positive) saving responses in the above sample.[34]

The conceptual framework also suggests that income effects are smaller when future incomes, Y_2, are large relative to initial income and wealth, Y_1, i.e., when the human wealth effect is

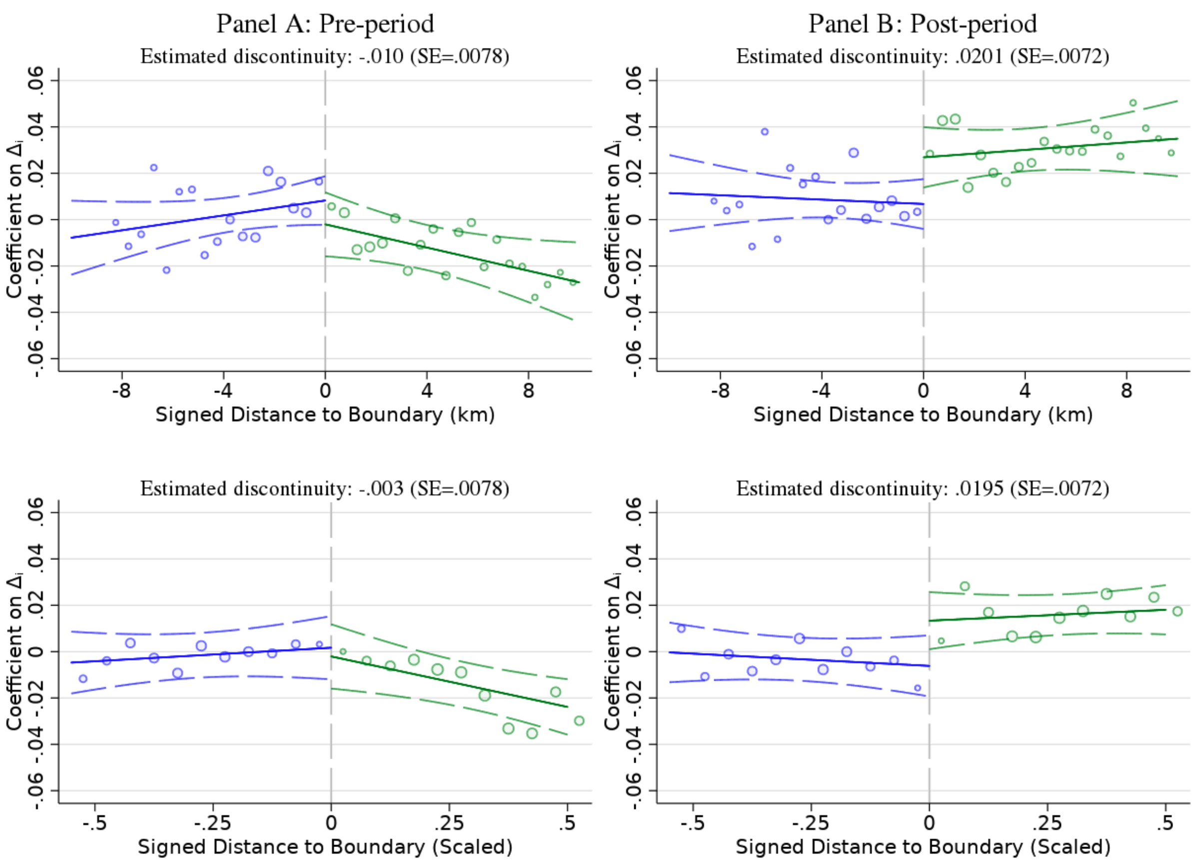
[34]These qualitative differences hold if we consider returns on Gross Financial Wealth (GFW). The marginal returns are the same, but the average returns must be adjusted, since TNW is typically larger than GFW. The effect on the average rate-of-return on GFW is thus larger. By scaling up the average rate-of-return effect by the ratio of the means of TNW and GFW in the respective samples, we get ratios of the effect on the marginal versus the average rates-of-return of 1.00 (=0.2675/((1741/1000)*0.1544)) in the full sample, 0.71 (=0.2169/((2541/1432)*0.1713)) in the above sample, and 1.45 (=0.3103/((758/466)*0.1312)) in the below sample.

larger. In my empirical setting, households whose initial taxable wealth was above the threshold differ from those initially below the threshold along three key characteristics, as shown in the summary statistics in Table A.1: (1) They are slightly older (2 years at the mean), but have similar current levels of labor incomes, thus likely face lower future incomes due to retirement. (2) They have more initial wealth as of 2009. These two elements suggest that households with initial taxable wealth above the threshold will save more or dissave less when subjected to increased tax assessment. If both income effects and substitution effects are large, this may offer differential effects (in terms of the sign of the savings response) based on initial taxable wealth.

5.3 Financial Saving

Figure 4: Graphical Presentation of the Effects of Increased Tax Assessment on Financial Saving

The graphs below show the reduced-form effect on savings of living in a boundary region where households face a one log-point tax assessment premium on the high assessment side. This effect is estimated separately for geographic bins, according to the different location measures. The discontinuities at zero represent the estimated reduced-form causal effect of a one log-point increase in (model-implied) tax assessment on household savings, measured as yearly log-differences of Gross Financial Wealth, adjusted for wealth tax payments. Panel (A) considers pre-period outcomes (2004–2009), and panel (B) considers post-period outcomes (2010–2015). The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal}_i)$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.



In this section, I provide the results on household financial saving. Figure 4 below shows the reduced-form results graphically for two of the main specifications. In Table 2, I provide the corresponding results, using an Instrumental Variables specification, for the full set of geographic running variables. Both Figure 4 and Table 2 consider the effect on wealth-tax adjusted financial saving. The wealth tax adjustment (discussed in the empirical section) removes the mechanical effects of increased taxation on savings, essentially by treating tax payments as saving. Table 3 shows results absent this adjustment, and also considers different components of financial wealth.

Figure 4, Panel (A), rows 1 and 2, both show the estimated effect of increased model-implied tax assessment on pre-period financial saving for the full analysis sample. The first row uses (unscaled) border distance in kilometers as the running variable, while the second uses scaled border distance. Neither specification can reject the null hypothesis of no pre-trends. The visual evidence is consistent with this conclusion. Panel (B), rows 1 and 2 show the effect on financial saving. Both specifications find that a 1 log-point increase in model-implied tax assessment increases saving by approximately 2%. Both discontinuities are significant at the 1% level.

Table 2: The (IV) Effect of Increased Tax Assessment on Household Financial Saving Behavior

This table shows the effect of changing tax assessment on financial saving during 2010–2015. $\log(TaxVal)$ is instrumented for with the model-implied variation in tax assessment. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main IV specification in equation 21. Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract level clustered standard errors are in parenthesis. Sample size is in brackets. F is the Kleinbergen-Paap rk-F statistic of the first-stage regression. One, two, and three stars indicate that estimates are statistically different from zero at the 10, 5, and 1 percent levels, respectively.

$\log(GFW_t + wtax_{t-1}) - \log(GFW_{t-1})$	(1)	(2)	(3)	**(4)**	(5)	(6)
	Full sample					
$\log(TaxVal)$	0.0229***	0.0252***	0.0231***	0.0238***	0.0270**	0.0232***
	(0.0011)	(0.0038)	(0.0083)	(0.0089)	(0.0127)	(0.0080)
N	[1842603]	[1842508]	[1459917]	[1472113]	[1472113]	[1649409]
F	40411	2960	845	683	326	837
	Households Initially Above Threshold					
$\log(TaxVal)$	0.0176***	0.0235***	0.0183*	0.0239**	0.0535***	0.0291***
	(0.0014)	(0.0050)	(0.0109)	(0.0121)	(0.0185)	(0.0113)
N	[1013476]	[1013369]	[817054]	[817214]	[817214]	[912464]
F	27576	1770	496	376	163	420
	Households Initially Below Threshold					
$\log(TaxVal)$	0.0291***	0.0265***	0.0234*	0.0216*	-0.0003	0.0166
	(0.0016)	(0.0057)	(0.0129)	(0.0129)	(0.0169)	(0.0111)
N	[829127]	[829022]	[642762]	[654804]	[654804]	[736853]
F	24703	1940	430	374	189	463
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

Table 2 provides the regression estimates using the IV specification, where tax-return observed tax assessment ($TaxVal$) is instrumented for by model-implied tax assessment ($\widehat{TaxVal}$). The first row of results show the estimated effects of the full sample. The estimates in columns (3) and (4) correspond to the reduced-form estimates in rows 1 and 2, respectively, in Panel (B) of Figure 4. The effects are consistent across specifications, and significantly different from zero at the 1% level for all but the specification that estimates slopes on border distance separately for each border area (column 5). Some of the specifications suggest that effects are weaker for households initially below the wealth tax threshold. This is consistent with the finding from Table 1 that these households are see relatively smaller income effects, since their marginal rate-of-returns is affected considerably more than the average rate-of-return.

To infer the implied saving propensity, namely how many NOK saving increases by for each additional NOK subject to a wealth tax, I perform the following calculation. Table 1 shows that the effect of a 1 log-point increase in $TaxVal$ increases the amount subject to a wealth tax by NOK 489,959, NOK 740,511 and NOK 237,618, for the full, above and below samples, respectively. To relate these effects do the IV estimates, we must divide these amounts by the first stage coefficients of 0.8194, 0.8450, and 0.7994, respectively. This addresses the fact that the IV estimates provide estimated effects of increases in $\log(TaxVal)$, while the estimates in Table 1 provides the estimated effects of increases in model-implied $\log(\widehat{TaxVal})$. While the resulting NOK effect on the amount subject to a wealth tax goes into the denominator, the following number enter the numerator. I take the estimated effects on the saving rate, 0.0238, 0.0239, and 0.0206, and multiply with the respective sample means of GFW of NOK 1,000,000, NOK 1,432,000, and NOK 466,000, respectively. This provides propensities (SEs) to increase saving out of increased wealth tax exposure of 0.0398 (0.0149), 0.0391 (0.0198), and 0.0339 (0.0202), respectively for the three samples.

For the full sample, these numbers suggest that households save approximately 4 NOK per additional NOK subject to a wealth tax. That includes approximately 1 NOK that goes towards paying the wealth taxes for that year. The residual 3 NOK may be interpreted as additional saving to offset future wealth tax payments. This seems reasonable from a life-cycle perspective, given that the average household is 62 years and thus not far from retirement.

I perform similar calculations to calculate the implied semi-elasticities of saving to the marginal rate-of-return. I divide the estimated coefficients by the (reduced-form) effect on the marginal rate of return, $r^{marginal}$, which themselves are divided by the first stage coefficients in column (1) of Table 1. This provides semi-elasticities of saving with respect to the marginal rate-of-return of -7.2904 (2.2762), -9.3541 (4.7357), and -5.5646 (3.3233), for the full, above, and below samples, respectively. This first estimate may read the following way. a 0.1 percentage point decrease in the marginal rate-of-return increases the *saving rate* out of financial wealth by 0.73 percentage points.

I perform a number of robustness tests. In Table A.3, I provide results when using triangular (distance-based) weights. All standard errors provided in the main text are clustered on the census-tract level. In A.4, I provide standard errors when clustering at the household or municipality level. Standard errors are slightly smaller when accounting for correlation in the error term across larger geographic areas (municipalities). In Tables A.6, A.7, and A.8, I provide estimated IV effects when varying the location measure cut-offs for the scaled border distance measure, border distance in KM, and the relative location measure, respectively. Effects are qualitatively similar when varying the bandwidths, but tend to be larger (and more noisily estimated) the more narrow the bandwidths are.

5.3.1 Decompositions of Financial Saving Effect

In Table 3 below, I consider the effects on saving, when not adjusting for wealth tax payments in column (2). In column (3), I further isolate responses to changes in third-party reported domestic deposits. In column (4), I only consider items that include self-reported items, such as foreign wealth and outstanding claims. Column (1) provides the the results from the baseline definition for reference. I provide results using scaled border distance as the geographic running variable.

Table 3: Decomposition of Financial Saving Response

In this table, I provide the IV effects using different measures of financial savings. Column (1) uses the baseline definition, which adjusts for wealth tax payments. Column (2) does not account for wealth tax payments. (3) Only considers domestically-held (and thus third-party reported) holdings of deposits. (4) Only considers self-reported wealth items: foreign financial assets and outstanding claims. Reduced-form standard errors are provided in parenthesis, and are clustered on the census tract level.

	Adj. $\Delta \log(GFW)$	Unadj. $\Delta \log(GFW)$	Δ log(Dom. Deposits)	Δ log(Self-Rep. GFW)
	(1)	(2)	(3)	(4)
	Full sample			
$\log(TaxVal)$	0.0238***	0.0149	0.0154	0.0254***
	(0.0089)	(0.0091)	(0.0099)	(0.0096)
	Households Initially Above Threshold			
$\log(TaxVal)$	0.0239**	0.0144	0.0093	0.0254
	(0.0121)	(0.0127)	(0.0141)	(0.0160)
	Households Initially Below Threshold			
$\log(TaxVal)$	0.0216*	0.0131	0.0201	0.0229**
	(0.0129)	(0.0129)	(0.0139)	(0.0097)
Controls				
Household Characteristics	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes
– Border specific	Yes	Yes	Yes	Yes
Border Distance Controls				
– Scaled	Yes	Yes	Yes	Yes

Comparing columns (1) and (2) suggest that around 0.149/0.0238=63% of the effect on saving comes from increased accumulation of financial wealth, and 37% comes from increased wealth tax payments. I perform the same exercise as in the previous section to calculate the implied propensities to save out of increased wealth tax exposure. I find that households in the full, above, and below samples, increase their savings (SE) by NOK 0.0249 (0.0152), 0.0235

(0.0208), and 0.0205 (0.0202) for each additional NOK subject to a wealth tax, respectively. The implied semi-elasticities are -4.5641 (2.7875), 5.6359 (4.9706), and 3.3748 (3.3233), respectively.

In column (3) I focus on changes in domestic holdings of deposits, which is the primary (financial) saving vehicle for Norwegian households. Deposits are broadly defined, and includes various forms of low-risk savings vehicles offered through banks. From the summary statistics in Table A.1, we see that deposits make up 98% (80%) of financial wealth for the median (mean) household in the full sample, and 94% (77%) for the median (mean) household in the above sample.[35]

While the estimated effect on deposit growth is not statistically significant, it is important to note that this specification does not account for the mechanical effects of increased wealth taxation. The relevant null hypothesis is therefore not zero, but the implied mechanical effect on deposits. Under the reasonable assumption that wealth taxes are paid out of deposits, the standard no-behavioral-response null-hypothesis would be a point estimate of -0.0045.[36] This would imply a t-statistic (column 3, full sample) of (0.0154+0.0045)/0.0099=2.01.

In column (4), I only consider self-reported items, and find qualitatively similar results. This suggests, that even though there is some scope for misreporting responses along asset classes such as foreign wealth, this does not seem to materialize itself as less reported wealth.

5.4 Debt

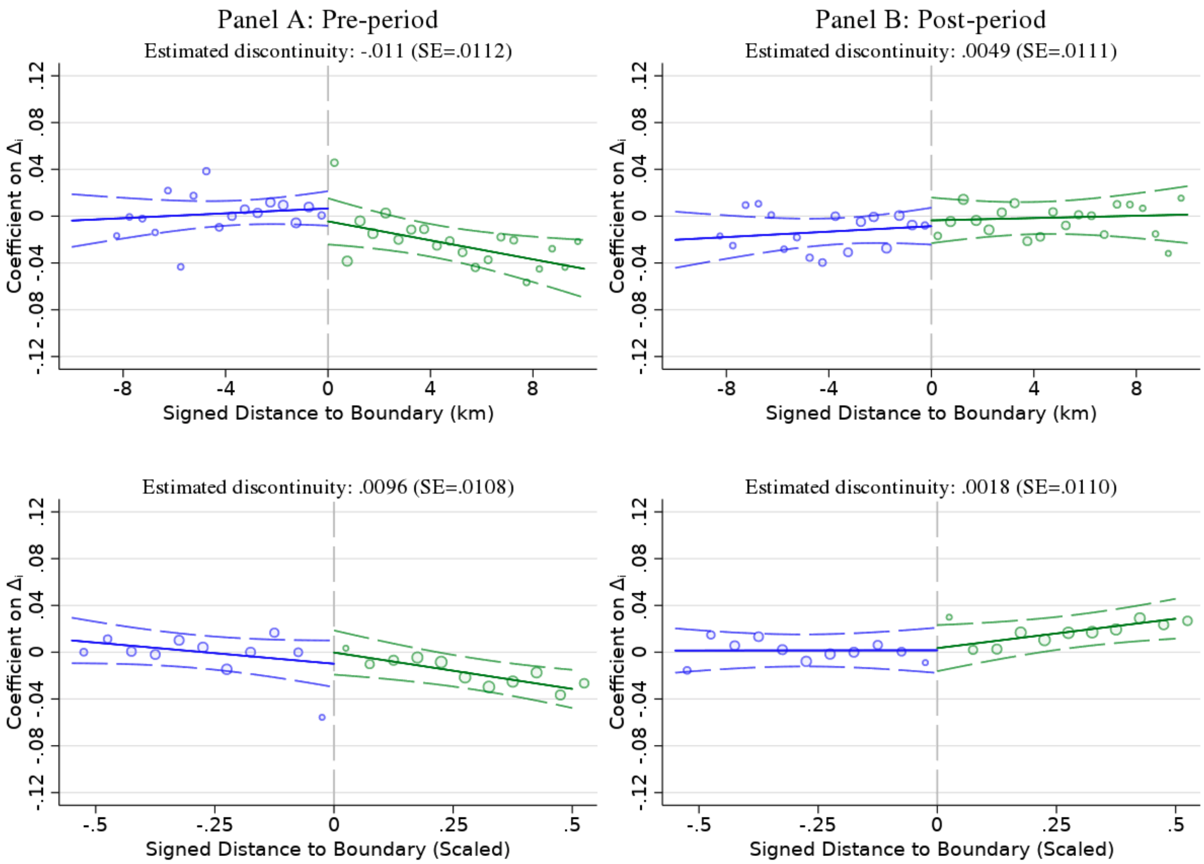
In this section, I explore the effects on household debt. Figure 5 shows the reduced-form results for the full sample. The first row uses the (unscaled) border distance specification, while the second row uses scaled border distance as the geographic running variable. Panel A finds no evidence of pre-trends in debt accumulation, and Panel B finds no evidence for any effect during the post-period, 2010–2015.

[35] This is higher than in the U.S. Fagereng et al. (2018) provides a comparison of the financial balance sheets of U.S. consumers present in the Survey of Consumer Finances (SCF) and Norwegian households by percentiles of financial wealth. This comparison accounts for the fact that pensions are largely provided by the government and therefore do not appear on household balance sheets. From their Table OA.1, I see that SCF households in the 90th to 95th percentile hold 47% of their (non-private equity) financial wealth in deposits and bonds. The comparable share for Norwegian households found from Table 1A is 78%.

[36] In Table 1, we see that a 1 log point increase in TaxVal reduces the average rate-of-return by 0.1544/0.8194=0.1884 pp. If I assume that the wealth taxes are paid out of Deposits (rather than out of all of TNW), the reduction in Deposits would be 0.1884% * 1741/722=0.4543%, where 722 is the mean amount of TNW and 722 is the mean amount of Deposits, in thousands of NOK, obtained from Table A.1.

Figure 5: Debt

The graphs below show the effect on log-differenced debt of living in a boundary region where households face a one log-point tax assessment premium on the high assessment side. This effect is estimated separately for geographic bins, according to the different location measures. The discontinuities at zero represent the estimated reduced-form causal effect of a one log-point increase in (model-implied) tax assessment on household savings, measured as yearly log-differences of Gross Financial Wealth, adjusted for wealth tax payments. Panel (A) considers pre-period outcomes (2004–2009), and panel (B) considers post-period outcomes (2010–2015). The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal}_i)$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.



In Table 4, I show the results from the IV specifications using the full set of specifications, and providing results separately for the different subsamples. All the specifications that control for unobserved geographic heterogeneity (columns 3–6) find no evidence that debt increases as a result of higher tax assessment. The coefficients in column (5) suggests that households reduce debt. However, this result can not be inferred from the other specifications.

Table 4: Debt

This table shows the effect of tax assessment on log-differenced debt during 2010–2015. log($TaxVal$) is instrumented for with the model-implied variation in tax assessment. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main IV specification in equation 21. Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract level clustered standard errors are in parenthesis. Sample size is in brackets. F is the Kleinbergen-Paap rk-F statistic of the first-stage regression. One, two, and three stars indicate that estimates are statistically different from zero at the 10, 5, and 1 percent levels, respectively.

Δ log($Debt$)	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample					
log($TaxVal$)	0.0300***	0.0265***	0.0057	0.0022	-0.0356*	-0.0060
	(0.0016)	(0.0058)	(0.0128)	(0.0135)	(0.0189)	(0.0119)
N	[1842624]	[1842529]	[1459935]	[1472130]	[1472130]	[1649429]
F	40414	2960	845	683	326	836
	Households Initially Above Threshold					
log($TaxVal$)	0.0296***	0.0249***	0.0153	-0.0019	-0.0408	-0.0025
	(0.0022)	(0.0080)	(0.0170)	(0.0186)	(0.0276)	(0.0168)
N	[1013495]	[1013388]	[817070]	[817229]	[817229]	[912482]
F	27580	1770	496	375	162	419
	Households Initially Below Threshold					
log($TaxVal$)	0.0307***	0.0286***	-0.0040	0.0098	-0.0162	-0.0024
	(0.0024)	(0.0082)	(0.0189)	(0.0194)	(0.0246)	(0.0164)
N	[829129]	[829024]	[642764]	[654806]	[654806]	[736855]
F	24704	1940	430	374	189	463
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

5.5 Portfolio Allocation

5.5.1 Risky Share of Financial Wealth

In this section I examine the effect of increased tax assessment on the share of financial wealth invested in the stock market. Figure 6 shows that there are no discontinuities during either the pre-period or post-period.

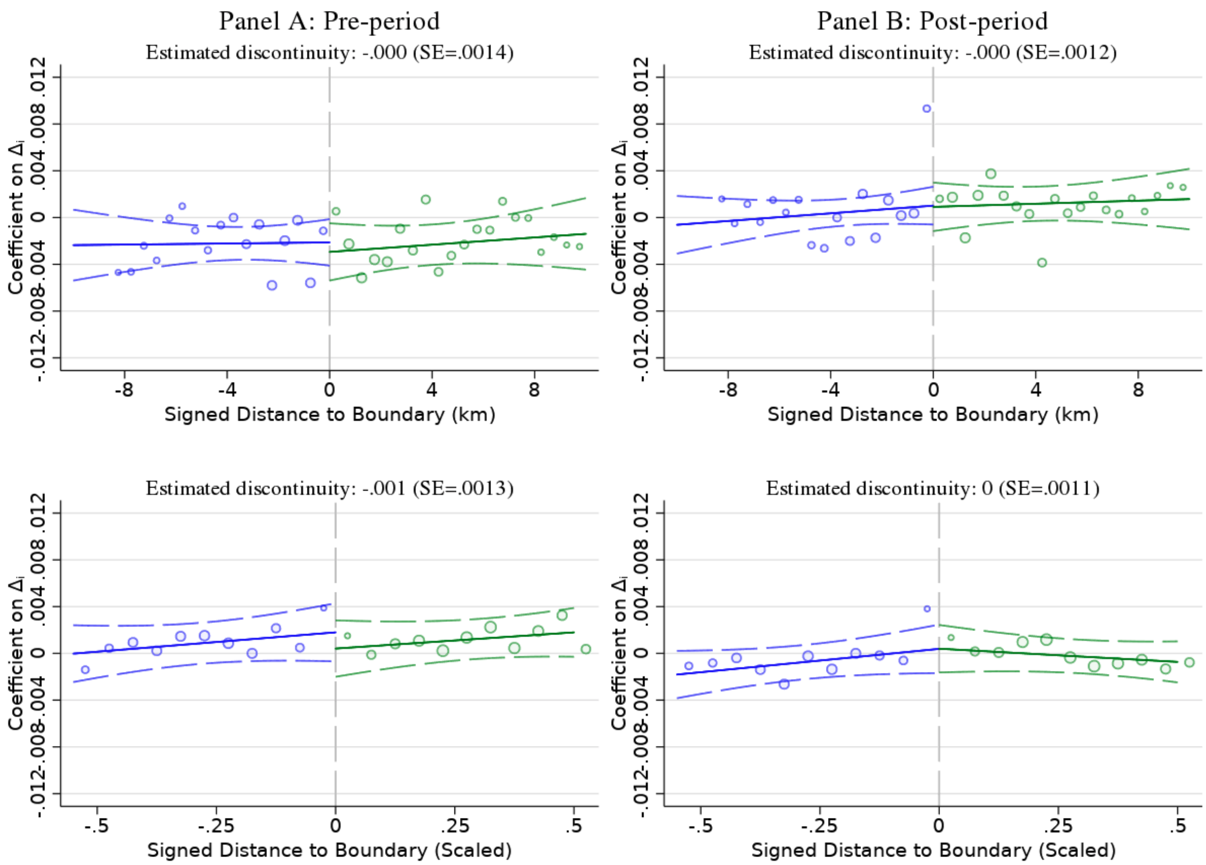
I perform a similar calculation as in the previous sections to calculate implied (semi-semi) elasticities. A 1 percentage point decrease in the after-tax rate-of-return changes the the stock market share by 0 (0.4288), -0.2348 (0.7828), and 0.2834 (0.5410) percentage points, for the full, above, and below samples, respectively, where standard errors are provided in parenthesis. In the full sample, I can thus rule out (at the 95% level) that a 1 percentage point reduction in the net-of-tax return reduces the stock market share by more than 0.84 percentage points and I can rule out an elasticity (dividing the effect by the mean share of financial wealth, and

1 percentage point by an average rate-of-return of 2%) larger than 0.17 in absolute value.

A potential hypothesis is that households take on more risk in order to increase their capital gains (in expectation) and thereby offset the effects of increased taxation. Focusing on the full sample, I multiply the estimated effect (and the SEs) by the average amount of financial wealth in the sample and divide by the estimated effect on the amount subject to the wealth tax to obtain a propensity to save in stocks out of wealth tax exposure. Given that this effect arises through changes in the stock market share, this would be a propensity to save in stocks, above-and-beyond what would be implied by maintaining a constant risky share. This exercise yields a 95% confidence interval of $\pm$1.96*0.0014*1000000/489959=[-0.0056,0.0056]. If we assume a risk premium of 5%, then the confidence interval on the effect on expected capital gains is [-0.0003,0.0003]. This does not come close to offsetting the effect on yearly wealth tax payments of around 0.0104 per additional NOK subject to the wealth tax.

Figure 6: Stock Market Share

The graphs below show the effect on the one-year differenced stock market share of living in a boundary region where households face a one log-point tax assessment premium on the high assessment side. This effect is estimated separately for geographic bins, according to the different location measures. The discontinuities at zero represent the estimated reduced-form causal effect of a one log-point increase in (model-implied) tax assessment on household savings, measured as yearly log-differences of Gross Financial Wealth, adjusted for wealth tax payments. Panel (A) considers pre-period outcomes (2004–2009), and panel (B) considers post-period outcomes (2010–2015). The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels the full sample of households with initial positive taxable net wealth in 2009. Scatter-points are estimated using the specification in equation 19, excluding geographic trend controls, **g**, allowing the coefficient on Δ_i to vary by bin. The size of each circle correspond approximately to the number of observations in that bin.



In Table A.5 in the Appendix, I provide results when instead considering a broader measure of the risky share of financial wealth. This measure also includes holdings of non-listed stocks, i.e., private equity. With this definition, the effects suggest a (statistically insignificant) reduction in risk-taking. For the full sample, a 1 percentage point reduction in the after-tax rate-of-return reduces the risky share of financial wealth by 0.8577 (SE=0.5514) percentage points.

Table 5: Stock Market Share

This table shows the effect of tax assessment on the (one-year differenced) share of wealth allocated to the stock market during 2010–2015. $\log(TaxVal)$ is instrumented for with the model-implied variation in tax assessment. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main IV specification in equation 21. Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract level clustered standard errors are in parenthesis. Sample size is in brackets. F is the Kleinbergen-Paap rk-F statistic of the first-stage regression. One, two, and three stars indicate that estimates are statistically different from zero at the 10, 5, and 1 percent levels, respectively.

ΔSMS	(1)	(2)	(3)	**(4)**	(5)	(6)
			Full sample			
$\log(TaxVal)$	0.0004**	0.0004	-0.0001	0.0000	-0.0012	0.0001
	(0.0002)	(0.0006)	(0.0014)	(0.0014)	(0.0020)	(0.0012)
N	[1835781]	[1835687]	[1454510]	[1466682]	[1466682]	[1643325]
F	41089	3022	857	698	332	850
			Households Initially Above Threshold			
$\log(TaxVal)$	0.0008***	0.0010	0.0004	0.0006	-0.0010	0.0007
	(0.0002)	(0.0008)	(0.0018)	(0.0020)	(0.0029)	(0.0017)
N	[1008692]	[1008586]	[813160]	[813335]	[813335]	[908140]
F	28321	1818	509	387	166	430
			Households Initially Below Threshold			
$\log(TaxVal)$	-0.0004	-0.0004	-0.0011	-0.0011	-0.0015	-0.0009
	(0.0003)	(0.0009)	(0.0022)	(0.0021)	(0.0028)	(0.0017)
N	[827089]	[826985]	[641250]	[653253]	[653253]	[735094]
F	24962	1952	431	378	192	466
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

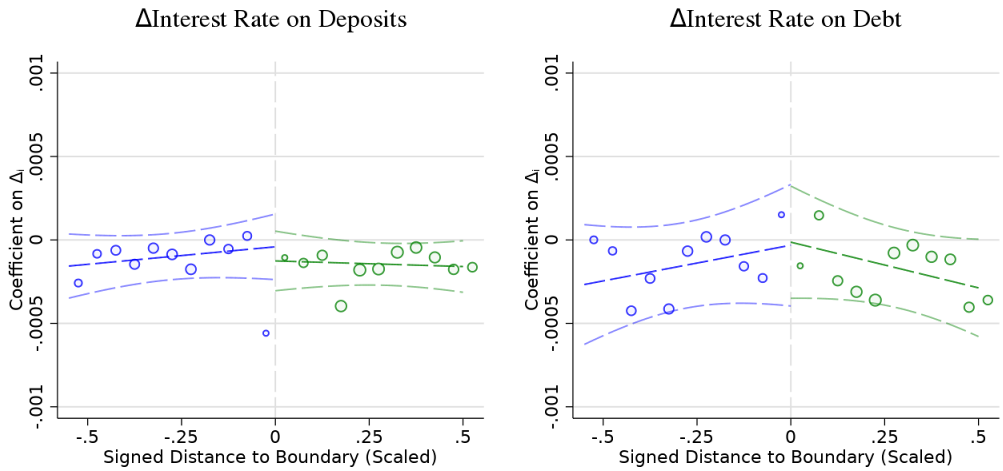
5.5.2 Interest Rates

Below I present the results for realized interest rates on deposits and debt. Figure 7 shows the results graphically, focusing on the scaled border distance specification.

I find no statistically significant effects on the interest rates on either deposits or debt. To speak to what effects I can rule out, I focus on the estimated effect on interest rates on deposits for the full sample, using the scaled border distance specification in column (2). This specification yields an (semi-semi) elasticity of -0.000101/(-0.002675/0.8194)=0.0309 (SE=0.0380). In other words, the effect of a 1 percentage point decrease in the marginal after-tax rate-of-return on realized returns has a 95% confidence interval of [-0.0437, 0.1053] percentage points.

Figure 7: Interest Rates on Deposits and Debt

The dependent variable is one-year differences realized interest rates on deposits (left) and debt (right). This graph only considers households with taxable net wealth above the wealth tax threshold in 2009 (full sample). The discontinuities at zero represent the estimated causal effect of a one log-point increase in tax assessment, and correspond to the estimated coefficients in Table 6 columns (1) and (3) for the left and right hand side figures, respectively. Both graphs use scaled border distance as the geographic running variable.



To calculate back-of-the-envelope bounds on how much of increased wealth tax payments is offset by realizing better returns on deposits, I perform the following exercise. Multiplying the coefficient in column (2) of -0.000101 (SE=0.000124) by the mean amount of deposits of 0.722 MNOK yields a NOK effect of 73 (89). At the same time, a 1 log increase in TaxVal will increase the amount of wealth subject to a wealth tax by 0.49MNOK/0.8194 $\approx$ 0.6 MNOK. The propensity to earn more interest out of increased wealth tax exposure is thus 0.000121 (SE=0.000148), which is small relative to the impact on the yearly wealth tax bill of around 1.04%.

Table 6: Effects of Increased Tax Assessment on Interest Rates on Deposits and Debt

This table shows the IV effects on realized interest rates on deposits and debt. Realized interest rates for $X \in \{\text{Deposits}, \text{Debt}\}$ are calculated as $\text{Interest(X)}_{i,t}/(0.5X_{i,t} + 0.5X_{i,t-1})$ Only households with debt in excess of NOK 10,000 are included when examining the interest rate on debt. Standard errors are provided in parenthesis, and are clustered on the census tract level. Sample sizes are provided in brackets.

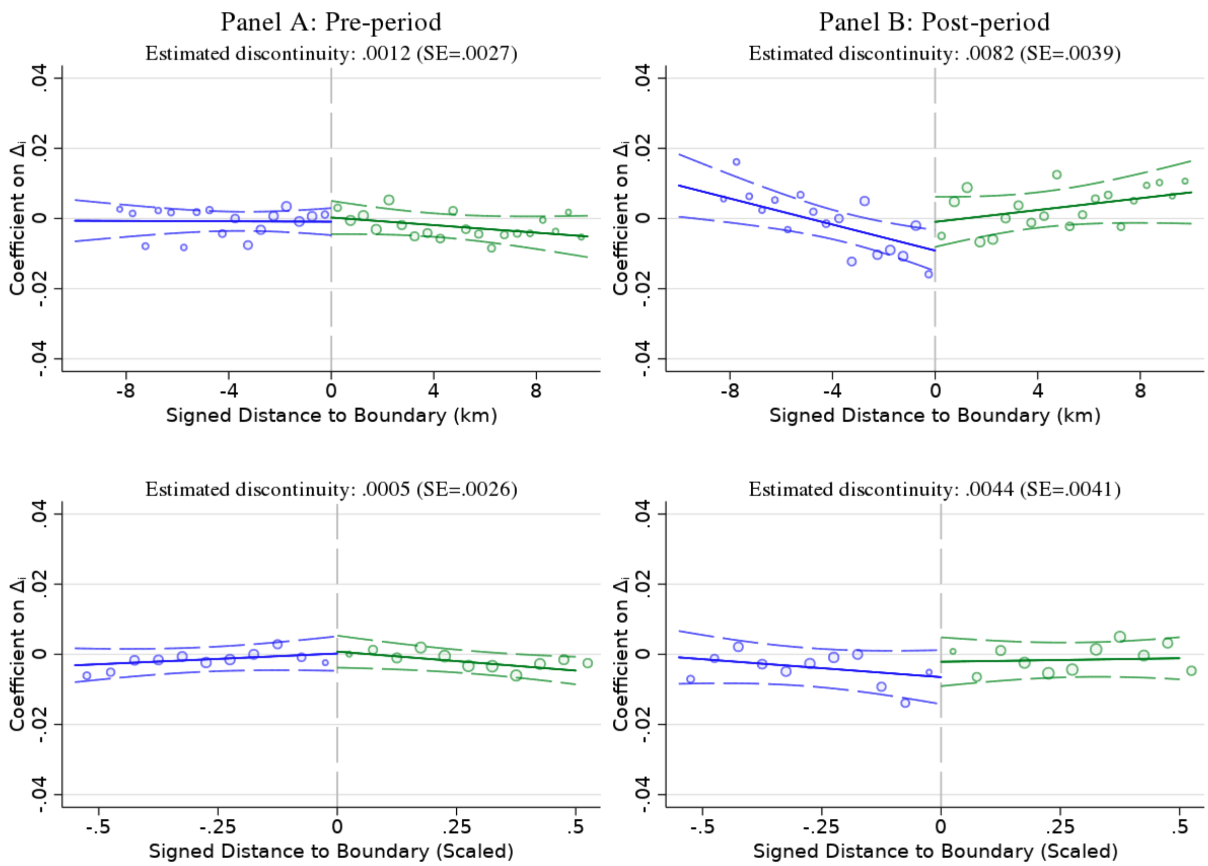
	Interest Rates on Deposits			Interest Rates on Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample					
$\log(TaxVal)$	-0.000145	-0.000101	-0.000131	0.000156	0.000023	0.000046
	(0.000119)	(0.000124)	(0.000107)	(0.000216)	(0.000228)	(0.000197)
N	[1230385]	[1240695]	[1390122]	[725506]	[726694]	[812642]
F	983	798	976	670	540	628
	Households Initially Above Threshold					
$\log(TaxVal)$	-0.000032	-0.000023	-0.000074	0.000379	-0.000053	-0.000262
	(0.000154)	(0.000167)	(0.000147)	(0.000393)	(0.000439)	(0.000409)
N	[689009]	[689150]	[769440]	[291170]	[286855]	[318960]
F	566	428	483	271	197	204
	Households Initially Below Threshold					
$\log(TaxVal)$	-0.000390**	-0.000218	-0.000224	0.000057	0.000137	0.000224
	(0.000193)	(0.000181)	(0.000159)	(0.000255)	(0.000262)	(0.000212)
N	[541270]	[551446]	[620587]	[434182]	[439676]	[493528]
F	504	438	535	455	378	464
Controls						
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
Housing Controls	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	Yes	Yes	Yes	Yes	Yes	Yes
Border Distance (KM)	Yes	–	–	Yes	–	–
Border Distance (Scaled)	–	Yes	–	–	Yes	–
Relative Location Control	–	–	Yes	–	–	Yes

5.6 Labor Income

5.6.1 Total Taxable Labor Income

Figure 8: Labor Income

The graphs below show the effect on one-year log-differenced labor income of living in a boundary region where households face a one log-point tax assessment premium on the high assessment side. This effect is estimated separately for geographic bins, according to the different location measures. The discontinuities at zero represent the estimated reduced-form causal effect of a one log-point increase in (model-implied) tax assessment on household savings, measured as yearly log-differences of Gross Financial Wealth, adjusted for wealth tax payments. Panel (A) considers pre-period outcomes (2004–2009), and panel (B) considers post-period outcomes (2010–2015). The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.



This section shows the results on household total taxable labor income. This definition of labor income includes transfers, such as UI benefits, labor-related pension payments, sickness and parental leave benefits. Figure A.9 shows the reduced-form effects for the full sample.

Table 7 shows estimated coefficients using different specifications and an IV setup. There is meaningful within-sample variation in the point-estimates across the specifications that address geographic heterogeneity (columns 3–6). To calculate implied elasticities and propensities, I take the average coefficient (and standard errors) across these specifications. Performing the same exercises as in the previous sections, I find an implied semi-elasticity (SE) of labor income to the marginal after-tax rate-of-return of -2.6190 (1.6388), -7.2798 (3.1996), and 0.0966 (1.6552), for the full, above, and below samples, respectively. A similar exercise yields propensities to earn (pre-tax) of 0.0099 (0.0062), 0.0147 (0.0065), and -0.0009 (0.0150), respectively.

Table 7: Labor Income

This table shows the effect of tax assessment on log-differenced labor income during 2010–2015. Labor Income is defined as the sum of wage earnings, self-employment earnings, pensions, and unemployment income. $\log(TaxVal)$ is instrumented for with the model-implied variation in tax assessment. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main IV specification in equation 21. Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract level clustered standard errors are in parenthesis. Sample size is in brackets. F is the Kleinbergen-Paap rk-F statistic of the first-stage regression. One, two, and three stars indicate that estimates are statistically different from zero at the 10, 5, and 1 percent levels, respectively.

$\Delta\log(Labor\ Income)$	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample					
$\log(TaxVal)$	0.0049***	0.0032	0.0094**	0.0053	0.0132*	0.0063
	(0.0006)	(0.0021)	(0.0045)	(0.0050)	(0.0074)	(0.0045)
N	[1844488]	[1844392]	[1461364]	[1473585]	[1473585]	[1651083]
F	40163	2968	839	681	327	837
	Households Initially Above Threshold					
$\log(TaxVal)$	0.0053***	0.0026	0.0161**	0.0139*	0.0325***	0.0119*
	(0.0009)	(0.0033)	(0.0065)	(0.0075)	(0.0115)	(0.0072)
N	[1014776]	[1014669]	[818080]	[818244]	[818244]	[913625]
F	27346	1754	494	375	164	420
	Households Initially Below Threshold					
$\log(TaxVal)$	0.0039***	0.0037	-0.0015	-0.0036	-0.0041	0.0005
	(0.0008)	(0.0026)	(0.0059)	(0.0063)	(0.0082)	(0.0053)
N	[829712]	[829607]	[643184]	[655247]	[655247]	[737367]
F	24628	1961	427	374	188	465
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

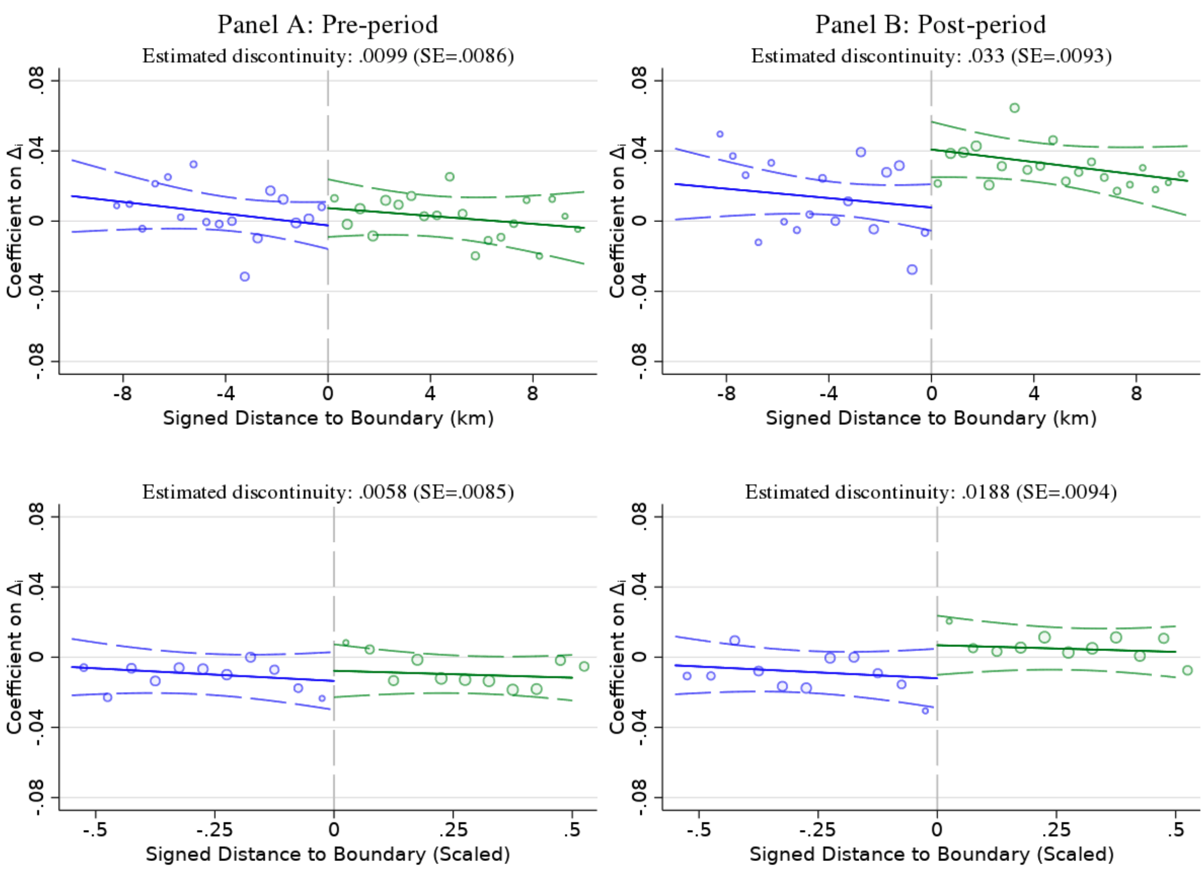
5.6.2 Salary and self-employment income

In this subsection, I focus on labor income in the form of salary and self-employment income. This is an important robustness check in my setting, since many of the households in my empirical setting are past retirement age. This means that a large part of their labor incomes come in the form of pensions that are the result of previous – unaffected – labor supply. The summary statistics in Table A.1 shows that for the full sample, the mean of salary and self-employment income is 62% that of the more comprehensive definition.

Figure 9 shows the results for the full sample using both the unscaled (km) and scaled border distance specification. The plots reveal a clear level shift in the post period. While there is no stark evidence of a pre-trend, the plots suggest slightly higher earnings growth in the pre-period. In Figure A.10, I provide the same plots, restricting the sample to those initially above the tax threshold. As I will show in Table 8 below, the effect is driven by this subsample, and it is thus reassuring that Figure A.10 shows no visual indication of pre-trends.

Figure 9: Salary and Self-Employment Income

The graphs below show the effect on one-year log-differenced labor income of living in a boundary region where households face a one log-point tax assessment premium on the high assessment side. This effect is estimated separately for geographic bins, according to the different location measures. The discontinuities at zero represent the estimated reduced-form causal effect of a one log-point increase in (model-implied) tax assessment on household savings, measured as yearly log-differences of Gross Financial Wealth, adjusted for wealth tax payments. Panel (A) considers pre-period outcomes (2004–2009), and panel (B) considers post-period outcomes (2010–2015). The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.



This change of definition reveals considerably stronger labor earnings responses. For the full sample, the coefficients from the IV regressions in Table 8 are 2–4 times larger than those using the more comprehensive definition in Table 7. For the above sample, estimated coefficients are around 3 times larger. This implies that estimated elasticities are correspondingly larger in magnitude.

Consistent with results using the previous definition, there is noticeable difference in the responses between households initially above versus those initially below the wealth tax threshold.

Table 8: The Effects of Increased Tax Assessment On Salary and Self-employment Income

This table shows the effect of tax assessment on log-differenced labor income, excluding pensions and transfers, during 2010–2015. Labor Income is defined as the sum of wage earnings, self-employment earnings, pensions, and unemployment income. $\log(TaxVal)$ is instrumented for with the model-implied variation in tax assessment. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main IV specification in equation 21. Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract level clustered standard errors are in parenthesis. Sample size is in brackets. F is the Kleinbergen-Paap rk-F statistic of the first-stage regression. One, two, and three stars indicate that estimates are statistically different from zero at the 10, 5, and 1 percent levels, respectively.

Δ log(Salary and Self-E. Income)	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample					
$\log(TaxVal)$	0.0162***	0.0159***	0.0379***	0.0230**	0.0233	0.0263***
	(0.0013)	(0.0049)	(0.0108)	(0.0116)	(0.0158)	(0.0099)
N	[1844488]	[1844392]	[1461364]	[1473585]	[1473585]	[1651083]
F	40163	2968	839	681	327	837
	Households Initially Above Threshold					
$\log(TaxVal)$	0.0166***	0.0194***	0.0528***	0.0336**	0.0290	0.0386***
	(0.0017)	(0.0069)	(0.0149)	(0.0161)	(0.0225)	(0.0142)
N	[1014776]	[1014669]	[818080]	[818244]	[818244]	[913625]
F	27346	1754	494	375	164	420
	Households Initially Below Threshold					
$\log(TaxVal)$	0.0149***	0.0112*	0.0133	0.0103	0.0094	0.0134
	(0.0019)	(0.0064)	(0.0154)	(0.0166)	(0.0215)	(0.0137)
N	[829712]	[829607]	[643184]	[655247]	[655247]	[737367]
F	24628	1961	427	374	188	465
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

Repeating the calculations in the previous subsection yields semi-elasticities (SEs) of -7.0453 (3.5533), -13.1505 (6.3013), and -2.6535 (4.2765), for the full, above and below samples, respectively. The implied propensities (SEs) to increase yearly pre-tax earnings out of increased wealth tax exposure are 0.0165 (0.0083), 0.0155 (0.0074), and 0.0159 (0.0257), for the full, above and below samples, respectively.

Both Tables 9 and 8 reveal much stronger (71% and 65%, respectively) labor earnings responses when using border distance in kilometers versus scaled border distance as the geographic running variable. This is consistent with the finding in Figure 3, that estimated discontinuity in wealth tax exposure (measured in NOK) is 77% larger when using border distance in kilometers. In other words, while the assessment effects are different, the propensities to earn out of wealth tax exposure are similar across specifications.

5.6.3 Labor-financed savings

This subsection provides some back-of-the-envelope calculations regarding the extent to which saving responses are financed by increased labor earnings.

In subsection 5.3.1 I found that the propensity to accumulate more financial wealth out of increased wealth tax exposure was 0.0249. If we also account for increased tax payments, this leads to a cumulative saving propensity over 5 years of 0.0249*5 + 5*1.04% = 0.1765. The yearly propensity to increase total taxable labor earnings of 0.0099 imply a cumulative earnings propensity of 0.0099 + 2*0.0099 + ... + 5*0.0099=0.1485. Over the course of five years, households will have earned 0.1485 NOK for each additional NOK subject to the wealth tax. If I assume an average marginal tax rate of 40%, this implies increased earnings over a 5-year period of 0.1485*(1-40%)=0.0891. Together, these numbers imply that around 0.0891/0.1765≈ 50% of the increase in saving was financed by increased labor earnings. If I instead use the implied propensity to earn (only salary and self-employment income) of 0.0150, this labor-financed share is instead (5+...+1)*0.0165*(1-40%)/(0.1765)=84%.

5.7 Year-by-year effects

In this section, I decompose the pooled post-period (2010–2015) results, by estimating coefficients separately for each year. I plot these results separately for the four main outcome variables in 10 below, using the scaled border distance specification.

Real responses to capital taxation are likely to be somewhat sluggish. It is therefore useful to investigate the dynamics of the effect. By showing that the main estimates are not driven by a single year is consistent with effects being driven by real responses, which are likely to be somewhat sluggish. For example, Zoutman (2018) finds that his estimated elasticities are driven by immediate responses, and thereby attributes the elasticity high to changes in reporting behavior.

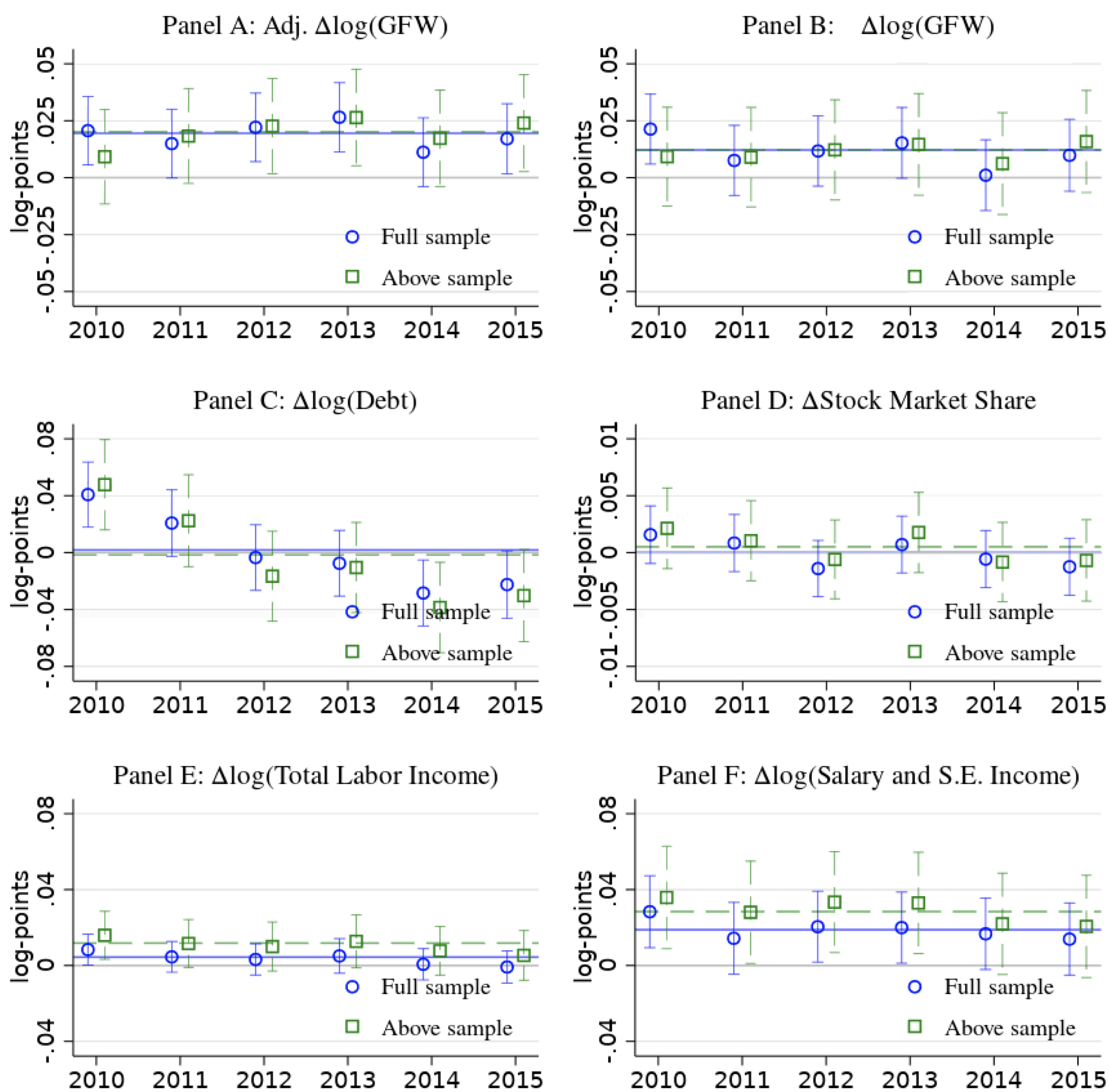
Panel (A) considers the effects on financial savings. As a reference, the estimated pooled coefficients from (corresponding Table 2 column (4) are plotted as horizontal lines. We see that the savings effect is persistent across the years, and that the yearly coefficients hover around the pooled coefficient. Panel (B) similarly shows the yearly estimated effects for the unadjusted saving measure.

Panel (C) considers the effects on debt. This reveals a somewhat more interesting pattern.

The immediate response appears to be to increase debt (or reduce it less), while the opposite seems to be the case towards 2014–2015. This could be consistent with constrained households lowering their debt payments to pay for their taxes, but eventually respond to the income effect and increase their saving by paying off their debts.

Figure 10: Yearly Decomposition of Estimated (Reduced-form) Effects

In this graph, I allow the estimated discontinuities, $\hat{\beta}$, to vary by year. Otherwise, the specifications are identical to those in column (4) of the respective tables, which includes the scaled border distance control. Standard errors are clustered on the census tract level, and plots indicate 95% confidence intervals for the point estimates. Coefficients are estimated in two different samples: *Full sample* corresponds to all households in the analysis sample, and the *Above sample* is the sample of households with Taxable Net Wealth above the wealth tax threshold in 2009. The blue solid (green dashed) line corresponds to the pooled regression estimates for the Full (Above) sample. Panel D considers total taxable labor income, which includes pension income.



Panel (D) considers the share of wealth allocated to the stock market. I see no dynamic effects, and all coefficients are close to zero, as are the pooled estimates.

Panel (E) considers total taxable labor income. The point-estimates suggest (although this suggestion is far from statistically significant) that initial responses are somewhat larger, and that the effects eventually dissipate. This seems highly reasonable, given that the average age of households is 62 (measured in 2009), and the modal age of retirement in Norway is 67, thus I expect the median household to retire during 2014–2015. We see a similar pattern in panel (F), which considers salary and self-employment incomes.

5.8 Subsequent Transaction Prices

In this section, I investigate the effects on subsequent transaction outcomes in terms of the likelihood of selling and the conditional subsequent sales price. I report these results in Table 9.

In columns (4)-(6) I consider extensive margin effects. The treatment in my setting is specific to the house that the households own. Households may therefore undo the treatment by selling their house and moving to an area with lower assessments. I do not believe that this is likely, given my impression of limited awareness of the geographic aspects of the pricing model, as well as the likely presence of sizable costs associated with moving. Consistent with this, I find statistically small effects on the propensity to sell. The estimates show that a 1 log-point increase in $\widehat{TaxVal}$ increases the likelihood of selling by 1.2 percentage points, and I can rule out any effects larger than 4.2 percentage points at the 5% level.

In columns (1)-(3) I consider conditional sales prices. The effect of increased tax assessment (which follows the house) on tax prices likely depends on the propensity of potential buyers to be subject to a wealth tax. Since most new home-owners finance their purchases with debt, the net-effect of a house purchase on your Taxable Net Wealth (TNW) is highly negative. This is because debt is deducted from TNW in its entirety, while the tax value of the house, on average, corresponds to around 25% of its market value. This causes new home buyers to generally have very low (negative) TNW. Any tax assessment premiums are therefore unlikely to affect these households' immediate wealth tax liabilities. I therefore do not expect the demand side to be highly sensitive to the tax assessments. Consistent with this, I find no effects on subsequent sales prices. The estimated point estimates from the preferred specifications in columns 1 and 3 are rather small at 0.035 and 0.016.

Since the confidence intervals in columns (1) through (3) are somewhat large, it makes sense to inquire whether the associated confidence intervals include a full capitalization effect. I evaluate this with a back-of-the-envelope calculation. If all potential buyers were well-above the wealth tax threshold, then a 10% increase in the tax assessment would increase yearly housing-induced wealth tax liabilities by around 10% times the average wealth tax rate of

0.0104. The NPV effect of this (over 30 years, discounted at 2%) would be 10%*0.0104*(1/0.02-1.02^{-30})=5.14%, which would be the upper bound for the magnitude of the potential capitalization effect. Using the estimates from column (1), I can rule out an effect outside of 10%*(0.035 ± 1.96*0.085)=[-1.32%, 2.02%]. Thus, the confidence interval contains 1.32/5.14=26% of the (back-of-the-envelope) potential full capitalization effect.

In Table 9, I also report estimated coefficients on the geographic trend controls. The purpose of this is to check whether the estimated coefficients on these variables correspond to my initial expectations. which were based on the motivating examples in the empirical specification section. My expectation was that the coefficients on the scaled border distance variable in column (1) would be 1, and the coefficient on the relative location variable in column (3) would be 0.5. The point-estimates are indeed very close to this, and statistically indistinguishable at the 5% level.

Table 9: Subsequent Transaction Outcomes

This table provides the effects of a one log-point increase in $\widehat{TaxVal}$ on transaction outcomes during 2010–2016. Columns (1)-(3) consider log transaction prices, and columns (4)-(6) examine extensive margin effects, in terms of a dummy which takes the value 1 if the house that the household lived in during 2009 was transacted during 2010–2016. Standard errors are provided in parenthesis, and are clustered on the census tract level.

	log(Transaction Price)			Sales Dummy		
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(\widehat{TaxVal})$	0.035	0.239***	0.016	0.002	0.004	0.002
	(0.085)	(0.084)	(0.072)	(0.003)	(0.003)	(0.002)
$\mathbb{1}[d_i < 0] * d_i^{scaled} * \Delta_i$	0.955***			0.005		
	(0.167)			(0.006)		
$\mathbb{1}[d_i > 0] * d_i^{scaled} * \Delta_i$	1.096***			0.004		
	(0.215)			(0.006)		
$\mathbb{1}[d_i < 0] * d_i^{KM} * \Delta_i$		0.050***			0.0003	
		(0.015)			(0.0004)	
$\mathbb{1}[d_i > 0] * d_i^{KM} * \Delta_i$		0.042***			-0.0002	
		(0.015)			(0.0004)	
RelativeLocation$_i$ * Δ_i			0.592***			0.003*
			(0.060)			(0.002)
N	44666	45422	50203	1553563	1540201	1740546
R^2	0.5381	0.5337	0.5257	0.1931	0.1972	0.1943
Household controls	Yes	Yes	Yes	Yes	Yes	Yes
Housing controls (Border spec.)	Yes	Yes	Yes	Yes	Yes	Yes
Geo-Controls						
– Scaled Border Distance	Yes	–	–	Yes	–	–
– KM Border Distance	–	Yes	–	–	Yes	–
– Relative Location	–	–	Yes	–	–	Yes

5.9 Regression-based Analysis of Pre-treatment Differences

5.9.1 Past transaction prices

In this subsection, I explore whether my empirical specifications are identifying discontinuities in past transaction prices. I show that my preferred specifications cannot reject the null of no discontinuity, while more naive approaches *do* identify such discontinuities. I also perform robustness by different subsets of past transactions.

Column (1) shows that model-implied tax assessment is highly correlated with past transaction prices if only the baseline housing characteristics (size, age, dummy for dense area) are controlled for. This specification uses both within-boundary and across boundary area variation in tax assessment. This correlation remains strong when controlling for these characteristics at the border area level in column (2), which only uses within-boundary area variation, but does not address geographic trends.

When addressing geographic heterogeneity by controlling for (unscaled) border distance in KM in column (3) these correlations are reduced considerably, but remain (largely) positive across subsamples. This is consistent with the visual evidence in Figure 2, where housing prices change non-linearly, thereby inviting the detection of a discontinuity in a linear specification.

Column (4) uses my main preferred geographic running variable, scaled border distance. This removes any statistically significant correlation between tax assessment and past transaction prices across subsamples. Column (5) estimates geographic slopes on border distance at the border area level. In 3 out of 4 subsamples I cannot reject the null of no correlation, but point estimates are consistently positive, and somewhat large. Column (6) corresponds to my second preferred specification, and also consistently keeps the null of no correlation between tax assessment and past transaction prices.

This exercise shows that scaled border distance and relative location perform well at keeping the null of no discontinuities, consistent with the visual evidence presented in Figure 2. The fact that estimates are slightly positive, at least in the first two samples, may be reflective of the fact that many of these transactions would have been present in the sample used to estimate the house price model coefficients. This is especially the case for the second sample, which restricts to transactions during 2004–2009.

Table 10: The Correlation Between Treatment and Observed Transaction Prices After Including Geographic Controls

This table reports estimated coefficients from a regressing log housing transaction prices on the log of model-implied tax assessment. Past transaction prices are obtained from the real-estate transaction register, covering all real-estate transactions during 1993–2017. The sample is limited to households who, in 2009, lived in a house that was transacted during 1993–2009. Panels (A)-(B) consider all households who owned a house during 2009; while Panels (A) and (C) restricts the sample to households in the main analysis sample. Panels (B) and (D) restricts the sample to transactions occurring during 2004–2009, the same sample period during the estimation of the house price model coefficients. Column (1) does not address geographic heterogeneity, and does not allow slopes on housing characteristics, H_i, to vary at the border area level. Column (2) allows slopes to vary at the border area level, but does not address within-border area geographic heterogeneity. Columns (3)-(6) address geographic heterogeneity according to the main reduced-form specification in equation 19. Column (4) corresponds to the preferred (scaled) border distance measure. Standard errors are provided in parenthesis, and are clustered on the census tract level. Sample sizes are provided in brackets.

log($Transaction\ Price$)	(1)	(2)	(3)	**(4)**	(5)	(6)
			(A) All homeowners, 2000–2009			
log($\widehat{TaxVal}$)	1.273***	0.769***	0.293***	0.086	0.148	0.148
	(0.017)	(0.051)	(0.108)	(0.102)	(0.091)	(0.094)
	[206712]	[206280]	[206280]	[199841]	[199841]	[199841]
			(B) All homeowners, 2004–2009			
log($\widehat{TaxVal}$)	1.258***	0.800***	0.422***	0.115	0.171*	0.144
	(0.020)	(0.062)	(0.121)	(0.121)	(0.102)	(0.110)
	[134304]	[133843]	[133843]	[129608]	[129608]	[129608]
			(C) Analysis sample, 2000–2009			
log($\widehat{TaxVal}$)	1.457***	0.773***	-0.067	-0.061	0.252	-0.109
	(0.040)	(0.110)	(0.244)	(0.224)	(0.202)	(0.211)
	[40211]	[39649]	[39649]	[38639]	[38639]	[38639]
			(D) Analysis sample, 2004–2009			
log($\widehat{TaxVal}$)	1.490***	0.946***	0.265	-0.086	0.332	-0.420
	(0.071)	(0.172)	(0.332)	(0.333)	(0.322)	(0.322)
	[18021]	[17429]	[17429]	[17035]	[17035]	[17035]
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM, KM^2	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

5.9.2 Pre-period Income, Wealth, Debt and Educational Attainment

In this section, I examine whether tax assessment discontinuities correlate with other pre-period household observables under my different specifications. The specifications in columns (1)-(2) do not address geographic heterogeneity and find a strong correlation between model-implied tax assessment and household observables. These correlations are reduced significantly in column (3), which uses border distance in kilometers as the running variable, but most correlations are still statistically different from zero. This occurs despite allowing the continuous loading on the assessment premium to be non-linear by including second-order polynomial terms in d_i^{KM} interacted with Δ_i.

My preferred specifications (4) and (6) show very small correlations. They are also fairly precise in the sense that the standard errors are an order of magnitude smaller than the point estimates in the baseline specifications (columns (1)–(2)). Column (5), which estimates border-specific slopes on border distance also performs reasonably well, except for a marginally significant (at the 10% level) correlation with having a college degree.

To illustrate the success of my approach in removing the correlation between tax treatment and household characteristics, consider the correlation between $\log(\widehat{TaxVal})$ and log(Labor Income). Column (1) shows that, when not addressing geographic heterogeneity at all, a one log-point increase in tax assessment is associated with 28.8% higher labor incomes. Addressing geographical confounders using my main preferred specification in column (4), I find that this correlation is reduced to 0, and I can rule out a correlation larger (in magnitude) than 5.5 percentage points, at the 5% level.

Table 11: The Correlation Between Treatment and Household Characteristics after Including Geographic Controls

This table reports the correlation between model-implied tax assessment, $\log(\widehat{TaxVal})$ and 2009 socioeconomic characteristics: Income, Gross Financial Wealth (GFW), Debt and education. College is a dummy equal to one if one of the household members (excluding children) have a college degree. Columns (1)-(4) add different sets of controls. Column (1) includes the baseline controls: structure-type specific slopes on log(size), the dense population dummy and age bracket indicators. Column (2) interacts the baseline controls with border area fixed effects. Column (3) further includes a control for the distance to border within a border area, estimated separately for each side, and interacted with Δ_i (border area and structure-type specific log(difference) in average assessed house prices between the sides of the border). Column (4) Includes the relative location control, also interacted with Δ_i. These two variables are defined in detail in the text. Standard errors are provided in parenthesis, and are clustered on the census tract level.

	(1)	(2)	(3)	**(4)**	(5)	(6)
	log(Labor Income)					
$\log(\widehat{TaxVal})$	0.288***	0.153***	0.067*	-0.000	-0.039	0.002
	(0.005)	(0.013)	(0.040)	(0.028)	(0.034)	(0.023)
	log(Gross Financial Wealth)					
$\log(\widehat{TaxVal})$	0.629***	0.409***	0.255***	-0.030	0.110	0.002
	(0.012)	(0.035)	(0.095)	(0.070)	(0.086)	(0.064)
	Stock Market Share					
$\log(\widehat{TaxVal})$	0.045***	0.028***	0.053***	0.000	0.010	0.002
	(0.002)	(0.005)	(0.014)	(0.010)	(0.013)	(0.009)
	log(Debt)					
$\log(\widehat{TaxVal})$	0.502***	0.350***	0.180	0.092	-0.009	0.097
	(0.015)	(0.046)	(0.143)	(0.099)	(0.125)	(0.082)
	College					
$\log(\widehat{TaxVal})$	0.239***	0.194***	0.159***	0.019	0.056*	0.028
	(0.005)	(0.015)	(0.040)	(0.028)	(0.033)	(0.024)
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM, KM^2	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

6 Bunching

In this section, I examine the extent towhich households bunch around the wealth tax threshold. The primary reason for this is to investigate the likely extent of evasion or avoidance opportunities. If these are abundant and not costly, I would expect to see sizable bunching (excess mass) around the wealth tax threshold. If this is indeed the case, one may worry that households can partially "untreat" themselves through these means. For example, if it is not costly for households to lower their wealth tax burden by moving financial assets that are observed by tax authorities into harder to tax assets, such as cash, the response that I observe in terms of growth in financial wealth may be a lower bound of the actual effect on savings.[37] This is not a concern for the qualitative conclusions of this paper, but it is nevertheless useful to evaluate since sizable bunching would suggest that the true saving responses may even higher.

I show my results in Figure 11 below. Panel (A) shows the results for my full analysis sample, and Panel (B) shows results for the full sample of Norwegian tax payers. I follow the approach from Chetty, Friedman, Olsen, and Pistaferri (2011) to perform the analysis.

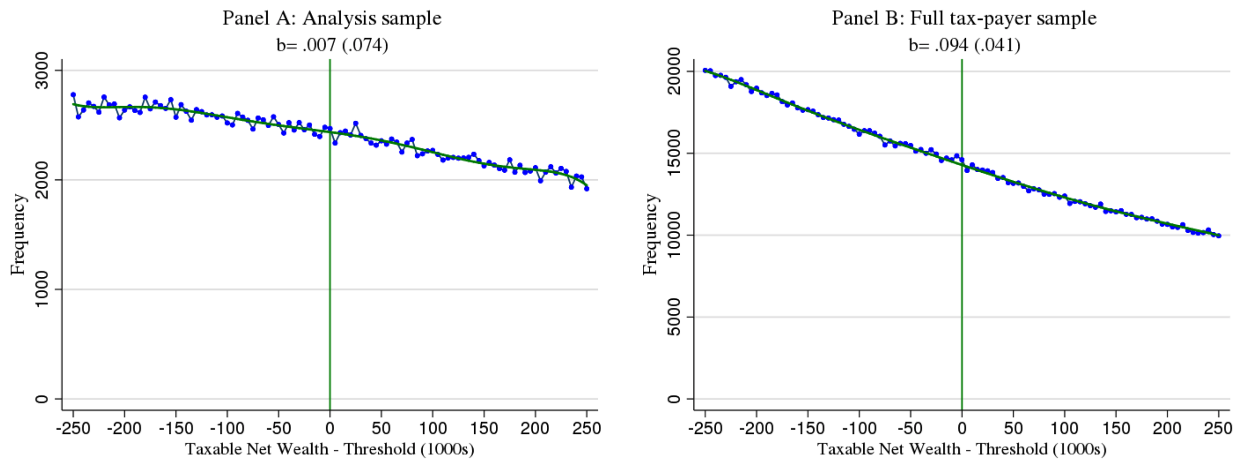
The visual evidence is quite clear in that there is no sizable bunching around the wealth tax threshold in either sample. Panel (B) estimates a statistically significant excess mass around the threshold, but the visual evidence is not very supportive. In panel (B), the excess mass, $b = 0.097$. This says, that there is 9.7% extra mass in the NOK 5,000 bin to the left of the kink. This number is calculated using the methodology from Chetty, Friedman, Olsen, and Pistaferri (2011), and the assumptions closely resemble those made in Seim (2017). First, a counterfactual distribution is calculated, by fitting a 7th order polynomial to all points bins outside $[-40k, 15k]$. Then the relative number of bunchers, $N(\%)$ is calculated as the relative difference between the number of agents in the empirical and counterfactual distributions within $[-40k, 15k]$. Then, all the bunchers are assumed to be bunching one bin to the left of the threshold. Multiplying $N(\%)$ by the number of NOK 5,000 bins in $[-40k, 15k]$, then yields b.

Multiplying b by NOK 5,000, tells us that $5,000b$ less TNW is being reported due to the wealth tax threshold. In relative terms, given an average threshold during this period of NOK 830,000, and $b = 0.097$, this implies that 0.05843% of TNW is being misreported. Since the average wealth tax rate during this sample period (2011–2014) was 1.075%, this yields a net-of-tax rate elasticity of taxable wealth (following the definition in Seim (2017)) of $0.05853\%/(0.01075/(1 - 0.01075)) = 0.054$. When translated to an elasticity with respect to a net-of-tax rate-of-return of 2%, the elasticity becomes $0.05853\%/(0.01075/(2\%)) = 0.0010$

[37] I do expect that withdrawing deposits to store as cash is quite costly for households. Even though it could reduce wealth taxes by 1 cent per dollar, it would also preclude them from any interest earnings of (on average) 2 cents, leading to a net loss.

Figure 11: Distribution of Households around the Wealth Tax Threshold

These figures show the distribution of taxable net wealth around the wealth tax threshold. Households are divided into NOK 5,000 bins, and households at zero have $[0, 5000)$ NOK in excess of the threshold. Panel (A) considers the full analysis sample, where thresholds are multiplied by 2 for married couples, and only couples with a non-changed marital status are included. Panel (B) considers the full sample of Norwegian tax-payers, where the analysis is done at the individual level. Plots and estimates are produced using the .ado file provided by Chetty, Friedman, Olsen, and Pistaferri (2011). The counterfactual distributions (green line) is constructed by fitting a 7 degree polynomial on all bins outside [-40,000, 15,000]. b is the estimated excess mass in this excluded range, normalized to be in the bin directly to the left of the threshold. Bootstrapped standard errors are in parenthesis. The analyses use pooled data for 2011–2014. Sample period is restricted due to limited sample years in the dataset covering the universe of tax-payers.



7 Implied Structural Parameters

In this section, I use a simple life-cycle model to explore what value of the EIS is most consistent with my empirical findings. The model environment is simple. It only contains the key elements necessary to replicate my empirical results and the shock to wealth tax exposure. Agents choose both how much to save and how much to work and they're shocked by more aggressive wealth taxation in a way where the effect on the marginal and average net-of-tax rates-of-return may differ.

7.1 A simple life-cycle model

Consider the following life-cycle model with perfect foresight. The model features a constant Elasticity of Intertemporal Substitution (EIS), $\frac{1}{\gamma}$, and a constant Frisch elasticity of labor supply, $\frac{1}{\nu}$.

$$\begin{aligned}
\max_{\{c_t, s_{t+1}\}_{t=0}^{T}} \quad & \textstyle\sum_{t=0}^{T} \quad \beta^t \left(\frac{1}{1-\gamma} c_t^{1-\gamma} - \psi \frac{l_t^{1+\nu}}{1+\nu} \right) && (25) \\
\text{s.t. } c_t + s_{t+1} \quad & = \quad y_t + l_t w_t && (26) \\
& + \quad s_t R(1 - \tau \mathbb{1}[s_t R + A - \bar{w} > 0]) + (A - \bar{w}) \tau \mathbb{1}[s_t R + A - \bar{w} > 0]
\end{aligned}$$

ψ is the (dis)utility weight on labor supply, and β is the discount factor. The endogenous variables are c_t, l_t, s_{t+1} for $t \geq 0$. Unearned income (pensions), y_t and initial wealth, s_0, are exogenous. $\bar{w}$ is the threshold applicable to all taxable wealth, savings with returns ($s_t R$) plus assessed housing wealth, A.

Define $\tilde{R}_t = R(1-\tau\mathbb{1}[s_t R + A - \bar{w} > 0])$ and $\tilde{V}_t = s^*\tau\mathbb{1}[s_t R + A - \bar{w} > 0]$. Budget constraint can be written as

$$c_t \quad = \quad y_t + l_t w_t + s_t \tilde{R}_t + \tilde{V}_t - s_{t+1} \tag{27}$$

I assume that households respond to changes in $\tilde{R}_t$ and $\tilde{V}_t$. I shut off the feedback mechanism of how changes in s_t can affect $\tilde{R}_t$ and $\tilde{V}_t$. This eliminates bunching around the tax threshold, and allows me to solve the model using the (binding) first order conditions, and the life-time budget constraint.

I do not model precautionary savings motives or bequests directly. Instead I assume that households live until they are 100 years old. This ensures that households do not dissave too quickly, and therefore still hold meaningful savings around the average (empirical) age of death in Norway, which is around 85 old.[38]

7.2 Simulation and calibration

I assume that households act according to the model in the previous section, and simulate their responses to shocks to $\tilde{R}_t$ and $\tilde{V}_t$, for $t \geq 1$, from a baseline $\tilde{R}_t = 1.02$ and $\tilde{V}_t = 0$. I simulate the responses in terms of their saving behavior and labor supply for EIS-Frisch combinations, $(\frac{1}{\gamma}, \frac{1}{\nu})$. The (dis)utility weight on labor supply, ψ is calibrated to ensure that simulated labor earnings at $t = 0$ equal observed after-tax labor earnings in 2009 and the consumption share of total incomes (labor earnings plus exogenous income) equals 80%.[39] I set $\beta = 0.98$.

Similar to Jakobsen et al. (2019), I model the responses of a representative agent. This agent sees shocks to $\tilde{R}_t$ and $\tilde{V}_t$ corresponding to those found for the full sample in Table 1, assuming a shock to $\log(\widehat{TaxVal}) = 0.5$. This implies $\Delta\tilde{R}_t = 0.13375$ p. p. and $\Delta\tilde{V}_t = s_t(\Delta\tilde{R}_t^{avg} - \Delta\tilde{R}_t) = s_t(-0.000772 - (-0.0013375))$ for $t \geq 1$, where the s_t used to determine $\Delta\tilde{V}_t$ is the s_t chosen absent the shock.

Savings, s_t, in my model corresponds to Gross Financial Wealth (GFW). Labor earnings

[38] Absent any mortality risk, this corresponds to (1) assuming that the bequest elasticity equals the EIS, and (2) that the strength of the (warm-glow) bequest motive ensures that households wish to bequest an amount large enough to finance their own planned consumption for 15 years given a continued flow of exogenous income.

[39] Choosing a consumption share of 80% ensures that agents choose labor supply is close to the empirical average in the sample. Setting it to 100% for example leads to very large (unshocked) labor supply in order to save enough to finance a higher level of consumption.

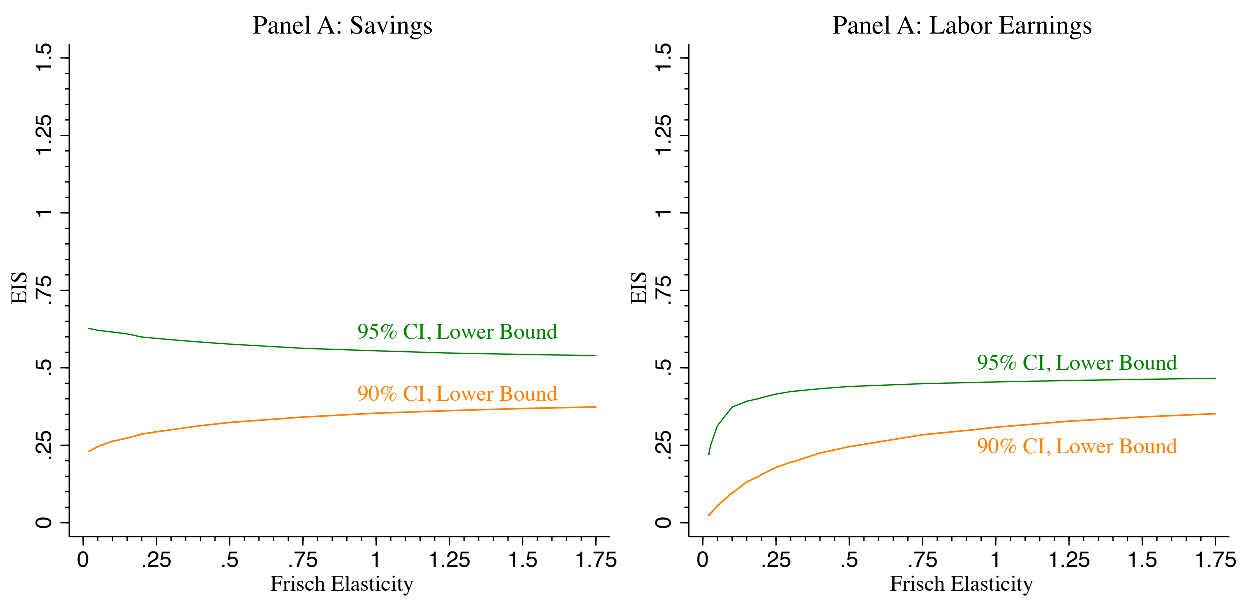
corresponds to salary and self-employment income. I set exogenous income, y_t, equal to the difference between total taxable income and labor earnings in 2009, whenever the agent's age is strictly below retirement age. This difference corresponds approximately to the average amount of pension income. Starting at retirement age, exogenous income (pension income) increases by 50% of the after-tax 2009 average of labor earnings. I induce retirement by making wages drop to zero over a 5 year period that starts at retirement age.

7.3 Simulated treatment effects

The benchmark empirical treatment effect on savings corresponds to a 0.5 log-point increase in tax assessment. My empirical estimates on savings growth (GFW) imply a yearly effect of 0.5*0.0149 percentage points each year, when not adjusting for the mechanical effects of increased taxation in Table 3. When cumulated over a 5 year period, this implies an effect of 5*0.5*0.0149= 0.0373 (SE=5*0.5*0.0091=0.0228). The lower bound of the 95% confidence interval is -0.0074.

Figure 12: The Frisch-EIS elasticities that provide simulated treatment effects inside the empirical confidence intervals

This figure shows the EIS and Frisch elasticity combinations that yield simulated cumulative treatment effects on savings (or labor earnings) over a five year period that correspond to the lower bound of the 95 percent confidence interval of my empirical findings. The baseline shock corresponds to a 0.5 log-point increase in tax assessment. EIS–Frisch combinations above the lines yield simulated treatment effects *below* the confidence intervals of my empirical findings.

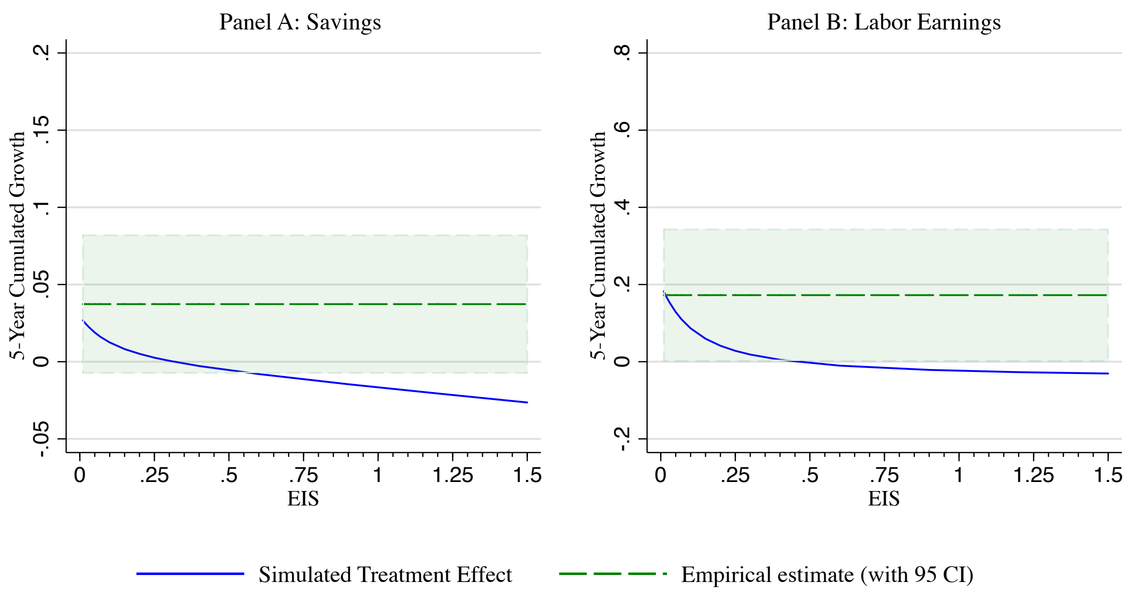


The benchmark empirical treatment effect for labor earnings is the following. I found that a 0.5 log-point increase in labor earnings increases labor earnings (salaries and self-employment income) by 0.5*0.0230 percentage points each year. The cumulative effect of this in terms of total earnings is 0.5*0.0230* (5+4+3+2+1), since the first year effect cumulates over 5 years, and so on. This gives a cumulative effect of 0.1725 (SE=0.5*0.0116* (5+4+3+2+1)=0.087). In other words, over 5 years, treated households earned 17.25% more than untreated households. The lower bound of the 95% confidence interval is 0.002.

In Figure 12 I plot the Frisch-EIS combinations (with the disutility weight on labor supply is calibrated) that provide treatment effects corresponding to the lower bound of the 95% confidence intervals of the cumulative treatment effects in green. The orange line similarly represents the lower part of the 90% confidence intervals. . Any EIS-Frisch combinations *above* these lines yield simulated treatment effects below the respective confidence intervals. In this figure, we see that either the labor earnings effect or the savings effect can be used to pin down similar bounds for the EIS, which only depend weakly on the Frisch elasticity.

Figure 13: Simulated Treatment Effects as a function of the EIS

This figure shows the simulated cumulative treatment effect (blue line) as a function of the EIS, assuming a Frisch elasticity o 0.5. The baseline shock corresponds to a 0.5 log-point increase in tax assessment. The green dashed the empirical point estimate, while the green shaded area constitutes the 95% confidence interval. The first column considers the effect on savings growth, while the second column considers the effect on earnings growth.



In Figure 13, I plot the simulated cumulative treatment effects as a function of the EIS, assuming a Frisch Elasticity of 0.5. It's clear from the figure that is difficult to parameterize the simple life-cycle model that I use in a way that exactly replicates my point estimates. This feature is not particularly robust to the assumptions of the model. For example, if we force the agent to pay a large bequest at death, increase β, or do other things that make the income effect larger, the blue line will shift slightly upwards. Similarly, we can make the blue line shift downwards by lowering the income effect, for example, by shortening the life-span of the agent.

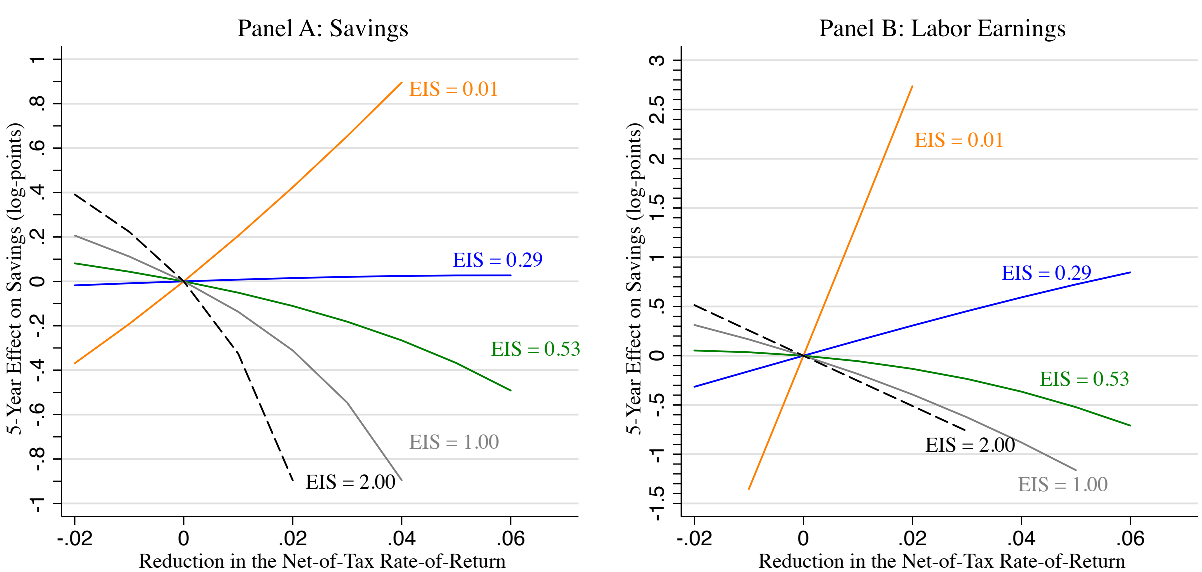
This section shows that in a simple life-cycle model, we need a fairly small value of the EIS to rationalize the empirical findings. We could also replicate the low-EIS behavior by exogenously imposing harsh consumption adjustment frictions, e.g., that $c_t \geq c_0$ for $t \geq 1$. In such an environment, the responses to wealth taxation becomes uninformative of the EIS.

7.4 Implied uncompensated elasticity of savings to the net-of-tax rate-of-return

In Figure 14, I simulate the effects of a reduction in the (net-of-tax) rate-of-return for different values of the EIS. The blue and green lines correspond to the the EIS that produced simulated treatment effects in Figure 13 corresponding to the lower bound of the confidence intervals of my empirical findings at the 90% and 95% levels, respectively.[40]

Figure 14: Simulated Saving Responses to Reducing the Net-of-tax Rate-of-return

This figure shows the simulated 5-year effect (in log points) of reducing the net-of-tax rate-of-return for different values of the EIS, assuming a Frisch elasticity of 0.5. Given a gross rate-of-return of 1.02 (assumed in the model), we can find the implied *uncompensated* savings elasticities to the net-of-tax rate-of-return by reading off (the negative of) the value on the y-axis for a given line when the x-axis takes a value of 0.02. In Panel A (Panel B) these values are 0.42, 0.01, -0.11, -0.31, and -0.90 (2.74, 0.31, -0.13, -0.40, and -0.51) for an EIS of 0.01, 0.29, 0.53, 1.00, 2.00, respectively.



I also plot the treatment effects for two other values of the EIS. The solid line assumes an EIS of 1, corresponding to log-utility in consumption. The dashed line corresponds to one of

[40]The EIS cut-offs are slightly different for whether we look at the savings results or labor earnings results, I therefore take the average EIS for a given confidence level.

the lower values of the EIS needed to calibrate the model in Jakobsen et al. (2019) to their empirical estimates. Given an average gross rate-of-return of 1.02 (assumed in the model), we can find the implied *uncompensated* elasticities to the net-of-tax rate-of-return by reading off (the negative of) the value on the y-axis for a given line when the x-axis takes a value of 0.02. From Panel A, we see that the implicit uncompensated five-year savings elasticity consistent with our empirical findings are below 0.10 and 0.02 at the 95% and 90% level, which can be read off of the green and blue lines, respectively. The implied elasticity when the EIS equals zero, which is the EIS that comes closest to explaining my results, is -0.42.

Figure 14 also highlights the strikingly different responses to, for example, tax-induced, rate-of-return shocks contained within the set of commonly used values of the EIS. A 2 percentage point reduction in the rate-of-return leads to a dissaving of 59% (=exp(-0.9)-1) if the EIS is 2, but an increase in savings of almost 50% (=exp(0.4)-1) if the EIS is zero. In the middle, we have an EIS of around 0.3, that gives barely any response at all. Research into the saving responses to capital taxation, and inferring the underlying parameters that drive them, therefore seems particularly prudent to inform tax policy.

Finally, it is worth noting that there are potential frictions that I do not model, which could lead agents to act as if they had a lower EIS. One example is the inability to adjust consumption downwards (Chetty and Szeidl, 2007).

8 Discussion

In this paper, I address an important and long-standing question in economics, namely how household savings and labor income respond to capital taxation.

Despite the importance of this question in terms of how it may inform a range of economic models, and in particular tax policy, there exists very little empirical evidence that is applicable to these models. This is in part due to a lack of exogenous identifying variation in the rate-of-return and capital taxation, but also due to the difficulty of isolating real responses from evasion and avoidance effects. By utilizing a novel source of identifying variation in wealth tax exposure in an empirical setting where responses are unlikely to be driven by evasion, I make an important contribution to this literature. An additional contribution lies in the novel examination of theoretically important margins of adjustment, such as labor earnings and portfolio allocation.

My results indicate that the distortionary effects of capital taxation may go in the opposite direction of what is typically assumed.[41] In addition, capital taxation may encourage households to supply more labor. This is important for policy-makers to consider when considering the

[41] Saez and Stantcheva (2018) consider feasible elasticities of capital to the net-of-tax rate-of-return to be 0.25, 0.5, and 1.

optimal mix of capital and labor income taxation. My findings suggest that capital taxation may offset some of the distortionary (tax revenue reducing) effects of labor income taxation on labor income. However, it is important to note, that my findings focus on distortionary effects arising in partial equilibrium in the household sector. Wealth taxation, and capital taxation in general, may have potentially adverse general equilibrium effects or effects operating through the corporate sector that are not considered in this paper.

My results on the savings effects of wealth taxation are qualitative different (and even of a different sign) from the existing empirical literature. The likely explanation is that my empirical setting, with largely third-party reported measures of savings, comes closer to estimating savings effects rather than strategic tax responses. Elasticities estimated elsewhere in the literature likely include evasion or avoidance responses, and will thus be larger (and may even be of a different sign) than pure savings elasticities. In Denmark for example, only households in the top 1% to 2% of the wealth distribution paid a wealth tax. Around half of these households are business owners with potentially sizable evasion opportunities since business wealth is self-reported. In Switzerland, financial wealth is completely self-reported by tax-payers.

I know of no obvious reason why the finding of a positive as opposed to a negative effect of wealth taxes on saving is would be driven by characteristics specific to Norway. If anything, the presence of more generous pension and social insurance programs should create an economic environment where savings motives, and thus income effects, would be weaker in Norway, and more easily dominated by the substitution effects associated with rate-of-return shocks.

At face value, the finding of a *positive* effect on savings is somewhat surprising. However, as I showed in the previous section, non-negative saving responses to a negative rate-of-return shock can be generated by plausible parameterizations of a life-cycle model. For example, The estimate of 0.1 in (Best et al., 2018) would, in the model calibrated to my empirical setting, produce simulated saving responses that take a positive sign. A value for the EIS of 0.1 is also contained in the confidence bounds around the empirical estimates of the EIS for stockholders in Vissing-Jørgensen (2002). This further highlights the possibility of counter-intuitively positively signed responses to adverse rate-of-return shocks.

Finally, as discussed in the introduction, my findings strengthen the premise upon which the recent macro-heterogeneity literature is built. In particular, my findings point towards a larger role for the partial-equilibrium mechanism in Auclert (2019) and the general-equilibrium mechanisms in Kaplan, Moll, and Violante (2018) in explaining aggregate responses to monetary policy. In addition, my results are driven by older, wealthier households, which suggests that these households may respond in the opposite way of that of a representative agent, highlighting the need to study the behavior of younger, constrained households, as in Wong (2019), where the mortgage refinancing channel is important.

References

Annette Alstadsæter, Niels Johannesen, and Gabriel Zucman. Tax evasion and inequality. *American Economic Review*, 109(6):2073–2103, 2019.

Anthony B Atkinson and Agnar Sandmo. Welfare implications of the taxation of savings. *The Economic Journal*, 90(359):529–549, 1980.

Adrien Auclert. Monetary policy and the redistribution channel. *American Economic Review*, 109(6):2333–67, 2019.

Spencer Bastani and Daniel Waldenström. How should capital be taxed? theory and evidence from Sweden. *Working Paper*, 2018.

Patrick Bayer, Fernando Ferreira, and Robert McMillan. A unified framework for measuring preferences for schools and neighborhoods. *Journal of Political Economy*, 115(4):588–638, 2007.

Michael Carlos Best, James Cloyne, Ethan Ilzetzki, and Henrik Kleven. Estimating the elasticity of intertemporal substitution using mortgage notches. Technical report, National Bureau of Economic Research, 2018.

Martin Beznoska and Richard Ochmann. The interest elasticity of household savings: a structural approach with german micro data. *Empirical Economics*, 45(1):371–399, 2013.

Sandra E Black. Do better schools matter? parental valuation of elementary education. *The Quarterly Journal of Economics*, 114(2):577–599, 1999.

Yosef Bonaparte and Frank Fabozzi. Estimating the elasticity of intertemporal substitution accounting for stockholder-specific portfolios. *Applied Economic Letters*, 24:923–927, 2017.

Michael J Boskin. Taxation, saving, and the rate of interest. *Journal of Political Economy*, 86 (2, Part 2):S3–S27, 1978.

Marius Brülhart, Jonathan Gruber, Matthias Krapf, and Kurt Schmidheiny. Behavioral responses to wealth taxes: Evidence from Switzerland. 2019.

Christophe Chamley. Optimal taxation of capital income in general equilibrium with infinite lives. *Econometrica: Journal of the Econometric Society*, pages 607–622, 1986.

Raj Chetty and Adam Szeidl. Consumption commitments and risk preferences. *The Quarterly Journal of Economics*, 122(2):831–877, 2007.

Raj Chetty, John N Friedman, Tore Olsen, and Luigi Pistaferri. Adjustment costs, firm responses, and micro vs. macro labor supply elasticities: Evidence from Danish tax records. *The Quarterly Journal of Economics*, 126(2):749–804, 2011.

Melissa Dell. The persistent effects of peru's mining mita. *Econometrica*, 78(6):1863–1903, 2010.

Andreas Fagereng, Luigi Guiso, Davide Malacrino, and Luigi Pistaferri. Heterogeneity and persistence in returns to wealth. Technical report, CESifo Working Paper 7101, 2018.

Jonathan Gruber. A tax-based estimate of the elasticity of intertemporal substitution. *Quarterly Journal of Finance*, 3(1), 2013.

Fatih Guvenen, Gueorgui Kambourov, Burhan Kuruscu, Sergio Ocampo, and Daphne Chen. Use it or lose it: Efficiency gains from wealth taxation. *Working Paper*, 2019.

Katrine Jakobsen, Kristian Jakobsen, Henrik Kleven, and Gabriel Zucman. Wealth taxation and wealth accumulation: Theory and evidence from denmark. Technical report, National Bureau of Economic Research, 2019.

Kenneth L Judd. Redistributive taxation in a simple perfect foresight model. *Journal of Public Economics*, 28(1):59–83, 1985.

Greg Kaplan, Benjamin Moll, and Giovanni L Violante. Monetary policy according to hank. *American Economic Review*, 108(3):697–743, 2018.

Juliana Londoño-Vélez and Javier Ávila-Mahecha. Can wealth taxation work in developing countries? quasi-experimental evidence from Colombia. *Working Paper*, 2018.

José; María Durán-Cabré, Alejandro; Esteller-Moré, and Mariona Mas-Montserrat. Behavioural responses to the (re)introduction of wealth taxes. evidence from Spain. *IEB Working Paper 2019/04*, 2019.

Emmanuel Saez and Stefanie Stantcheva. A simpler theory of optimal capital taxation. *Journal of Public Economics*, 162:120–142, 2018.

Emmanuel Saez and Gabriel Zucman. Progressive wealth taxation. *Brookings Papers on Economic Activity*, 2019.

Lawrence Schmidt and Alexis Akira Toda. Bad news and robust comparative statics for the elasticity of intertemporal substitution. *Working Paper*, 2019.

David Seim. Behavioral responses to wealth taxes: Evidence from Sweden. *American Economic Journal: Economic Policy*, 9(4):395–421, 2017.

Ludwig Straub and Iván Werning. Positive long run capital taxation: Chamley-judd revisited. Technical report, National Bureau of Economic Research, 2019.

Annette Vissing-Jørgensen. Limited asset market participation and the elasticity of intertemporal substitution. *Journal of Political Economy*, 110(4):825–853, 2002.

Arlene Wong. Refinancing and the transmission of monetary policy to consumption. *Working Paper*, May 2019.

Floris T. Zoutman. The elasticity of taxable wealth: Evidence from the Netherlands. *Working Paper*, 2018.

A Empirical Appendix

A.1 Defining the geographic running variable

My setting includes many border areas that differ significantly in terms of residential density. While neighbors may be kilometers away in the arctic northern parts of Norway, they may only be meters away in rural Oslo. This is problematic when pooling boundary areas in order to obtain precision, because for a fixed differential, Δ_i, house prices must change more rapidly whenever the border area is smaller. When pooling boundary areas, by construction, households closer to the border (in kilometers) will be drawn from smaller (denser) areas,[42] where the slope of house prices will be steeper. I provide a graphical example of the issue in Figure A.6 in the Appendix. This example shows that, despite geographically smooth – even linear – house prices within a border area, a pooled regression may easily detect discontinuities due to strong non-linearities arising.

Below I describe a simple motivating example where house prices move linearly within border areas, and the geographic slope varies only with two key characteristics, namely the difference in average house prices (Δ) and residential density.

Fix a boundary area, b, populated by households, i. Assume that true house prices, p, move linearly along some geographic measure, k: $p_i = p(k(\mathbf{c}_i)) = \xi_b k_i = \xi_b k(\mathbf{c}_i)$. We can think of k as border distance in kilometers. There are two sides, $S = L, H$. Then assume that $\mathbb{E}[k(c_i)|i \in S] = k(\mathbb{E}[c_i|i \in S])$ (a linearity assumption)[43]. Then the mean price in S equals the price at $k()$ valued at the centroid of S: $\mathbb{E}[p_i|i \in S] = p\Big(k\Big(\mathbb{E}[\mathbf{c}_i|i \in S]\Big)\Big)$, since p is linear in k. Define the coordinate centroid of side S as: $\mathbf{c}_S = \mathbb{E}[\mathbf{c}_i|i \in S]$. Applying the formula for a line, given two points, we get that the slope of p on $k(\mathbf{c}_i)$ is: $(p(k(\mathbf{c}_H)) - p(k(\mathbf{c}_L)))/(k(\mathbf{c}_H) - k(\mathbf{c}_L))$.

Define Δ_b as the difference in mean house prices: $\Delta_b = \mathbb{E}[p_i|i \in H] - \mathbb{E}[p_i|i \in L]$, and the centroid distance, $CD_b = k(\mathbf{c}_H) - k(\mathbf{c}_L)$, and we can write the slope of prices, p, on our geographic measure, k, as Δ_b/CD_b

$$p_i = \frac{k_i}{CD_b}\Delta_b$$

This example contains the two key elements: (1) house prices have larger geographic gradients when the differences in averages are higher; and (2) more dense (less scattered) areas have larger geographic gradients. I illustrate this in Figure A.1. The boundary region in (A) and (B) differ only in that the average prices in (A) are 1 price unit higher in (A). Boundary regions (B) and (C) differ only in that (C) is spread out geographically (all g_is in C are twice

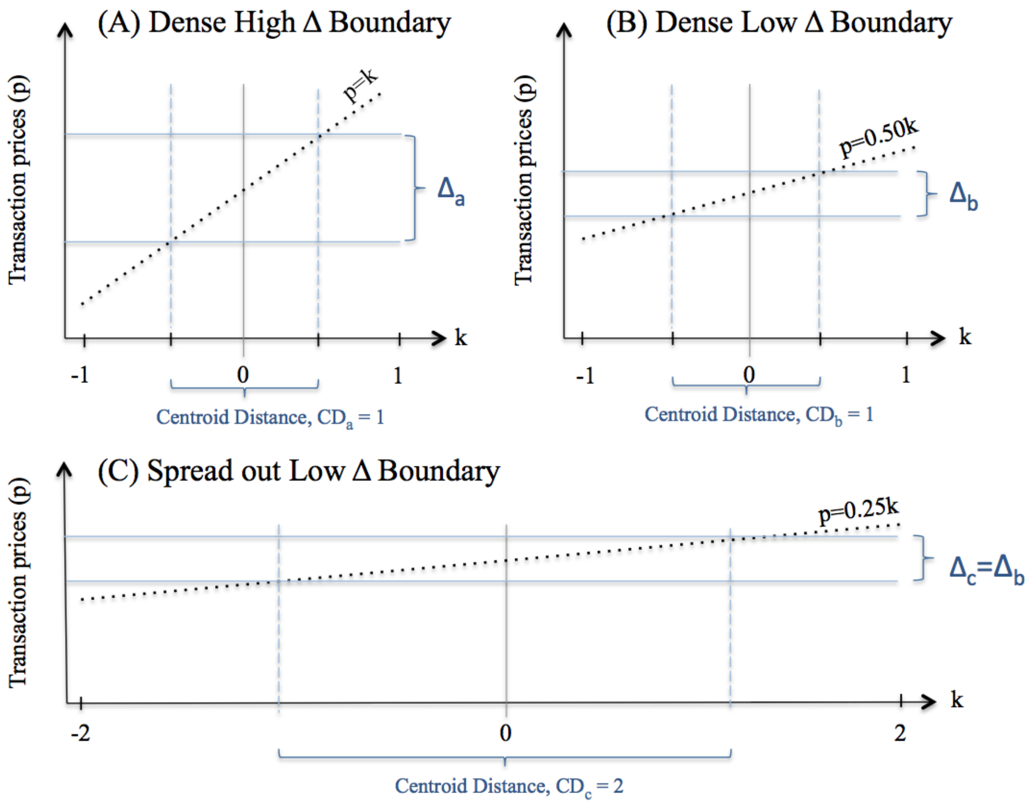
[42] In Panel A of Figure A.2, I show that households located near the boundary (in a kilometer sense) live in much denser areas than those further away.

[43] In reality, this is more of an approximation, as coordinates generally will not map linearly into border distance.

that in B). In all three cases, the slope of prices, p, on our geographic measure, k, is simply Δ_b/CD_b. Below I outline four approaches based on this example.

Figure A.1: Border Area Heterogeneity: Motivating Example

This figure provides some simple examples to motivate my empirical specifications. I plot house prices (dotted lines) against a geographic measure (e.g., border distance) for three hypothetical border areas. The geographic slope of house prices is linear within each border area. Panel (A) and (B) differ only in that the difference in average house prices between each side of the boundary is higher in (A) than in (B). Panel (B) and (C) differ only in that (C) is more spread out, while the differences in averages is still the same. The commonality between all border areas, b, is that the slope of house prices on the geographic measure is Δ_b/CD_b, where CD_b is the distance between the centroids of the two sides of a given boundary area, b.



Approach 1 (Benchmark: Border Distance in km). This approach uses signed border distance, d_i, as the relevant within-boundary area geographic measure: $k(\mathbf{c}_i) = d_i$. It ignores heterogeneity in residential density by assuming a (normalized) centroid distance, $CD_b = 1$. This invites the problem of non-linear slopes on d_i, that are potentially very steep near boundaries in a pooled regression. This can be visualized by envisioning the slope of house prices on g, when pooling border areas (B) and (C). I provide an example of what this might look like when pooling multiple border areas in Figure A.6. This issue also becomes apparent in the results section. Despite this, it serves as a useful benchmark for the other approaches.

$$\text{(Unscaled) Border Distance term: } g_b(\mathbf{c}_i) = \gamma \cdot d_i \tag{28}$$

Approach 2 (Scaled Border Distance). This approach also uses signed border distance, d_i, but incorporates the heterogeneity in density, by scaling the measure by $CD_b = Dist(\mathbf{c}_{b,H}, \mathbf{c}_{b,L})$. The following term then captures the within-border area geographic variation in house prices, where the expectation is that $\hat{\gamma} = 1$.

$$\text{Scaled Border Distance term: } g_b(\mathbf{c}_i) = \gamma \cdot \frac{d_i}{CD_b} \tag{29}$$

Approach 3 (Relative Location). I set $k()$ equal to the differential distance to the centroids of the L versus H side of the boundary. This provides, in meters, how much closer c_i is to $\mathbf{c}_H$ than $\mathbf{c}_L$: $k(c_i) = \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,L}) - \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,H})$. In this setting, $k(\mathbf{c}_H) - k(\mathbf{c}_L) = 2 \cdot \text{Dist}(\mathbf{c}_{b,H}, \mathbf{c}_{b,L})$. I omit this scaling by 2, which leads to the expectation that $\hat{\gamma} = \frac{1}{2}$.

$$\text{Relative Location term: } g_b(\mathbf{c}_i) = \gamma \cdot \frac{\text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,L}) - \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,H})}{\text{Dist}(\mathbf{c}_{b,H}, \mathbf{c}_{b,L})} \in [-\gamma, \gamma] \tag{30}$$

The Relative Location variable is novel in the BDD setting. It is based on the hypothesis that the true house price for some sampled house within a boundary area is a weighted average of estimated average house prices on each side of a boundary, where weights are assigned based on how much closer (or less far away) a house is located to the centroids of the estimation samples on the two sides.[44]

Approach 4 (Border-specific slopes). Finally, I set $k()$ equal to signed border distance, $d(\mathbf{c}_i) = d_i$ where households on the low-assessment side receive $k < 0$. I estimate slopes separately for each border area, and thus do not scale by CD_b, since this does not vary within a border area.

$$\text{Border-specific Border Distance: } g_b(\mathbf{c}_i) = \gamma_b \cdot d_i \tag{31}$$

While the motivating example does not contain side-specific slopes, I follow the standard approach in the RDD literature and allow slopes γ, to be estimated separately for $d_i < 0$ and $d_i > 0$ in approaches 1, 2, and 4.

In the specifications using border distance, there is the concern that treated units on one side of the border may indeed be very far away from any control units on the other side. This may be caused by housing clusters near a border, where the other side of the border is vacant due to the presence of a forest or mountain. If this happens frequently enough, observable characteristics may seem discontinuous, even if they truly are smooth (and even linear) along other dimensions of proximity, such as (unobservable) travel distance. I partially address this concern by measuring border distance as the distance to the nearest owner-occupied residences on the

[44] I use the centroids of all *residences* to proxy for the centroid of the actual estimation sample. Some areas see very few or no housing transactions, thus using all residences provides a more widely applicable measure.

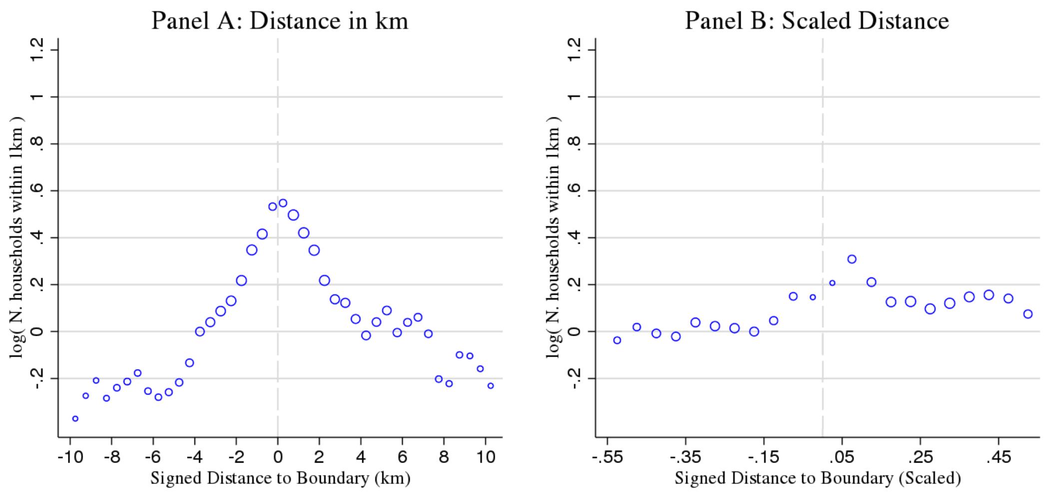
other side of the border.[45] This nearest-neighbor approach also avoids some computational issues in calculating border distance when borders take complicated forms, since I can calculate border distance by minimizing the distance to residences in neighboring municipalities.[46]

B Appendix

B.1 Descriptive Figures and Tables

FIGURE A.2: RESIDENTIAL DENSITY AROUND BORDERS

This graph shows how residential density varies with border distance. Density is defined as at the household level as the log of the number of households living within 1 km. The figures plot estimated coefficients of living in a given distance bin. The regressions include the baseline housing controls $\mathbf{H}_{i,2009}$, but these are not allowed to vary at the border area level. Panel A uses distance in kilometers, and panel B uses scaled distance. All households in the analysis sample (with taxable net wealth ≥ 0 in 2009) are included.



[45]The exact algorithm for calculating the distance variables is provided in the appendix.

[46]Complex borders may require linearization or division of the border into a finite set of points. This could lead to sizable approximation errors, in relative terms, for households very close to the border.

Figure A.3: Geographic Distribution of Households

This figures provides histograms illustrating the distribution of households in the analysis sample according to the different distance measures. All households in the analysis sample (with taxable net wealth ≥ 0 in 2009) are include, except those with distance measures outside the visible range of the graphs. Panel A uses (signed) distance in kilometers, Panel B uses scaled distance, and Panel C uses relative location, where the darker shade indicates membership to the high-assessment side of the boundary.

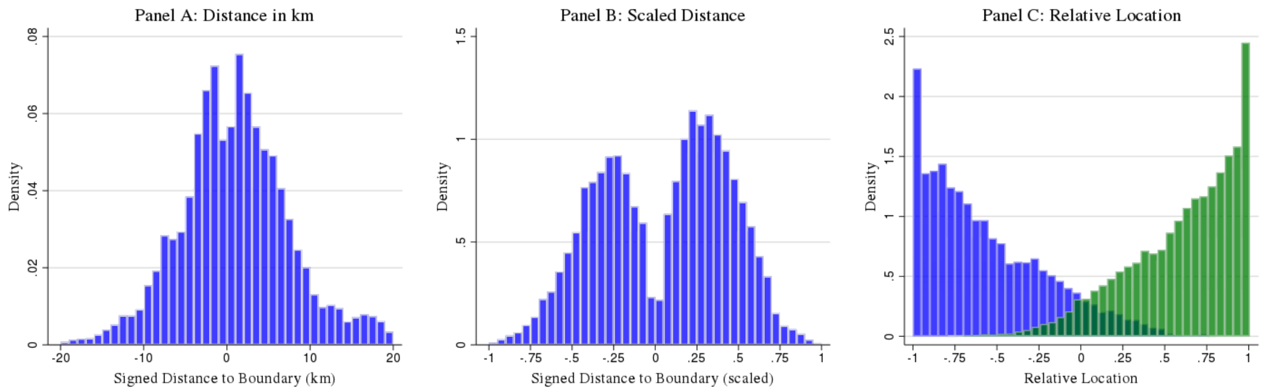


Figure A.4: Verifying The House Price Model Coefficients

This figure plots actual assessed tax values against tax values predicted using the real-estate data and coefficients from the hedonic pricing model. The Y-axis has the actual tax values are retrieved from individuals' tax returns for 2010, presumably based on the coefficients from the model estimated with 2004–2009 data. The X-axis has predicted tax values based on 2009 real-estate data and coefficients estimated with 2004–2008 data, which are the same coefficients used in providing preliminary tax values to households in during 2010. Predicted and actual values may differ for the following main reasons: (1) coefficients changed due to the inclusion of 2009 data in the estimation sample; (2) households can move or have a complaint approved that assessed tax values are too high; or (3) households may own a second home.

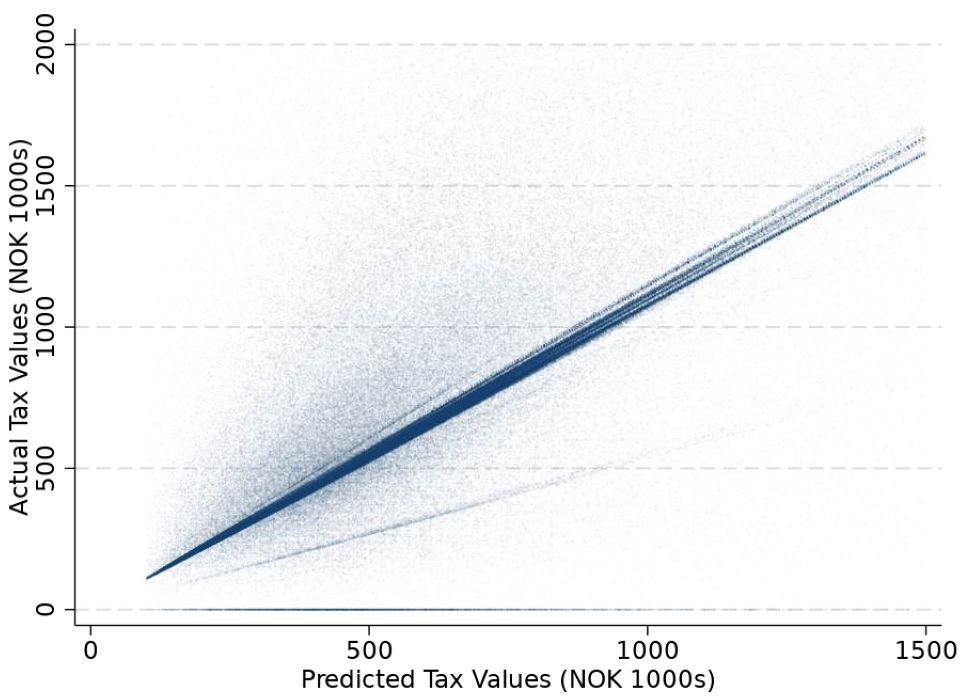


Figure A.5: Example of Data Source for House Price Model Coefficients

The regression output below is for s=detached homes, in the price region, R, corresponding to Aust-Agder county. Estimated coefficients are: $\alpha_R = 11.83711$, $\gamma_1 = 0$, $\gamma_2 = -0.15054$,..., $\gamma_7 = -0.72255$, $\zeta_R^{size} = -0.38555$, $\zeta_R^{Dense} = 0.06373$, $\zeta_{1,R}^{Age} = 0$, $\zeta_{2,R}^{Age} = -0.09434$,..., $\zeta_{4,R}^{Age} = -0.21287$, and $\sigma_R = 0.28800$.

Eneboliger i Aust-Agder
170

The REG Procedure
Model: MODEL1
Dependent Variable: lnkvmpris

Number of Observations Read	4196
Number of Observations Used	4196

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Value	Pr > F
Model	16	312.92646	19.55790	235.79	<.0001
Error	4179	346.63448	0.08295		
Corrected Total	4195	659.56094			

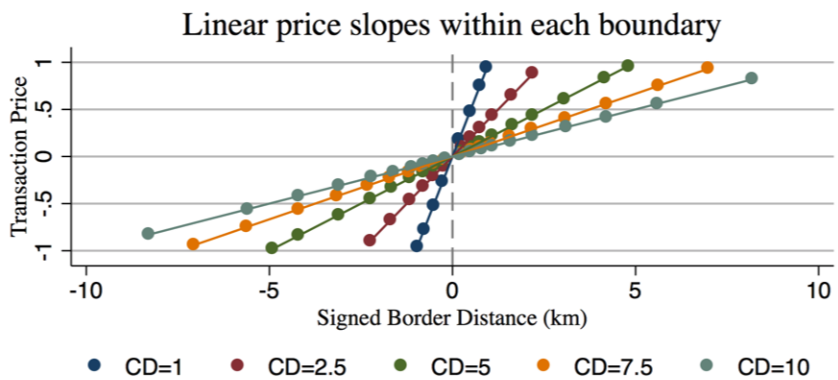
Root MSE	0.28800	R-Square	0.4744
Dependent Mean	9.35767	Adj R-Sq	0.4724
Coeff Var	3.07774		

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	t Value	Pr > |t|
Intercept	1	11.83711	0.07555	156.68	<.0001
lnareal	1	-0.38555	0.01447	-26.65	<.0001
tett	1	0.06373	0.01197	5.33	<.0001
aar1	1	-0.43694	0.01599	-27.33	<.0001
aar2	1	-0.36403	0.01544	-23.57	<.0001
aar3	1	-0.26966	0.01494	-18.05	<.0001
aar4	1	-0.09556	0.01496	-6.39	<.0001
aar5	1	-0.05457	0.01537	-3.55	0.0004
alder2	1	-0.09434	0.01954	-4.83	<.0001
alder3	1	-0.21329	0.01615	-13.21	<.0001
alder4	1	-0.21287	0.01475	-14.43	<.0001
sone2	1	-0.15054	0.01850	-8.14	<.0001
sone3	1	-0.26613	0.02581	-10.31	<.0001
sone4	1	-0.24332	0.01699	-14.32	<.0001
sone5	1	-0.36361	0.02706	-13.44	<.0001
sone6	1	-0.43013	0.02127	-20.23	<.0001
sone7	1	-0.72255	0.02323	-31.10	<.0001

Figure A.6: Example of non-linearities in pooled boundary region

I create this example the following way. First create 100 border areas, indexed by b. Each border area has a length of 200*b, and thus a centroid distance in thousands, e.g., kilometers, (CD) of $100b/1000$ Each b has 100 households, equidistantly populated in G=[-100b, 100b]. Within each b, house prices move linearly according to their location, $g \in G$: $p = \frac{\Delta}{100}$. By construction, the mean difference between houses with $g < 0$ (low-side) and $g > 0$ (high side) is constant across bs, and is Δ. I set Δ to 1. In the first plot, I provide a binscatter of ps against g, separately for $b = 10, 25, 50, 75, 100$. In the second plot, I perform a pooled binscatter of p, for $b \in \{10, 25, 50, 75, 100\}$. The red line is a second-order RD polynomial, estimated separately for each side, allowing for a discontinuity at zero. Point estimates correspond to the within-bin means for 20 equal-sized bins.



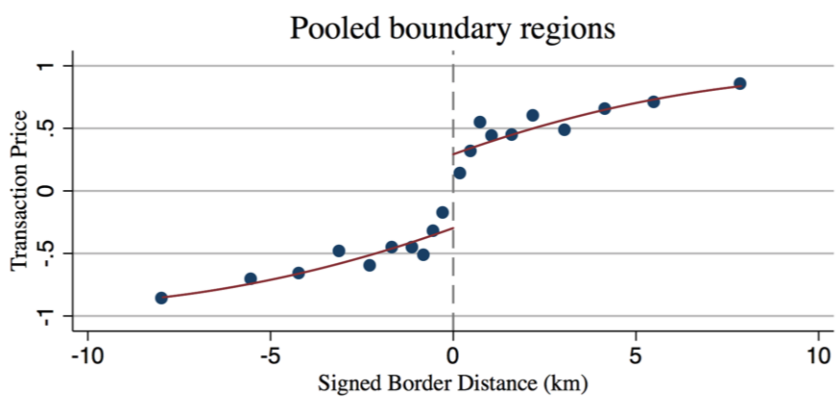


Table A.1: Summary Statistics by Initial Wealth (Appendix)

Summary statistics are provided for households in the sample. Only households who are assigned a signed border distance variable are included. GFW is Gross Financial Wealth. GFW, censored winsorizes GFW at the 95th percentile. TGW is Taxable Gross Wealth. TNW is TGW minus Debt. Total Taxable Labor Income is the sum of wage earnings, self-employment income, UI benefits and labor-related pension income, and transfers (e.g., parental leave). SMW is the sum of mutual-fund and direct holdings of listed stocks. $TaxVal$ is the assessed tax value (housing wealth) as observed in tax returns. $wtax$ is the amount of wealth taxes paid. $wtax/GFW$ is set to be the min(wtax/GFW,1). RiskyShare is the ratio of SMW, plus foreign held financial assets (excl. deposits), plus nonlisted stocks (e.g., private equity) to GFW. $Foreign/GFW$ is the share of GFW that is held abroad. $Self\text{-}rep/GFW$ is the share of GFW that belongs to self-reported asset classes, such as outstanding claims and foreign assets. $wtax > 0$ is a dummy for whether a household paid wealth taxes. $r, Deposits$ is the realized (symmetric) return on deposits. $r, Debt$ is similarly defined, but excludes household who in either the current or subsequent period had $Debt < 10,000$.

	Full sample						Households above wealth tax threshold in 2009						Households below wealth tax threshold in 2009					
	N	mean	sd	p25	p50	p75	N	mean	sd	p25	p50	p75	N	mean	sd	p25	p50	p75
2010–2015																		
GFW	1820892	1177	2300	224	580	1321	1005096	1739	2859	491	1026	2014	815796	484	933	117	284	579
GFW, censored	1820892	1000	1109				1005096	1432	1235				815796	466	589			
Debt	1820892	510	1035	0	138	617	1005096	417	1144	0	6	332	815796	625	869	71	385	813
TGW	1772724	2261	2765	970	1550	2600	977374	2967	3369	1353	2100	3437	795350	1394	1314	750	1097	1648
TNW	1772724	1741	2559	586	1181	2157	977374	2541	3061	1139	1837	3022	795350	758	1154	285	638	1081
Labor Income	1824466	692	483	378	579	891	1007252	698	526	365	567	887	817214	685	424	392	595	895
Earnings	1824466	429	566	0	203	741	1007252	405	602	0	60	676	817214	459	515	0	355	793
SMW	1820892	151	559	0	0	82	1005096	225	717	0	8	153	815796	59	223	0	0	31
Deposits	1820892	722	970	153	408	916	1005096	1029	1154	308	700	1337	815796	343	448	87	215	445
TaxVal	1780154	828	706	466	659	969	981670	908	805	495	707	1062	798484	730	545	436	609	865
wtax	1819622	11	25	0	2	13	1004801	18	30	2	10	22	814821	2	9	0	0	1
2004–2009																		
TaxVal	1873872	406	222	257	367	511	1038598	434	237	277	391	544	835274	371	196	236	340	471
2010–2015																		
SMW/GFW	1805548	0.108	0.202	0.000	0.000	0.118	995132	0.115	0.203	0.000	0.008	0.135	810416	0.099	0.199	0.000	0.000	0.092
RiskyShare	1805548	0.164	0.266	0.000	0.006	0.224	995132	0.189	0.282	0.000	0.032	0.282	810416	0.133	0.242	0.000	0.000	0.152
Deposits/GFW	1805548	0.803	0.291	0.690	0.977	1.000	995132	0.772	0.308	0.615	0.944	1.000	810416	0.841	0.264	0.780	1.000	1.000
Foreign/GFW	1805548	0.008	0.054	0.000	0.000	0.000	995132	0.009	0.058	0.000	0.000	0.000	810416	0.006	0.049	0.000	0.000	0.000
Self-rep/GFW	1805548	0.025	0.106	0.000	0.000	0.000	995132	0.030	0.113	0.000	0.000	0.000	810416	0.019	0.096	0.000	0.000	0.000
wtax/GFW	1797249	0.009	0.036	0.000	0.005	0.011	990509	0.012	0.037	0.004	0.009	0.013	806740	0.006	0.035	0.000	0.000	0.004
wtax>0	1819622	0.600	0.490	0.000	1.000	1.000	1004801	0.817	0.387	1.000	1.000	1.000	814821	0.332	0.471	0.000	0.000	1.000
2009																		
r, Deposits	315208	0.0202	0.0116	0.0117	0.0203	0.0274	174786	0.0237	0.0108	0.0174	0.0244	0.0299	140422	0.0158	0.0111	0.0075	0.0148	0.0218
r, Debt	196473	0.0441	0.0144	0.0384	0.0433	0.0499	79305	0.0408	0.0169	0.0361	0.0416	0.0485	117168	0.0464	0.0118	0.0397	0.0441	0.0508
Age (hh. avg)	315328	61.93	13	53.0	62.0	71.0	174814	64.08	13	55.0	64.0	73.0	140514	59.25	13	50.0	59.0	68.0

B.2 Additional figures

Figure A.7: Graphical Presentation of the Reduced-Form Effects on Pre-Period Wealth Tax Exposure

This graph shows the reduced-form effects of increased tax assessment on the following pre-period outcomes: (A) How much of household savings is subject to a wealth tax, i.e., the amount of wealth above the tax threshold; (B) Whether or not a household pays a wealth tax. These outcomes are measured during 2010–2015. The first row uses distance in kilometers, where households on the low-assessment side are given a negative distance. The second row uses (similarly signed) distance scaled by the distance between the two municipal centroids. The fitted lines and discontinuities for correspond to reduced-form regressions using the regression specification in equation 19. 95% Confidence bands are represented by dashed lines. All panels consider post-period saving outcomes for the full sample of households with initial positive taxable net wealth in 2009. Scatter-points stem from estimating a coefficient on Δ_i using equation 23 separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. The size of each circle correspond approximately to the relative number of observations in that bin.

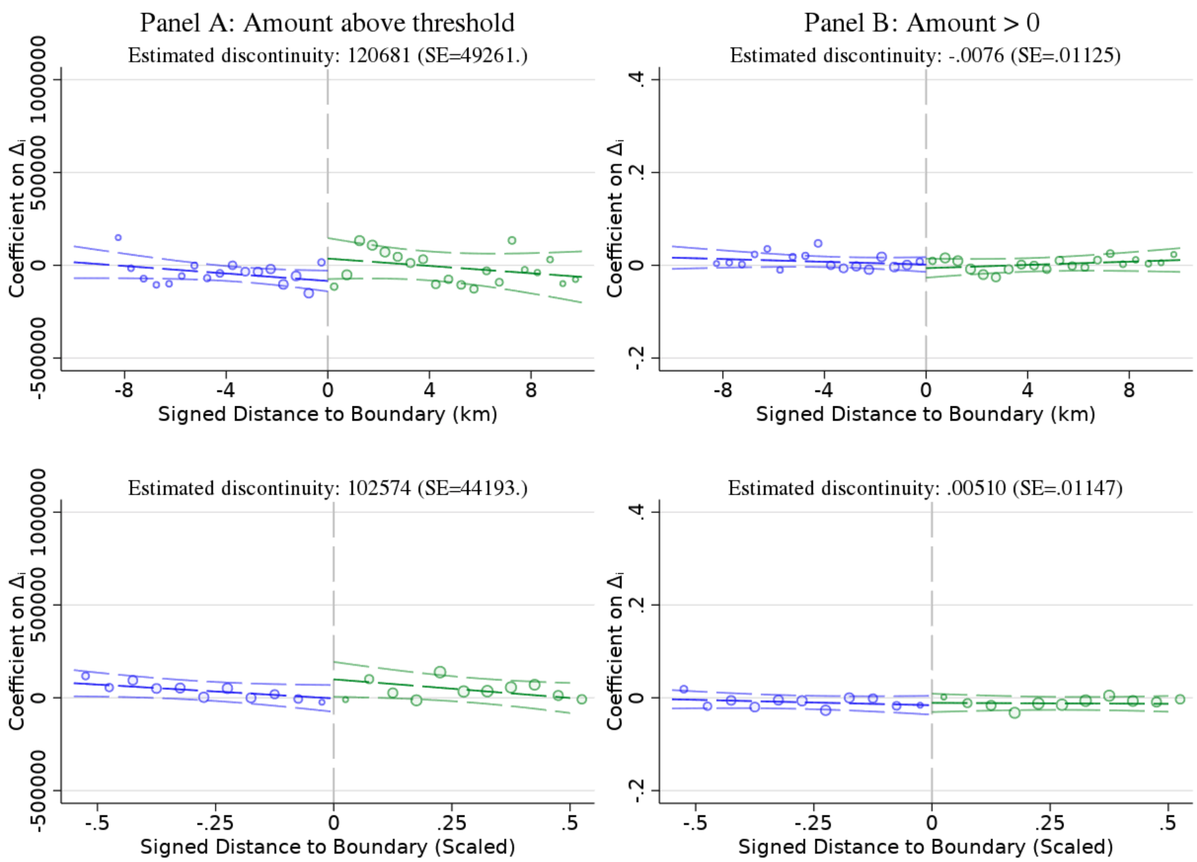
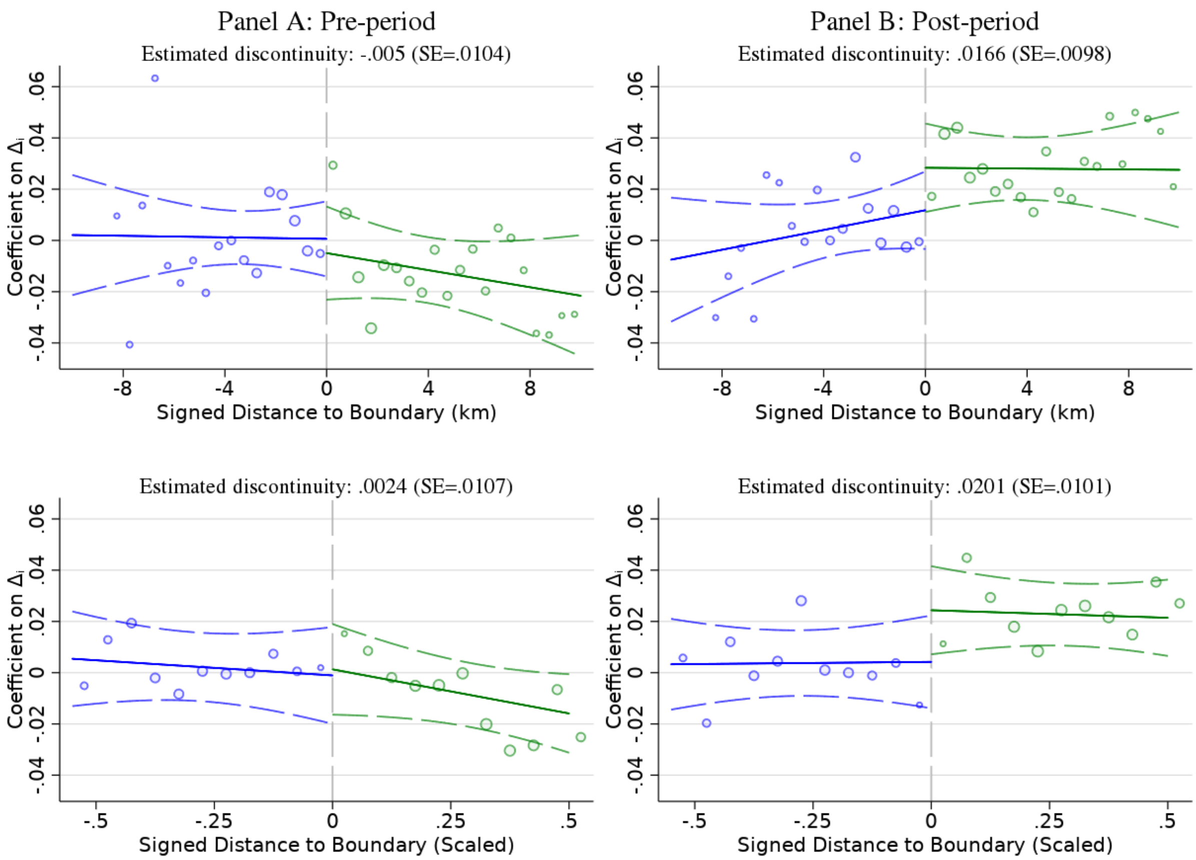


Figure A.8: Graphical Presentation of the Effects of Increased Tax Assessment on Financial Saving

Above Sample



B.3 Additional results figures

Figure A.9: Total Taxable Labor Income
Subsample: Households initially above tax threshold

The dependent variable is one-year log-differenced labor income. This figure provides results for the sample of households above the tax threshold in 2009. See description in Figure 8.

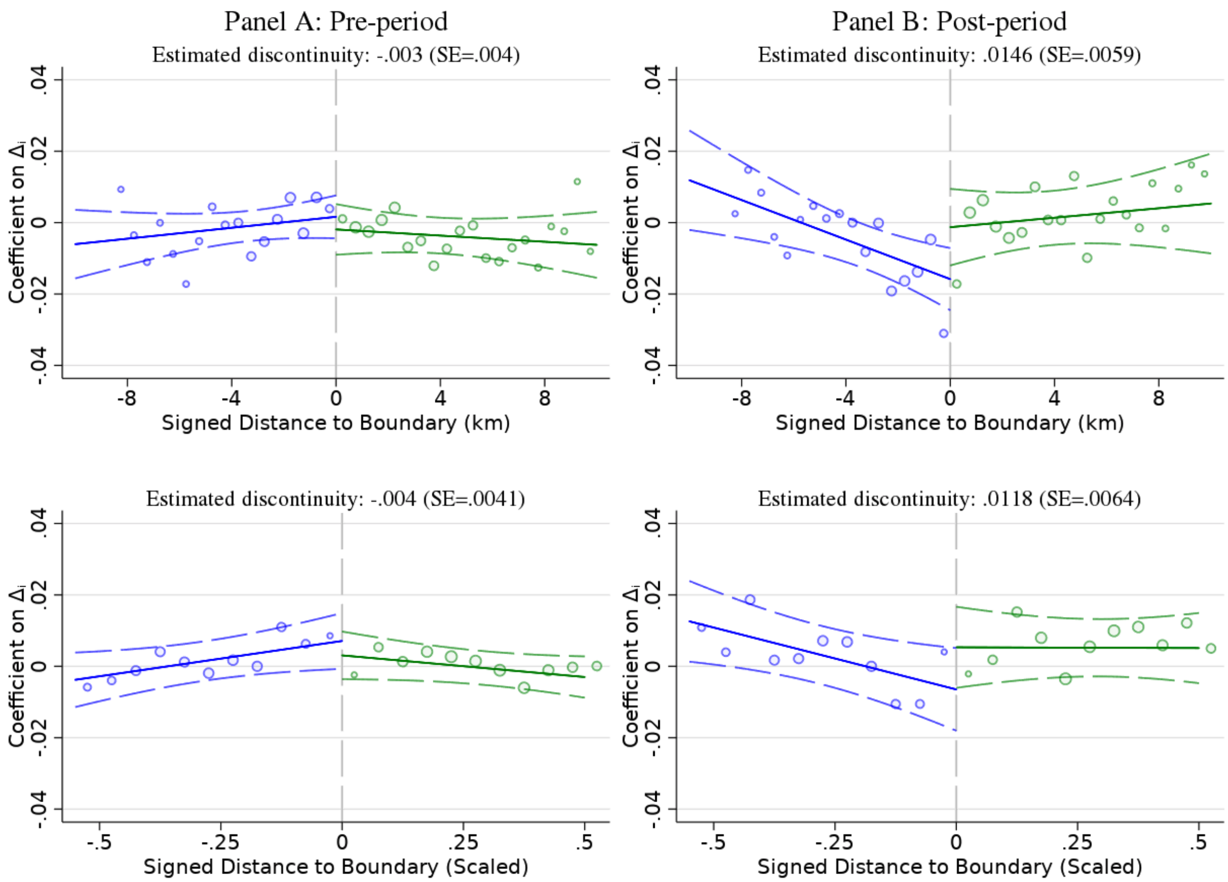
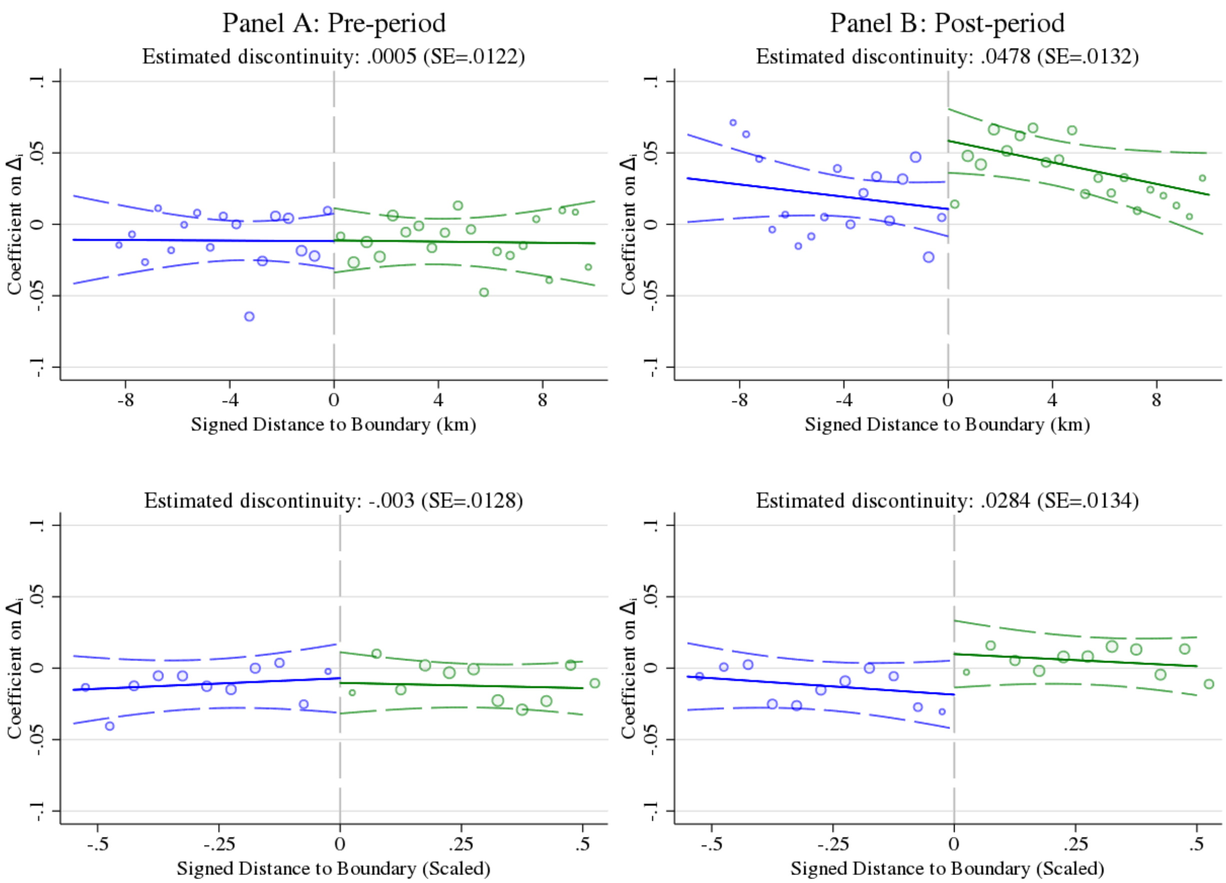


Figure A.10: Salaries and Self-Employment Income
Subsample: Households initially above tax threshold

The dependent variable is one-year log-differenced salary and max(Self-Employment Income,0). This figure provides results for the sample of households above the wealth tax threshold in 2009 See description in Figure 9.



B.4 Robustness

B.5 Additional Results Tables

Table A.2: Robustness: Excluding 2015 and controlling for property tax rate

This table provides the main (reduced-form) results when controlling for municipal property tax rates and excluding 2015.

	Adj. Δ log(GFW)	Δlog(Debt)	ΔSMS	Δlog(Labor Income) Broad	Δ log(Labor Income) Salary + Self. Empl.
	(1)	(2)	(3)	(4)	(5)
			Full sample		
log($\widehat{TaxVal}$)	0.0211***	0.0004	0.0003	0.0044	0.0191
	(0.0077)	(0.0123)	(0.0014)	(0.0045)	(0.0119)
Prop. Tax (%)	-0.0062*	-0.0009	-0.0009	0.0024*	0.0086***
	(0.0032)	(0.0051)	(0.0006)	(0.0014)	(0.0028)
N	[1232832]	[1232845]	[1229001]	[1234293]	[1234293]
			Households Initially Above Threshold		
log($\widehat{TaxVal}$)	0.0219**	-0.0131	0.0017	0.0135**	0.0344***
	(0.0107)	(0.0180)	(0.0018)	(0.0066)	(0.0127)
Prop. Tax (%)	-0.0056	0.0025	-0.0003	0.0008	0.0122***
	(0.0041)	(0.0067)	(0.0008)	(0.0020)	(0.0043)
N	[685156]	[685168]	[682439]	[686177]	[686177]
			Households Initially Below Threshold		
log($\widehat{TaxVal}$)	0.0192*	0.0173	-0.0013	-0.0042	0.0038
	(0.0107)	(0.0182)	(0.0021)	(0.0057)	(0.0168)
Prop. Tax (%)	-0.0064	-0.0029	-0.0015**	0.0054***	0.0055
	(0.0046)	(0.0063)	(0.0007)	(0.0018)	(0.0042)
N	[547581]	[547582]	[546467]	[548022]	[548022]
Geo-Controls					
Scaled Border Distance	Yes	Yes	Yes	Yes	Yes

Table A.3: Robustness: Main IV Estimates using Triangular Weights

This table provides regression results when the main IV regression specifications are run with triangular weights. For the case of scaled border distance, $d_i^{scaled} \in [-0.6, 0.6]$, weights are assigned as 1-abs(d_i^{scaled}/0.6). For the relative location measure, $RL_i \in [-1, 1]$, weights are assigned as 1-abs(RL_i).

	Adj. $\Delta \log(GFW)$		$\Delta \log(Debt)$		Δ Stock Market Share		Δlog(Labor Income)		Δlog(Salary + SE Income)	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
					Full sample					
$\log(TaxVal)$	0.0264***	0.0254***	0.0022	-0.0060	0.0000	0.0001	0.0061	0.0074	0.0263**	0.0230**
	(0.0102)	(0.0086)	(0.0135)	(0.0119)	(0.0014)	(0.0012)	(0.0056)	(0.0048)	(0.0126)	(0.0104)
N	1472113	1649293	1472130	1649429	1466682	1643325	1473585	1650966	1473585	1650966
					Households above tax threshold in 2009					
$\log(TaxVal)$	0.0345**	0.0261**	-0.0019	-0.0025	-0.0004	-0.0008	0.0164*	0.0107	0.0307*	0.0320**
	(0.0137)	(0.0120)	(0.0186)	(0.0168)	(0.0022)	(0.0020)	(0.0085)	(0.0074)	(0.0178)	(0.0158)
N	817214	912399	817229	912482	813335	908075	818244	913560	818244	913560
					Households below tax threshold in 2009					
$\log(TaxVal)$	0.0170	0.0183	0.0098	-0.0024	0.0003	0.0001	-0.0038	0.0038	0.0206	0.0161
	(0.0145)	(0.0117)	(0.0194)	(0.0164)	(0.0023)	(0.0018)	(0.0069)	(0.0058)	(0.0181)	(0.0139)
N	654804	736802	654806	736855	653253	735043	655247	737315	655247	737315
Geo-Controls										
Scaled Border Distance	Yes	–	Yes	–	Yes	–	Yes	–	Yes	–
Relative Location	–	Yes	–	Yes	–	Yes	–	Yes	–	Yes

Table A.4: Robustness: Significance and Standard Errors of Main Reduced-form Results using Different Levels of Clustering

Table reports estimated standard errors on $\log(\widehat{TaxVal})$ using different levels of clustering. All regressions are reduced-form, and consider post-period (2010–2015) outcomes.

	Adj. $\Delta\log(GFW)$		$\Delta\log(Debt)$		ΔSMS		$\Delta\log(Labor\ Income)$		$\Delta\log(Salary + S.E.Income)$	
Level of clustering	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)	(5a)	(5b)
				Full sample						
Household	0.0195***	0.0182***	0.0018	-0.0047	0.0000	0.0001	0.0044	0.0049	0.0191**	0.0214***
	(0.0076)	(0.0063)	(0.0113)	(0.0093)	(0.0012)	(0.0010)	(0.0043)	(0.0036)	(0.0091)	(0.0075)
Census tract	0.0195***	0.0182***	0.0018	-0.0047	0.0000	0.0001	0.0044	0.0049	0.0188**	0.0207***
	(0.0073)	(0.0063)	(0.0111)	(0.0093)	(0.0012)	(0.0010)	(0.0041)	(0.0035)	(0.0095)	(0.0078)
Municipality	0.0195***	0.0182***	0.0018	-0.0047	0.0000	0.0001	0.0044	0.0049	0.0188**	0.0207***
	(0.0073)	(0.0060)	(0.0104)	(0.0091)	(0.0013)	(0.0011)	(0.0039)	(0.0033)	(0.0105)	(0.0075)
				Households above tax threshold in 2009						
Household	0.0201*	0.0230**	-0.0016	-0.0020	0.0005	0.0006	0.0118*	0.0094*	0.0290**	0.0316***
	(0.0108)	(0.0090)	(0.0163)	(0.0135)	(0.0017)	(0.0014)	(0.0069)	(0.0057)	(0.0132)	(0.0110)
Census tract	0.0201**	0.0230***	-0.0016	-0.0020	0.0005	0.0006	0.0118*	0.0094*	0.0284**	0.0306***
	(0.0102)	(0.0089)	(0.0157)	(0.0133)	(0.0017)	(0.0014)	(0.0064)	(0.0057)	(0.0135)	(0.0112)
Municipality	0.0201**	0.0230***	-0.0016	-0.0020	0.0005	0.0006	0.0118**	0.0094*	0.0284**	0.0306***
	(0.0091)	(0.0075)	(0.0158)	(0.0132)	(0.0015)	(0.0012)	(0.0059)	(0.0051)	(0.0122)	(0.0101)
Geo-Controls										
Scaled Border Distance	Yes	–	Yes	–	Yes	–	Yes	–	Yes	–
Relative Location Controls	–	Yes	–	Yes	–	Yes	–	Yes	–	Yes

Table A.5: Effect on the Risky Share of Gross Financial Wealth which includes non-listed stocks and foreign-held financial assets (excl. foreign deposits).

This table shows the (IV) effect of tax assessment on the (one-year differenced) share of wealth allocated to "risky assets" during 2010–2015. Risky Assets is defined as the sum of listed and non-listed stocks, plus securities (excl. deposits) held abroad. This implies $RiskyShare \geq SMS$.

ΔRiskyAssets/GFW	(1)	(2)	(3)	**(4)**	(5)	(6)
			Full sample			
log($TaxVal$)	-0.0003	-0.0005	-0.0006	-0.0028	-0.0035	-0.0019
	(0.0002)	(0.0008)	(0.0017)	(0.0018)	(0.0026)	(0.0016)
N	[1835781]	[1835687]	[1454510]	[1466682]	[1466682]	[1643325]
F	41012	3021	857	697	331	849
			Households Initially Above Threshold			
log($TaxVal$)	-0.0001	0.0009	-0.0014	-0.0024	-0.0030	-0.0019
	(0.0003)	(0.0011)	(0.0023)	(0.0026)	(0.0039)	(0.0023)
N	[1008692]	[1008586]	[813160]	[813335]	[813335]	[908140]
F	28269	1819	509	386	165	430
			Households Initially Below Threshold			
log($TaxVal$)	-0.0009***	-0.0020*	-0.0006	-0.0035	-0.0035	-0.0021
	(0.0003)	(0.0011)	(0.0027)	(0.0026)	(0.0035)	(0.0021)
N	[827089]	[826985]	[641250]	[653253]	[653253]	[735094]
F	24918	1955	432	379	193	466
Controls						
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes	Yes
Border Distance Controls						
– KM	–	–	Yes	–	–	–
– Scaled	–	–	–	Yes	–	–
– Border specific	–	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	–	Yes

Table A.6: Bandwidth Robustness, Scaled Border Distance.

This table shows the results from the main IV specification, using different cut-offs for scaled border distance. bw=0.9 implies keeping all households with a scaled border distance inside [-0.9,0.9]. The main specification uses bw=0.6. All specifications consider outcomes during the post-period of 2010–2015.

Bandwidth	Adj. $\Delta\log(GFW$ (1)	$\Delta\log(GFW)$ (2)	$\Delta\log(Debt)$ (3)	ΔSMS (4)	$\Delta\log(Income)$ (5)	$\Delta\log(Salary + S.E.Inc.)$ (5)
			Full sample			
bw = .9	0.0199**	0.0110	-0.0032	0.0002	0.0044	0.0311***
	(0.0078)	(0.0080)	(0.0119)	(0.0012)	(0.0044)	(0.0100)
bw = .8	0.0208***	0.0118	-0.0020	0.0000	0.0027	0.0305***
	(0.0079)	(0.0081)	(0.0121)	(0.0012)	(0.0044)	(0.0102)
bw = .7	0.0212***	0.0123	-0.0027	0.0003	0.0027	0.0295***
	(0.0080)	(0.0082)	(0.0124)	(0.0013)	(0.0045)	(0.0105)
bw = .5	0.0204**	0.0110	0.0067	0.0007	0.0054	0.0293**
	(0.0100)	(0.0102)	(0.0153)	(0.0016)	(0.0056)	(0.0125)
bw = .4	0.0247**	0.0147	0.0081	0.0003	0.0043	0.0200
	(0.0119)	(0.0122)	(0.0179)	(0.0019)	(0.0066)	(0.0146)
bw = .3	0.0390**	0.0282*	-0.0081	-0.0010	0.0121	0.0353*
	(0.0156)	(0.0159)	(0.0236)	(0.0025)	(0.0086)	(0.0193)
bw = .2	0.0647**	0.0526**	0.0015	-0.0013	0.0121	0.0647**
	(0.0253)	(0.0256)	(0.0365)	(0.0037)	(0.0135)	(0.0275)
			Households above tax threshold in 2009			
bw = .9	0.0201*	0.0114	-0.0004	0.0015	0.0094	0.0412***
	(0.0104)	(0.0109)	(0.0160)	(0.0016)	(0.0066)	(0.0136)
bw = .8	0.0201*	0.0111	-0.0018	0.0014	0.0060	0.0375***
	(0.0106)	(0.0111)	(0.0164)	(0.0017)	(0.0066)	(0.0138)
bw = .7	0.0178*	0.0085	-0.0086	0.0014	0.0058	0.0347**
	(0.0107)	(0.0113)	(0.0168)	(0.0017)	(0.0067)	(0.0143)
bw = .5	0.0263*	0.0168	0.0088	0.0008	0.0147*	0.0350**
	(0.0135)	(0.0142)	(0.0210)	(0.0023)	(0.0085)	(0.0177)
bw = .4	0.0395**	0.0292*	0.0036	-0.0008	0.0137	0.0161
	(0.0161)	(0.0169)	(0.0251)	(0.0027)	(0.0102)	(0.0205)
bw = .3	0.0526**	0.0415*	-0.0118	-0.0045	0.0270**	0.0474*
	(0.0214)	(0.0223)	(0.0340)	(0.0036)	(0.0134)	(0.0281)
bw = .2	0.0875**	0.0733*	0.0148	-0.0052	0.0204	0.1084**
	(0.0376)	(0.0387)	(0.0585)	(0.0060)	(0.0227)	(0.0481)
			Households below tax threshold in 2009			
bw = .9	0.0194*	0.0102	-0.0048	-0.0018	-0.0010	0.0195
	(0.0114)	(0.0113)	(0.0174)	(0.0019)	(0.0055)	(0.0148)
bw = .8	0.0214*	0.0122	-0.0006	-0.0023	-0.0006	0.0218
	(0.0115)	(0.0114)	(0.0176)	(0.0019)	(0.0056)	(0.0149)
bw = .7	0.0235**	0.0146	0.0057	-0.0016	-0.0015	0.0235
	(0.0119)	(0.0119)	(0.0182)	(0.0019)	(0.0058)	(0.0153)
bw = .5	0.0149	0.0055	0.0005	0.0002	-0.0045	0.0215
	(0.0142)	(0.0142)	(0.0215)	(0.0023)	(0.0070)	(0.0180)
bw = .4	0.0074	-0.0021	0.0151	0.0008	-0.0049	0.0290
	(0.0176)	(0.0175)	(0.0260)	(0.0028)	(0.0084)	(0.0215)
bw = .3	0.0146	0.0043	0.0016	0.0017	-0.0036	0.0222
	(0.0225)	(0.0223)	(0.0319)	(0.0037)	(0.0106)	(0.0285)
bw = .2	0.0423	0.0328	0.0141	0.0026	0.0104	0.0296
	(0.0331)	(0.0329)	(0.0440)	(0.0049)	(0.0149)	(0.0373)

Table A.7: Bandwidth Robustness, Border Distance (km)

This table shows the results from the main IV specification, using different cutoffs for scaled border distance. bw=10 implies keeping all households with a border distance, in km, inside [-10,10]. The main specification uses bw=10. All specifications consider outcomes during the post-period of 2010–2015.

Bandwidth	Adj. $\Delta \log(GFW$ (1)	$\Delta \log(GFW)$ (2)	$\Delta \log(Debt)$ (3)	ΔSMS (4)	$\Delta \log(Income)$ (5)	$\Delta \log(Salary + S.E.Inc.)$ (5)
				Full sample		
bw = 14	0.0228***	0.0111	0.0022	0.0012	0.0057	0.0322***
	(0.0070)	(0.0072)	(0.0109)	(0.0012)	(0.0039)	(0.0091)
bw = 12	0.0226***	0.0102	0.0059	0.0005	0.0055	0.0367***
	(0.0077)	(0.0079)	(0.0118)	(0.0013)	(0.0042)	(0.0101)
bw = 8	0.0252***	0.0119	0.0101	0.0001	0.0110**	0.0379***
	(0.0094)	(0.0097)	(0.0145)	(0.0016)	(0.0050)	(0.0120)
bw = 6	0.0327***	0.0179	0.0025	-0.0005	0.0102*	0.0290**
	(0.0109)	(0.0113)	(0.0176)	(0.0019)	(0.0059)	(0.0143)
bw = 4	0.0436***	0.0285**	0.0156	-0.0023	0.0109	0.0319*
	(0.0139)	(0.0143)	(0.0216)	(0.0023)	(0.0075)	(0.0174)
bw = 2	0.0361	0.0184	-0.0276	-0.0055	-0.0002	0.0650**
	(0.0251)	(0.0258)	(0.0396)	(0.0041)	(0.0132)	(0.0307)
bw = 1	-0.0238	-0.0504	0.0132	-0.0170*	-0.0239	0.0278
	(0.0549)	(0.0573)	(0.0860)	(0.0099)	(0.0283)	(0.0684)
				Households above tax threshold in 2009		
bw = 14	0.0184**	0.0073	0.0017	0.0014	0.0126**	0.0437***
	(0.0093)	(0.0098)	(0.0147)	(0.0015)	(0.0057)	(0.0124)
bw = 12	0.0183*	0.0071	0.0058	0.0009	0.0127**	0.0482***
	(0.0101)	(0.0106)	(0.0159)	(0.0017)	(0.0060)	(0.0136)
bw = 8	0.0218*	0.0101	0.0194	0.0003	0.0164**	0.0489***
	(0.0123)	(0.0130)	(0.0195)	(0.0020)	(0.0073)	(0.0165)
bw = 6	0.0381***	0.0245	0.0051	-0.0017	0.0187**	0.0324*
	(0.0142)	(0.0150)	(0.0226)	(0.0024)	(0.0085)	(0.0190)
bw = 4	0.0460**	0.0329*	-0.0115	-0.0031	0.0166	0.0288
	(0.0179)	(0.0190)	(0.0279)	(0.0030)	(0.0109)	(0.0231)
bw = 2	0.0302	0.0190	-0.0476	-0.0044	-0.0050	0.0231
	(0.0301)	(0.0321)	(0.0479)	(0.0048)	(0.0180)	(0.0356)
bw = 1	0.0005	-0.0184	0.0195	-0.0091	-0.0057	0.0414
	(0.0595)	(0.0636)	(0.0853)	(0.0090)	(0.0317)	(0.0634)
				Households below tax threshold in 2009		
bw = 14	0.0245**	0.0119	0.0045	0.0007	-0.0035	0.0126
	(0.0109)	(0.0109)	(0.0163)	(0.0019)	(0.0053)	(0.0135)
bw = 12	0.0243**	0.0107	0.0080	0.0000	-0.0040	0.0197
	(0.0118)	(0.0118)	(0.0174)	(0.0020)	(0.0059)	(0.0145)
bw = 8	0.0246*	0.0091	0.0010	-0.0004	0.0009	0.0148
	(0.0144)	(0.0144)	(0.0211)	(0.0025)	(0.0064)	(0.0174)
bw = 6	0.0190	0.0025	0.0050	0.0012	-0.0034	0.0148
	(0.0174)	(0.0174)	(0.0255)	(0.0030)	(0.0077)	(0.0217)
bw = 4	0.0376*	0.0197	0.0529*	-0.0019	0.0043	0.0251
	(0.0221)	(0.0220)	(0.0311)	(0.0038)	(0.0098)	(0.0267)
bw = 2	0.0422	0.0169	0.0316	-0.0057	0.0036	0.1192**
	(0.0405)	(0.0398)	(0.0579)	(0.0070)	(0.0185)	(0.0530)
bw = 1	-0.0978	-0.1517	0.0493	-0.0276	-0.0623	-0.0352
	(0.1304)	(0.1346)	(0.1747)	(0.0232)	(0.0601)	(0.1548)

Table A.8: Bandwidth Robustness, Relative Location

This table shows the results from the main IV specification, using different cutoffs for scaled border distance. bw=0.5 implies keeping all households with Relative Location in inside [-0.5,0.5]. The main specification uses bw=1.0. All specifications consider outcomes during the post-period of 2010–2015.

Bandwidth	Adj. $\Delta\log(GFW$ (1)	$\Delta\log(GFW)$ (2)	$\Delta\log(Debt)$ (3)	ΔSMS (4)	$\Delta\log(Income)$ (5)	$\Delta\log(Salary + S.E.Inc.)$ (5)
	Full sample					
bw = .9	0.0223***	0.0129	-0.0090	-0.0002	0.0059	0.0230**
	(0.0082)	(0.0084)	(0.0128)	(0.0013)	(0.0046)	(0.0103)
bw = .8	0.0217**	0.0121	-0.0034	0.0003	0.0040	0.0206*
	(0.0088)	(0.0091)	(0.0140)	(0.0014)	(0.0051)	(0.0109)
bw = .7	0.0250***	0.0152	-0.0038	-0.0000	0.0058	0.0182
	(0.0095)	(0.0098)	(0.0149)	(0.0015)	(0.0054)	(0.0113)
bw = .6	0.0312***	0.0201*	0.0047	-0.0012	0.0094	0.0188
	(0.0104)	(0.0107)	(0.0164)	(0.0017)	(0.0060)	(0.0127)
bw = .5	0.0326***	0.0221*	-0.0090	-0.0010	0.0127*	0.0280**
	(0.0117)	(0.0120)	(0.0180)	(0.0018)	(0.0066)	(0.0134)
bw = .4	0.0321**	0.0196	-0.0154	-0.0002	0.0134*	0.0181
	(0.0130)	(0.0133)	(0.0193)	(0.0020)	(0.0072)	(0.0149)
bw = .3	0.0352**	0.0205	-0.0283	-0.0000	0.0086	0.0213
	(0.0146)	(0.0150)	(0.0235)	(0.0022)	(0.0083)	(0.0171)
	Households above tax threshold in 2009					
bw = .9	0.0259**	0.0157	-0.0071	-0.0000	0.0110	0.0383**
	(0.0118)	(0.0124)	(0.0185)	(0.0019)	(0.0074)	(0.0152)
bw = .8	0.0218*	0.0115	-0.0018	-0.0000	0.0068	0.0349**
	(0.0127)	(0.0134)	(0.0202)	(0.0021)	(0.0080)	(0.0164)
bw = .7	0.0234*	0.0126	0.0004	-0.0003	0.0045	0.0281
	(0.0137)	(0.0145)	(0.0218)	(0.0022)	(0.0085)	(0.0174)
bw = .6	0.0292**	0.0174	0.0074	-0.0018	0.0088	0.0195
	(0.0148)	(0.0158)	(0.0234)	(0.0024)	(0.0092)	(0.0189)
bw = .5	0.0367**	0.0252	-0.0057	-0.0027	0.0138	0.0217
	(0.0169)	(0.0179)	(0.0268)	(0.0026)	(0.0100)	(0.0208)
bw = .4	0.0256	0.0116	0.0033	-0.0025	0.0228**	0.0240
	(0.0182)	(0.0192)	(0.0288)	(0.0030)	(0.0105)	(0.0225)
bw = .3	0.0326	0.0152	-0.0225	-0.0026	0.0167	0.0251
	(0.0211)	(0.0225)	(0.0381)	(0.0034)	(0.0126)	(0.0283)
	Households below tax threshold in 2009					
bw = .9	0.0197*	0.0109	-0.0051	-0.0006	0.0009	0.0079
	(0.0116)	(0.0115)	(0.0170)	(0.0018)	(0.0056)	(0.0143)
bw = .8	0.0215*	0.0127	0.0005	-0.0001	0.0006	0.0082
	(0.0124)	(0.0124)	(0.0187)	(0.0020)	(0.0061)	(0.0152)
bw = .7	0.0174	0.0084	0.0030	-0.0005	0.0057	0.0109
	(0.0131)	(0.0130)	(0.0194)	(0.0021)	(0.0065)	(0.0155)
bw = .6	0.0182	0.0081	0.0120	-0.0008	0.0083	0.0232
	(0.0144)	(0.0143)	(0.0219)	(0.0023)	(0.0074)	(0.0174)
bw = .5	0.0111	0.0012	-0.0006	0.0005	0.0103	0.0400**
	(0.0156)	(0.0155)	(0.0228)	(0.0024)	(0.0081)	(0.0180)
bw = .4	0.0192	0.0072	-0.0159	0.0026	0.0012	0.0208
	(0.0171)	(0.0169)	(0.0247)	(0.0027)	(0.0089)	(0.0202)
bw = .3	0.0194	0.0057	-0.0098	0.0028	-0.0002	0.0232
	(0.0187)	(0.0186)	(0.0278)	(0.0030)	(0.0094)	(0.0215)

Table A.9: First Stage Effects on Wealth Tax Outcomes: Using Distance in KM

This table provides the reduced-form using scaled border distance as the geographic measure. Column (1) considers the tax value of housing, as observed in tax returns. Column (2) considers the effect on being above the wealth tax threshold. Column (3) considers the effect on the marginal rate-of-return, by isolating extensive margin effects from wealth taxation. This is done by defining the dependent variable to be $-\tau_t \mathbb{1}[TNW_{i,t} > Threshold_t]$. Column (4) examines the effect on the amount above the wealth tax threshold, $\mathbb{1}[TNW_{i,t} > Threshold_t](TNW_{i,t} - Threshold)$. Column (4) considers isolates the effect of increased wealth taxation on the average rate-of-return. This is done by defining the dependent variable as $-\tau_t \mathbb{1}[TNW_{i,t} > Threshold_t](TNW_{i,t} - Threshold)/TNW_{i,t}$, which is evaluated as 0 if $TNW_{i,t} \leq 0$. pp is short for percentage points, and indicates that coefficients (SEs) are multiplied by 100. Standard errors, provided in parenthesis, are clustered at the census tract level.

		Extensive Margin		Extensive and intensive margin	
	$\log(TaxVal)$	$\mathbb{1}[TNW > Threshold]$	$r^{marginal}$	AmountAbove	$r^{average}$
	(1)	(2)	(3)	(4)	(5)
	Full sample				
$\log(\widehat{TaxVal})$	0.872693***	0.259342***	-0.002724***	865278***	-0.001898***
	(0.030300)	(0.012781)	(0.000133)	(87379)	(0.000082)
$F(\hat{\beta} = 0)$	830	412	419	98	539
	Households above tax threshold in 2009				
$\log(\widehat{TaxVal})$	0.906123***	0.157144***	-0.001654***	1158019***	-0.001737***
	(0.041042)	(0.014319)	(0.000149)	(135316)	(0.000103)
$F(\hat{\beta} = 0)$	487	120	123	73	284
	Households below tax threshold in 2009				
$\log(\widehat{TaxVal})$	0.827092***	0.386843***	-0.004059***	375749***	-0.002017***
	(0.040108)	(0.020063)	(0.000209)	(58352)	(0.000115)
$F(\hat{\beta} = 0)$	425	372	378	41	309
Border Distance Controls – KM	Yes	Yes	Yes	Yes	Yes

B.6 Effect on municipal finances

Households in high taxation municipalities may see the negative income effect partially offset by a higher provision of public goods or a lowering of municipal fees. While this may generally be a cause for concern, I argue that this effect is likely negligible in my empirical setting for the following key reasons: First, wealth taxes are disproportionately paid by the very wealthy, who were not disproportionately affected by this reform given that the housing wealth accounts for a very small fraction of net worth among the very wealthy.[47] Thus, changes in tax assessments are not likely to lead to meaningful changes in the aggregate amount of wealth tax revenues in a given municipality. In addition, wealth taxes only account for 10% of aggregate municipal tax revenues, a share that drops to only 4% of when considered relative to aggregate municipal total incomes. Finally, due to the government's revenue equalization scheme, increasing per capita

[47]See Fagereng, Guiso, Malacrino, and Pistaferri (2018)

tax revenues by 1 NOK lowers transfers from the central government by 0.6 NOK. Therefore, even if wealth tax revenues do change, the effect on local public services would be likely muted, due to a limited effect on municipality finances. Calculations that I present below, suggest that a municipality where assessed tax values of housing are 0.5 log points higher will have 0.26% more revenue.[48] Thus any reasonable bounds on household sensitivity to municipal finances suggest that the effect will be negligible.

B.7 Communication of policy change

The implementation of a new methodology to assess housing wealth was primarily communicated through a letter sent to all home-owners in August of 2010. The letter was titled "Information for the calculation of new tax values for residential properties,"[49] and provided registered information about the house, namely structure type, construction year and size. Home-owners were asked to verify and possibly correct this information, either by via mail or online. At the same time, "tax calculators" were made available online on the tax authorities' website, where households could enter the characteristics of their home and see their estimated new tax value. This tax value differed somewhat from the actual assessed values, since the online calculators used pricing coefficients based on 2004–2008 transaction data, while the final assessment for 2010 used coefficients based on 2004–2009 data. The fact that a new assessment methodology was introduced was therefore salient, and the effect on a household's wealth tax base (TNW) readily available already in the early fall of 2010. On December 15th 2010, preliminary tax information ("tax cards") was sent out to all tax payers, containing estimated taxes to be paid for that year, which included the new housing assessment and TNW. Households should thus have been aware of the financial impact of the new assessment methodology before Christmas of 2010, at the latest.

On the tax authorities' website, they inform that tax values are assessed as the size of the home multiplied with a price-per-square meter coefficient, which is based on Statistics Norway's real estate transaction statistics:"Boligens boligverdi er lik boligens areal multiplisert med kvadratmeterpris basert påstatistikk over omsatte boliger." (March 2019). See the tax

[48]I utilize the distribution of wealth tax payers from SSB (https://www.ssb.no/statbank/table/08231/tableViewLayout1/), and assume that this distribution holds for all municipalities. In my empirical setting, 0.5 log point increase in $TaxVal$ increases the amount subject to a wealth tax by 478,000 for households initially above the wealth tax threshold. This increases wealth tax payments by approximately 5,000. Using the distribution of wealth tax payers, I increase everyone's tax payments by 5,000, and find an increase in total tax payments of 25%. Assume that this occurred in one municipality, but not it's neighbor. Since the municipal share of the wealth tax is only 64%, the high-side municipality now has 0.64*0.25=16% more wealth tax revenue. The wealth tax's share of tax revenue is 10%. Thus the high-side will have 1.6% more tax revenue, but only 1.6% * 40% = 0.64% more total revenue, since tax revenues account for 40% of total incomes on average. Only 40% of this difference will pass through after applying the government revenue equalization scheme, leaving only. 0.26% more revenue for the high assessment side municipality.

[49]My own translation.

authorities' website. There are no details provided on the exact methodology.

B.8 Calculating distances

For each household i, d_i^U,is the signed distance, in meters, to the nearest residential building in a different municipality (or within-city district), m_i', with which m_i shares a border which is not in the ocean or a fjord.[50,51] In the following, m is referred to as a municipality, where, for purposes of brevity, I denote within-city districts in the largest 4 cities as municipalities. Household i is then assigned to the border area $b_i = m_i \cup m_i' = m_i' \cup m_i$. Household i is defined to be in the low assessment side, if the average household in b_i would receive a lower assessment, were it assessed as if it were in m_i. If the assessment would be higher in m_i, household i lives on the high assessment side. If they are equal, the household (and all households in this border area) is dropped from the sample. If household i lives in the low (high) assessment side, d_i^U is signed to be negative (positive).

For all municipal pairs, m, m', distance between their housing centroids is calculated. A centroid is the vector average of the coordinates of all owner-occupied houses in a municipality. This disregards which b_i you belong to. This number then enters in the denominator when scaling d_i^U to get the scaled (and signed) border distance, d_i.

B.9 Two-period model with endogenous labor supply

Consider the modified household optimization problem. $L \geq 0$ is hours worked in period 1, W is the exogenous hourly wage, $\frac{1}{\nu} > 0$ is the Frisch Elasticity, and $\psi > 0$ is the (dis)utility weight on labor supply. Y_1, and Y_2 are the exogenous incomes in periods 1 and 2, respectively.

$$\begin{aligned}
\max_{C_1,C_2,S,L} \quad U(C_1,C_2,S,L) &= \frac{1}{1-\gamma}C_1^{1-\gamma} - \psi\frac{L^{1+\nu}}{1+\nu} + \beta\frac{1}{1-\gamma}C_2^{1-\gamma} \\
\text{s.t.} \quad C_1 + S &= Y_1 + LW \\
\text{and} \quad C_2 &= Y_2 + \tilde{R} + \tilde{V}
\end{aligned} \tag{32}$$

The first order conditions with respect to S, together with the budget constraints, imply that:

[50] All distances are calculated as the euclidean distance between coordinate vectors measured in meters. This assumes that the world is flat, which seems like a reasonable approximation for distances between neighboring municipalities. This assumption speeds up the time it takes to calculate a large combination of distances required to find the nearest household in a different municipality.

[51] For computational reasons, only one building per 100^2-square-meter square in neighboring municipalities is kept, and the search is conducted over a grid of the coordinates of these "representative" buildings.

$$S = \frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}(Y_1 + LW)}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}} - \frac{Y_2 + \tilde{V}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}} \tag{33}$$

The first order conditions with respect to L, together with the budget constraints, imply that:

$$dL \cdot W = \frac{\gamma[Y_1 + LW - S]^{-\gamma-1}W^2}{(\psi\nu L^{\nu-1} + \gamma[Y_1 + LW - S]^{-\gamma-1}W^2)} dS \equiv f dS \tag{34}$$

Since $\psi\nu L^{\nu-1} > 0$, and $C_1 = Y_1 + LW - S > 0$, this implies that the labor earnings response is a fraction, $f \in (0, 1)$, of the savings response to rate-of-return shocks.

I now totally differentiate equation 33 with respect to $\tilde{R}$, and substitute $dL \cdot W$ with the expression in equation 34, and solve for $dS/d\tilde{R}$ to get:

$$\frac{dS}{d\tilde{R}} = \left(1 - f\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right)^{-1} \left((Y_1 + LW)\frac{1-\gamma}{\gamma}\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2} + (Y_2 + \tilde{V})\frac{1 + \frac{\beta}{\gamma}[\beta\tilde{R}]^{\frac{1}{\gamma}-1}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2}\right) \tag{35}$$

I then totally differentiate 33 with respect to $\tilde{V}$, and substitute $dL \cdot W$ with the expression in equation 34, and solve for $dS/d\tilde{V}$ to get:

$$\frac{dS}{d\tilde{V}} = \left(1 - f\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right)^{-1} \left(-\frac{1}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right) \tag{36}$$

The expressions for $dS/d\tilde{R}$ and $dS/d\tilde{V}$ are qualitatively similar to the case without endogenous labor supply, but are scaled up (in magnitude), since $(1 - f[\beta\tilde{R}]^{\frac{1}{\gamma}}/(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}))^{-1} > 1$. The expression for the rate-of-return sensitivity in equation 35 now also contains labor earnings as period 1 income. There is therefore no change to the qualitative conclusions in the previous section. The new conclusion is that the effect on labor earnings should be of a same sign as, but smaller in magnitude than, the effect on savings.