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

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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 a boundary discontinuity approach together with third-party reported data on saving behavior. I find that wealth taxation causes households to save more, and that this increase in saving is primarily financed by increased labor earnings. These responses are the combination of small negative effects of increasing the marginal tax rates on wealth and relatively larger positive effects of increasing average tax rates. These results imply that income effects may dominate substitution effects in household responses to rate of return shocks, which has important implications for both optimal capital taxation and macroeconomic modeling.

Keywords: Wealth Taxes, Savings, Capital Taxation, Intertemporal Substitution
JEL: G51, D14, D15, H20, H31, E21, J22

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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 2020, 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 different transmission channels (Auclert 2019, Wong 2019, Caramp and Silva 2020). It is also informative of whether saving responses dampen or amplify downward movements in the natural interest rate (Mian, Straub, and Sufi 2021, Summers and Rachel 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 that involve 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. Identifying variation in the after-tax rate of return is scarce. Even exogenous shocks to the interest rate may have general equilibrium effects that inhibit 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 presents important problems related to both identification and measurement. 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 and tax evasion. This is problematic, since evasion responses are uninformative of responses to other rate-of-return shocks, such as interest rate changes, or even capital taxation when evasion opportunities are limited.

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, 2020) 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 described above. The source of identifying variation

[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 cutoff to around 0.45 in my setting (Section 5).

in the net-of-tax rate of return comes from capital taxation in the form of an annual net-wealth tax. While wealth taxation and capital income taxation are equivalent in standard models,[3] wealth taxation differs from traditional capital income taxation by requiring regular assessments of the stock of capital. The steps 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 that exceeds a given threshold. The relatively low threshold subjects 12 to 15 percent of taxpayers to the wealth tax,[4] and primarily affects households in the top 10% of the life-time income distribution (Halvorsen and Thoresen, 2021). The main components of the tax base are financial wealth and housing wealth. While financial wealth may be assessed at third-party reported market values (which limits the 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 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 novel 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 0.038. 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 almost three times larger than what is necessary to maintain the same level of wealth after taxes are collected, consistent with households increasing their savings to offset future wealth tax payments.

My findings indicate that this increase in saving is primarily financed by increases in labor supply. Corresponding to the geographic discontinuities in wealth tax exposure, I find clear discontinuities in household labor earnings. These discontinuities constitute novel evidence of a meaningful cross-elasticity between labor supply and the net-of-tax return on capital. Translating these estimates into an earnings propensity, I find that households increase their after-tax earnings by around 0.027 for each additional NOK subjected to the wealth tax, enough to finance a majority of the additional saving.

I also present new evidence on the effect of capital taxation on portfolio allocation. I first consider the effect on the share of financial wealth invested in the stock market. The perhaps

[3]This includes Chamley (1986) and Judd (1985). For further discussion, see, e.g., Bastani and Waldenström (2018), Scheuer and Slemrod (2021), Guvenen, Kambourov, Kuruscu, Ocampo, and Chen (2019), and Lu (2021)

[4]This refers to the years 2010–2015, which is the time period 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 (Saez and Stantcheva, 2018)

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. The alternative view is that households respond to a drop in the after-tax return by "reaching for yield" or capital incomes, which may entail substituting low-interest deposits for higher-return stock holdings (see, e.g., Lian, Ma, and Wang 2019; Daniel, Garlappi, and Xiao 2021; Campbell and Sigalov 2021). Consistent with this ambiguity, I find no effect on the risky share and can rule out economically large effects. I further present the hypothesis that the adverse income effect of increased taxation may induce households to enjoy less financial leisure, in the sense that they exert greater effort toward financially optimizing the returns they receive on their low-risk savings. My findings, however, lend meager support to this hypothesis. For the average household, there is little effect on realized interest rates on deposits, which is the dominant form of risk-free saving.

I proceed by using a simple life-cycle model to illustrate which values of the EIS can rationalize my empirical findings. This exercise shows how both the saving and labor earnings responses are determined by the EIS. Depending on how preferences for leisure are parameterized, my point estimates are consistent with an EIS between 0.02 and 0.15. When the EIS exceeds 0.5, the life-cycle model produces positive saving and labor supply responses that are outside of the 95% confidence intervals of my empirical findings.

The theoretical implication of my main findings on saving and labor supply responses is that income effects dominate intertemporal substitution effects. The positive income effects associated with increasing the average tax rate on wealth (ATR) must be larger in magnitude than the negative substitution effects caused by increasing the marginal tax rate (MTR). However, recent research shows that consumers may suboptimally confuse marginal and average prices (Ito, 2014). If this applies to taxes as well, then traditional approaches to modeling non-linear, progressive taxation would be fundamentally misguided. In light of this, I test whether households respond to marginal and average tax rates as theory would prescribe. I use an instrumental-variables framework that exploits the fact that assessment discontinuities had differential effects on ATRs and MTRs depending on households' ex-ante characteristics, such as initial taxable wealth. My findings are consistent with the underlying mechanism of the life-cycle model: I estimate positive ATR effects that dominate weaker, negative MTR effects.

This paper contributes to multiple literatures. First, I contribute to the new literature providing a rich picture of behavioral responses to capital taxation (see, e.g., Boissel and Matray 2021; Glogowsky 2021; Lavecchia and Tazhitdinova 2021; Martínez-Toledano 2020; Bjørneby, Markussen, and Røed 2020; Bach, Bozio, Guillouzouic, and Malgouyres 2020). A central finding is that wealth taxation reduces the amount of taxable wealth that households report (Zoutman 2018; Seim 2017; Londoño-Vélez and Ávila-Mahecha 2020a; Londoño-Vélez and Ávila-Mahecha 2020b; Jakobsen, Jakobsen, Kleven, and Zucman 2020; Durán-Cabré, Esteller-Moré, and Mas-Montserrat 2019; Brülhart, Gruber, Krapf, and Schmidheiny 2019). However, these findings do not necessarily imply that wealth taxes cause households to save less, as evasion responses may dominate (real) saving responses.[6] Consistent with this ambiguity, I find strikingly different

[6] Jakobsen et al. (2020) note that their estimated elasticities may be a combination of real, avoidance, and evasion responses. Zoutman (2018) writes that the immediate responses he observes are unlikely to indicate real adjustments to savings. Seim (2017) complements his reporting (bunching) analysis with a DiD design

effects when limiting the role for evasion by (i) focusing on savings in the form of financial wealth, which in Norway is primarily third-party reported, and by (ii) obtaining identifying variation in wealth tax exposure from below the top 1%, where evasion is less prominent.[7]

My central contribution to this literature is to emphasize the real responses to capital taxation, which are crucial for informing optimal taxation. While evasion and intertemporal substitution both reduce tax revenues, their *combined* effect cannot alone inform micro-founded models of optimal taxation. This is because tax enforcement may reduce evasion, but not households' preferences for intertemporal substitution. My paper thus complements empirical work on evasion behavior in providing the necessary distinct moments to inform models where these different margins of adjustment are modeled separately (see, e.g., Rotberg and Steinberg 2021). This contribution is strengthened by providing new evidence on how labor supply is affected, which is a key parameter in optimal taxation (see, e.g., Atkinson and Sandmo 1980). My paper is also the first to empirically decompose MTR and ATR effects. This allows me to estimate the uncompensated elasticities that are needed for optimal taxation without relying on the assumption of no income effects (Saez and Stantcheva 2018).

By examining real saving responses to (net-of-tax) rate-of-return shocks, I also contribute to the empirical literature that considers the sensitivity of saving (see, e.g., Boskin 1978 and Beznoska and Ochmann 2013) or debt accumulation (Cespedes 2019; Fagereng, Gulbrandsen, Holm, and Natvik 2021) to the net-of-tax interest rate. Nevertheless, Saez and Stantcheva (2018) consider there to be a "paucity of empirical estimates" that can be used to inform optimal capital taxation. Finally, since the outcomes I consider are tightly connected to the Elasticity of Intertemporal Substitution, I contribute to the diverse empirical literature aimed at estimating it (see, e.g., Attanasio and Weber 1995; Gruber 2013; Vissing-Jørgensen 2002; Bonaparte and Fabozzi 2017; Crump, Eusepi, Tambalotti, and Topa 2015; Cashin and Unayama 2016). The EIS needed to rationalize my findings is in the lower range of EIS estimates reviewed by Havránek (2015). However, my evidence is consistent with the modest intertemporal substitution effects found in recent quasi-experimental work by Best, Cloyne, Ilzetzki, and Kleven (2020). Their estimated EIS of 0.1 produces treatment effects well inside the confidence intervals of my empirical findings.

Relative to this combined body of work, I make two main contributions. The first is to provide micro-level evidence and to do so by comparing households who are similar on socioeconomic observables. While 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 net-of-tax returns

that exploits shifts in the wealth tax threshold. He finds no evidence of real responses, but does not provide (confidence intervals on the) implied savings elasticities comparable to my results. Using supplemental survey data on a smaller subset of their sample, Brülhart, Gruber, Krapf, and Schmidheiny (2021) find no indication that the effect on reported wealth is driven by saving responses. See Advani and Tarrant (2021) for a review.

[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 et al., 2020). Alstadsæter et al. (2019a) show that wealth tax evasion primarily occurs above the 99th percentile of the wealth distribution.

by using differential tax treatment that arises from differences in characteristics such as wealth, income, and asset shares. My second contribution is to provide evidence on how shocks to the net-of-tax rate of return affect portfolio decisions. This has received little empirical attention, despite its importance for economic modeling. By showing how (i) the risky share of financial wealth and (ii) the realized returns on risk-free assets are (un)affected by rate-of-return shocks, I directly assess the validity of treating returns as exogenous in partial equilibrium.

This paper has implications for the growing literature on 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.

This paper is further related to the literatures surrounding property taxation and housing wealth effects on labor supply (see, e.g., Zator 2020; Zhao and Burge 2017; Li, Li, Lu, and Xie 2020; Atalay, Whelan, and Yates 2016; Disney and Gathergood 2018; Wong 2020; Fu, Liao, and Zhang 2016). This literature finds that households do in fact respond to reductions in their (net-of-tax) housing wealth by supplying more labor, which is at odds with the common finding of immaterial income effects in labor supply decisions (see, e.g., Gruber and Saez 2002; Kleven and Schultz 2014; and the discussion in Giupponi 2019). Importantly, this literature does not speak directly to how labor supply is affected by a net wealth tax. This is because wealth taxation lowers the marginal tax rates on savings, which produces intertemporal substitution effects. Whether intertemporal substitution effects dominate the income effects on labor supply is an open question. My contribution is to show that they do not, which suggests a broader applicability of the findings in this literature.

Finally, this paper contributes to the literature that employs pooled boundary discontinuity designs (see, e.g., Black 1999 and Bayer, Ferreira, and McMillan 2007). In this literature, it is common to pool multiple geographic discontinuities that have varying first-stage effects. Yet, there is no established approach that facilitates a graphical representation of the resulting BDD estimates.[8] My new, simple semi-parametric approach (i) exploits all identifying variation while facilitating standard regression discontinuity design plots and (ii) directly addresses the fact that potential confounding unobserved heterogeneity is correlated with treatment intensity. This methodology has applicability in settings that incorporate treatment discontinuities whose first-stage effects vary mechanically varies with observables, such as geographic location.

The paper proceeds as follows. Section 2 discusses the institutional features and assessment model for housing wealth. Section 3 introduces the data, the identification strategy, and the reduced-form specification. Section 4 the presents the main, reduced-form findings. Section 5 uses a simple life-cycle model to illustrate the relationship between my empirical findings and the EIS. Section 6 present results from the instrumental variables methodology that distinguishes between MTR and ATR effects. Section 7 concludes.

[8]One approach is to discard identifying variation on the intensive margin and do a graphical, binary comparison of low- and high-treatment sides of boundaries (Bayer, Ferreira, and McMillan 2007).

2 Institutional Details

In Norway, wealth taxes are assessed according to the following formula:[9]

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

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. This threshold gradually rose from NOK 470,000 (USD 78,000) to NOK 1,200,000 (USD 208,000) during 2010–2015.[10] Since wealth levels grew over the same period, the over-all fraction of households paying a wealth tax remained relatively stable at 12-15%. The tax rate, τ, was 1.1% during 2009–2013, 1% in 2014, and 0.85% in 2015.[11]

The wealth tax base, 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).[12] The market value of all financial assets held through or borrowed from domestic financial institutions are third-party reported each year. The tax value of unlisted stocks is reported directly by the stock issuer as part of their financial reporting to the tax authorities. These numbers are pre-filled onto households' tax returns. The tax is assessed on individuals, but married couples are free to shuffle assets and liabilities between them, which effectively taxes married households on the sum of their taxable net wealth in excess of two times the wealth tax threshold.

The wealth tax formula in equation (1) provides a good starting point to illustrate the gist of my empirical setting. In this paper, I identify effects from quasi-random variation in $TNW_{i,t}$ that arises 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 net-of-tax return by τ_t or, equivalently, increases the MTR by τ_t. The presence of a wealth tax threshold is a crucial ingredient in this empirical setting. It allows quasi-random variation in the assessment of *housing* wealth to provide variation in the marginal return on *financial* wealth. By affecting $wtax_{i,t}$, the identifying variation in $TNW_{i,t}$ also affects the average tax rate (ATR) on wealth, defined as $wtax_{i,t}/TNW_{i,t}$.

2.0.1 A Hedonic House Price Model with Built-in Discontinuities

In 2010, the Norwegian tax authorities implemented a major change to how they assess the housing wealth component of TNW. Prior to 2010, assessed housing wealth was set to an inflated

[9] See subsection A.1 for how this formula is implemented in the data.

[10] Assumes the 2010 USD/NOK exchange rate of around 6.

[11] 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%.

[12] This assessment discount is the reason why only 12-15% of households pay a wealth tax despite the relatively low wealth tax thresholds. 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.

multiple of the initial tax assessment, which typically corresponded to 30% of construction cost.[13] This approach grew unpopular, because some regions experienced larger house price growth than others. This produced 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. The implementation of a new assessment methodology was communicated to homeowners in a letter sent out in August 2010. I describe this communication in more detail in Section B.6.

Using a large national dataset on property transactions during 2004–2009, the hedonic pricing model was estimated according to equation (2) below.[14]

$$\begin{aligned} \log\left(\frac{Price_i}{Size_i}\right) &= \alpha_{R,s} + \gamma_{Z,s} + \zeta^{size}_{R,s}\log(Size_i) + \zeta^{Dense}_{R,s} DenseArea_i \\ &+ \zeta^{Age}_{1,R,s}\mathbb{1}\{Age_i \in [10,19]\} + \zeta^{Age}_{2,R,s}\mathbb{1}\{Age_i \in [20,34]\} + \zeta^{Age}_{3,R,s}\mathbb{1}\{Age_i \geq 35\} + \varepsilon_i \end{aligned} \tag{2}$$

where $Price$ is the recorded transaction price and $Size$ is the size of the house in square meters. $DenseArea$ is a dummy for whether the dwelling was located in a cluster of at least 50 housing units. Age is the number of years since construction. As the subscripts indicate, the equation is estimated separately for each of the three structure types, $s \in$ {Detached, Non-detached, Condominium}, and for each region, R. A region is either one of the 20 Norwegian counties or one of the four largest cities (Oslo, Bergen, Stavanger, and Trondheim).[15] Municipalities, or within-city districts for the largest four cities, were assigned to within-region price zones, Z, separately for each structure type-region combination.[16] These price zone fixed effects, $\gamma_{Z,s}$, make up a key component of the pricing model.

All of the estimated coefficients from a total of 44 regressions are provided in regression output form (see Figure A.5 for an example). These coefficients were then provided to the tax authorities, who applied the estimated coefficients to data from real estate registers and homeowner-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 a given residence:

[13] The tax value of a house would first enter at construction cost. Then each year the tax value is changed by some percentage; e.g., -5%, 0%, 10%. The practice of using initial construction cost is described in the government budget of 2010 (FINDEP, 2009). These yearly changes provide Berzins, Bøhren, and Stacescu (2020) with a novel source of identifying variation in shareholder liquidity that they use to examine the effects of wealth-tax induced adverse liquidity shocks on firm financing and real outcomes.

[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 were used. When actual tax values were assigned, 2009 data was included.

[15] For non-detached housing and condominiums, for which there were fewer transactions, some counties were combined, presumably to increase sample size in each regression.

[16] Municipalities were assigned to price zones depending on "analyses of past price levels" (my translation of a comment in the 2009 pricing-model report), 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 within that region, regardless of geographic proximity. (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.)

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

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. The estimated house value enters at a discounted rate of 0.25.

We can use equations (2) and (3) to write $\log(\widehat{TaxVal}_i)$ as

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

From this, we see that tax assessments will be geographically discontinuous even if past transaction prices are truly smooth. This implies 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. For a given structure type, s, the geographic variation within a region, R, comes from $\hat{\gamma}_{R,Z,s}$. Across regions, all of the estimated coefficients change. This provides identifying variation that depends on structure characteristics, such as $Size$ and Age. In Section 3.2, I discuss how I exploit (and isolate) all of this geographic variation.

I collect all the data necessary to replicate the assessed house values from Statistics Norway's reports. In subsection A.3 in the Appendix, I show how using these coefficients and the real estate registers allows me to accurately predict assessed tax values observable in household tax returns. Keeping housing characteristics fixed, Figure 1 shows how a standard house would be assessed in different municipalities.

3 Data and the Empirical Specification

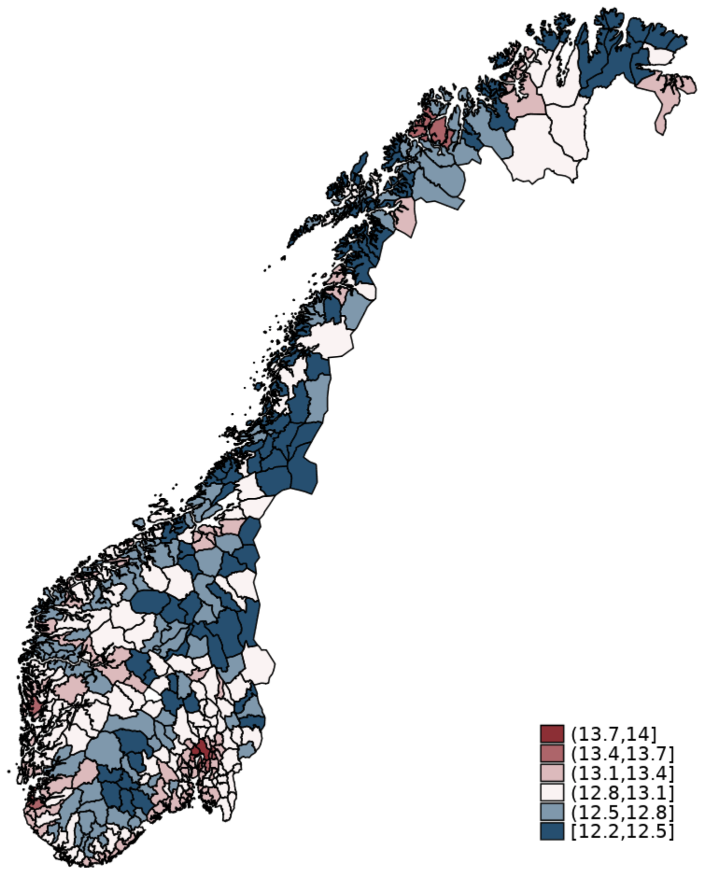
3.1 Identification

I identify household responses to an increased exposure to wealth taxation caused by higher tax assessments on housing. By focusing on households who made their residential choices prior to the development and implementation of the new pricing models, selection into a given tax treatment is not a concern. However, since treatment is assigned based on 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. Furthermore, housing wealth is highly correlated with other important determinants of saving behavior 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 exploit the fact that treatment varies discontinuously at geographic boundaries, which allows me to remove the effects of potential confounders that vary smoothly.

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 that is assessed as if it were located inside one of the municipalities below. These hypothetical assessments come from applying the coefficients from the tax authorities' hedonic pricing model to equation (4). Each distinct (shade of) color corresponds to a bin of $\widehat{TaxVal}$ with a width of 0.3 log-points. I assume a house size of 130 m^2 (1,400 square ft), an age of 25–34 years, and a location in an area defined as densely populated. 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.



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. In addition, my parameterization of the BDD regression equations must not confuse smooth variation with geographic discontinuities. This is not straightforward in a setting with treatment discontinuities that vary across boundaries. In the next subsection, I describe my parameterization in more detail. Throughout the results section, I show that a *correctly* specified BDD framework does not pick up any discontinuities in potential confounders such as housing wealth, pre-period income, or financial wealth. The fact that the identifying variation in my BDD framework is essentially orthogonal to household observables allows me to include a wide range of controls without reducing the amount of residual identifying variation.

A 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.3 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.

Relatedly, some Norwegian municipalities also levy a property tax on residential homes. This is problematic to the extent that municipalities apply the tax authorities' assessment methodology. Fortunately, municipalities were not allowed to use the tax authorities' assessment values until the very end of my sample period, and only a small subset, which I remove from the sample, opted to do so (see the discussion in Section B.4 in the Appendix). The main results are virtually unaffected by removing these observations, as well as controlling for municipal property tax rates.

3.2 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.[17]

I will refer to boundary areas, b, as sets of households assigned to the same municipal boundary. Within a boundary area, b, households are defined as being on the high-assessment side if the average household within that boundary would see a higher tax assessment on that side.[18] 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 $\mathbf{H}_i = \{log(Size)_i,\ DenseArea_i,\ \{\mathbb{1}[Age_i \geq a], a = 0, 10, 20, 35\}\}$ 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.

$$\Delta_i \equiv \log(\widehat{TaxVal_i})\Big|_{d_i>0} - \log(\widehat{TaxVal_i})\Big|_{d_i<0} \tag{5}$$

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.

$$y_{i,t} = \beta \log(\widehat{TaxVal_i}) + g_b(\mathbf{c}_i)\Delta_i + \delta'_{b,s}\mathbf{H}_i + \gamma'_t\mathbf{X}_i + \varepsilon_{i,t}. \tag{6}$$

[17] 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.

[18] Within a boundary area, a municipality is defined as being on the high-assessment side if 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 are no differences for single family homes, i.e., they are in the same price region and price zone, I conduct 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 $\mathbb{1}[d_i > 0]\Delta_i$. $\hat{\beta}$ thus identifies the effect on households on the high assessment side of the boundary ($d_i > 0$) of seeing a Δ_i log-point increase in $\widehat{TaxVal}$.

While the estimator for β identifies the effect of a *discontinuous loading* on Δ_i, the estimated coefficient on $g_b(\mathbf{c}_i)\Delta_i$ is meant to capture the effect of anything that loads continuously on Δ_i. I describe the parameterization of $g_b(\cdot)$ 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, denoted by $\mathbf{X}_i$, which 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]), a dummy for whether any of the adults have college degree, a debt dummy, log(Debt), the share of GFW invested in the stock market [SMW], the log of the previous tax-return-observed assessed tax value of housing [$TaxVal$], a dummy for whether the tax returns indicate ownership of other real estate as well as the log of the assessed value, and finally a dummy for whether the household owns non-listed stocks [PE Dummy].

I note that while my specification allows the effect of geographic discontinuities in the estimated coefficients to covary with $\mathbf{H}_i$ (per the definition of Δ_i), estimating border-area-specific coefficients on $\mathbf{H}_i$ accounts for the fact that those with larger houses in higher-priced areas may have have different (unobservable) characteristics.

Observations are pooled by treatment period, where the pre-period is 2004–2009 and the post-period is 2010–2015. Equation (6) is then estimated separately for these periods, allowing the slopes without a t subscript to vary by treatment period. While the initial hypothesis is that the assessment discontinuities are not predictive of differences in pre-period characteristics, outcome variables, $y_{i,t}$, are generally differenced, which accommodates this possibility. The main outcome variable that captures saving responses is log-differenced financial wealth. This differencing effectively takes out household fixed effects in the amount of (financial) wealth they hold. This growth rate in financial wealth is not itself (double-) differenced, which is inline with previous papers using the (nondifferenced) log of wealth as the outcome variable while estimating household fixed effects (see, e.g., Jakobsen et al. 2020).

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).[19]

My main specification imposes equal weights on all observations and clusters at the census-

[19] 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 (2020a). There are only negligible differences for similar changes in the main outcome variables. For example, a change in 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.

tract level (*grunnkrets*). I provide results using triangular (distance-based) weights for my main results in Table B.3 in the Appendix. Results using different levels of clustering (household and municipality) are reported in Table B.4. Neither standard errors nor estimates are sensitive to these specifications.

Addressing continuous geographic loading on Δ_i. My method for capturing 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 heterogeneous loading on Δ_i. Similar to Dell (2010), I test multiple such specifications. The baseline specification involves controlling for (signed) border distance in kilometers:

$$g_b(\mathbf{c}_i) = \gamma^- \mathbb{1}[d_i < 0]d_i + \gamma^+ \mathbb{1}[d_i > 0]d_i, \tag{7}$$

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 Section A.2 in the Appendix. 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.[20] 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.[21]

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 socioeconomic controls, $\mathbf{X}_i$, from the main specification in equation (6). The coefficient of interest is β.

$$y_i = \beta \log(\widehat{TaxVal_i}) + g_b(\mathbf{c}_i)\Delta_i + \delta'_{b,s}\mathbf{H}_i + \varepsilon_i. \tag{8}$$

3.2.1 Empirical specification relative to the BDD literature

The similarity between my empirical specification and that of the existing BDD literature (e.g., Black 1999; Bayer, Ferreira, and McMillan 2007; Livy 2018; Harjunen, Kortelainen, and Saarimaa 2018) that incorporates cross-boundary area variation in treatment intensity can be seen by acknowledging that $\mathbb{1}[d_i > 0]\Delta_i$ in equation (6) could be replaced with $\log(\widehat{TaxVal})$ and I would 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 standard Regression Discontinuity Design

[20] This measure is the distance between the two centroids of the two municipalities (or within-city districts) whose residents occupy a given border area, b. Centroids are municipality (or within-city district)-specific, and do not vary for households within a municipality. The centroid distance measure is b-specific, and thus depends on which border is the closest.

[21] Households at $RelLoc \in [-1, 1]$ must travel (as the crow flies) $RelLoc \cdot X$ km farther 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 low and high sides.

representation of estimates.[22]

Beyond this graphical contribution, my approach differs from the existing approach in how it deals with potentially confounding geographic heterogeneity. First, my approach differs by directly 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 use a specification similar to the baseline regression specification in equation (6) without directly controlling for geographically smooth heterogeneity, but rather *uniformly* reduce the cutoffs (bands) that determine which observations, i, would be included, based on d_i alone.

Applying the traditional approach to my empirical setting would entail comparing households whose d_i's were similar in order to limit the potential influence of confounders that covary with d_i. To preserve identifying variation one would then consider households whose d_is are close to the treatment cutoff of 0. This approach would be unsatisfactory to the extent that confounders vary more rapidly in areas where treatment discontinuities are larger. For example, we may plausibly expect that socioeconomic differentials are increasing in estimated house prices differences. If this is the case, then imposing uniform cutoffs implies that the boundary areas that offer the most identifying variation will also have the most dissimilar control group. My approach directly addresses this concern.

Second, my approach differs from the traditional approach by addressing—rather than discarding—geographic heterogeneity in residential density. Addressing heterogeneity in density is important whenever potential confounders may change more rapidly, in a geographic sense, in denser areas. My solution to address 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 in order 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.

3.3 Data

I combine a wide range of administrative registers maintained by Statistics Norway. These 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. (2020).

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, and listed stocks. They also include the sum of household liabilities. I can further distinguish between third-party reported domestic wealth holdings (e.g., domestic deposits) and self-reported foreign holdings of real estate, deposits, and other securities, separately. The tax data include a breakdown of household income, such as self-employment income, wage earnings, pensions,

[22] Prior 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 cutoff, but their main estimation strategy leverages the full identifying variation, which allows treatment discontinuities to vary across border areas. Jakobsen and Søgaard (2020) make a similar contribution by providing a clever graphical framework to analyze the effects of threshold-related variation in marginal income tax rates.

UI income, and the sum of government transfers. They also contain a detailed breakdown of capital income, such as interest income from domestic or foreign deposits, and realized gains or realized losses. These data span 1993 to 2015.

Real estate data. Real-estate ownership registers provide end-of-year data on 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 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 facilitates the calculation of distances to geographic boundaries 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. I collapse the dataset at 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 and thus exclude 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.

3.3.1 Discussion of sample restrictions and characteristics

Sample restrictions. 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 on their house in 2009.[23] In addition, I require that their residence be registered as larger than 50 square meters (approx. 540 square feet). This is to limit the possibility that the size is mismeasured, or that this is not their intended long-term residence. 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. I further exclude households in which the average age of adults is less than 25 years.

I then only keep households with 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 causes the sample to be fairly balanced with respect to whether households paid wealth taxes. The unconditional propensity to pay a wealth tax during 2010–2015 is around 50%. In Section 6, I include households with $TNW_{2009} >$ -3,000,000 per adult to obtain additional first-stage variation.

[23]I drop households whose tax records indicate ownership in building co-ops in 2009, due to the lack of data on housing unit assignment within co-ops.

The primary reason for incorporating the upper bounds on TNW_{2009} is that these households will contribute very little to the identifying variation. This is because housing accounts for a much smaller share of their over-all TNW. Therefore, omitting them allow summary statistics to more accurately reflect the sample for which I obtain identifying variation. In addition, it may aid precision by limiting the extent of outliers in control and outcome variables.

Age. An immediate consequence of focusing on households with initial positive TNW is that the resulting sample has a fairly high average age of 62 (see Panel A in Table A.1), and is thus fairly close to retirement. This is close to the average age of 61 in Jakobsen et al. (2020).[24] 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 in older households.[25] A recent report from Statistics Norway shows that the average age of wealth tax payers was 63 years in 2015, and that individuals above 65 years of age account for 48% of wealth tax payers.[26]

High-liquidity households. Another consequence of focusing on positive-TNW_{2009} households is that sample participants have fairly high levels of GFW. Even at the 25th percentile, households hold about NOK 230,000 (USD 38,000) in GFW. Going further toward the left tail of the liquidity distribution, I find that only 7% of households have less than NOK 50,000 in GFW. I also find that wealth tax payments only exceed one quarter of GFW for 0.2 percent of the sample. This suggests that liquidity constraints are unlikely to play a first-order role in my setting. This is relevant for the interpretation of my findings, as liquidity constraints would essentially mute any responses to wealth taxation toward zero. I discuss this as well as consider analyses on subsets of particularly-liquid households in section E.2 in the Appendix.

Geographic cutoffs. I also impose some geographic cutoffs. When considering border distance in kilometers, I only consider households within 10 km 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 cutoff similarly retains approximately 80% of the sample. The main purpose 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.4 in the Appendix, I show how households are distributed according to the different distance measures.

The identifying variation will largely come from households near price zone borders. In my preferred BDD specification, these households are in fact very similar to the average household in the sample.[27]

[24] If we adjust for reductions in mortality over 20+ years that have passed since the Jakobsen et al. (2020) sample was drawn, my sample may in some regards even be a bit younger on average.

[25] 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.

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

[27] This can be seen by comparing columns (1) and (3) in Table A.2 in the Appendix. Households near the boundary in a scaled-distance sense have, for example, only 3.8% more GFW and 3.5% higher labor earnings. Households near the boundaries in a KM-sense are considerably more different, which I discuss more when introducing the first-stage results.

I provide summary statistics for the main sample in Panel A of Table A.1 in the Appendix. Table A.2 in the Appendix shows sample means for the over-all sample, as well as for households closer to the assessment boundaries. Table A.2 also provides sample means that are weighted by pre-determined measures of the extent to which assessment discontinuities provide extensive- versus intensive-margin variation in wealth tax exposure. I discuss these statistics in more detail when presenting the first-stage effects on wealth tax exposure.

4 Main Reduced-form Evidence

4.1 A Graphical Overview

In this section, I first graphically verify the existence of discontinuities in assessed housing wealth. I then show that these discontinuities do not correlate with jumps in past transaction prices or household income levels. I perform this exercise using two different geographic measures to sort households relative to the treatment boundary. This illustrates the benefits of using scaled distance, rather than distance in kilometers, as the geographic running variable. These findings are presented in Figure 2.

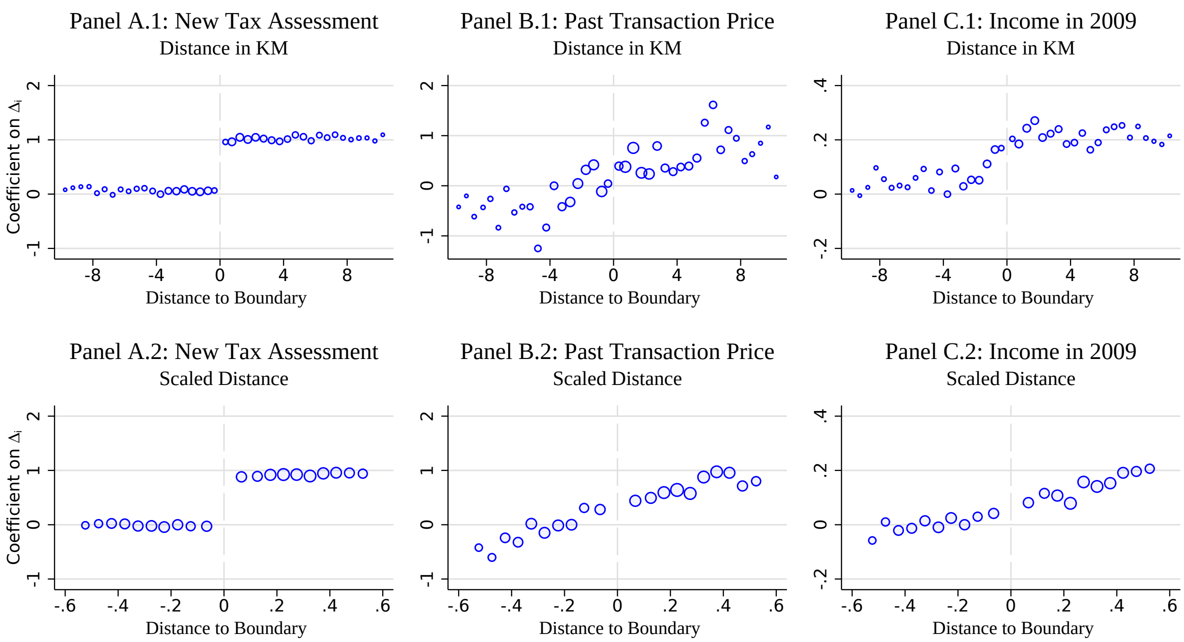
Panel A shows that for a given model-implied treatment discontinuity, Δ_i, assessed housing wealth does indeed rise by close to Δ_i log-points. This verifies that the tax authorities do use the model-implied tax assessment, $\widehat{TaxVal}$ to assess housing wealth, $TaxVal$, for wealth tax purposes. Other than the jump at zero, there is no other geographic variation in tax assessment. Regression results that verify the statistical significance of this discontinuity are provided in subsection 4.2.

Panel B shows how past transaction prices vary geographically. Regardless of which geographic measure is used, past transaction prices do not jump at the treatment boundary. Comparing Panels A and B shows that it is the reliance on geographic fixed effects rather than true underlying transaction-price discontinuities that create the cross-border variation in tax assessment. I perform formal tests of the presence of discontinuities in past transaction prices in Table 2 in subsection 4.9.

In Panel C, I consider pre-period household income. Consistent with no jump in past transaction prices, there is also shift in income levels at the boundary. However, consistent with a positive correlation between house prices and income levels, we see that incomes rise as we enter the high-assessment side of the boundary. In terms of the BDD methodology, an important take-away from these plots is that past transaction prices and, particularly, labor incomes change nonlinearly relative to border distance measured in kilometers, but linearly relative to scaled border distance.

Figure 2: Assessed Tax Values and Observable Characteristics

The graphs below show how actual tax assessment, $TaxVal$ (as observed in tax returns), past transaction prices, and pre-treatment incomes vary with border distance in a boundary region where the hedonic pricing model coefficients imply a 1-log-point assessment premium on the high-assessment side. Specifically, the scatter points stem from estimating coefficients on Δ_i in equation (8) separately for distance (d_i) bins, rather than estimating the discontinuity, i.e., the slope on $\log(\widehat{TaxVal}_i)$, and the geographic slopes, $g_b(c_i)\Delta_i$. Panel A considers $\log(TaxVal)$ in 2010 to verify the treatment discontinuity. Panel B considers the smoothness of observed past log transaction prices (2000–2009). Panel C considers log total taxable labor income (TTLI) 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. The size of the circles corresponds approximately to the relative size of that bin in the estimation sample. Table 2 provides the corresponding estimated discontinuities, as well as further balance tests. Figure A.4 in the Appendix shows the geographic distribution of households under the different distance measures.



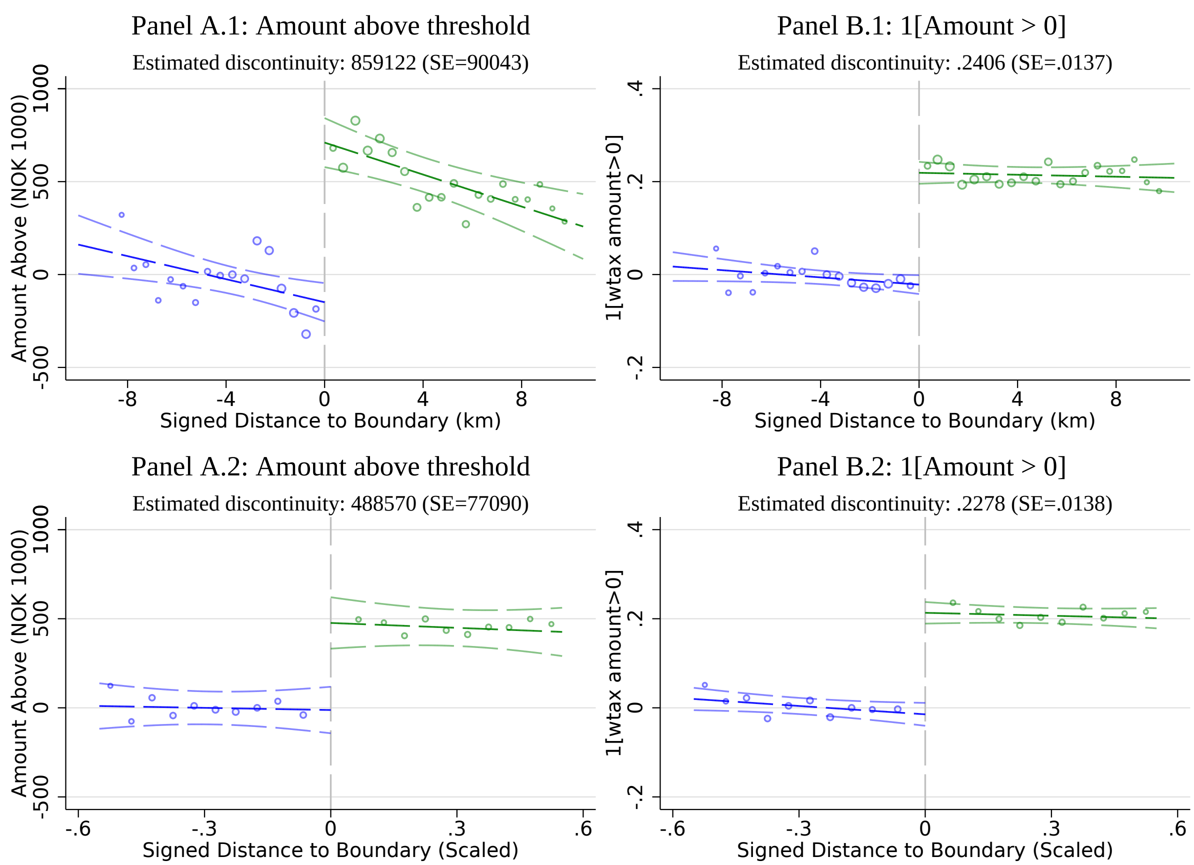
While visual inspection of Panel C.1 does not suggest a discontinuity in labor incomes, a formal test in Table 2, column 3, shows a discontinuity of 0.067, significant at the 10% level. Thus in order for regression estimates to agree with our visual inspection, we either need to use polynomials of an even higher order or to further limit the sample, both of which would have adverse effects on precision. Using scaled distance addresses this issue. The discontinuity estimate corresponding to Panel C.2 is essentially zero and has a standard error of only 0.028.

The non-linear relationships between observable characteristics and distance in km appear to be driven by a steeper gradient near the boundary. I find that this is likely driven by the fact that households drawn from near the boundary tend to live in much denser areas. In Figure A.3 in the Appendix, I show how residential density varies with border distance. Panel A shows that households who live within 1 *kilometer* of the border have 0.8 log-points (120%) more neighbors than those who live 5 to 10 km away from the border. When instead using scaled border distance as the geographic running variable, this hump-shaped relationship between density and border distance nearly vanishes. Using scaled border distance induces more homogeneity in socioeconomic characteristics with respect to the running variable. This allows me to account for geographic heterogeneity with a more parsimonious RDD specification, and reduces the external-validity concern that results are only applicable to households in densely populated areas.

4.2 First-Stage Effects: Assessment Discontinuities and Wealth Taxation

Figure 3: Graphical Presentation of the Reduced-Form Effects on Wealth Tax Exposure

These graphs illustrate how geographic discontinuities in tax assessment, $\widehat{TaxVal}$, affect intensive- and extensive-margin wealth tax exposure during 2010–2015. Panel A considers the effect on the amount of wealth above the threshold and thereby subject to the wealth tax. Panel B considers the extensive-margin effect on whether households are above the threshold and thereby must face wealth taxation of marginal savings. ∘ The graphs show the reduced-form effect on these outcomes of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ∘ 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. ∘ Scatter-points stem from estimating a coefficient on Δ_i using equation (6) separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. One negative-distance bin is normalized to be zero. ∘ The sample includes households with $TNW_{2009} > 0$. The size of each circle corresponds to the relative number of observations in that bin. Standard errors are clustered at the census-tract level. The pre-period (placebo) version of these graphs can be found in Figure B.1 in the Appendix.



The assessment discontinuities create variation in assessed housing wealth, and thereby overall TNW. This affects both whether households have to pay a wealth tax and how much they pay. Quantifying these two exposure effects is necessary to map the reduced-form estimates on, e.g., saving behavior into elasticities or saving propensities. Both of these first-stage effects are also necessary to map the findings to theory. Extensive-margin variation lowers the marginal net-of-tax rate of return and should elicit stronger dissaving responses the stronger is the EIS. Intensive-margin variation, on the other hand, should cause an increase in saving for consumption-smoothing households.

I show the first-stage effects graphically in in Figure 3. Panel A.1 and A.2 show clear evidence of a discontinuous treatment effect in terms of the amount subject to a wealth tax. On average, a $\Delta = 1$ increase in tax assessment leads to a MNOK 0.48 to 0.85 increase in the amount of wealth subject to the wealth tax. The estimate is considerably larger when sorting households according to their distance in kilometers. This is reasonable, and exactly what we would expect, in light of the discussion in the previous section. Households nearer the boundary—in a kilometer sense—tend to be drawn from more densely populated areas with higher house prices levels. A given percentage point rise in assessment thus has a larger impact in NOKs. This is not the case when using the scaled distance specification, which is reflected in the smaller jump and flatter geographic slopes on either side of the boundary in Panel A.2.

We further see that a 1-log point increase in tax assessment increases the propensity to pay a wealth tax by about 23-24 percentage points, or roughly 50% relative to a mean of 46%. Since this number arises from a comparison of otherwise similar individuals, this effect accounts for the fact that some households might accumulate more wealth and therefore find themselves above the wealth tax threshold even absent any change in $TaxVal$. This fairly large extensive-margin effect is in part due to the fairly low wealth tax threshold as well as populating the sample with only households who had positive TNW in 2009. This implies that the sample fairly densely populates the area of the TNW distribution that surrounds the wealth tax threshold. Since the wealth tax threshold gradually rose during 2010–2015, even households who were above the wealth tax threshold in 2009 were significantly affected.[28]

In Table 1, column (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, e.g., moving.[29]

In column (2), I consider the propensity to be located above the wealth tax threshold. The estimated coefficient for the full sample corresponds to the estimated discontinuity in Panel B, row 2, in Figure 3. Column (3) shows the effect on the marginal tax rate on wealth. A Δ_i log-point increase in tax assessment decreases the MTR by $0.24\Delta_i$ percentage points. This is roughly the coefficient in column (2) multiplied by the average wealth tax rate.

Column (4) shows the effect on the amount of wealth above the threshold, corresponding to Panel A, row 2, in Figure 3. Column (5) considers the effect on the average tax rate on wealth, which I find to be precisely estimated at 11 percentage points.

One important take-away from Table 1 is that the MTR effect is considerably larger than the ATR effect. This is what would be the case for a range of wealth tax reforms. First, in the presence of a wealth tax threshold, any increase in the nominal tax rate would map one-to-one into the MTR, but the effect on the ATR would be lower, as not all wealth is subject to the change in marginal rates. Second, if policymakers remove the wealth tax, households

[28]See Figure B.6 in the Appendix for an illustration. The point estimate is slightly larger for households initially above the threshold. This is likely because these households were more likely to counterfactually find themselves close enough to the wealth tax threshold for $TaxVal$ to play a decisive role.

[29]I provide further discussion of the first-stage relationship in section A.3.

initially subject to it would see a larger change in their MTR than ATR, as some fraction of their wealth was previously shielded by the threshold. From a theoretical perspective, we may roughly think of MTR effects as driving substitution effects, and ATR effects as driving income effects.[30] There is thus little reason to expect that different tax reforms will have the same effects on saving behavior—even if the underlying MTR change is the same.

Table 1: First Stage Effects on Wealth Tax Outcomes

This table provides reduced-form effects using scaled border distance as the geographic measure in equation (6). 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 as $-\tau_t \mathbb{1}[TNW_{i,t} > Threshold_t]$. 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) 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. Sample size is in brackets. Standard errors, provided in parentheses, are clustered at the census-tract level. Table B.2 in the Appendix provides first-stage estimates using the km-distance specification.

		Extensive margin		Extensive and intensive margin	
	$\log(TaxVal)$	$\mathbb{1}[TNW > Threshold]$	MTR (pp.)	AmountAbove	ATR (pp.)
	(1)	(2)	(3)	(4)	(5)
$\log(\widehat{TaxVal})$	0.8194***	0.2279***	0.2416***	488571***	0.1111***
	(0.0316)	(0.0139)	(0.0146)	(77091)	(0.0146)
	[1475162]	[1441985]	[1441985]	[1441985]	[1441985]
$F(\hat{\beta} = 0)$	672	269	274	40	274
Scaled Border Distance	Yes	Yes	Yes	Yes	Yes

In general, quasi-first-stage estimates, such as those in Table 1, should be interpreted with some caution, since they may be affected by behavioral saving responses. For example, if household savings, and thereby TNW, 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 toward 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 provides useful quantities with which to compare the subsequent estimates of how increases in tax assessment affect household behavior.

4.3 The Effect on Financial Saving Behavior

In this subsection, I consider the effect on financial saving. My measure of the level of financial savings is Gross Financial Wealth (GFW), which 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.[31]

[30] See, e.g., Gruber and Saez 2002 who formalize this in a static model of labor earnings

[31] Foreign deposits and foreign financial assets are self-reported. Outstanding claims are primarily self-reported. Third-party reported components include unpaid wages. For the average household, the potentially self-reported components of GFW account for less than 3%. For a detailed description of wealth variables see subsection A.1 in the Appendix.

I measure saving as 1-year log-differences of financial savings (GFW). Log-differencing wealth variables is standard in the wealth tax literature.[32] I further follow Jakobsen et al. (2020) 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.

$$\begin{aligned} \text{Adjusted } \Delta \log(GFW_{i,t}) \quad &\equiv \quad \log(GFW_{i,t} + wtax_{i,t-1}) - \log(GFW_{i,t-1}) \\ &\approx \quad \Delta \log(GFW_{i,t}) + \frac{wtax_{i,t-1}}{GFW_{i,t-1}} \end{aligned} \tag{9}$$

The main graphical evidence is provided in Figure 4. I first perform a placebo test to verify that households subjected to higher tax assessment do not differ in terms of pre-period saving behavior. Panels (A) and (C) plot how 2004–2009 saving rates vary according to scaled and unscaled border distance. Neither of these plots indicate any jumps in saving behavior occurring at the boundaries.

Panels (B) and (C), on the other hand, show a clear jump in post-period saving rates for households who face discontinuously higher tax assessment. Both exercises reveal an increase in the saving rate out of financial wealth of about 1.6 percentage points. To grasp the economic significance, it is useful to cast these findings in terms of saving propensities. I define the saving propensity, out of wealth tax exposure, as

$$\text{Saving Propensity} = \frac{\text{Coefficient on Adj. } \Delta \log(GFW) \times \Delta_i \times \overline{GFW}}{\text{First-stage coefficient on AmountAbove} \times \Delta_i}. \tag{10}$$

This approximates the change in the amount of saving by multiplying the effect on the log-differenced saving by the mean amount of saving.

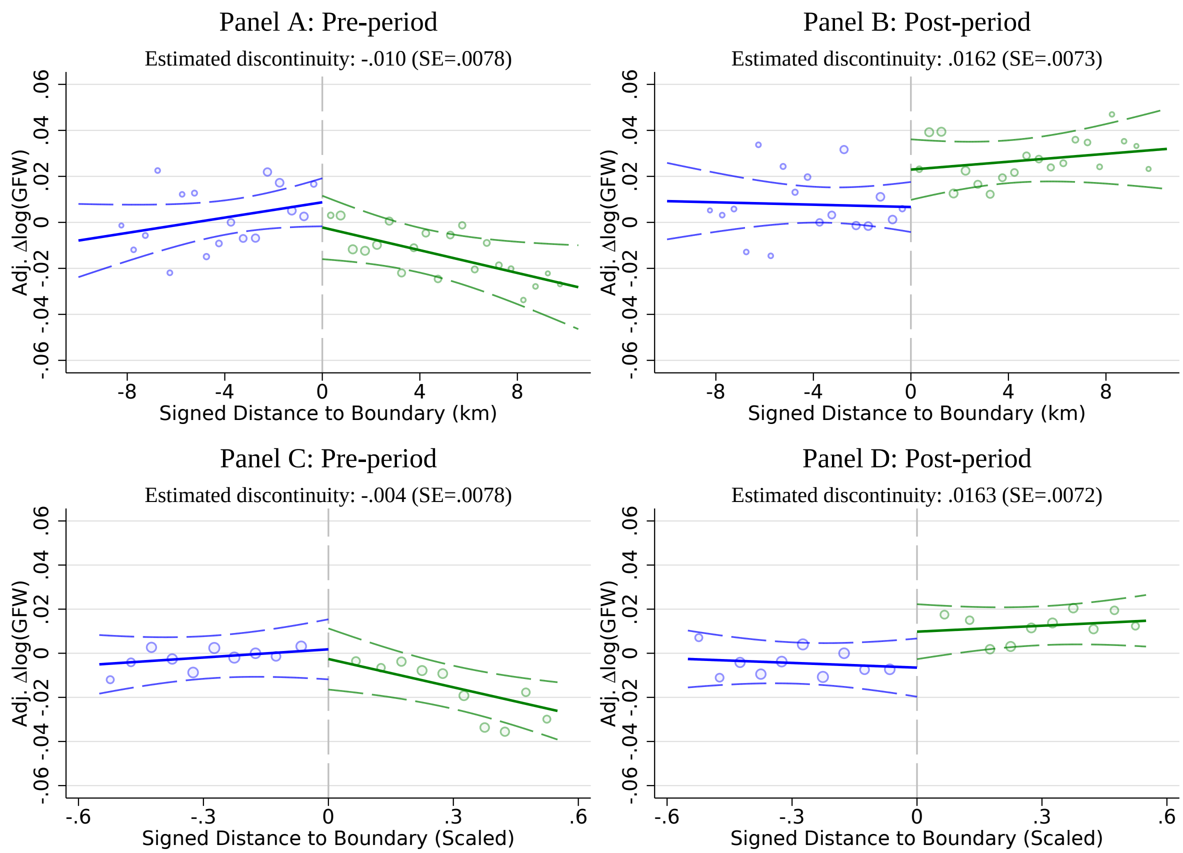
From Panel A of Table A.2, we see that the mean amount of GFW in the sample is 1.171 MNOK. Combining this with the first-stage coefficient of 0.48 MNOK in column (4) of Table 1, provides a saving propensity of 0.038.[33] If households saved exactly enough to maintain the same level of wealth after wealth taxes are paid, this coefficient should equal the average nominal wealth tax rate of 1.045%. A saving propensity of 0.038 implies that households save almost three times more than what is needed to maintain the same level of wealth after wealth taxes are paid. This is reasonable given the fact that the average household in the sample is a little over 60 years old, and is thus nearing retirement. The average treated household may thus wish to effectively pre-pay future wealth tax payments now, while their incomes are high, in order to offset the negative effect on future net-of-wealth-tax capital incomes.

[32] Zoutman (2018) considers 1- to 3-year log differences; Brülhart et al. (2019) consider 3-year log differences; Jakobsen et al. (2020) consider log-values, but incorporate household fixed effects to produce estimated effects on 1- to 8-year log-differenced wealth.

[33] This is based on the scaled-distance specification. If we instead use boundary distance in kilometers, the first-stage coefficient is 0.85. In this case, it is useful to consider the mean amount of GFW near the boundary in a KM sense. This is provided in column (2) of Panel (A) in Table A.2, and is considerably larger at 1.45 MNOK. This results in a comparable saving propensity of 0.028.

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

These graphs consider the effect on financial saving, which is adjusted for wealth tax payments as in equation 9. Panel A considers pre-period outcomes (2004–2009) and Panel B considers post-period outcomes (2010–2015). ∘ The graphs below show the reduced-form effect on financial saving of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ∘ 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 sample includes households with initial $TNW > 0$. Scatter-points stem from estimating a coefficient on Δ_i using equation (6) separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal}_i)$ and $g_b(c_i)\Delta_i$. One negative-distance bin is normalized to be zero. The size of each circle corresponds approximately to the relative number of observations in that bin. Standard errors are clustered at the census-tract level.



The discussion above is based on wealth-tax adjusted growth in financial wealth. Figure B.7 in the Appendix shows results absent this adjustment. This reveals a coefficient that is approximately 1/4th smaller. This says that approximately 1/4th of the effect I find on adjusted saving goes toward paying off wealth taxes, which is consistent with the previous back-of-the-envelope calculation that households save approximately 3 times more than is needed to maintain their level of financial wealth.

Robustness tests. In Table B.3, I provide results when using triangular (distance-based) weights. All standard errors provided in the main text are clustered at the census-tract level. Depending on the geographic cutoffs applied to the different distance measures, this provides around 9,000–10,000 clusters. In Table B.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 Table B.5, I provide estimated effects when varying the location measure cutoffs 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 narrower the bandwidth.

Debt. The most intuitive way to save to offset future tax liabilities is arguably to increase gross financial wealth. This may may be particularly true for wealth-tax payers who tend to be wealthier and older and thus either hold less debt or be closer in time to having completed their debt repayments.[34] Using the existing framework, I find no evidence of discontinuities in (either pre-period or) post-period debt accumulation Figure B.2 in the Appendix.

4.4 Portfolio Allocation

4.4.1 Stock Market Share of Financial Wealth

In this section, I examine the effect of increased wealth tax exposure on the share of financial wealth allocated to the stock market. Portfolio allocation plays a key role in the dynamics of wealth inequality (Martínez-Toledano, 2020), and both theory and evidence from the household finance literature suggest that the risky share of financial wealth may be affected by a wealth-tax induced reduction in the rate of return.

As a theoretical benchmark, it is useful to consider constant relative risk aversion (CRRA) agents who allocate a fixed share of their life-time wealth to the stock market. Increased wealth taxation, as in my empirical setting, largely lowers the future component of life-time wealth. Since current wealth remains largely unaffected, but stock holdings go down, we would expect the stock market share of financial wealth to decrease.

However, there are two reasons why I would expect the stock market share to remain unaltered. First, it is possible that the effect of increased wealth tax exposure on life-time wealth is fully offset by the behavioral responses that I document. Thus, if life-time wealth remains the same, we would also expect the level of stock market holdings to remain fixed.

Second, there is the alternative view that households may respond to a wealth-tax induced reduction in the risk-free rate by "reaching for yield" as in, e.g., Lian, Ma, and Wang (2019). Essentially, households may wish to offset the adverse effect on their portfolio-wide expected rate of return by allocating more wealth to higher-expected-return assets. Relatedly, households may wish to allocate more wealth to assets that yield higher income flows, which may entail unloading deposits or bonds in favor of dividend-paying stocks (Daniel, Garlappi, and Xiao, 2021).

I present my empirical results in Panel A of Figure 5. This plot reveals no change in the stock market share for households more exposed to wealth taxation. The associated confidence interval is [-0.0024, 0.0022]. A back-of-the-envelope calculation suggests that we can rule out a yearly increase in capital incomes above NOK 110.[35] There is little evidence with which

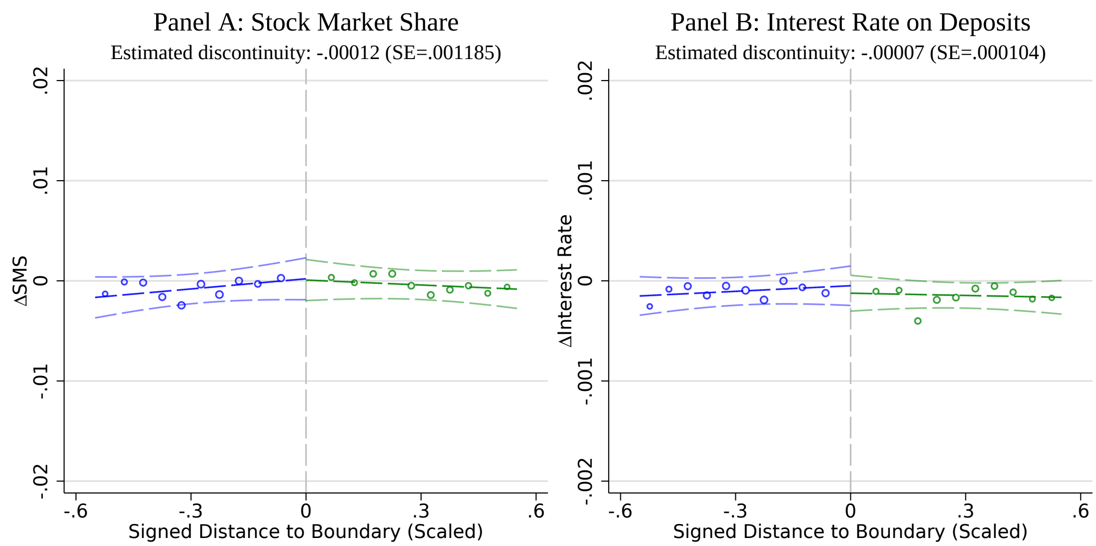
[34] The median household only has NOK 138,000 (USD 23,000) in debt (see Table A.1).

[35] 110 = 0.0022*1000000*5%. I multiply the upper bound of the confidence interval by the mean amount of GFW (1,000,000) and further by an assumed risk premium of 5% to obtain an estimate the effect on additional capital incomes.

to compare these findings. While, for example, Alan, Atalay, Crossley, and Jeon (2010) find evidence that capital taxation affects portfolio allocation in Canada, their findings are driven by a reallocation toward tax-favored assets. In contrast, the identifying variation in my setting has no direct, differential effect on the returns on safe versus risky assets.

Figure 5: The Effects on Portfolio Allocation: Stock Market Share and Realized Pre-tax Returns on Safe Assets

These graphs consider the effect on (Panel A) changes in the stock market share (SMS), which is the ratio of stock market wealth (SMW) to gross financial wealth (GFW) and (Panel B) changes in the realized interest rates on deposits. ◦ The graphs below show the reduced-form effect of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ◦ See Figure 4 for a full figure description. ◦ Figures B.3 and B.4 in the Appendix provide pre-period placebo results and estimates based on the KM-distance specification.



4.4.2 The effect on realized returns on safe assets

I also consider the effect on realized returns on deposits. Instead of allocating more wealth to risky assets, households may exert more effort toward optimizing their risk-free return. The banking literature has documented considerable dispersion in the (net-of-fee) interest rates on deposits (see, e.g., Azar, Raina, and Schmalz 2019). This large dispersion may be supported by switching costs that render the deposit rates less competitive (Sharpe, 1997). I propose the hypothesis that households may choose to suffer these non-pecuniary costs, i.e., supply more effort, in order to offset the adverse effects of more aggressive wealth taxation. I test this by considering the average realized returns on bank deposits,

$$\text{Interest Rate on Deposits}_{i,t} = \frac{\text{Total Taxable Interest Income}_{i,t}}{0.5 \cdot Deposits_{t-1} + 0.5 \cdot Deposits_{i,t}}. \tag{11}$$

I report the main result in Panel B of Figure 5. The evidence is inconsistent with my initial hypothesis. Households' realized returns appear quite unaffected by the wealth tax treatment.

As a benchmark, it is useful to establish what a hypothetical, large effect would be. Table

A.1 shows that the difference between the 75th and 50th percentiles of the realized interest rate is 0.61 percentage points. If every household pushed above the threshold increased their interest rates by 0.61 percentage points, the estimated coefficient in Panel A should be around 0.0014.[36] This hypothetical effect is 26 times larger than the upper bound of the 95% confidence interval in Panel A. In other words, my findings are inconsistent with a substantial "searching for interest" channel. However, this does not imply that I can rule out scale dependence in returns, which is discussed in the context of optimal capital taxation by Schulz (2021). This is because the behavioral saving response is likely too modest to trigger an increasing-returns-to-scale effect.

4.5 Reduced-form Effect of Wealth Taxation on Labor Earnings

In this subsection, I present results on how increased wealth tax exposure affects households' labor earnings,

$$\text{Labor Earnings}_{i,t} = \text{salary and wage earnings}_{i,t} + \max(\text{self-employment income}_{i,t}, 0). \tag{12}$$

I focus on pre-tax labor earnings in the form of salary, wages, and self-employment income. This excludes other components of total taxable labor income (TTLI), such as pensions, that are largely unaffected by concurrent labor supply. Under the reasonable assumption of interconnected municipal labor markets, wages will not be affected in my setting. The implication is that changes to labor earnings may serve as a proxy for labor supply.

Studying labor supply responses is particularly useful because it circumvents common measurement issues in studying the effects of wealth taxation on saving behavior. Firstly, household labor supply is not directly affected by incentives to misreport wealth. Secondly, it is not directly affected by how wealth taxation may encourage households to save in harder-to-tax (or easier-to-evade) asset classes such as art or durable consumption goods. Both of these phenomena may cause a downward bias in the inferred effect on saving behavior. Focusing on third-party reported financial saving will shut down the first bias in my setting. However, the propensity to save in unobserved asset classes may still be affected.[37]

Beyond addressing measurement issues, the effect on labor earnings is independently informative of the EIS (see, e.g., the two-period model in section E in the Appendix). This strong link between saving and labor supply responses to net-of-tax rate-of-return shocks implies that understanding labor earnings responses becomes crucial for optimal taxation, as taxing capital may have first-order effects on revenues from taxing labor incomes.

[36] This equals 0.61 p.p. times the first-stage coefficient on $1[TNW > Threshold]$ of 0.2279

[37] Although such spillovers are unlikely to be substantial, as the gains from such evasion or avoidance are limited. For example, evaded cash would still underperform deposits by the difference between the average rate on deposits (2%) and the nominal wealth tax rate (about 1%). Other assets, such as art may be easy to evade, but carry much more risk than, e.g., deposits or bonds, and is particularly illiquid.

Figure 6: The Effect of Wealth Taxation on Household Labor Earnings

These graphs consider the effect on labor earnings growth, $\Delta\log(\text{LaborEarnings})$. Labor earnings is defined as the sum of salaried and wage income as well as max(self-employment income,0). ∘ The graphs below show the reduced-form effect of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ∘ Panel A sorts households by the KM-distance to the geographic boundary, assigning negative values to those in the low-assessment side. Panel B uses similarly signed border distance, which is scaled by the distance between the two municipal centroids. ∘ The sample includes households with initial $TNW > 0$. Scatter-points stem from estimating a coefficient on Δ_i using equation (6) separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. One negative-distance bin is normalized to be zero. The size of each circle corresponds approximately to the relative number of observations in that bin. Standard errors are clustered at the census-tract level. The pre-period (placebo) version of these graphs are provided in Figure B.5 in the Appendix.

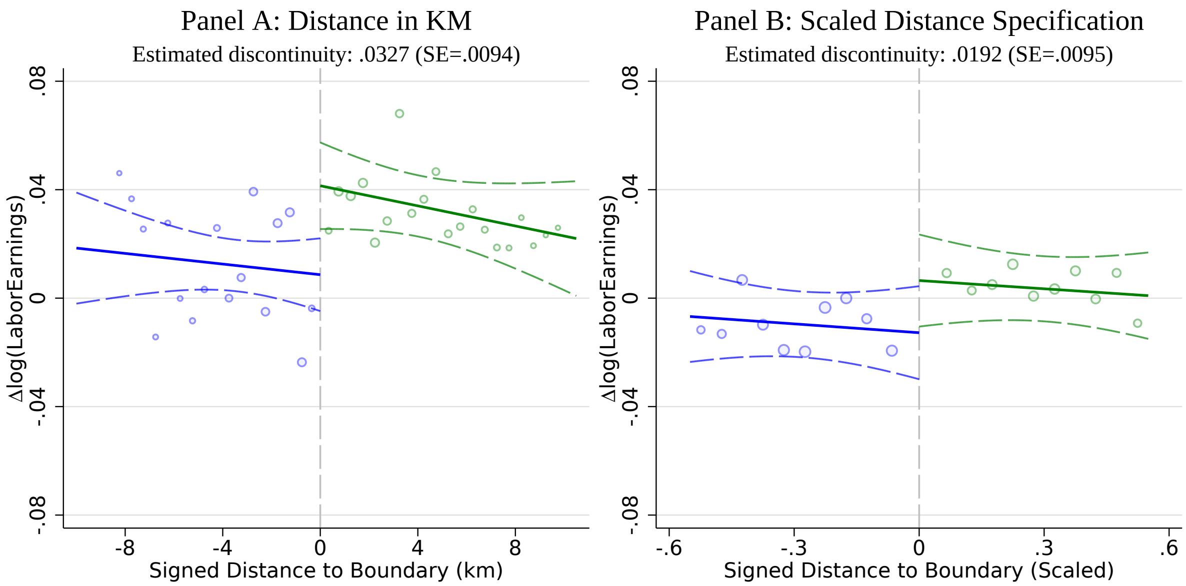


Figure 6 shows the main results, using both the km and scaled distance specifications. We see that labor earnings decrease somewhat as we move toward and into the high assessment sides of the boundaries. However, at the assessment discontinuities, household labor earnings growth jumps by about 1.9-3.3 percentage points. In order to relate these findings to the effect on saving behavior, I define an earning propensity, similar to the saving propensity in equation (10). Since labor earnings is a flow variable, and I am considering the effect on its growth rate, I cumulate the growth over a 5-year period, and divide again by 5 to obtain an average-earning propensity.

$$\text{Earning Propensity} = \frac{1}{5}\frac{\sum_{t=1}^{5} t \times \text{Coef. on } \Delta\log(\text{LaborEarnings}) \times \Delta_i \times \overline{LaborEarnings}}{\text{First-stage coefficient on AmountAbove} \times \Delta_i}. \tag{13}$$

I obtain the benchmark levels of Labor Earnings of 0.659 and 0.588 MNOK from Table A.2 for households near the boundary in a KM- and scaled-distance sense, respectively. I divide by the respective effects on the amount of wealth above the threshold of 0.846 and 0.477 MNOK. This provides pre-tax earnings propensities of 0.060 and 0.054. Assuming a marginal tax rate of 50% leads to after-tax earnings propensities of 0.030 and 0.027. These are similar in magnitude to the saving propensity that I uncovered earlier, suggesting that a large part of the increase in yearly saving is financed by increased labor supply. Of course, there will be estimation error both above and below the numerator which precludes me from putting narrow intervals this ratio of propensities. However, this exercise shows that labor supply may be an important margin of adjustment for households facing more aggressive capital taxation.

4.6 Boundary Discontinuity Estimates With Different Specifications

In this section, I discuss how the main reduced-form results vary depending on which specification is used. The underlying regression estimates are presented in Appendix Table B.6, where each column provides estimates using different approaches to address geographic heterogeneity.

The first column ignores unobserved geographic heterogeneity. The second column introduces boundary area fixed effects, and thus compares households in adjacent municipalities or districts in the four largest cities. We can see that the point estimates do not change much when adding these fixed effects. This is likely due sosio-economic control variables already absorbing a large part of this variation.

Columns (3)-(5) introduce geo-coded control variables that account for unobserved heterogeneity *within* a boundary area. For most of the outcome variables, we see that this does not materially change the estimated coefficients relative to column (2) where only boundary area fixed effects were included. This is not surprising given the graphical evidence presented in the previous subsections that show how the estimated discontinuities correspond to the differences in means across the boundaries.

Introducing geo-coded control variables do have a substantial impact on the estimated effect on debt, however. Columns (1) and (2) both wrongly suggest that higher wealth tax exposure leads to more debt accumulation. Figure B.2 in the Appendix, show that this is driven by a gradual increase in debt accumulation as we move into the high-assessment side of the boundary. This figure also shows how using a differences-in-differences design, rather than a boundary-discontinuity design, would exacerbate this problem. In the pre-period, household debt accumulation is decreasing in border distance; a differences-in-differences approach would thus produce large positive effects on debt accumulation.

While columns (3)-(4) show that the KM and scaled-distance specifications generally agree on the coefficients, the KM-specification suggests a larger effect on labor earnings. However, when accounting for the fact that the KM-specification is associated with a higher first-stage effect, the implied earnings propensities are almost identical.[38]

4.7 Year-by-year Effects

When estimating the discontinuities year by year, I find that treatment effects are quite similar over time. This is a useful exercise to inform the mechanism behind the observed saving responses. For example, Zoutman (2018) finds that his estimated elasticities are driven by rather immediate responses. He therefore attributes the high elasticity to changes in reporting

[38] When drawing households near the boundary in a KM sense, we are more likely to draw households from more densely-populated, higher-income areas. These boundary areas have higher average house price levels, which causes a given relative increase in tax assessment to have a larger effect on wealth tax exposure measured in the amount of wealth above the threshold. See calculations of earnings propensities in the preceeding subsection 4.5 and the discussion surrounding the first-stage effects in subsection 4.2.

behavior, since real responses arising from changes in consumption and labor supply will likely occur gradually. Below, I show that the main estimates are not driven by a single year, which is consistent with real responses caused by gradual consumption and labor supply adjustments. These findings are provided in Figure B.8 in the appendix. While there is some variation in the effect on the growth rate of debt over time, we see that it is zero on average, and eventually goes negative, consistent with a positive effect on net saving.

4.8 The Effect on House Prices and the Propensity to Sell

In this subsection, I report my findings on the effect on whether households subject to higher wealth tax exposure move, and whether more highly-assessed houses sell for less. These results are provided in Table B.1 in the Appendix.

Propensity to sell. Panel A of Table B.1 finds no effect on the propensity to sell. Since tax assessments stay with the house, households may "untreat" themselves 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 detailed 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 using the main, scaled-distance specification shows that a 1-log-point increase in $\widehat{TaxVal}$ increases the likelihood of selling by only 0.2 percentage points. I can rule out any effects larger than 0.8 percentage points at the 5% significance level.

Subsequent transaction prices. Panel B of Table B.1 finds no effect on 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 homeowners finance their purchases with debt, the net effect of a house purchase on their 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 statistically significant effect 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.[39]

[39]These estimates are somewhat imprecise, since I only a limited number of subsequent transactions. It therefore explore 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}/0.02)=2.33%. This would be the upper bound for the magnitude of the potential capitalization effect. Using the estimates from column (5), I can rule out an effect outside of 10%*(0.035 $\pm$ 1.96*0.085)=[-1.32%, 2.02%]. Thus, the confidence interval contains (only) 1.32/2.33=57% of the (back-of-the-envelope) potential full capitalization effect.

4.9 Regression-based Analysis of Pre-treatment Differences

In this subsection, I explore whether my identifying variation in wealth tax exposure, arising from geographic assessment discontinuities, is correlated with pre-period observables.

Table 2: 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, GFW, debt, and education. College is a dummy equal to one if any of the household adults 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 parentheses, and are clustered at the census-tract level. Sample sizes are provided in brackets.

	(1) *No geo.*	(2) *Boundary FEs*	(3) *KM Distance*	**(4)** *Scaled Distance*	(5) *Rel. Location*
			log Past Transaction Price, Post-2000		
$\log(\widehat{TaxVal})$	1.461***	0.776***	-0.064	-0.061	-0.112
	(0.040)	(0.110)	(0.244)	(0.224)	(0.211)
	[40310]	[39748]	[39748]	[38728]	[38728]
			log Past Transaction Price, Post-2004		
$\log(\widehat{TaxVal})$	1.494***	0.943***	0.251	-0.095	-0.442
	(0.071)	(0.172)	(0.333)	(0.335)	(0.323)
	[18062]	[17469]	[17469]	[17073]	[17073]
			log Total Taxable Labor Income in 2009		
$\log(\widehat{TaxVal})$	0.288***	0.153***	0.067*	-0.000	0.002
	(0.005)	(0.013)	(0.040)	(0.028)	(0.023)
	[261300]	[260774]	[260769]	[236241]	[254364]
			log Gross Financial Wealth in 2009		
$\log(\widehat{TaxVal})$	0.629***	0.409***	0.255***	-0.030	0.002
	(0.012)	(0.035)	(0.095)	(0.070)	(0.064)
	[261300]	[260774]	[260769]	[236241]	[254364]
			Stock Market Share in 2009		
$\log(\widehat{TaxVal})$	0.045***	0.028***	0.053***	0.000	0.002
	(0.002)	(0.005)	(0.014)	(0.010)	(0.009)
	[261139]	[260612]	[260607]	[236096]	[254208]
			log Debt in 2009		
$\log(\widehat{TaxVal})$	0.502***	0.350***	0.180	0.092	0.097
	(0.015)	(0.046)	(0.143)	(0.099)	(0.082)
	[261300]	[260774]	[260769]	[236241]	[254364]
			College Degree		
$\log(\widehat{TaxVal})$	0.239***	0.194***	0.159***	0.019	0.028
	(0.005)	(0.015)	(0.040)	(0.028)	(0.024)
	[260599]	[260075]	[260070]	[235610]	[253687]
Controls					
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes
Border Distance Controls					
– KM, KM^2	–	–	Yes	–	–
– Scaled	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	Yes

The first variable that I consider in Table 2 is past transaction prices. While the hedonic pricing model is estimated on transactions after 2004, I also include the years 2000–2003 in the first row, in order to increase precision. In the second row, I restrict my analysis to transactions after 2004. Column (1) shows that houses in higher-assessed areas are indeed more expensive.[40] This strong positive relationship persists when I control for boundary area fixed effects, with greatly limits the potential amount of geographic heterogeneity. However, these differences lose their significance once I address within-boundary-area heterogeneity in columns (3)-(5). This is consistent with the visual evidence in Figure 2 where past transaction prices are plotted against the distance measures.

In terms of point estimates, it is the scaled distance measure in column (4) that estimates the smoothest price path across the boundary. Given the fairly nonlinear relationship between ex-ante observables and border distance in kilometers presented Figure 2, I allow for a more flexible, second-order relationship in the regressions underlying Table 2. This is mainly to give it more of a fighting chance against the preferred scaled-distance specification in column (4).

Since there is no indication of jumps in past transaction prices at the assessment boundaries, it is not surprising that the subsequent rows reveal no discontinuities in other socioeconomic characteristics. If house prices are in fact smooth across geographic boundaries, it is reasonable to expect that other household characteristics are as well. Table 2 shows that the identifying variation in $\log(\widehat{TaxVal})$ is uncorrelated with past incomes, financial wealth, portfolio allocation, debt, as well as educational attainment, when geographic heterogeneity is accounted for with the scaled-border specification in column (4). We see that the relative location measure also performs particularly well, but that the kilometer specification struggles. This is reasonable given what we saw earlier in Figure 2, that the relationship between 2009 incomes and border-distance in kilometers is highly nonlinear.

5 Implied Structural Parameters

The degree to which economic agents are willing to substitute consumption across periods is one of the most important modeling choices in economics. In standard models, this choice is reflected in the Elasticity of Intertemporal Substitution (EIS) or, equivalently in models with no uncertainty, the inverse of the coefficient of relative risk aversion. While the central role of the EIS in macroeconomic models is well appreciated, its importance in public finance may have been obfuscated by the classical result that, regardless of the EIS, the optimal long-run tax rate on savings is zero (Chamley 1986 and Judd 1985). Recently, however, this result has been overturned by Straub and Werning (2020) in the same models in which it arose. Whether it is optimal to tax capital does indeed depend crucially on the EIS in classical models. In this section, therefore, I use a simple life-cycle model to examine which value of the Elasticity of Intertemporal Substitution (EIS) is most consistent with my empirical findings.

The model environment is simple: It only contains the core elements necessary to replicate

[40] It should be noted, however, that this does not verify the ability of the hedonic pricing model to price houses out of sample, as these transactions would have been included in the model's estimation sample.

my empirical results and the shock to wealth tax exposure. Agents choose both how much to save and how much to work, and importantly, they're shocked by more aggressive wealth taxation in such a way that the effect on the marginal and average net-of-tax rates-of-return may differ. The model environment accounts for the fact that the average household in my sample is close to retirement and thus faces lower incomes in the near future. To simplify the analysis, I abstract from frictions, but discuss how they may play a role in interpreting the mapping between my empirical findings and the EIS.

5.1 A simple life-cycle model

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

$$\max_{\{c_t, s_{t+1}, l_t\}_{t=0}^{T}} \quad \textstyle\sum_{t=0}^{T} \quad \beta^t \left(\frac{1}{1-\gamma} c_t^{1-\gamma} - \psi \frac{l_t^{1+\nu}}{1+\nu} \right), \tag{14}$$

$$\text{s.t. } c_t + s_{t+1} \quad = \quad y_t + l_t w_t \tag{15}$$

$$+ \quad s_t R - wtax_t(s_t).$$

ψ is the (dis)utility weight on labor supply, and β is the time discount factor. Households choose how much to consume, c_t, work, l_t, and save, s_{t+1} each period. Unearned income (pensions), y_t and initial wealth, s_0, are exogenous. Households earn a gross rate of return of R, but must pay wealth taxes, $wtax_t$, that depend on s_t.

Agents face a wealth tax schedule where any savings, s_t, in excess of the threshold, $\bar{s}$, is subject to a tax rate of τ, according to the following formula.

$$wtax(s_t) = (s_t - \bar{s})\mathbb{1}[s_t > \bar{s}]\tau. \tag{16}$$

Rewritten budget constraint. Now define $MTR_t = \mathbb{1}[s_t > \bar{s}]\tau$ and $ATR_t = wtax(s_t)/s_t$. This allows us to rewrite the budget constraint as

$$c_t + s_{t+1} \quad = \quad y_t + l_t w_t + \underbrace{s_t(R - MTR_t)}_{\substack{\text{Linearized Gross} \\ \text{Capital Income}}} + \underbrace{s_t(MTR_t - ATR_t)}_{\text{Virtual Income}}, \tag{17}$$

where the second-to-last term is the gross, net-of-wealth-tax capital incomes the agent would obtain if there were no wealth tax threshold. Since there is such a threshold, the last term contains the necessary virtual-income compensation. This decomposition allows for a straightforward mapping between my first-stage estimates and the shocks to the budget constraint experienced by the life-cycle agent.

5.2 Calibration

In my main specification, I follow Jakobsen et al. (2020) in modeling the responses of a representative agent. I set $R = 1.02$. The baseline MTR and ATR are both set to zero. The unshocked (counterfactual) agent sees no changes to MTR or ATR. The shocked agent sees their MTR shocked by $dMTR$, which equals the empirical first-stage estimate on MTR in Table 1. Since I model the responses in terms of GFW, the shocked agent sees $dATR = \widehat{dATR^{GFW}}$. This is the first-stage coefficient on the average tax rate relative to GFW, $ATR^{GFW} = wtax_{i,t}/GFW_{i,t}$, which is reported in Table B.12. The virtual income shock is set to $s'_t(dMTR - dATR)$, where s'_t is the simulated savings path of the unshocked agent.

I simulate the responses in terms of their saving behavior and labor supply for EIS-Frisch combinations, $(\frac{1}{\gamma}, \frac{1}{\nu})$. I set $\beta = 0.96$. 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, assuming an average income tax rate of 0.3, and that the consumption share of total incomes (labor earnings plus exogenous income) equals 80%.[41]

The role of the exogenous income, y_t, is to imitate pension income. For agents below a nominal retirement age of 65, I set this equal to the difference between mean total taxable labor income and labor earnings that I observe in the data. Once agents reach nominal retirement age, y_t increases by 60% of the average observed labor earnings. Pensions are then taxed at a linear rate of 0.3. This procedure accounts for some households in the data already being retired before the age of 65. I induce agents to retire by making wages drop to zero over a 5-year period that starts at age 65. To simplify the analyses, I do not model bequests motives directly. Instead, I assume that households live until they are 100 years old and do not receive pension incomes after age 90. 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 years.[42]

5.3 Simulated Treatment Effects

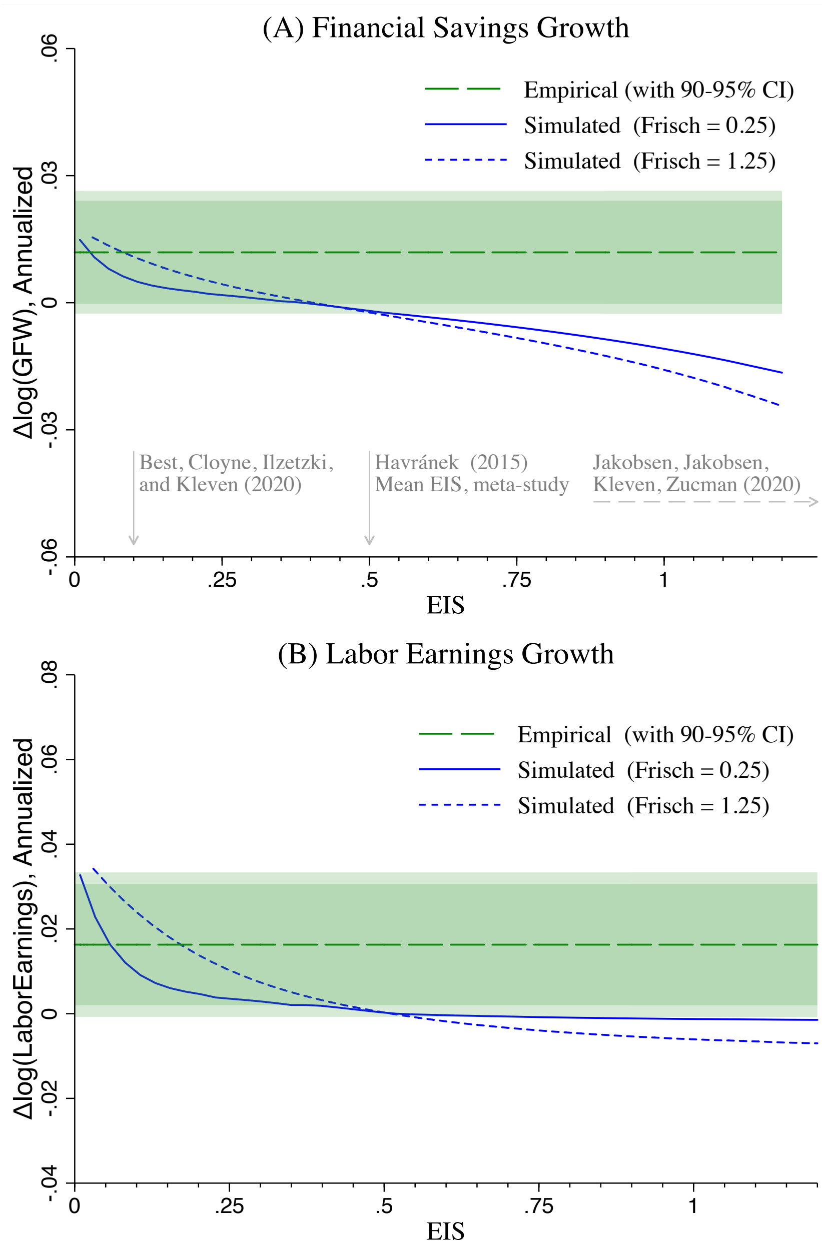
Figure 7 shows simulated treatment effects for different values of the EIS. Panel A considers the effect on the growth rate of financial saving absent the wealth tax adjustment (corresponding to the empirical findings in Figure B.7). We see that the cut-off for when we see a change in the sign of the saving response is around 0.45. This is lower than the canonical cut-off of 1 in a pure-capitalist model due to human wealth effects offsetting the income effect (Elmendorf, 1997). The figure shows that an EIS of about 0.02–0.08 can replicate my empirical findings. This is only moderately dependent on the Frisch elasticity of labor supply. We see that savings growth is somewhat more responsive when labor supply adjustments are less costly, i.e., when the Frisch elasticity is higher. However, as long as it is sufficiently positive, the Frisch elasticity plays a modest role in mediating the responses.

[41] Choosing a consumption share of 80% ensures that agents choose labor supply 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.

[42] Absent any mortality risk, this roughly 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 bequeath an amount large enough to finance their own planned consumption for 15 years. If instead agents ended their life-cycle at age 85 with zero residual assets, income effects would be weaker, and even lower values of the EIS would be needed to obtain simulated treatment effects consistent with the confidence intervals on my empirical findings.

Figure 7: Simulated Treatment Effects as a Function of the EIS

This figure shows the relationship between simulated saving and labor earnings responses and the Elasticity of Intertemporal Substitution (EIS). The long-dashed green lines provide the empirical point estimates, with surrounding 90% and 95% confidence intervals. The solid blue line provides the simulated treatment effect for different values of the EIS when the Frisch elasticity, $1/\nu$, is 0.25. The dashed blue line uses a Frisch elasticity of 1.25. Panel A considers financial savings growth, without the wealth tax adjustment, where the empirical point estimate comes from Figure B.7. Panel B considers labor earnings growth, where the point estimate comes from Panel B of Figure 6. The citations in grey correspond to existing estimates of the EIS. Best et al. 2020 estimate an EIS of 0.1. Havránek 2015 finds that the mean of existing estimates is 0.5. The calibrated EIS in Jakobsen et al. 2020 ranges from 2 to 6. Figure B.9 in the Appendix repeats this exercise while allowing for first-stage heterogeneity. When Frisch=0.25 (1.25), the lowest computationally feasible EIS was 0.0088 (0.02). Simulated effects are smoothed by using a local 5th-order polynomial fit.



This figure also shows that empirically finding positive saving responses to wealth taxation is not too surprising. For example, the EIS of 0.1 found by Best, Cloyne, Ilzetzki, and Kleven (2020) produces simulated saving responses that are statistically indistinguishable from my empirical findings. The same applies to recent evidence from India and Japan, where the EIS is found to be 0.022 (Agarwal, Chua, Ghosh, and Song, 2020) and 0.21 (Cashin and Unayama, 2016).

While there is little U.S. evidence, recent work by Baker, Johnson, and Kueng (2021) finds that an EIS of 0.19 best explains long-run responses to variation in sales taxes. Havránek (2015) reviews existing estimates of the EIS more broadly and finds a mean of 0.5, but considerable dispersion. While this suggests that a large swath of empirical estimates are consistent with my estimated treatment effects, Havránek (2015) points out that the mean microeconometric estimates published in Top-5 journals is close to 1.[43] From this, I would draw the conclusion that my findings are not inconsistent with previous empirical evidence, as the existing empirical evidence itself is far from consistent.

It is also useful to consider values of the EIS derived from wealth taxation. Jakobsen, Jakobsen, Kleven, and Zucman (2020), using different identification strategies, find that the implied EIS ranges from 2 to 6. Values of the EIS that are this large are clearly not reconcilable with my empirical evidence. However, they are also not inconsistent, as Jakobsen et al. (2020) emphasize that their calibrated EIS may be driven by evasion or avoidance.

Panel B considers the effect on labor earnings. To map the simulated responses to those found in Panel B of Figure 6, I consider the cumulative labor earnings response, which I average over time.[44] Interestingly, labor earnings responses are almost as sensitive to EIS as the savings responses. The EIS cut-off below which we see positive earnings responses is only slightly higher at about 0.5. In order to replicate the empirical treatment effect, I need an EIS of about 0.05–0.15. This range overlaps considerably with the one for savings responses.

First-stage heterogeneity. Figure B.9 shows results when incorporating first-stage heterogeneity. This produces a qualitatively and quantitatively similar relationship between treatment effects and the EIS. I incorporate first-stage heterogeneity by allowing the first-stage coefficients on MTR and ATR^{GFW} to vary by which 2009 quartile of TNW a household belongs to. These coefficients, and summary statistics by quartiles, are provided in Table B.12. This exercise thus accounts for the fact that high TNW households on average saw a modest effect on their marginal tax rates.

Credit constraints. If households were financially constrained, they would wish to front-load consumption more than they are already doing. In other words, they have an unmet preference for dissaving. This would clearly mute substitution effects, as households are already unable to dissave more. However, it would also work against finding large income effects. Positive income effects on observed saving behavior would only materialize to the extent that they exceed the pre-existing unmet demand for dissaving. Essentially, the income effects would have to change the optimal saving path enough to render the agent unconstrained. This becomes

[43] There is considerable spread in assumed values of the EIS in quantitative modeling as well. For example, while Guvenen (2009) uses an EIS between 0.1 and 0.3, Kaplan and Violante (2014) assume a value of 1.5.

[44] In the simple model used for simulating treatment responses, labor supply responses are immediate. This is because labor supply is determined through the intratemporal first-order conditions, which leaves the level of labor earnings log-proportional to consumption. The adjustment to increased taxation thus comes immediately as the level of consumption is decreased. This differs from my empirical findings, in which household labor earnings growth is affected smoothly across time. This may be caused by households readjusting at different points in time, or that households have a preference for smoothing labor supply adjustments. Since it is unclear how to model labor supply adjustments in a way that produces a smooth response over time, I take the following simpler route. I calculate the cumulative, simulated labor earnings response, and then calculate the average, as if responses occurred smoothly over time.

problematic to the extent that there is considerable EIS heterogeneity (which runs counter to the findings in Best et al. 2020). In that case, credit constraints would fully mute the responses of high-EIS households and only allow us to observe partially-muted responses of low-EIS, high-income-effect households. However, this is hard to square with my empirical findings for a few reasons.

Firstly, treated households are fairly old and thus face declining income paths. This is at odds with material credit constraints for rational agents, since it is unclear against what future incomes they wish to borrow against. Second, the households in my sample have ample liquidity. Even at the 25th percentile, households hold NOK 224,000 (USD 37,000) in financial wealth out of which NOK 152,000 (USD 25,000) is in deposits (See Panel A of Table A.1). Going further toward the left tail of the liquidity distribution, I find that only 6-7% of households have less than NOK 50,000 (USD 8,333) in GFW, and that wealth taxes only exceed one quarter of GFW for about 0.2% of my sample (See Panel A of Table A.2). These statistics do not materially change when instead considering households positioned to experience larger shocks. It is therefore unlikely that financial frictions play an important role or that taxation induces financial hardship as in Wong (2020).

Frictions to adjusting consumption. 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 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 become uninformative of the EIS. The fact that I cannot statistically rule out that increases in savings are fully paid for by increased labor earnings (rather than downward adjustments to consumption) lends support to such an economic environment. However, perhaps more reasonably, the strength of consumption frictions are reduced over time. In that case, the presence of consumption frictions would still allow me to detect responses consistent with a high EIS: If the EIS is high (and thus the substitution effect is strong), households would want to shift more consumption from the future. If consumption frictions don't bind in the future, households would optimally shift consumption toward the present by dissaving. My finding of a positive effect on saving behavior is inconsistent with this.

6 Disentangling Marginal and Average Tax Rate Effects

The previous section used a standard life-cycle model to show that a small EIS is necessary to rationalize my empirical findings. The negative relationship between the EIS and the saving and labor supply responses is a built-in feature in standard life-cycle models. This is because the EIS determines the strength of intertemporal substitution effects. By lowering the EIS, we lower the substitution effects, and thereby allow income effects to dominate. The underlying mechanism dictates that the substitution effects are driven by changes in the marginal net-of-tax rate of return, while income effects are driven by changes in the average net-of-tax rate.

In this section, I decompose the effects of increased wealth tax exposure into the effects of changing the marginal (MTR) and average tax rate (ATR) on wealth. This allows me to directly test the mechanisms underlying the life-cycle model. First, an increase in the MTR lowers the marginal net-of-tax rate one for one, and should thereby cause dissaving. Second, an increase in the ATR lowers the average net-of-tax rate, and should cause more saving. Since my empirical results indicate a combined effect that is positive, I should find MTR effects that are smaller in magnitude than the ATR effects.

The gist of the exercise is that I use pre-period characteristics to create new variables that predict the extent to which geographic discontinuities in $TaxVal$ change households MTRs versus ATRs. I then use these variables, interacted with the geographic discontinuities, as instruments in an instrumental variables approach. This allows to separately identify the effects.

6.1 Two Variables that Differentially Predict Extensive and Intensive-margin Effects of Assessment Discontinuities

The following expression provides the amount of wealth taxes a household must pay following a relative increase in $TaxVal_{i,t}$ of δ.

$$wtax(\delta)_{i,t} \quad = \quad \max(TNW_{i,t} + \delta \cdot TaxVal_{i,t} - T_t, 0). \tag{18}$$

Now consider the following two empirical moments. These tell us the conditional marginal tax rate contribution (MTC) and the average tax rate contribution (ATC) of a 100%-increase in tax assessment. This is calculated by linearizing the effect around the given δ, which equals 0.25 in the main specification. These conditional expectations (sample means) are functions of the conditional joint distribution of $TNW_{i,t}$ and $TaxVal_{i,t}$. Conditional means are calculated separately for each year, t.

$$\begin{aligned} MTC^t(\delta|W_{i,2009}) \quad &= \quad \frac{1}{\delta}\mathbb{E}^t\Big[\tau_t\big(\mathbb{1}[wtax(\delta)_{i,t} > 0] - \mathbb{1}[wtax(0)_{i,t} > 0]\big)\ \Big|W_{i,2009}\Big], && (19)\\ ATC^t(\delta|W_{i,2009}) \quad &= \quad \frac{1}{\delta}\mathbb{E}^t\Big[\big(wtax(\delta)_{i,t} - wtax(0)_{i,t}\big)/TNW_{i,t}\Big|W_{i,2009}\Big]. && (20)\end{aligned}$$

When these two expressions are the same, assessment shocks induce rate-of-return changes that, on average, resemble those of a linear (e.g., capital income) tax. When MTC is substantially larger than ATC, assessment shocks tend to lead to marginal (short-term compensated) return shocks.

There are two natural choices for the set of conditioning variables, $W_{i,2009}$. The first is is $TNW_{i,2009}$ due to persistence in wealth levels. The second is the tax assessment households would see absent the existence of an assessment discontinuity in their boundary area, $\widehat{TaxVal_i^{\approx}}$. This counterfactual assessment is based on 2009 housing characteristics, $H_{i,2009}$, and is the average of the counterfactual low- and high-side tax assessments.[45] The larger this counterfactual assessment is, the larger is the impact of a δ-increase on $TNW_{i,t}$.

Conditioning on the predetermined $TNW_{i,2009}$ and $\widehat{TaxVal_i^{\approx}}$ ensures that the conditional

[45]More specifically, this may be written as $\widehat{TaxVal_i^{\approx}} \equiv \big(\widehat{TaxVal_i}\big|_{d_i<0} + \widehat{TaxVal_i}\big|_{d_i>0}\big)/2$

joint distribution of $(TNW_{i,t}, TaxVal_{i,t})$ is not affected by the assessment discontinuity. I further account for the fact that TNW growth rates likely depend on age and income levels by also conditioning on age and household income as of 2009. More specifically, I assign households into one of 2,700 bins that are made up of 30 TNW_{2009} bins, 10 $\widehat{TaxVal^{\approx}}$ bins, 3 income bins, and 3 age bins. $W_{i,2009}$ then indicates membership in a combination of these bins. While all groups include more than one hundred household, and typically substantially more, I strengthen exogeneity by calculating conditional means using the leave-me-out method. For each individual household, i, this method calculates means under the assumption that i does not belong to their own group.

Figure 8: Effects of Higher Tax Assessment on Marginal and Average Wealth Tax Rates

This figure shows the tax-driven effect of increasing housing assessments on the marginal tax rate (MTC, blue circles) and average tax rate (ATC, orange squares), and how this varies according to 2009-valued TNW. These are calculated as in equations (19) and (20). The frequency count (gray bars) corresponds to the right-hand-side y-axis.

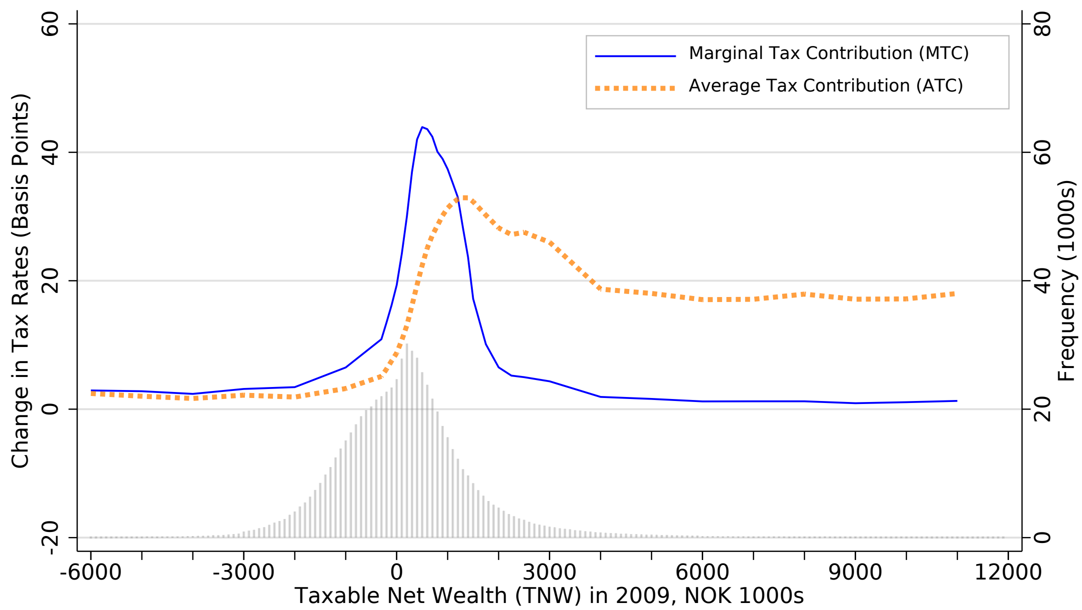


Figure 8 illustrates how the resulting MTCs and ATCs covary with TNW_{2009} by averaging over non-TNW characteristics. We see that for $TNW_{i,2009} < -2\ 000\ 000$, the two lines are very similar, and close to zero. This means that very low-TNW households' wealth tax exposure were not significantly affected during 2010–2015. For higher TNW, there is initially a more rapid increase in MTC than ATC. These are households who are likely to be pushed above, but not far above, the wealth tax threshold. This implies that these households are likely to experience a larger effect on marginal than average tax rates. For $TNW_{i,2009} > 1,000,000$, we see a slight decrease in ATC and a rapid decrease in MTC. These are households who are likely to be—and stay—above the tax threshold even without an increase in tax assessment. These households are thus unlikely to see an effect on their marginal tax rates, but will see a full impact on their average tax rates as more of their wealth is taxed on the intensive margin. The slight decrease that we observe is caused by the fact that TNW rises faster than $TaxVal$, which means that a given percentage point increase in assessment has a decreasing effect on the ratio of wealth taxes to TNW.

6.2 IV Methodology to Distinguish Between MTR and ATR Effects

This subsection describes how the two variables, MTC and ATC, are used as interaction variables to obtain first-stage heterogeneity in how households' MTR and ATR are affected. In an IV framework, I can then separately identify the effects of changing average versus marginal tax rates.

While the previous analyses in section 4 only included households with initial $TNW > 0$, I now also include households with initial negative TNW. As Figure 8 shows, there is a large number of households with initial $TNW \leq 0$ for whom MTRs should be non-trivially affected by an inflated $TaxVal$.

My IV specification is built on, and uses the same notation as, the reduced-form regression equation (6). There are two endogenous variables, MTR and ATR. The instrumental variation is obtained from interacting the assessment discontinuity term with $\mathbf{z}_i^t = \{MTC_i^t,\ ATC_i^t\}$. These variables have superscript t, as they account for changes in the wealth tax threshold and nominal tax rate over time. I include $\mathbf{z}_i^t$ interacted with the slope terms, as well as the potential increase in tax assessment, Δ_i, as controls. I also include year-specific controls in $\mathbf{z}_i^t$. All coefficients are estimated separately for households initially below and above the threshold (indexed by a), which ensures that the residualized geographic variation in saving behavior documented in Section 4 is unaltered.

$$\begin{aligned} w_{i,t} = \quad & \beta^w_{a,w}\Delta_i \mathbb{1}[d_i > 0]'\mathbf{z}_i^t && + g_{b,a,w}(\mathbf{c}_i)\Delta_i\mathbf{z}_i^t + \xi_a^w \Delta_i \mathbf{z}_i^t + \rho^w_{t,a}\mathbf{z}_i^t && (21) \\ & && + \delta'_{b,s,a,w}\mathbf{H}_i + \gamma'_{t,a,w}\mathbf{X}_i + \epsilon^w_{i,t}, \quad \text{for } w \in \{MTR, ATR\}, && \\ y_{i,t} = \quad & \beta^{MTR} MTR_i + \beta^{ATR} ATR_i && + g_{b,a}(\mathbf{c}_i)\Delta_i\mathbf{z}_i^t + \xi_a \Delta_i\mathbf{z}_i^t + \rho_{t,a}\mathbf{z}_i^t && (22) \\ & && + \delta'_{b,s,a}\mathbf{H}_i + \gamma'_{t,a}\mathbf{X}_i + \varepsilon_{i,t}. && \end{aligned}$$

This type of analysis is based on Gruber and Saez (2002). My primary innovation to this approach is to exploit persistent, differential tax exposure that arises due to tax assessment discontinuities. The secondary innovation is to use the information from the conditional distribution of TNW to enhance precision rather than assuming that TNW remains constant when constructing my instruments. This allows me to obtain a stronger instrument by accounting for, e.g., the extent to which TNW tends to rise for young high-income households but decline for those that are older and lower income.

6.2.1 Saving Behavior and Labor Earnings

I provide the main results from estimating the system of equations (21)-(22) in Table 3. These IV analyses all use the main, scaled border distance specification. Columns (1)-(3) consider the main wealth-tax adjusted financial saving measure. Column (1) estimates the effect of changing MTR alone, using only the MTC interaction as the instrument. This reveals virtually no effect on saving behavior. Column (2) similarly only estimates the ATR effect. This shows a positive effect on saving, albeit not quite statistically significant, with a t-statistic of 1.5. Column (3) jointly estimates MTR and ATR effects. This shows a larger, and highly significant sensitivity of saving to the average tax rate. The sensitivity of saving to the marginal rate is now found to be negative, but insignificant, and small relative to the ATR effect. The increase in the magnitude of both the estimated coefficients is consistent with theory ascribing opposite effects, and the fact that changes in MTR and ATR changes are positively correlated.

The finding of a positive effect of increasing the ATR on financial saving is exactly what a simple life-cycle model would predict given the main BDD estimates. If households respond to wealth taxation by saving more, this must be associated with positive income effects caused by lowering the average after-tax rate of return, or equivalently, increasing the ATR, on wealth. What is particularly interesting is to quantify the relative contribution of MTR effects. These are found to be of the theoretically appropriate negative sign. While the MTR effect is modest in size and significance relative to the ATR effect, it is consistent with other research that has shown that increases in the *marginal* return on savings leads to more wealth accumulation.[46]

Table 3: Differential Saving and Labor Earnings Responses to Changing Marginal and Average Tax Rates on Wealth

This table reports IV estimates of the effect of changing the marginal (MTR) and average tax rates (ATR) on saving behavior and labor earnings. The IV specification is provided in equations (21)-(22). Columns (1)-(3) consider financial saving adjusted for wealth tax payments, $\log(GFW_{i,t} + wtax_{i,t-1}) - \log(GFW_{i,t-1})$. Columns (4)-(6) consider log-differenced labor earnings, $\Delta \log(\text{Labor Earnings})$. Geographic assessment discontinuities, $1[d_i > 0]\Delta_i$, interacted with MTC_i^t and ATC_i^t are used as instruments. MTC_i^t and ATC_i^t are based on 2009 observables and time t tax rules to provide the simulated impact of tax-assessment increases on MTR and ATR. The estimation sample also includes households with $TNW_{i,2009} < 0$; and all coefficients, on instruments and control variables, are allowed to vary by whether $TNW_{i,2009} > 0$. $ATR + MTR$ provides the sum of the coefficients on MTR and ATR. Standard errors on the sum are calculated using the covariance matrix of the estimated coefficients. Stars indicate significance at the 10%, 5%, and 1% levels. Standard errors are clustered at the census-tract level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adj. $\Delta\log(GFW)$			$\Delta\log(GFW)$	$\Delta\log(LaborEarnings)$		
MTR	-0.03		-3.95	-4.62	3.04		-0.23
	(1.96)		(2.97)	(3.03)	(2.35)		(3.49)
ATR		6.55	13.79**	11.24**		14.75**	12.02*
		(4.38)	(5.50)	(5.70)		(6.36)	(7.29)
ATR+MTR			9.84	6.62			11.78
s.e. (ATR+MTR)			3.61	3.77			5.24
First-stage F-statistic	253.40	150.16	81.30	81.29	253.41	150.10	81.29
N	2471793	2471793	2471793	2471819	2471820	2471820	2471820
Instruments							
$1[d_i > 0]\Delta_i \times MTC_i^t$	Yes	–	Yes	Yes	Yes	–	Yes
$1[d_i > 0]\Delta_i \times ATC_i^t$	–	Yes	Yes	Yes	–	Yes	Yes

As in the standard Gruber and Saez (2002) framework, my estimates are local to households around the tax threshold. More specifically, the MTR and ATR effects are largely identified by

[46]E.g., the effect of financial inclusion on wealth accumulation (Célerier and Matray, 2019).

high-MTC and high-ATC households, respectively. As is clear from Figure 8, MTR effects are not identified from the wealthiest households. This is also the case for the ATR effects, since the sample density becomes quite low as initial TNW exceeds around 3 MNOK.[47]

In standard models, the theoretical ambiguity of how saving responds to changes in the net-of-tax rate refers to unadjusted changes in savings. Therefore, in column (4), I consider $\Delta \log(GFW)$, which includes any mechanical effects of increased wealth taxation. This reveals qualitatively similar findings. Although the MTR effect increases in magnitude, it is still only 40% as large as the ATR effect. We also see that the sum of the coefficients, which give the implied effect of a linear change in the tax rate, is still positive and significant (t=1.76).

To better understand how the IV methodology produces the estimates in Table 3, I provide first-stage and reduced-form estimates when not allowing first-stage and reduced-form coefficients to vary with $1[TNW_{i,2009} > 0]$ (discarding the a subscript) in Table B.10. This lighter specification provides nearly identical IV estimates, and facilitates easier translation of first-stage and reduced-form coefficients to the final IV estimates. Panel B of Table B.10 shows that the IV estimates are essentially mildly-rescaled versions of the reduced form coefficients on the assessment discontinuity terms, $\mathbb{1}[d_i > 0]\Delta_i * MTC$ and $\mathbb{1}[d_i > 0]\Delta_i * ATC$. As expected, we see in Panel C that $\mathbb{1}[d_i > 0]\Delta_i * MTC$ contributes more to the MTR than the ATR. This inverse holds true for $\mathbb{1}[d_i > 0]\Delta_i * ATC$, which provides a strong instrument for the average tax rate but does not predict marginal rates when both instruments are included.

6.2.2 Portfolio Allocation

I use the IV methodology to reconsider the effect on portfolio allocation in Table B.8 in the Appendix. Panel A shows that the stock market share is not affected by changes to either the marginal or average tax rates. Panel B re-examines the effect on realized returns on safe assets, in the form of the achieved pre-tax interest rates on deposits. This exercise suggests that the net effect of zero that I found in section 4.4.2 is a combination of negative MTR and positive ATR effects. While a one percentage point (pp.) increase in the MTR leads to a 0.1 pp. decrease, a one pp. increase in the ATR leads to a 0.18 pp. increase in realized pre-tax interest rates. The positive, albeit marginally statistically significant (p <0.1), effect on ATR is consistent with my earlier hypothesis that income effects may induce households to enjoy less financial leisure. The negative effect on the marginal tax rate (p <0.1), however, is a bit more puzzling. This result is somewhat harder to rationalize since the wealth tax is, to a first order, a tax on the stock of savings, and only to a second order, a tax on the return on savings. One possible explanation is that households are somehow discouraged from financial optimization on the extensive margin, but on the intensive-margin income effects kick in, leading to a combined effect that is weakly positive.

[47] This may limit the applicability of my findings to the ultra wealthy. However, while it is clear that ultra-wealthy households may be more elastic in terms of evasion responses, it is not obvious that they would be more elastic in terms of real responses. If anything, a higher ratio of financial to human wealth suggests a lower EIS threshold below which positive saving responses occur (Schmidt and Toda, 2019), which makes positive saving responses more likely.

6.2.3 Tax Behavior

The existing wealth-tax literature has focused on estimating tax base semi-elasticities, namely the responsiveness of $\log(TNW)$ to the wealth tax rate. To provide more comparable estimates, I consider the responsiveness of TNW to the marginal and average tax rates in Appendix Section C. I find no evidence that the wealth-tax base is particularly elastic, which suggests that only considering mechanical effects can provide a good approximation for the total response (see, e.g., Smith, Zidar, and Zwick 2020). Consistent with the saving results, I find a weakly negative effect of increasing MTRs (p-value=0.58), and a larger, positive effect of increasing ATRs (p=0.27). The implied semi-elasticity with respect to a linear wealth-tax rate is modestly positive at 4.67 (se=3.52). I further consider the effect on self-reported financial wealth, where evasion or avoidance responses may play a larger role. Interestingly, I find a reversal of the MTR and ATR effects. While the point estimates are not all significant with p-values of 0.07 and 0.25, they may be indicative of households being more vigilant about reporting their non-third-party-reported holdings once they reach the threshold, while becoming more likely to misreport once the impact on the annual wealth tax bill becomes larger.

In Section C, I also discuss the point estimates in more detail and relate them to existing findings in the wealth-tax literature. Relatedly, Appendix Section D introduces new evidence of no bunching around the Norwegian wealth-tax threshold, which further points toward a limited role for evasion and avoidance in my empirical setting.

7 Discussion

In this paper, I address an important and long-standing question in economics, namely, how household savings and labor earnings 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 the difficulty of isolating real responses from evasion and avoidance effects. By using a novel source of identifying variation in wealth tax exposure in an empirical setting in which observed 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. In addition, capital taxation may encourage households to supply more labor. This is important for policymakers to consider when considering the 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 that arise in partial equilibrium in the household sector. Wealth taxation, and capital taxation in general, may have potentially adverse general equilibrium effects or effects that operate through the corporate sector that are not considered in this paper.[48] To account for these and other effects, researchers may need to employ a macroeconomic model as in Rotberg and Steinberg (2021) or Guvenen, Kambourov, Kuruscu, Ocampo, and Chen (2019), or estimate effects at a less-disaggregated (e.g., state) level as Agersnap and Zidar (2020) do,and account for effects on asset prices (Mason and Utke 2021; Bjerksund and Schjelderup 2021). When wealth taxation depends on the taxpayer's residence, one would also need to consider the effects on migration as in Agrawal, Foremny, and Martínez-Toledano (2020).

My results on the savings effects of wealth taxation are qualitatively different from the main findings in 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. Taxable wealth 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.[49] In Denmark, for example, only households in the top 1% to 2% of the wealth distribution paid a wealth tax. 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.

In terms of external validity, it is not clear why finding a positive as opposed to a negative effect of wealth taxes on saving 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 in which 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. The same applies to the fact that I study moderately- rather than ultra-wealthy households. From a theoretical perspective, ultra-wealthy households are even more likely to display positive saving responses to adverse rate-of-return shocks than the moderately wealthy. This is because ultra-wealthy households are closer to the pure capitalist modeled in, e.g., Straub and Werning (2020), who respond by saving more as long as the EIS is below the fairly high cutoff of 1.

At face value, the finding of a *positive* effect on savings is somewhat surprising. However, as I showed in Section 5, nonnegative saving responses to a negative rate-of-return shock can be generated by plausible parameterizations of a life-cycle model. For example, the EIS estimate of 0.1 in Best, Cloyne, Ilzetzki, and Kleven (2020) would, in the model calibrated to my empirical setting, produce simulated saving responses statistically indistinguishable from my findings. 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 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 to a larger role for the partial-equilibrium mechanism of Auclert (2019) and the general-equilibrium mechanisms of Kaplan, Moll, and Violante (2018) in explaining aggregate responses to monetary policy.

[48]Interestingly, however, Boissel and Matray (2021) find evidence consistent with income effects dominating substitution effects in how owner-managers respond to more aggressive dividend taxaton.

[49]This offers an interesting analogy to Martinez et al. 2021 who find a near-zero intertemporal labor supply elasticity for individuals with fewer avoidance opportunities.

In addition, my results are driven by older, wealthier households, which suggests that these households may respond in the opposite way to that of a representative agent, highlighting the need to study the behavior of younger, constrained households, as in Wong (2019).

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Online Appendices

A Data and Empirical Appendix

A.1 Wealth Variables

- Deposits is the sum of deposits in Norwegian banks, which is Tax Return (TR) item 4.1.1, as well as deposits held abroad (TR 4.1.9). This includes savings and checking accounts, and accounts with higher interest and limitations on the timing/number of withdrawals. Information on deposit holdings are reported directly from banks to the tax authorities.
- Stock Market Wealth (SMW) is the sum of listed domestic stocks (4.1.7.1), mutual fund (equity) holdings (4.1.4), other taxable capital abroad (TR 4.6.2). The first two items are third-party reported. The third is largely self-reported and includes foreign financial securities (excl. deposits in foreign banks).
- Private Equity (PE) is the value of directly held unlisted stocks (TR 4.1.8). The PE share of GFW is 5.6% at the mean and 0% at the 75th percentile for the full sample during 2010–2015. The value of unlisted stocks are reported by the stock issuer as part of their annual tax returns. If the stock issuer owns domestic financial securities (e.g., listed stocks, deposits), then this is reported directly to the tax authorities the same way as it would be reported for individuals.
- GFW is the sum of SMW, deposits, deposits in foreign banks (TR 4.1.9), PE, bonds (TR 4.1.5 and TR 4.1.7.2), and outstanding claims (TR 4.1.6). Outstanding claims contain unpaid wages (reported by the firm) and loans to friends and family (self-reported).
- TaxVal is the tax value of housing wealth (TR 4.3.2).
  - This is the sum of the tax value of primary housing (which is what I instrument for) and the tax value of secondary housing.
  - If TaxVal is missing, but GFW or Total Taxable Labor Income (TTLI) is not (2.7% of observations), it is first replaced with the average of the lag and lead. If the lead is missing, it is replaced with the lag, if the lag is missing, it is replaced with zero. This addresses the concern that housing transactions may render TaxVal missing, but that repeated missing values most likely indicates non-ownership.
  - When taking logs, the base is shifted (for all wealth variables, by NOK 10,000 or approximately USD 1,667), which retains households who sold their house in the sample (by avoiding log(0) returning missing values). For the other variables, it ensures that small level changes do not lead to extreme log-differences (e.g., an increase in GFW from NOK 1 to NOK 100 ($\approx$ \$17) would otherwise lead to a log-difference of 4.7).
  - This process thus ensures that households who might sell their house in response to the treatment remain in the sample.
- TGW is the sum of GFW, TaxVal, holiday homes, forest property and other property (TR 4.3.3, TR 4.3.4, TR 4.3.5), real estate held abroad (TR 4.6.1), capital in housing coops (TR 4.5.3), home contents/moveable property (TR 4.2).
- TNW is TGW minus debt (TR 4.8).
  - The taxable net wealth definition used by the tax authorities to calculate wealth taxes also include less-frequently used posts TR 4.4 and TR 4.5, which are not in my data. To calculate wealth tax payments, the net wealth variable "netto formue" (nto_form) from the FD-Trygd database is used instead, as it includes all such posts. I also use this to create dummy variables indicating whether a household is above the wealth tax threshold. The nto_form variable is not available for 2015. I then use the previous definition of TNW to calculate *wtax*.
  - The variable nto_form is also bottom-coded for many years (displaying zeros if no wealth taxes are paid), which is not an issue for the first-stage analyses since those only need taxable wealth *in excess of the tax threshold*. However, when this variable is used for the bunching analysis, I must limit the analyses to years in which it is not bottom-coded.

  - The median relative difference (TNW - nto_form)/(0.5*TNW + 0.5*nto_form) for 2014 (when there is no bottom-coding) is 0, and the 10th and 90th percentiles are -0.0386 and 0.0543, respectively.
- wtax is $\tau_t(nto\_form - Threshold_t)$ for all years except 2015, when it equals $\tau_t(TNW - Threshold_t)$.
- Stock Market Share (SMS) is the ratio of SMW to GFW. Risky Share is (SMW + PE)/GFW.
- The foreign share (Foreign/GFW) is defined as (deposits in foreign banks plus other taxable capital abroad)/GFW.
- The deposit share (Deposits/GFW) is the ratio of deposits to GFW.

A.2 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,[50] 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 nonlinearities arising.

Below, I describe a simple motivating example in which house prices move linearly within border areas, and the geographic slope varies only with two key characteristics: 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, $S = L, H$. Assume that $\mathbb{E}[k(c_i)|i \in S] = k(\mathbb{E}[c_i|i \in S])$ (a linearity assumption)[51]. 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.

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

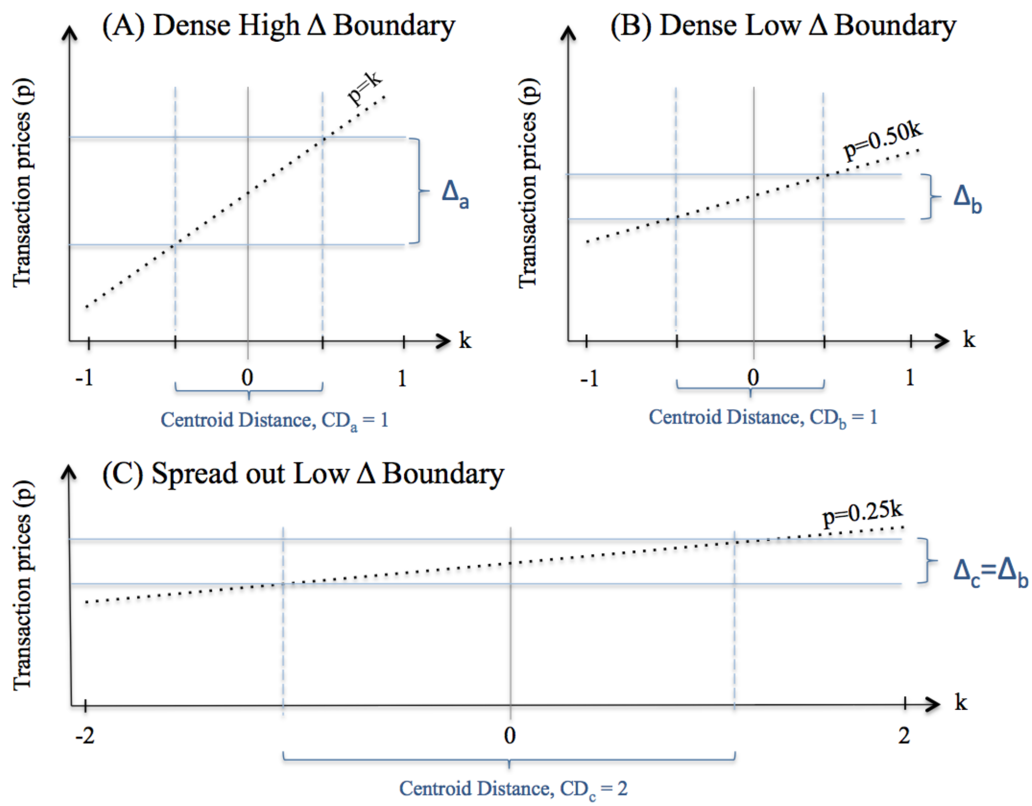
This example contains the two key elements: (1) house prices have larger geographic gradients when the differences in averages are higher and (2) more dense (less scattered) areas have larger geographic gradients. I illustrate this in Figure A.1. The boundary regions in (A) and (B) differ only in that 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 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.

[50]In Panel A of Figure A.3, I show that households located near the boundary (in terms of kilometers) live in much denser areas than those farther away.

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

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. Panels 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). Panels 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: $k(\mathbf{c}_i) = d_i$. d_i is the distance to the nearest household on the opposite side of a municipal (or within-city district) boundary. It ignores heterogeneity in residential density by assuming a (normalized) centroid distance, $CD_b = 1$. This invites the problem of nonlinear slopes on d_i, which 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.

$$\text{(Unscaled) Border Distance term: } g_b(\mathbf{c}_i) = \gamma \cdot d_i \tag{23}$$

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 the centroid distance in b, CD_b. Households are assigned to a b based on the municipality (within-city district) of the geographically closest residence on the opposite side of a municipal or (within-city district) boundary. This measure provides the distance between the centroids of the two municipalities (or within-city districts) that constitute the border area b. All individuals in a given municipality share the same centroid vector, but may face different CD_bs to the extent that they differ in which neighboring municipality they are closest to. For the purposes of this analysis, we can define $CD_b = Dist(\mathbf{c}_{b,H}, \mathbf{c}_{b,L})$. The subscripts (b, L) indicate that we are concerned with the centroid of the municipality on the low (L) assessment side of the boundary b. The following term then captures the within-border area geographic variation in house prices, where the expectation is that $\hat{\gamma} = 1$.

$$\text{Scaled Border Distance term: } g_b(\mathbf{c}_i) = \gamma \cdot \frac{d_i}{CD_b} \tag{24}$$

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 $\mathbf{c}_{b,H}$ than $\mathbf{c}_{b,L}$: $k(c_i) = \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,L}) - \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,H})$. 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 $\hat{\gamma} = \frac{1}{2}$.

$$\text{Relative Location term: } g_b(\mathbf{c}_i) = \gamma \cdot \frac{\text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,L}) - \text{Dist}(\mathbf{c}_i, \mathbf{c}_{b,H})}{\text{Dist}(\mathbf{c}_{b,H}, \mathbf{c}_{b,L})} \in [-\gamma, \gamma] \tag{25}$$

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.[52]

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 and 2.

In the specifications using border distance, there is a 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 other side of the border. 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.[53]

A.3 Model-implied v. Actual Tax Value of Housing Wealth

In Figure A.2 below, I show the mapping in a scatter-plot format from model-implied tax assessments to the actual tax assessments of housing wealth observed in the tax returns.

The actual tax values may differ from predicted tax values for a few reasons. First, the coefficients I use are based on estimating equation (2) on 2004–2008 data. These are, to the best of my knowledge, the same coefficients that were used to inform households of their new tax assessments *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

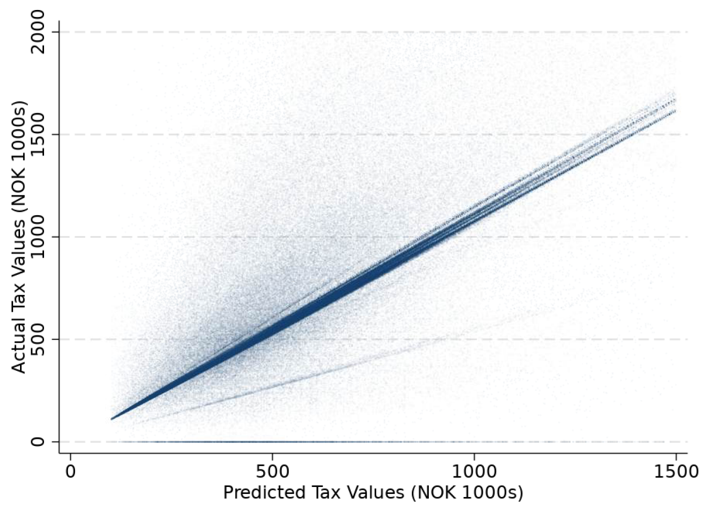
[52] I use the centroids of all *residences* to proxy for the centroid of the actual estimation sample. Some areas see very few or no housing transactions; thus using all residences provides a more widely applicable measure.

[53] 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.

would slightly impact the coefficients and the assessed tax values. Second, the amount of housing wealth observed in the tax returns, $TaxVal$ (no hat), also includes the value of secondary homes, while I estimate model-predicted tax values, $\widehat{TaxVal}$, only considering primary residences. This leads to a few cases in which $TaxVal > \widehat{TaxVal}$. This is inconsequential for the analysis, since the main first-stage regressions use measures of wealth-tax exposure (e.g., MTR or ATR on wealth) and not $\widehat{TaxVal}$ by itself as the dependent variable. Third, households may have moved during 2010. Finally, they may have filed a complaint regarding the tax assessment. While assessed tax values are meant to equal $0.25\times$ market value, households who can document that their assessment exceeds $0.30\times$ market value may have the assessment lowered to $0.30\times$ market value, but not to $0.25\times$. In other words, even if the assessment is 20% too high, there are no incentives to complain. This ensures that the possibility of households' complaining does not materially lower the explanatory effect of the model coefficients on actual tax assessments.

Figure A.2: 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 that 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.



A.4 Descriptive Figures and Tables

Figure A.3: Residential Density Around Borders

This graph shows how residential density varies with border distance. Density is defined 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.

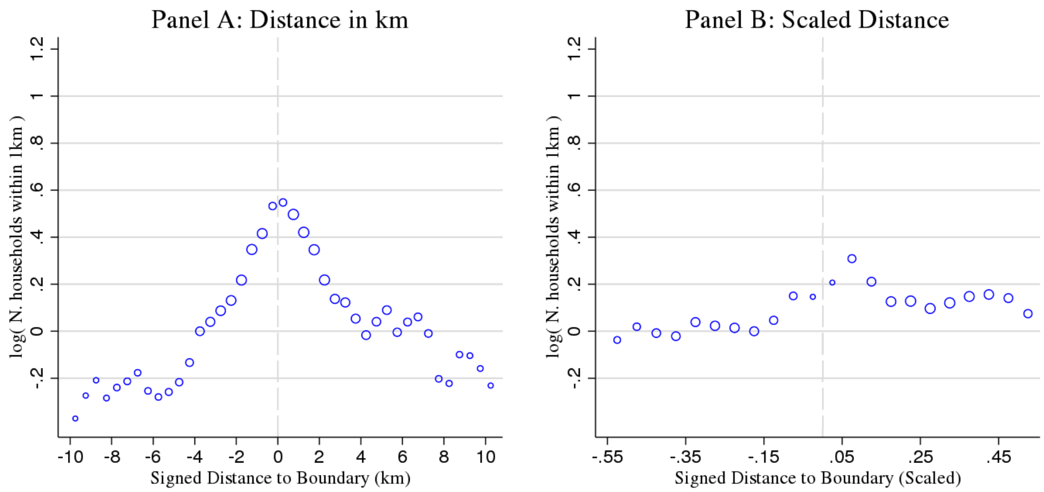


Figure A.4: Geographic Distribution of Households

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 included, 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 green shade indicates membership in the high-assessment side of the boundary.

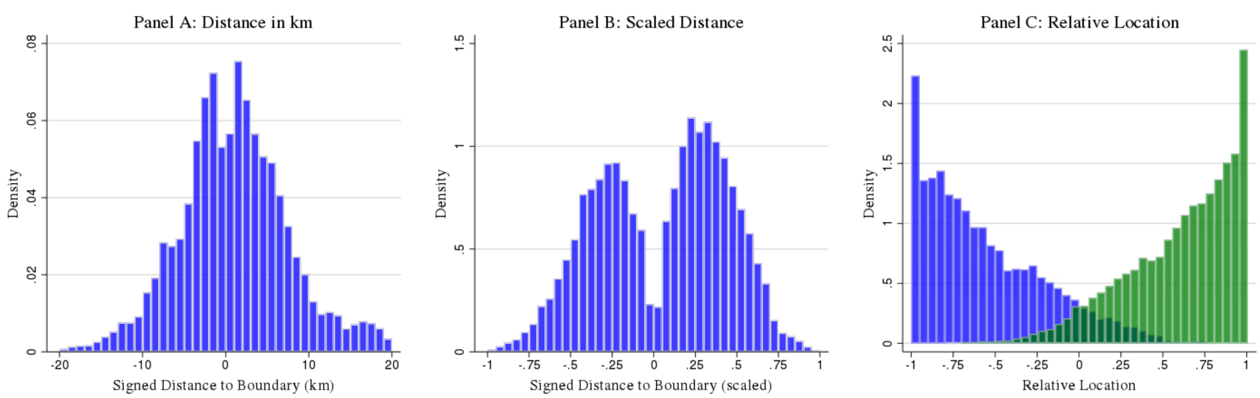


Figure A.5: Example of Data Source for House Price Model Coefficients

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 $\sigma_R = 0.28800$.

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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

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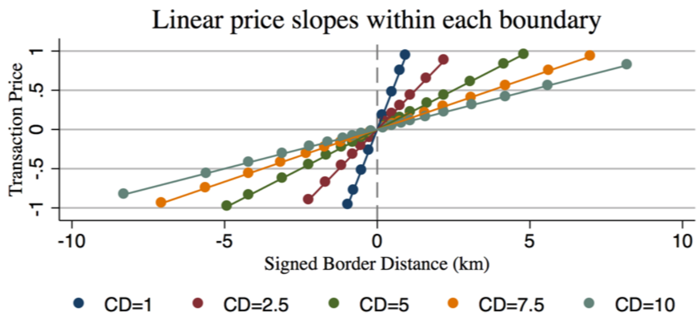
Figure A.6: Example of nonlinearities in a pooled boundary region

I employ the following procedure to create this graphical example. First, I 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.



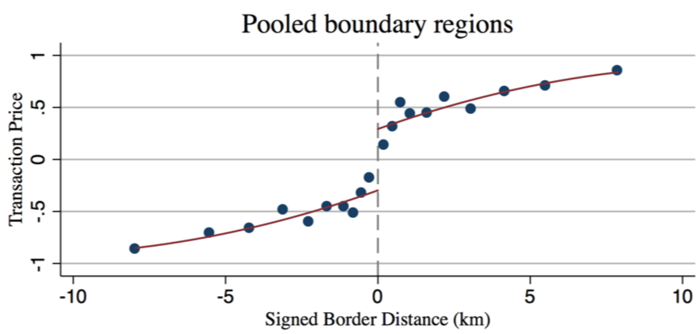


Table A.1: Summary Statistics

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* indicates that GFW is winsorized (censored) at the 95th percentile. TGW is Taxable Gross Wealth. TNW is TGW minus Debt. Labor Earnings (LE) is the sum of wage and salary earnings and max(self employment income, 0). Total Taxable Labor Income (TTLI) is the sum of LE, UI benefits and other transfers, and labor-related pension income. SMW is the sum of mutual-fund holdings, direct holdings of listed domestic stocks and financial securities (excl. deposits) held abroad. $TaxVal$ is the assessed tax value (housing wealth) observed in the 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 non-listed 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 households who in either the current or subsequent period had $Debt < 10,000$. Further information on the wealth variables can be found in subsection A.1 in the Appendix.

	(A) Main sample Used in main reduced-form analyses $TNW_{2009} > 0$						(B) Extended sample Used for MTR-ATR decomposition TNW_{2009} >-3 MNOK Obs. weighted by MTC+ATC					
	N	mean	sd	p25	p50	p75	N	mean	sd	p25	p50	p75
GFW, censored	1540156	1000	1109	224	580	1325	2376567	866	953	226	544	1109
2010–2015, NOK 1000s												
GFW	1540156	1178	2144	223	580	1324	2376567	964	1721	225	543	1109
Debt	1540156	515	1047	0	141	621	2376567	631	1184	0	165	794
TGW	1500236	2268	2628	972	1555	2611	2376567	2142	2171	1083	1612	2484
TNW	1500236	1743	2409	586	1182	2163	2376567	1512	1979	683	1218	1923
T. Taxable L. Income	1543195	694	487	378	581	893	2376567	698	519	355	556	898
Labor Earnings	1543195	432	570	0	209	744	2376567	453	601	0	236	753
SMW	1540156	153	567	0	0	82	2376567	132	465	0	0	83
Domestic Deposits	1540156	722	974	152	407	914	2376567	611	793	145	376	789
TaxVal	1506487	833	711	468	664	977	2376567	953	721	554	788	1153
wtax	1540258	8	22	0	0	8	2376539	7	17	0	0	7
2004–2009, NOK 1000s												
TaxVal	1506487	833	711	468	664	977	2376567	953	721	554	788	1153
2010–2015												
SMW/GFW	1527230	0.108	0.202	0.000	0.000	0.119	2371941	0.119	0.214	0.000	0.000	0.139
RiskyShare	1527230	0.164	0.266	0.000	0.006	0.225	2371941	0.169	0.269	0.000	0.009	0.238
Deposits/GFW	1527230	0.802	0.292	0.687	0.977	1.000	2371941	0.794	0.296	0.663	0.970	1.000
Foreign/GFW	1527230	0.008	0.054	0.000	0.000	0.000	2371941	0.009	0.062	0.000	0.000	0.000
Self-rep/GFW	1527230	0.026	0.108	0.000	0.000	0.000	2371941	0.029	0.116	0.000	0.000	0.000
wtax/GFW	1521005	0.006	0.033	0.000	0.000	0.007	2371917	0.009	0.048	0.000	0.001	0.008
wtax>0	1540258	0.456	0.498	0.000	0.000	1.000	2376539	0.535	0.499	0.000	1.000	1.000
2009												
r, Deposits (pp.)	1315492	1.94	1.07	1.16	2.03	2.64	2020663	1.94	1.08	1.18	2.01	2.62
r, Debt (pp.)	790456	4.04	1.56	3.39	4.07	4.79	1524925	3.94	1.47	3.31	3.96	4.65
Age (avg. of adults)	1513409	61.49	13	52	61	70	2291999	61.39	13	52	61	71

Table A.2: Summary Statistics When Restricting to Households Near Boundary and/or Weighting by Propensity to Experience Extensive- Versus Intensive-Margin Effects

This table provides means for variables during 2010–2015. • Column (1) does not apply any weights or limit the sample to households near the boundary. Column (2) limits the sample to the 25% of households nearest the boundary in terms of kilometers, by keeping the subsample of households with abs(d_i^{km}) below the 25th percentile. Column (3) similarly restricts the sample to households closest to the boundary in terms of d_i^{scaled}. Column (3) weights households by their MTC_i, defined in equation (19), which assigns a higher value to households who would see a greater effect of increases in $TaxVal$ on their marginal tax rate (MTR) . Column (4) weights households by their ATC_i, defined in equation (20). This variable similarly assigns a larger value to households who would see a greater effect on their average tax rate (ATR). Columns (6)-(7) weight by MTC, but consider the 25% subset of households nearest the boundary. Columns (8)-(9) are similar to (6)-(7) but instead weight by ATC. • Gross financial wealth (GFW) is the sum of stocks, mutual fund holdings, bonds, and deposits. Taxable net wealth (TNW) equals taxable gross wealth (TGW) minus debt. Stock market wealth (SMW) consists of holdings in listed stocks and mutual funds. TaxVal is assessed housing wealth. T. Taxable L. Income is total taxable labor income, which includes labor earnings, as well as pensions and other transfers. Labor earnings is the sum of salary and wage payments and max(self employment income,0). wtax is the amount of wealth taxes accrued in a given year. Age is measured in 2009. Deposits/GFW is the share of GFW held in deposits (bank savings). Foreign/GFW is the share of GFW held abroad, and not subject to third-party reporting. Self-rep/GFW is the share of GFW that is potentially self-reported; some subitems may be third-party reported. wtax/GFW is the ratio of wtax to GFW. Int. R. Deposits is the realized average return on deposits. 1[wtax>0] indicates whether a household had to pay any wealth taxes. 1[GFW<0,000] indicates whether a household had less than NOK 50,000 in GFW. 1[wtax>0.25*GFW] indicates whether a household accrued wealth taxes in excess of 25% of their GFW.

Weighted	–	–	–	MTC	ATC	MTC	MTC	ATC	ATC
Near Boundary	–	km	scaled	–	–	km	scaled	km	scaled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Main Sample 0 < TNW_{2009} per adult < 6 MNOK								
NOK 1000s									
GFW	1171	1452	1216	757	1352	822	769	1481	1376
Debt	510	652	553	467	510	618	516	640	562
TGW	2252	2878	2378	1795	2637	2104	1875	3050	2743
TNW	1732	2213	1815	1328	2126	1486	1358	2410	2180
SMW	150	219	167	93	191	115	99	234	204
TaxVal	826	1119	891	842	1026	1059	898	1281	1096
Domestic Deposits	719	827	731	528	813	539	526	842	811
T. Taxable L. Income	691	784	715	645	703	719	668	778	728
Labor Earnings	429	510	451	390	437	462	414	504	460
wtax	8	12	9	4	12	5	4	14	12
$TaxVal_{2009}$	532	595	558	537	586	581	562	628	613
Age (avg. of adults)	62	61	61	62	63	62	62	63	63
SMS = SMW/GFW	0.108	0.126	0.113	0.105	0.124	0.117	0.108	0.138	0.129
Deposits/GFW	0.803	0.768	0.793	0.823	0.770	0.803	0.817	0.748	0.761
Foreign/GFW	0.008	0.012	0.009	0.007	0.011	0.010	0.008	0.014	0.012
Self-rep/GFW	0.025	0.035	0.029	0.023	0.034	0.029	0.025	0.041	0.038
wtax/GFW	0.006	0.009	0.007	0.007	0.013	0.011	0.008	0.018	0.015
TaxVal/TGW	0.478	0.498	0.484	0.535	0.474	0.573	0.545	0.511	0.485
Int. R. Deposits (pp.)	1.94	2.05	1.97	1.88	2.13	1.90	1.89	2.15	2.14
1[wtax>0]	0.453	0.564	0.472	0.456	0.743	0.501	0.462	0.779	0.748
1[GFW<50,000]	0.073	0.062	0.073	0.063	0.041	0.068	0.067	0.042	0.042
1[wtax>0.25*GFW]	0.002	0.004	0.003	0.004	0.006	0.007	0.005	0.010	0.008
	Panel B: Expanded Sample -3 MNOK < TNW_{2009} per adult < 6 MNOK								
NOK 1000s									
GFW	806	1005	837	726	1287	781	736	1394	1306
Debt	1106	1318	1159	627	625	844	694	783	683
TGW	1854	2370	1955	1807	2588	2122	1887	2977	2687
TNW	733	1034	780	1180	1963	1278	1194	2193	2003
SMW	107	156	118	93	184	116	99	223	195
TaxVal	827	1098	884	881	1044	1111	940	1297	1112
Domestic Deposits	488	565	496	496	770	497	492	787	765
T. Taxable L. Income	757	857	780	676	721	763	702	800	747
Labor Earnings	564	659	588	439	468	530	467	542	493
wtax	5	7	5	3	11	4	4	13	11
$TaxVal_{2009}$	513	563	535	534	582	569	557	619	608
Age (avg. of adults)	55	55	55	61	62	60	60	61	62
SMS = SMW/GFW	0.120	0.137	0.123	0.112	0.128	0.128	0.116	0.143	0.133
Deposits/GFW	0.794	0.764	0.788	0.814	0.767	0.789	0.806	0.744	0.758
Foreign/GFW	0.007	0.011	0.008	0.008	0.011	0.011	0.009	0.014	0.012
Self-rep/GFW	0.027	0.034	0.030	0.025	0.034	0.031	0.027	0.041	0.038
wtax/GFW	0.004	0.006	0.004	0.007	0.012	0.010	0.008	0.017	0.014
TaxVal/TGW	0.573	0.586	0.577	0.554	0.492	0.594	0.565	0.529	0.503
Int. R. Deposits (pp.)	1.72	1.83	1.75	1.84	2.08	1.86	1.85	2.10	2.09
1[wtax>0]	0.274	0.345	0.285	0.417	0.697	0.446	0.419	0.722	0.698
1[GFW<50,000]	0.172	0.145	0.171	0.083	0.055	0.089	0.087	0.057	0.057
1[wtax>0.25*GFW]	0.002	0.003	0.002	0.004	0.006	0.006	0.005	0.010	0.007

B General Appendix

B.1 Additional figures

FIGURE B.1: GRAPHICAL PRESENTATION OF THE REDUCED-FORM EFFECTS ON PRE-PERIOD WEALTH TAX EXPOSURE

These graphs illustrate how geographic discontinuities in tax assessment, $\widehat{TaxVal}$, affect intensive- and extensive-margin wealth tax exposure during **2004–2009**. Panel A considers the effect on the amount of wealth above the threshold and thereby subject to the wealth tax. Panel B considers the extensive-margin effect on whether households are above the threshold and thereby must face wealth taxation of marginal savings. ∘ The graphs show the reduced-form effect on these outcomes of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ∘ 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. ∘ Scatter-points stem from estimating a coefficient on Δ_i using equation (6) separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal}_i)$ and $g_b(c_i)\Delta_i$. One negative-distance bin is normalized to be zero. ∘ The sample includes households with $TNW_{2009} > 0$. The size of each circle corresponds to the relative number of observations in that bin. Standard errors are clustered at the census-tract level. First-stage effects during 2010–2015 are provided in Table 3 in the main text.

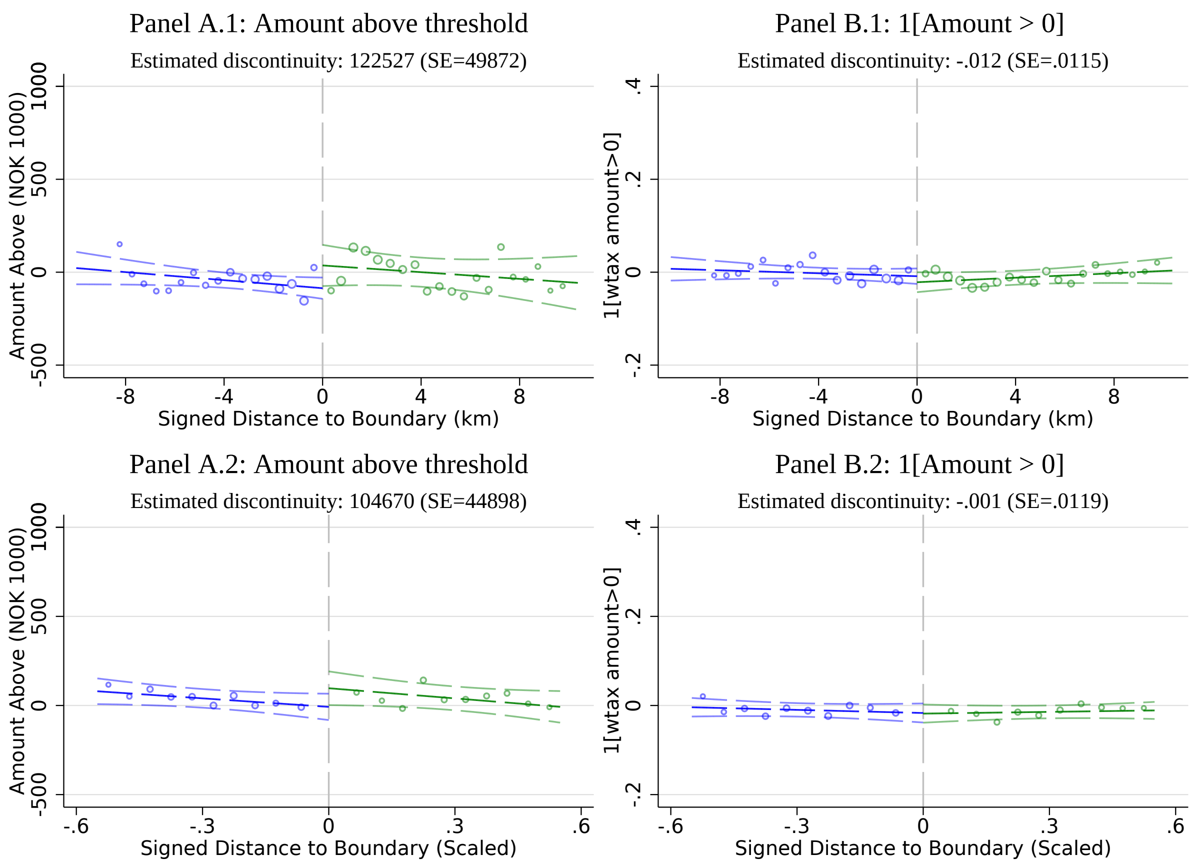


Figure B.2: Household Debt Accumulation Figure, Extensive Version

These graphs consider the effect on household debt accumulation, $\Delta \log(Debt)$, during 2004–2009 and 2010–2015 in Panels A and B, respectively. 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.

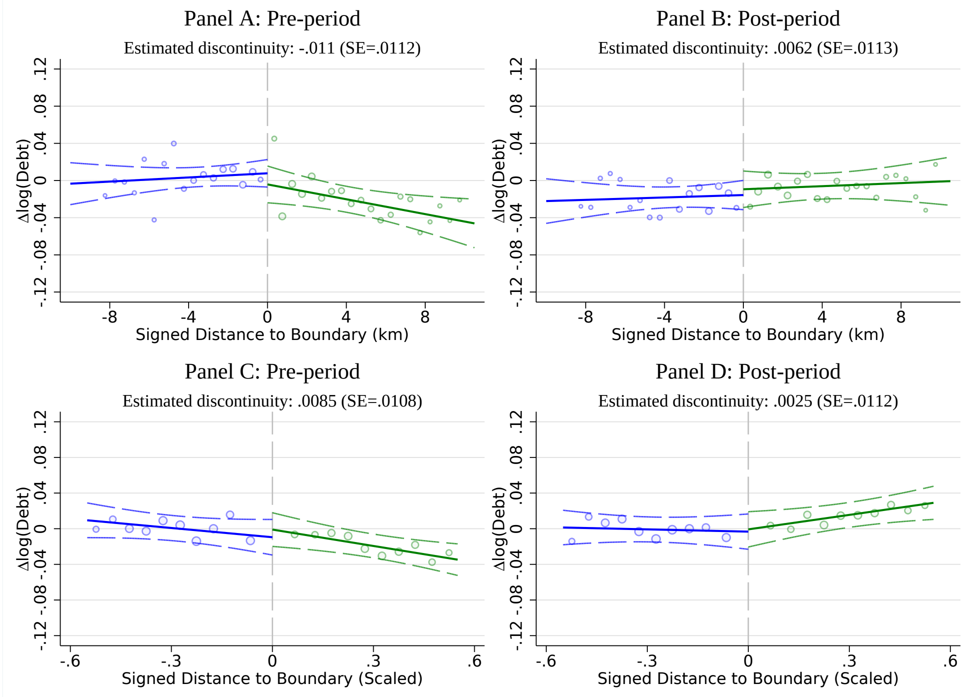


Figure B.3: Stock Market Share Figure, Extensive Version

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 sample includes households with initial $TNW > 0$.

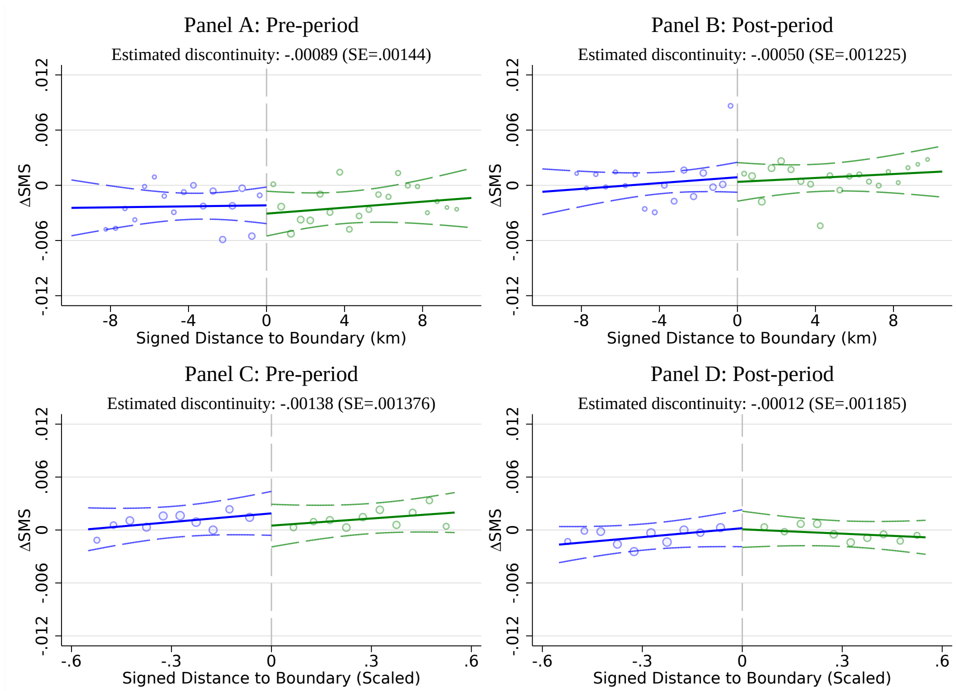


Figure B.4: Interest Rates on Deposits, Extensive Version

These graphs consider the effect on changes in the realized interest rates on deposits. Panel A considers pre-period outcomes (2004–2009) and Panel B considers post-period outcomes (2010–2015). ◦ The graphs below show the reduced-form effect on ΔSMS of living in a boundary region where households face a 1-log-point tax assessment premium on the high-assessment side. Circles provide the estimated effect for a given geographic bin. Solid lines provide the linear fit. The discontinuity at zero, jumping from the blue to the green solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{TaxVal}$. ◦ 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 sample includes households with initial $TNW > 0$. Scatter-points stem from estimating a coefficient on Δ_i using equation (6) separately for d_i bins, rather than estimating coefficients on $\log(\widehat{TaxVal_i})$ and $g_b(c_i)\Delta_i$. One negative-distance bin is normalized to be zero. The size of each circle corresponds approximately to the relative number of observations in that bin. Standard errors are clustered at the census-tract level.

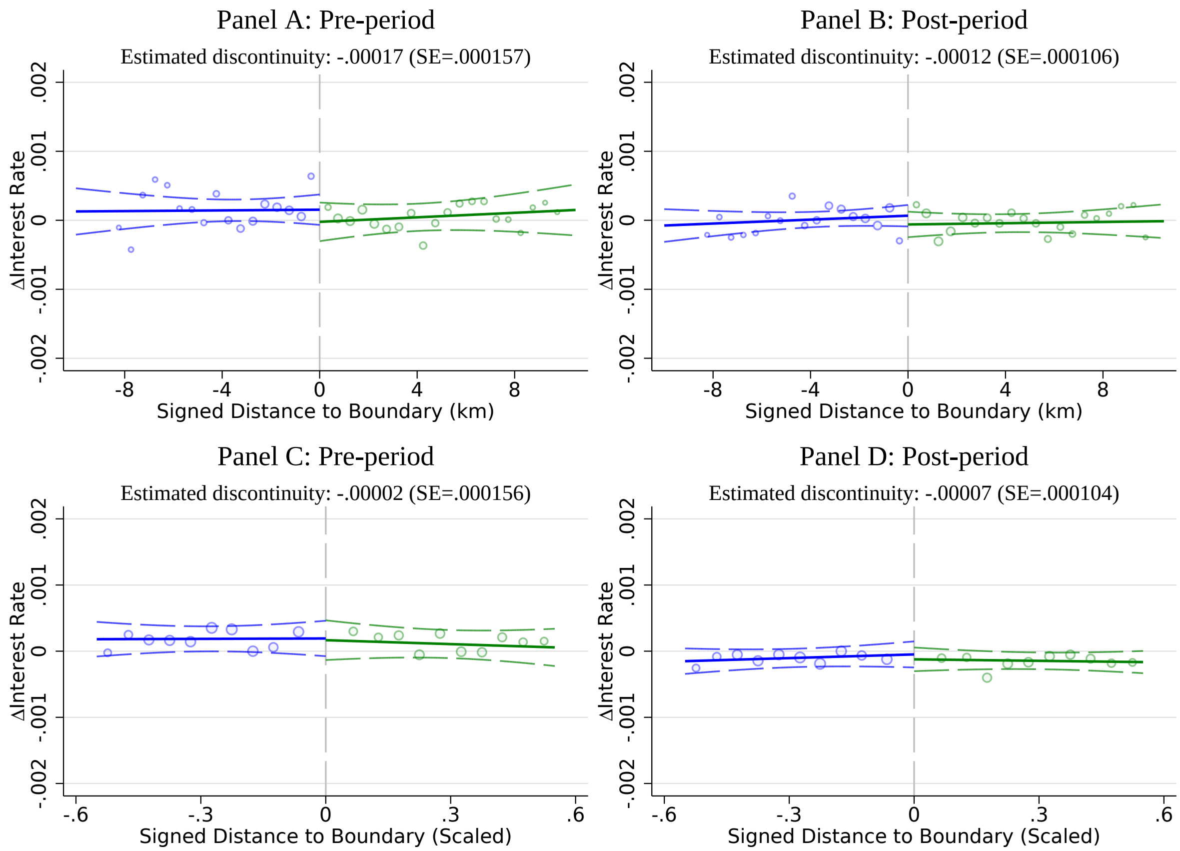


Figure B.5: Pre-Period Version of Labor Earnings Figure

- Figure 6 in the main text provides the estimated effect during the 2010–2015 period.

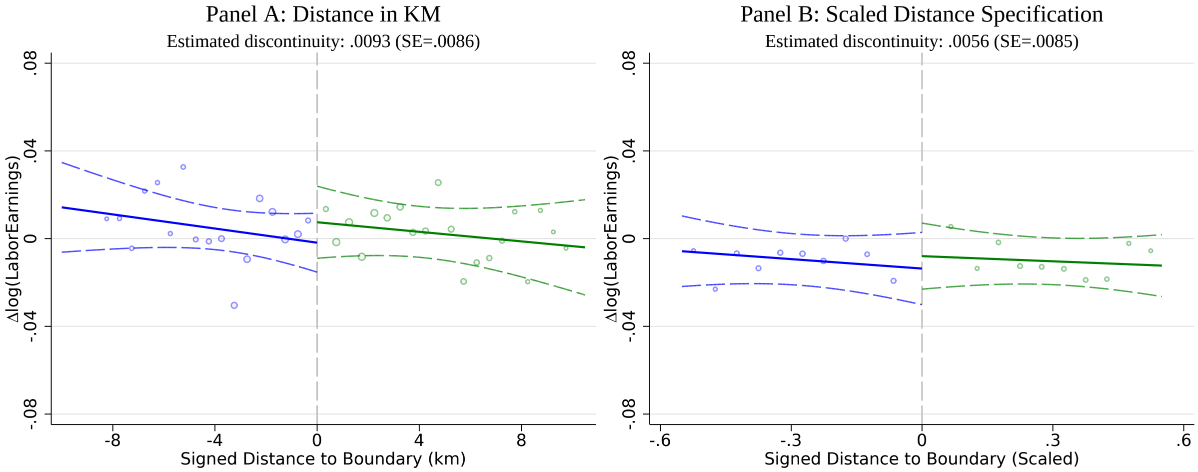


Figure B.6: Extensive-Margin Wealth Tax Exposure for Households with TNW Above the Wealth Tax Threshold in 2009

This figure illustrates the effect of assessment discontinuities on extensive-margin wealth tax exposure for households initially above the wealth tax threshold (TNW_{2009} per adult >470,000). This figure otherwise mirrors Panel B.2 of Figure 3.

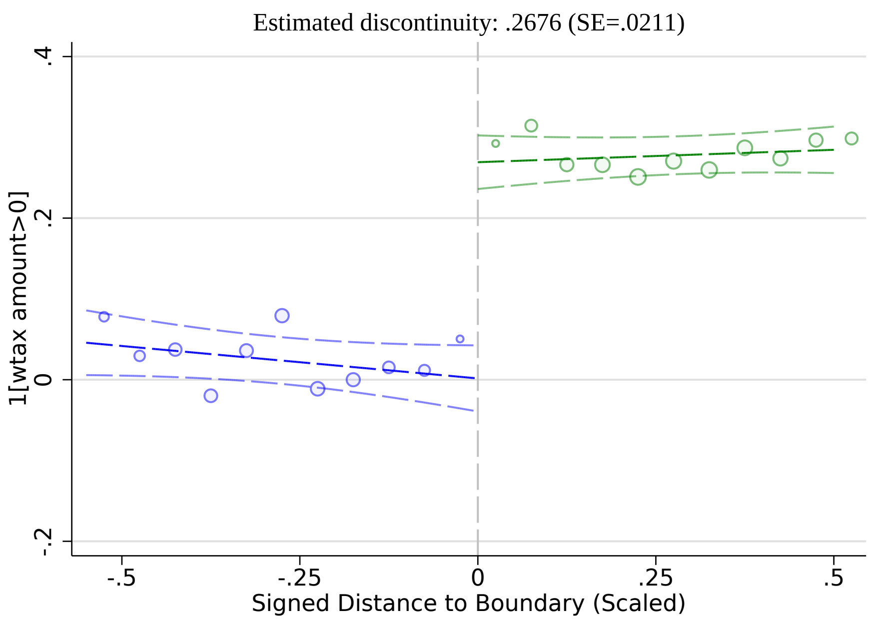


Figure B.7: Results on Unadjusted Financial Savings Growth

This figure provides the reduced-form effect on unadjusted financial saving, defined as $\Delta \log(GFW)$. This measure does not account for the mechanical wealth-reducing effect of increased wealth tax exposure, and so gives a net-of-wealth-tax saving measure. The sample and methodology mirrors that of in-text Figure 4, Panel C.

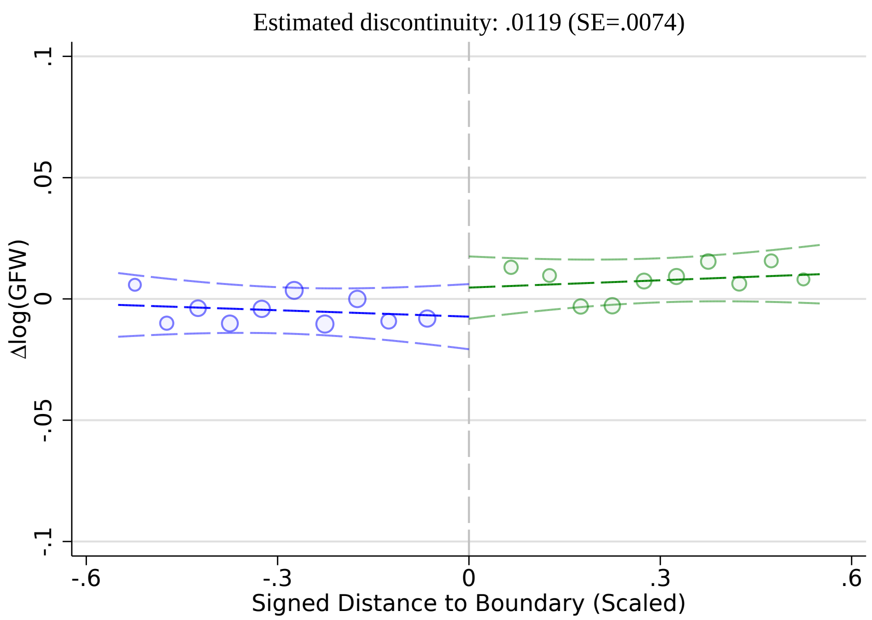


Figure B.8: Year-by-Year Boundary Discontinuity Estimates

In this graph, I allow the estimated discontinuities, $\hat{\beta}$, to vary by year. The dashed blue lines provide the main (pooled) estimates. Horizontal lines provide 95% confidence intervals.

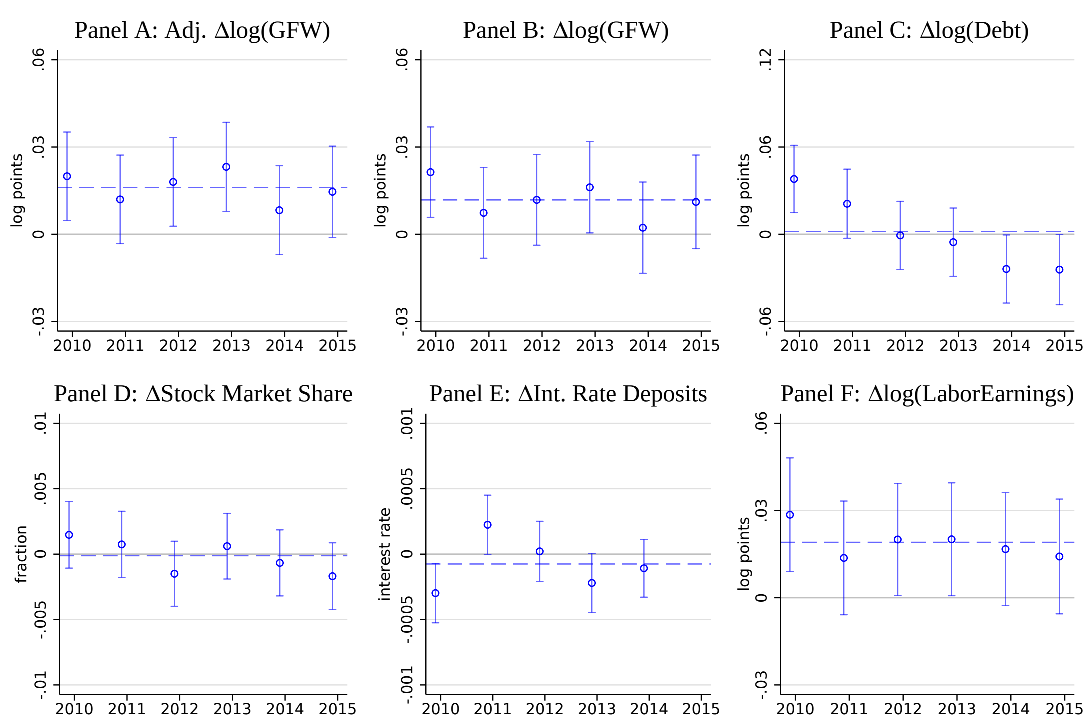
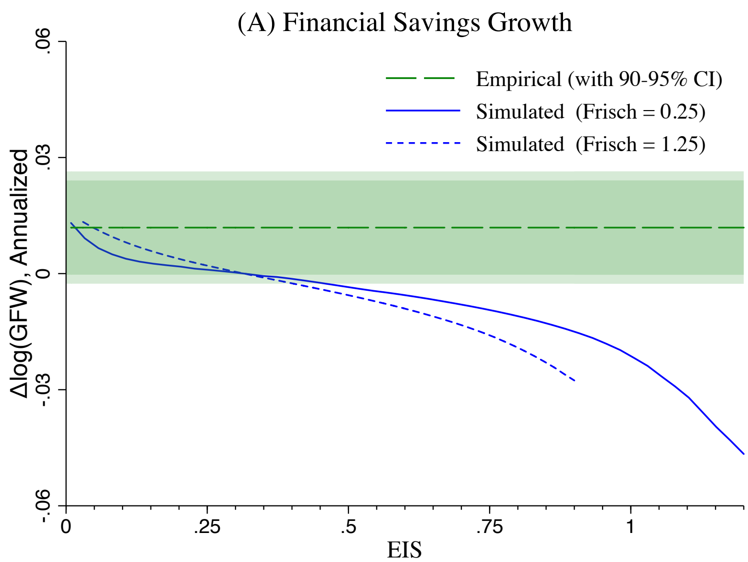


Figure B.9: Simulated Treatment Effects as a Function of the EIS when incorporating first-stage heterogeneity

This figure shows the how the relationship between simulated treatment effects and the EIS when incorporating first-stage heterogeneity. The sample is split into four quartiles based on TNW_{2009}. First-stage coefficients on MTR and ATR^{GFW} are estimated separately for each group. Table B.12 provides the estimation results and the subsample-specific summary statistics that underlie this simulation. This contrasts with in-text Figure 7 which models the responses of a single shocked agent. High-EIS high-Frisch simulation results are omitted due to computational (numerical) issues.



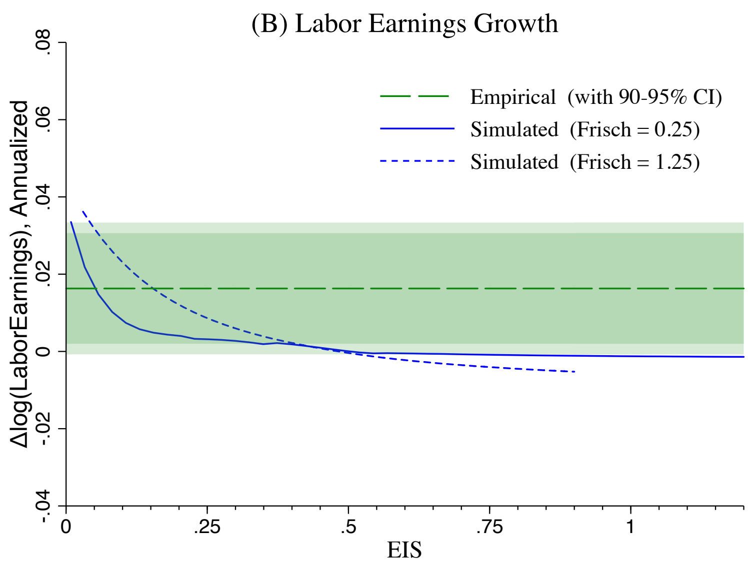


Table B.1: Subsequent Transaction Outcomes

This table provides the effects of a 1-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 that takes the value 1 if the house the household lived in during 2009 was transacted during 2010–2016. Standard errors are provided in parentheses, and are clustered at the census-tract level. • The geographic slopes in columns (5) and (6) are statistically consistent with the motivating examples discussed in subsection A.2 in the Appendix. These motivating examples would imply coefficients on the scaled border distance variables in column (5) of 1, and a coefficient on the relative location variable in column (6) would of 0.5.

	Panel A			Panel B		
	Sale Dummy			log(Transaction Price)		
	(1)	(2)	(3)	(4)	(5)	(6)
$\log(\widehat{TaxVal})$	0.0036	0.0021	0.0016	0.2401***	0.0355	0.0160
	(0.0029)	(0.0028)	(0.0023)	(0.0835)	(0.0853)	(0.0721)
$\mathbb{1}[d_i < 0] * d_i^{KM} * \Delta_i$	0.0003			0.0504***		
	(0.0004)			(0.0152)		
$\mathbb{1}[d_i > 0] * d_i^{KM} * \Delta_i$	-0.0002			0.0414***		
	(0.0004)			(0.0152)		
$\mathbb{1}[d_i < 0] * d_i^{scaled} * \Delta_i$		0.0047			0.9567***	
		(0.0060)			(0.1670)	
$\mathbb{1}[d_i > 0] * d_i^{scaled} * \Delta_i$		0.0044			1.0943***	
		(0.0064)			(0.2152)	
Relative Location$_i$ * Δ_i			0.0031*			0.5922***
			(0.0018)			(0.0596)
N	1540201	1553575	1740546	45422	44667	50203
R^2	0.1972	0.1931	0.1942	0.5336	0.5380	0.5256
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

B.2 Additional Results Tables

Table B.2: First-Stage Effects on Wealth Tax Outcomes: Using Distance in KM

This table provides the reduced-form estimates that uses border distance in km (as opposed to scaled border distance) as the geographic measure. The sample and methodology is otherwise identical to in-text Table 1 that uses the scaled distance specification.

		Extensive margin		Extensive and intensive margin	
	$\log(TaxVal)$	$\mathbb{1}[TNW > Threshold]$	$r^{marginal}$	AmountAbove	$r^{average}$
	(1)	(2)	(3)	(4)	(5)
$\log(\widehat{TaxVal})$	0.8727***	0.2406***	0.2556***	859122.4803***	0.2556***
	(0.0303)	(0.0137)	(0.0144)	(90043.0316)	(0.0144)
	[1462961]	[1426446]	[1426446]	[1426446]	[1426446]
$F(\hat{\beta} = 0)$	830	308	316	91	316
Border Distance Controls					
– KM	Yes	Yes	Yes	Yes	Yes

Table B.3: Main Reduced-form Estimates with 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. $\Delta\log(GFW)$	$\Delta\log(Debt)$	ΔSMS	ΔInt. R. Deposits	$\Delta\log(L.\ Earnings)$
	(1)	(2)	(3)	(4)	(5)
	Panel A: Scaled Border Distance				
$\log(\widehat{TaxVal})$	0.0182**	0.0025	0.0000	-0.0000	0.0209**
	(0.0082)	(0.0123)	(0.0013)	(0.0001)	(0.0103)
N	1442739	1442756	1437504	1240649	1444211
	Panel A: KM Border Distance				
$\log(\widehat{TaxVal})$	0.0143*	0.0080	-0.0005	-0.0001	0.0295***
	(0.0079)	(0.0128)	(0.0014)	(0.0001)	(0.0102)
N	1392561	1392579	1387513	1199969	1393962
	Panel A: Relative Location Measure				
$\log(\widehat{TaxVal})$	0.0168**	-0.0053	-0.0001	-0.0000	0.0189**
	(0.0068)	(0.0107)	(0.0011)	(0.0001)	(0.0083)
N	1614851	1614871	1608987	1389962	1616524

Table B.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. Slightly different point-estimates may occur when clustering at the household or municipality level rather than the census-tract level, as a small number of households are not assigned census tracts.

	adj. $\Delta\log(GFW)$	$\Delta\log(Debt)$	ΔSMS	ΔInt. R. Deposits	$\Delta\log(L.\ Earnings)$
	(1)	(2)	(3)	(4)	(5)
	Panel A: Scaled Border Distance				
Household	0.0161**	0.0019	-0.0001	-0.0001	0.0191**
	(0.0076)	(0.0114)	(0.0012)	(0.0001)	(0.0092)
Census tract	0.0163**	0.0025	-0.0001	-0.0001	0.0192**
	(0.0073)	(0.0112)	(0.0012)	(0.0001)	(0.0096)
Municipality	0.0161**	0.0019	-0.0001	-0.0001	0.0191*
	(0.0072)	(0.0101)	(0.0013)	(0.0001)	(0.0107)
	Panel A: KM Border Distance				
Household	0.0163**	0.0058	-0.0005	-0.0001	0.0331***
	(0.0075)	(0.0115)	(0.0012)	(0.0001)	(0.0091)
Census tract	0.0163**	0.0062	-0.0005	-0.0001	0.0328***
	(0.0073)	(0.0113)	(0.0012)	(0.0001)	(0.0094)
Municipality	0.0163**	0.0058	-0.0005	-0.0001	0.0331***
	(0.0071)	(0.0118)	(0.0012)	(0.0001)	(0.0096)
	Panel A: Relative Location Measure				
Household	0.0147**	-0.0049	-0.0000	-0.0001	0.0209***
	(0.0063)	(0.0094)	(0.0010)	(0.0001)	(0.0076)
Census tract	0.0146**	-0.0048	-0.0000	-0.0001	0.0209***
	(0.0063)	(0.0094)	(0.0010)	(0.0001)	(0.0078)
Municipality	0.0147**	-0.0049	-0.0000	-0.0001	0.0209***
	(0.0058)	(0.0092)	(0.0011)	(0.0001)	(0.0075)

Table B.5: Main results under different geographic bandwidth choices

This table shows the results from the main regressions using different bandwidths (geographic cutoffs). Panel A considers the scaled distance measure. bw=0.9 implies retaining all households with a scaled border distance inside [-0.9,0.9]. The main specification uses bw=0.6. Panel B considers border distance in kilometers (KM). bw=10 implies retaining all households with a border distance, in km, inside [-10,10]. The main specification uses bw=10. Panel C considers the relative location measure. bw = 0.6 implies retaining all households where their differential distance to the centroid of the high (versus low) side of the boundary is smaller than 0.6 times the distance between the two centroids. The main specification uses bw = 1.0.

Bandwidth	adj. $\Delta \log(GFW)$ (1)	$\Delta \log(Debt)$ (2)	ΔSMS (3)	ΔInt. R. Deposits (4)	$\Delta \log(L. Earnings)$ (5)
	Panel A: Scaled Border Distance				
bw = .9	0.0144**	-0.0022	0.0001	-0.0001	0.0264***
	(0.0065)	(0.0099)	(0.0010)	(0.0001)	(0.0085)
bw = .8	0.0147**	-0.0008	-0.0001	-0.0001	0.0260***
	(0.0066)	(0.0102)	(0.0010)	(0.0001)	(0.0086)
bw = .7	0.0156**	-0.0007	0.0001	-0.0001	0.0263***
	(0.0068)	(0.0105)	(0.0011)	(0.0001)	(0.0090)
bw = .6	0.0163**	0.0025	-0.0001	-0.0001	0.0192**
	(0.0073)	(0.0112)	(0.0012)	(0.0001)	(0.0096)
bw = .5	0.0145*	0.0065	0.0005	-0.0001	0.0228**
	(0.0081)	(0.0125)	(0.0013)	(0.0001)	(0.0103)
bw = .4	0.0170*	0.0067	0.0002	-0.0002	0.0154
	(0.0095)	(0.0143)	(0.0015)	(0.0001)	(0.0118)
bw = .3	0.0260**	-0.0027	-0.0006	-0.0000	0.0265*
	(0.0116)	(0.0178)	(0.0019)	(0.0002)	(0.0147)
bw = .2	0.0482***	0.0028	-0.0013	0.0005**	0.0462**
	(0.0175)	(0.0254)	(0.0025)	(0.0002)	(0.0190)
	Panel B: KM Border Distance				
bw = 14	0.0183***	0.0048	0.0007	-0.0002*	0.0314***
	(0.0063)	(0.0098)	(0.0011)	(0.0001)	(0.0083)
bw = 12	0.0172**	0.0073	0.0001	-0.0001	0.0340***
	(0.0067)	(0.0105)	(0.0011)	(0.0001)	(0.0089)
bw = 10	0.0163**	0.0062	-0.0005	-0.0001	0.0328***
	(0.0073)	(0.0113)	(0.0012)	(0.0001)	(0.0094)
bw = 8	0.0184**	0.0114	-0.0002	-0.0001	0.0327***
	(0.0081)	(0.0126)	(0.0014)	(0.0001)	(0.0104)
bw = 6	0.0238***	0.0047	-0.0007	0.0000	0.0225*
	(0.0092)	(0.0150)	(0.0016)	(0.0001)	(0.0122)
bw = 4	0.0333***	0.0164	-0.0019	0.0001	0.0239
	(0.0118)	(0.0188)	(0.0020)	(0.0002)	(0.0151)
bw = 2	0.0186	-0.0180	-0.0048	0.0004	0.0492**
	(0.0195)	(0.0315)	(0.0032)	(0.0003)	(0.0240)
bw = 1	-0.0138	0.0325	-0.0120*	0.0009	0.0249
	(0.0391)	(0.0626)	(0.0070)	(0.0006)	(0.0478)
	Panel C: Relative Location				
bw = 1	0.0146**	-0.0048	-0.0000	-0.0001	0.0209***
	(0.0063)	(0.0094)	(0.0010)	(0.0001)	(0.0078)
bw = .9	0.0151**	-0.0079	-0.0002	-0.0000	0.0188**
	(0.0066)	(0.0102)	(0.0011)	(0.0001)	(0.0083)
bw = .8	0.0148**	-0.0030	0.0003	-0.0001	0.0171**
	(0.0070)	(0.0110)	(0.0011)	(0.0001)	(0.0087)
bw = .7	0.0176**	-0.0028	0.0002	-0.0000	0.0147
	(0.0076)	(0.0118)	(0.0012)	(0.0001)	(0.0091)
bw = .6	0.0217***	0.0038	-0.0009	-0.0001	0.0146
	(0.0080)	(0.0128)	(0.0013)	(0.0001)	(0.0100)
bw = .5	0.0227**	-0.0067	-0.0006	-0.0000	0.0234**
	(0.0092)	(0.0141)	(0.0014)	(0.0001)	(0.0106)
bw = .4	0.0219**	-0.0143	0.0000	-0.0000	0.0173
	(0.0107)	(0.0158)	(0.0016)	(0.0001)	(0.0123)
bw = .3	0.0247**	-0.0292	-0.0001	-0.0001	0.0184
	(0.0124)	(0.0197)	(0.0019)	(0.0002)	(0.0144)

Table B.6: Boundary Discontinuity Estimates Under Different Specifications

This table shows the effect of changing tax assessment on financial saving during 2010–2015. $\log(TaxVal)$ is instrumented 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)-(5) address geographic heterogeneity according to the main specification in equation (6). Column (4) corresponds to the preferred (scaled) border distance measure. Census-tract-level clustered standard errors are in parentheses. 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 level, respectively.

	(1)	(2)	(3)	**(4)**	(5)
	No geo.	*Boundary FEs*	*KM Distance*	*Scaled Distance*	*Rel. Location*
	Δ log(Gross Financial Wealth), adjusted for wealth-tax payments				
$\log(\widehat{TaxVal})$	0.0183***	0.0181***	0.0164**	0.0161**	0.0147**
	(0.0010)	(0.0031)	(0.0073)	(0.0073)	(0.0063)
N	1802589	1802494	1426960	1442739	1614965
	Δ log(Debt)				
$\log(\widehat{TaxVal})$	0.0266***	0.0217***	0.0057	0.0019	-0.0049
	(0.0015)	(0.0048)	(0.0113)	(0.0112)	(0.0094)
N	1802610	1802515	1426978	1442756	1614985
	ΔInterest Rate on Deposits				
$\log(\widehat{TaxVal})$	-0.000142***	-0.000030	-0.000118	-0.000075	-0.000099
	(0.000014)	(0.000047)	(0.000106)	(0.000104)	(0.000086)
N	1552870	1552772	1230322	1240649	1390060
	ΔStock Market Share				
$\log(\widehat{TaxVal})$	0.000291*	0.000238	-0.000521	-0.000119	-0.000040
	(0.000169)	(0.000492)	(0.001217)	(0.001181)	(0.000974)
N	1796010	1795917	1421759	1437504	1609101
	Δlog(LaborEarnings)				
$\log(\widehat{TaxVal})$	0.0144***	0.0135***	0.0329***	0.0191**	0.0209***
	(0.0012)	(0.0041)	(0.0095)	(0.0096)	(0.0078)
N	1804474	1804378	1428407	1444211	1616639
Controls					
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Housing Characteristics	Yes	Yes	Yes	Yes	Yes
– Border specific	–	Yes	Yes	Yes	Yes
Border Distance Controls					
– KM	–	–	Yes	–	–
– Scaled	–	–	–	Yes	–
Relative Location Controls	–	–	–	–	Yes

B.3 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 housing wealth accounts for a very small fraction of net worth for the very wealthy (see Fagereng, Guiso, Malacrino, and Pistaferri (2020)). 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 account for only 10% of aggregate municipal tax revenues, and 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 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.[54] Thus any reasonable bounds on household sensitivity to municipal finances suggest that the effect will be negligible.

B.4 Property Taxation

In 2015, 242 out 428 municipalities levied property taxes on residential homes.[55] Starting in 2015, a subset (49) of municipalities began using the tax authorities' assessments ($\widehat{TaxVal}$) to assess property taxes. Prior to 2014, municipalities were not allowed to access or use these assessments. In order to allow municipalities to reduce costs by limiting the need to perform independent assessments, the tax authorities allowed municipalities to use their assessments as of 2014. Initially, municipalities were discouraged from using the measure, by only being allowed to assess property taxes based on a downward-adjusted (by 33%) version of $\widehat{TaxVal}$, which would limit municipal property tax revenues. This disincentive was partially reduced in 2015, when $\widehat{TaxVal}$ only needed to be reduced by 20%. A continuing disincentive is that the tax authorities do not allow muncipalities to use their own information to adjust or fine tune $\widehat{TaxVal}$. This may be problematic, as municipalities may want to extract higher taxes from houses with better locations within an area (e.g., a view of the ocean or larger property size). Neither of these two factors are accounted for in $\widehat{TaxVal}$.

The potential use of $\widehat{TaxVal}$ for property-tax purposes implies some scope for the exclusion restriction to be violated: Border discontinuities in $\widehat{TaxVal}$ may affect property taxes for a subset of households as of 2015, thereby amplifying (over-stating) the income effects associated with a pure wealth tax treatment. To ensure that this is not driving my results, I perform two robustness exercises. In Panel A of B.7, I exclude 2015 completely. In Panel B, I omit municipality-year observations in which $\widehat{TaxVal}$ is reported to be used for property taxation

[54] I use 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, a 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 taxpayers, 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 its 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.

[55] Source: Statistikkbanken at Statistics Norway, series 12503: Eiendomskatt (K) 2007–2019. 180 is the number of municipalities that report collecting strictly positive property taxes on a standard house (Enebolig, 120kvm).

purposes. Reassuringly, both Panels A and B yield virtually identical results as those estimated with the baseline sample criteria, which are provided in Panel C.

An additional argument against property taxation playing a confounding role is that the estimated effects are not driven by the last years of my sample. This is clear from considering the year-by-year decomposition of saving and labor earnings responses in Figure B.8: I find similar effects for all years, including the early years where there was no opportunity for municipalities to base their property taxes on $\widehat{TaxVal}$. While its certainly possible that Norwegian households respond to increased property taxation, this response (in terms of e.g., increased saving) is likely to occur *after* my sample ends in 2015, when this subset of households realized they would face higher future property taxes. Property taxation is also more likely to disproportionately affect lower-TNW households than those providing most of the identifying variation in my setting, namely those near or above the wealth tax threshold. Thus, my finding that the positive saving effect is driven by ATR effects rather than MTR (which is more significantly affected for lower-TNW households) is inconsistent with property taxation playing a confounding role.

Table B.7: Robustness Exercises Related to Property Taxation

This table provides results for the main sample of households with $TNW_{2009} > 0$. Panel A omits observations during 2015, and adds a control variable for the municipal property tax rate. This panel clusters standard errors on the municipality level. Panel B keeps observations during 2015 and, unlike the main baseline sample restriction, it keeps observations where the households live in municipalities that report using TaxVal for property tax purposes. Panel C provides the main results, using the baseline sample restrictions, as a reference. Panels B and C cluster standard errors on the census-tract level, as per the main specification.

	Adj. $\Delta\log(GFW)$	Δlog(Debt)	ΔSMS	ΔInt. R. Dep.	Δ log(L. Earnings)
	(1)	(2)	(3)	(4)	(5)
	Panel A: Omit 2015; and control for prop. tax rate				
$\log(\widehat{TaxVal})$	0.0180**	0.0008	0.0002	-0.0001	0.0191
	(0.0077)	(0.0122)	(0.0013)	(0.0001)	(0.0119)
Prop. Tax (%)	-0.0061*	-0.0009	-0.0009	-0.0001	0.0084***
	(0.0032)	(0.0050)	(0.0006)	(0.0001)	(0.0028)
N	[1234147]	[1234160]	[1230315]	[1233282]	[1235608]
	Panel B: Keep 2015; and obs. where muni uses TaxVal for prop. tax				
$\log(\widehat{TaxVal})$	0.0167**	0.0023	0.0000	-0.0001	0.0190**
	(0.0073)	(0.0111)	(0.0012)	(0.0001)	(0.0095)
N	[1473681]	[1473698]	[1468248]	[1242017]	[1475153]
	Panel C: Main specification for reference				
$\log(\widehat{TaxVal})$	0.0163**	0.0025	-0.0001	-0.0001	0.0192**
	(0.0073)	(0.0112)	(0.0012)	(0.0001)	(0.0096)
N	[1444279]	[1444296]	[1439042]	[1241961]	[1445751]
Geo-Controls					
Scaled Border Distance	Yes	Yes	Yes	Yes	Yes

B.5 MTR-ATR decomposition of portfolio allocation effects

Table B.8: Differential Portfolio Responses to Changing Marginal and Average Tax Rates on Wealth

This table reports IV estimates of the effect of changing the marginal (MTR) and average tax rates (ATR) on portfolio allocation as referred to in subsection 6.2.2 in the main text. Columns (1)-(3) consider the stock market share of financial wealth. Columns (4)-(6) consider the realized interest rate on bank savings. Geographic assessment discontinuities, $1[d_i > 0]\Delta_i$, interacted with MTC_i^t and ATC_i^t are used as instruments. MTC_i^t and ATC_i^t are based on 2009 observables and time t tax rules to provide the simulated impact of tax-assessment increases on MTR and ATR. The estimation sample also includes households with $TNW_{i,2009} < 0$; and all coefficients, on instruments and control variables, are allowed to vary by whether $TNW_{i,2009} > 0$. $ATR + MTR$ provides thee sum of the coefficients on MTR and ATR. Standard errors on the sum are calculated using the covariance matrix of the estimated coefficients. Stars indicate significance at the 10%, 5%, and 1% levels. Standard errors are clustered at the census-tract level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: ΔSMS			Panel B: Δ Int. Rate on Deposits		
MTR	-0.10		-0.01	-0.05		-0.10*
	(0.33)		(0.50)	(0.03)		(0.05)
ATR		-0.41	-0.27		-0.05	0.18*
		(0.87)	(1.01)		(0.08)	(0.10)
ATR+MTR			-0.28			0.08
s.e. (ATR+MTR)			0.71			0.07
First-stage F-statistic	253.41	150.25	81.04	253.95	152.31	86.71
N	2469362	2469362	2469362	2124479	2124479	2124479
Instruments						
$1[d_i > 0]\Delta_i \times MTC_i^t$	Yes	–	Yes	Yes	–	Yes
$1[d_i > 0]\Delta_i \times ATC_i^t$	–	Yes	Yes	–	Yes	Yes

B.6 Communication of policy change

The implementation of a new methodology to assess housing wealth was primarily communicated in a letter sent to all homeowners in August of 2010. The letter was titled "Information for the calculation of new tax values for residential properties,"[56] 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 postal service 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) was already available in the early fall of 2010. On December 15 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

[56] My own translation.

the latest.

The tax authorities' website states 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 authorities' website, where no details are provided on the exact methodology.

C Tax Behavior: Tax-base Elasticity and Effect on Self-Reported Wealth

(Referred to in Section 6.2.3 of the main text)

Taxable Net Wealth (TNW). I first study the effect on the wealth-tax base, TNW, in columns (1)-(3) of Table B.9. I define adjusted $\log(TNW)$ the following way.

$$\text{Adj. } \log(TNW) = \log(q_t + \max(TNW_{i,t} - TaxVal_{i,t} + TaxVal_{i,2009}, 0)), \tag{26}$$

where q_t shifts the log-argument away from zero by an inflation-adjusted NOK 100,000,[57] and the mechanical effects of the assessment discontinuities are removed by replacing concurrent housing assessments with the initial assessment. This definition removes variation from households with two periods of negative TNW, which is reasonable given that these households typically don't see a large effect on their wealth-tax exposure.

The main result is in column (3) of Table B.9, where I decompose the response into a marginal and average tax rate effect. To my knowledge, the estimated combined effect of 4.58 (se=3.47) is the first direct estimate of an uncompensated wealth tax base elasticity.

How do these estimates compare to the existing literature? Brülhart, Gruber, Krapf, and Schmidheiny (2021) provide a summary of existing estimates in their Appendix Table A.1. Their own findings indicate 5-year semi-elasticities of -43 and -96. For simplicity, I wrongfully assume that their variation is fully driven by MTR effects. Since my MTR effects are negative, and the ATR effects positive, this provides the best hope for producing consistent findings. By multiplying the MTR coefficient in column (3) by 5, I obtain a 5-year MTR elasticity of -9.15, with a lower bound on the 95% confidence interval of -22.65. Thus, I can I statistically rule out that my findings are consistent with their point estimates. This, however, is not surprising given the evidence they provide in favor of substantial evasion responses in their setting. Using Table A.1 in Brülhart, Gruber, Krapf, and Schmidheiny (2021), we also see that the implied 8-year elasticity in Jakobsen, Jakobsen, Kleven, and Zucman (2020) is -17 to -25. Rescaled to 5-year elasticities, they are -11 to -16. Thus their findings are statistically comparable to mine, but only to the extent that ATR effects in their setting are very small.[58]

This comparison illustrates the usefulness of the MTR-ATR decomposition when considering

[57] TNW levels are typically considerably larger than GFW levels, hence the higher log-shifter.

[58] Jakobsen, Jakobsen, Kleven, and Zucman (2020) discuss how marginal tax rates were affected, but not how this corresponded to changes in average tax rates.

the effects on the wealth tax base. Any particular tax reform can, to some extent, be summarized as a convex combination of a reduction in marginal and average tax rates, which maps directly into linear-return and virtual-income decomposition (see Section E in the Appendix).

In sum, my findings here suggest that behavioral responses are not a large concern in a setting with limited evasion opportunities. This is in line with the views on wealth taxation elicited in surveys by Fisman, Gladstone, Kuziemko, and Naidu (2020). Using textual analyses of respondents' justifications of various wealth tax schemes, they find little evidence of any expressed concerns about the tax base being elastic.

Table B.9: Differential Tax Adjustments to Changing Marginal and Average Tax Rates on Wealth

This table reports IV estimates of the effect of changing the marginal (MTR) and average tax rates (ATR) on the wealth tax base, TNW, as well as the self-reported component of GFW. The adjusted log(TNW) is defined in (27). Columns (1)-(3) consider TNW, which is adjusted to remove the mechanical correlation caused by higher $TaxVal$. This is done by replacing $TaxVal_{i,t}$ with $TaxVal_{i,2009}$. Columns (4)-(6) consider the self-reported components of GFW. Geographic assessment discontinuities, $1[d_i > 0]\Delta_i$, interacted with MTC_i^t and ATC_i^t are used as instruments. MTC_i^t and ATC_i^t are based on 2009 observables and time t tax rules to provide the simulated impact of tax-assessment increases on MTR and ATR. The estimation sample also includes households with $TNW_{i,2009} < 0$; and all coefficients, on instruments and control variables, are allowed to vary by whether $TNW_{i,2009} > 0$. $ATR + MTR$ provides the sum of the coefficients on MTR and ATR. Standard errors on the sum are calculated using the covariance matrix of the estimated coefficients. Stars indicate significance at the 10%, 5%, and 1% levels. Standard errors are clustered at the census-tract level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Δ log(Adj. TNW)			Panel B: Δ log(Self-reported GFW)		
MTR	-0.47		-1.51	3.51*		5.56*
	(1.86)		(2.73)	(1.96)		(3.07)
ATR		6.46	5.67		8.38	-8.11
		(4.32)	(5.14)		(6.02)	(6.97)
ATR+MTR			4.16			-2.55
s.e. (ATR+MTR)			3.52			5.01
First-stage F-statistic	252.84	149.70	81.23	253.41	150.10	81.29
N	2464590	2464590	2464590	2471819	2471819	2471819
Instruments						
$1[d_i > 0]\Delta_i \times MTC_i^t$	Yes	–	Yes	Yes	–	Yes
$1[d_i > 0]\Delta_i \times ATC_i^t$	–	Yes	Yes	–	Yes	Yes

Self-reported Wealth. While the economic significance of self-reported financial assets in my setting is limited,[59] I follow the existing literature in providing new country-specific evidence on the potential for evasion behavior. To do this, I zoom in on the largely self-reported components of financial wealth, such as securities and real-estate held abroad, in columns (4)-(6) of Table B.9. Both columns (4) and (5) suggest a perhaps surprisingly positive effect of wealth taxation on reported wealth. Interestingly, the decomposition in column (6) shows that this is driven by extensive-margin MTR variation. This is exactly the opposite of what we found for the main saving measure. This may be indicative of households being more vigilant about reporting their non-third-party-reported holdings once they reach the threshold, while becoming

[59] The summary statistics in Table A.1 show that self-reported financial assets only account for 2.6% of GFW at the mean, and 0% for more than three-quarters of the sample.

more likely to misreport once the impact on the annual wealth tax bill becomes larger.

Since this effect appears to be driven by extensive-margin exposure to taxes, it is useful to examine the potential extent of evasion around the tax threshold in using a bunching analysis. Interestingly, this analysis reveals no evidence of an excess mass of of households located at the wealth tax threshold (see analyses in in Section D in the Appendix). This seems consistent with the non-negative effect that we found in column (6) arising from extensive-margin exposure. My finding of no bunching at the threshold is in stark contrast with previous findings from Denmark, Sweden, Colombia, and Switzerland.[60] The findings of Seim (2017) and Londoño-Vélez and Ávila-Mahecha (2020a) suggest that evasion may be greatly restricted if self-reporting is limited. My findings are consistent with this, suggesting that evasion is addressable by limiting the extent of self-reporting, as Saez and Zucman (2019) argue. However, these findings come with the important caveat that evasion primarily occurs among the very wealthiest individuals whom are already considerably to the right of the threshold. Their responses evasion behavior would not be picked up my either my bunching analysis or my above analysis in Table B.9.

D Bunching

There is clear evidence of Norwegian households evading and avoiding capital taxation (see, e.g., Alstadsæter and Fjærli 2009, Alstadsæter, Kopczuk, and Telle 2019b). However, the existing empirical evidence shows that this occurs primarily among very wealthy households. In my empirical setting, I obtain most of my identifying variation from households near the wealth tax threshold, considerably below the top 1% of the wealth distribution. These households primarily hold assets that are third-party reported, which greatly lowers the potential extent for evasion. Yet, it is useful to confirm this graphically. Traditional wealth-tax threshold bunching analyses inform the behavior of agents near the tax and is thus well suited for this purpose.

While households may not underreport the amount of deposits they hold in domestic banks, they may, for example, be induced by wealth taxation to shift savings into harder-to-tax asset classes, such as art. This would imply that my saving measure, based on changes in largely third-party reported financial wealth, would understate the true effect on saving. I shed some light on this question following the approach in Seim (2017).

I show my results in Figure B.10 below. Panel A shows the results for my full analysis sample, and Panel B shows results for the full sample of Norwegian taxpayers. We see that the wealth tax threshold is located at a fairly dense place in the wealth distribution. In Panel A, each NOK 5,000 bin ($\approx$ USD 833, using the 2010 USD/NOK exchange rate of around 6.) contains about 625 households on average in a given year (2500/4 years).

The visual evidence is quite clear, in that there is no sizable bunching around the wealth tax threshold in either sample. I perform more formal bunching analyses using the the approach of Chetty, Friedman, Olsen, and Pistaferri (2011). Panel B estimates a statistically significant excess mass around the threshold, but the visual evidence is not very supportive. In Panel B,

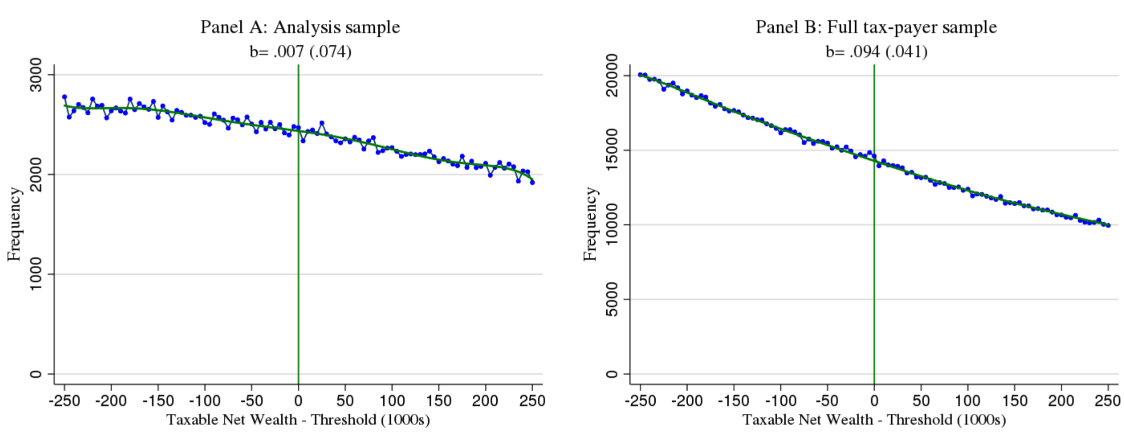
[60] See Jakobsen et al. (2020); Seim (2017); Londoño-Vélez and Ávila-Mahecha (2020a); and Brülhart et al. (2019), respectively.

the excess mass, b, equals 0.097, which means 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 of Chetty et al. (2011), and the assumptions closely resemble those made by 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 of 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 $0.05853\%/(0.01075/(2\%)) = 0.0010$.

Figure B.10: 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 two for married couples, and only couples with a non-changed marital status are included. Panel B considers the full sample of Norwegian taxpayers, 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 distribution (green line) is constructed by fitting a 7th degree polynomial on all bins outside [-40,000, 15,000]. b is the estimated excess mass inside [-40,000, 15,000], 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. The sample period is restricted due to limited sample years in which the relevant net wealth variable, nto_form, is not bottom-coded at the level of the tax threshold.



These findings point toward limited avoidance or evasion behavior in my sample, which is consistent with evasion or avoidance responses being unlikely to lead my estimated effects on financial saving to greatly understate the effect on over-all saving behavior. This is in large part because I am estimating treatment effects among (only) moderately wealthy households. Wealthier households likely have access to more low cost evasion or avoidance technologies, which is consistent with the findings of Alstadsæter, Johannesen, and Zucman (2019a). One example is the ease of placing wealth in foreign financial assets. Alstadsæter, Johannesen, and Zucman (2018) find that only 0.03% of households in the bottom 99% of the wealth distribution (covering almost my entire sample) reported foreign wealth holdings under the protection of a tax amnesty. In the top 0.1% of the wealth distribution, on the other hand, a more substantial 6% of households reported foreign wealth holdings. While moderately wealthy households may benefit, in wealth tax sense, by placing assets into harder-to-tax assets such as art, the net benefit is not clear. Art is considerably less liquid and likely much riskier than financial assets such as deposits.

Finally, it is worth noting that little-to-no bunching by itself does not rule out extensive evasion or avoidance behavior. Such behavior may materialize in ways that is hard to detect by bunching techniques. For example, it may be the very wealthiest who evade and avoid the most, and they might abstain from doing it to the extent where they end up very close to the wealth tax threshold. However, these findings are consistent with the institutional details in my setting that likely limit such responses for the households that I study.

E 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.[61] 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.

Consider the following simple two-period model, in which households choose how much to consume in each period, C_t and may save S in period 1. 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 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.

$$\max_{C_1,C_2,S} \quad U(C_1, C_2, S) = \frac{1}{1-\gamma}C_1^{1-\gamma} + \beta\frac{1}{1-\gamma}C_2^{1-\gamma}, \tag{27}$$

$$\text{s.t.} \quad C_1 + S = Y_1 \tag{28}$$

$$\text{and} \quad C_2 = Y_2 + RS - \tau\mathbb{1}[SR + A - \bar{W} > 0](SR + A - \bar{W}). \tag{29}$$

[61] In the last section, in which I calibrate a life-cycle model to infer which EIS my results 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.

We can rewrite the constraint for period 2 as:

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

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. The problem then becomes:

Simplified optimization problem.

$$\begin{aligned}
\max_{C_1, C_2, S} \quad U(C_1, C_2, S) &= \frac{1}{1-\gamma} C_1^{1-\gamma} + \beta \frac{1}{1-\gamma} C_2^{1-\gamma}, && (30)\\
\text{s.t.} \quad C_1 + S &= Y_1 && (31)\\
\text{and} \quad C_2 &= Y_2 + \tilde{R}S + \tilde{V}. && (32)
\end{aligned}$$

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

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

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:

$$\frac{dS}{dA} = \underbrace{\frac{dS}{d\tilde{R}} \frac{d\tilde{R}}{dA}}_{\text{Rate-of-return Effect}} + \underbrace{\frac{dS}{d\tilde{V}} \frac{d\tilde{V}}{dA}}_{\text{Virtual Income Effect}}. \tag{34}$$

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.

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

The first term in equation (36) 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", whereby 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 overall ambiguity more clearly in the presence of a human wealth effect.

$$\frac{dS}{d\tilde{R}} = \frac{1}{\gamma}\frac{[\beta R]^{\frac{1}{\gamma}}}{(\tilde{R}+[\beta\tilde{R}]^{\frac{1}{\gamma}})^2}\left(Y_1+\frac{Y_2+\tilde{V}}{\tilde{R}}\right) - \frac{S}{\tilde{R}+[\beta\tilde{R}]^{\frac{1}{\gamma}}}. \tag{36}$$

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.[62]

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

$$\frac{dS}{d\tilde{V}} = -\frac{1}{\tilde{R}+[\beta\tilde{R}]^{\frac{1}{\gamma}}}. \tag{37}$$

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:[63]

$$\frac{d\tilde{V}}{dA} \quad = \quad S\left(\frac{d\tilde{R}^{avg}}{dA} - \frac{d\tilde{R}}{dA}\right). \tag{38}$$

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

[62] As an example, consider the case in which $\beta\tilde{R}=1$. 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 $1/\gamma\leq 0.38$.

[63] 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.

effects of tax-assessment shocks on the marginal versus average net-of-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 (35) when the EIS is zero to isolate income effects. I use the expression for $dS/d\tilde{R}$ from equation (37), substitute in for $d\tilde{V}/dA$, and reorganize to get:

$$\frac{dS}{dA} = -\frac{S}{\tilde{R}+1}\left[\underbrace{\left(\frac{d\tilde{R}}{dA}\right)}_{\text{Income Effect}} - \underbrace{\left(\frac{d\tilde{R}}{dA} - \frac{d\tilde{R}^{avg}}{dA}\right)}_{\text{Virtual Income Effect}}\right]. \tag{39}$$

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 $\mathbb{E}[d\tilde{R}^{avg}/dA]$, which 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 $\mathbb{E}[d\tilde{R}^{avg}/dA]$, this suggests that income effects are amplified by a factor of two.

Endogenous labor supply. In Section E.1 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 the 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.

E.1 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.

$$\begin{aligned} \max_{C_1,C_2,S,L} \quad U(C_1, C_2, S, L) &= \frac{1}{1-\gamma}C_1^{1-\gamma} - \psi\frac{L^{1+\nu}}{1+\nu} + \beta\frac{1}{1-\gamma}C_2^{1-\gamma} \\ \text{s.t.} \quad C_1 + S &= Y_1 + LW \\ \text{and} \quad C_2 &= Y_2 + \tilde{R} + \tilde{V} \end{aligned} \tag{40}$$

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

$$S = \frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}(Y_1 + LW)}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}} - \frac{Y_2 + \tilde{V}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}. \tag{41}$$

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

$$dL \cdot W = \frac{\gamma[Y_1 + LW - S]^{-\gamma-1}W^2}{(\psi\nu L^{\nu-1} + \gamma[Y_1 + LW - S]^{-\gamma-1}W^2)} dS \equiv f dS. \tag{42}$$

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 42 with respect to $\tilde{R}$ and substitute $dL \cdot W$ for the expression in equation (43), and solve for $dS/d\tilde{R}$ to get:

$$\frac{dS}{d\tilde{R}} = \left(1 - f\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right)^{-1} \left((Y_1 + LW)\frac{1-\gamma}{\gamma}\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2} + (Y_2 + \tilde{V})\frac{1 + \frac{\beta}{\gamma}[\beta\tilde{R}]^{\frac{1}{\gamma}-1}}{(\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}})^2}\right) \tag{43}$$

I then totally differentiate 42 with respect to $\tilde{V}$, and substitute $dL \cdot W$ with the expression in equation (43), and solve for $dS/d\tilde{V}$ to get:

$$\frac{dS}{d\tilde{V}} = \left(1 - f\frac{[\beta\tilde{R}]^{\frac{1}{\gamma}}}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right)^{-1} \left(-\frac{1}{\tilde{R} + [\beta\tilde{R}]^{\frac{1}{\gamma}}}\right). \tag{44}$$

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 44 now also contains labor earnings as period 1 income. There is therefore no change in the qualitative conclusions drawn in the case with only exogenous (labor) income. The new insight is that the effect on labor earnings should be of a same sign as, but smaller in magnitude than, the effect on savings.

Table B.10: Detailed IV Table: Saving Behavior and Labor Earnings
Results when not allowing coefficients on instruments to vary by $1[\text{TNW}_{i,2009} > 0]$

This table provides results using a slightly simplified version of the regression specification underlying the main results in Table 3. This specification does not allow the first-stage or reduced-form coefficients on the instruments to covary with $1[\text{TNW}_{i,2009} > 0]$, which leaves fewer first-stage and reduced-form coefficients to be estimated.

Panel A: IV Estimates

	Adj. Δ log(GFW)			Δ log(GFW), unadj.			Δ log(LaborEarnings)		
MTR	-0.46		-4.13	-1.84		-4.77	3.13		0.06
	(1.98)		(2.99)	(2.02)		(3.06)	(2.36)		(3.49)
ATR		5.70	13.34**		0.62	10.68*		14.75**	11.79
		(4.43)	(5.52)		(4.66)	(5.73)		(6.37)	(7.27)
ATR+MTR			9.20			5.91			11.85
s.e. (ATR+MTR)			3.63			3.79			5.24
First-stage F-statistic	503.63	297.77	159.51	503.63	297.67	159.48	503.64	297.67	159.48
N	2471793	2471793	2471793	2471819	2471819	2471819	2471820	2471820	2471820

Panel B: Reduced-form Regressions

	Adj. Δ log(GFW)			Δ log(GFW), unadj.			Δ log(LaborEarnings)		
$1[d_i > 0]\Delta_i * MTC_i$	-0.42		-2.95	-1.70		-3.37	2.89		-0.03
	(1.82)		(2.12)	(1.84)		(2.14)	(2.17)		(2.44)
$1[d_i > 0]\Delta_i * ATC_i$		3.62	6.92**		0.40	4.71*		9.36**	8.24**
		(2.88)	(2.75)		(2.96)	(2.80)		(4.03)	(3.80)
N	2471793	2471793	2471793	2471819	2471819	2471819	2471820	2471820	2471820

Panel C: First-stage Regressions

	Adj. Δ log(GFW)				Δ log(GFW), unadj.				Δ log(LaborEarnings)			
	MTR	ATR	MTR	ATR	MTR	ATR	MTR	ATR	MTR	ATR	MTR	ATR
$1[d_i > 0]\Delta_i * MTC_i$	0.92***		0.69***	-0.01	0.92***		0.69***	-0.01	2.89		0.69***	-0.01
	(0.04)		(0.04)	(0.02)	(0.04)		(0.04)	(0.02)	(2.17)		(0.04)	(0.02)
$1[d_i > 0]\Delta_i * ATC_i$		0.63***	0.57***	0.70***		0.63***	0.57***	0.70***		9.36**	0.57***	0.70***
		(0.04)	(0.08)	(0.03)		(0.04)	(0.08)	(0.03)		(4.03)	(0.08)	(0.03)
N	2471793	2471793	2471793	2471793	2471819	2471819	2471819	2471819	2471820	2471820	2471820	2471820

E.2 Exploring the role of liquidity constraints

To the extent that households in my sample face binding liquidity constraints, the effects of increased wealth tax exposure may be muted toward zero. Firstly, if substitution effects dominate, liquidity constrained households *cannot* dissave further, thus we would not expect to see considerable dissaving. Secondly, income effects would be muted by the unmet borrowing demand. We would only observe a positive saving response if income effects were strong enough to cancel out the unmet borrowing demand and render the household unconstrained.

However, if there is some amount of preference heterogeneity, it becomes possible that liquidity constraints disproportionately mute the dissaving responses of high-EIS households. For this to have a material effect, a meaningful portion of the households in my sample would have to face binding liquidity constraints. My descriptive statistics are inconsistent with this. Households generally have high levels of liquid wealth, and wealth taxes almost never account for a very large share of GFW.[64] While it is unlikely that the small amount of low-GFW households are driving my results, I provide a robustness exercise below.

In Table B.11 below, I provide the main MTR-ATR decomposition results when dropping households with initial GFW below NOK 100,000 (approx. USD 16,667). This leads to a very liquid sample. Even the 10th percentile of financial wealth is above NOK 125,000 (> USD 20,000) during the sample period, which was not guaranteed given that I only conditioned on *initial* GFW. When focusing on the households that provide the identifying variation, i.e., those with high MTCs and ATCs (defined in Section 6.1), the unconditional 10th percentile is even higher. At the means and medians, we see that GFW is above 0.5 MNOK and 1 MNOK respectively.

The results found in this subsample are qualitatively very similar to those found in Table 3 in the main text, and the differences are statistically modest.

To compare the estimates, I scale the differences by the standard errors in Table 3. I find that MTR effects increase somewhat in magnitude, by about half of a standard error in column (1), and by about one quarter of a standard error in columns (2)-(3). The ATR effects decrease in magnitude for the two saving measures: By around one-third standard error in column (1) and almost half a standard in column (2). For labor earnings, the ATR effect *increases* by a little over one-half standard errors.

[64] See Table A.1. Even at the 25th percentile, GFW exceeds NOK 220,000 (USD 36,667). Table A.2 shows that wealth taxes account for more than 25% of GFW for less than 1% of the sample, including when we limit the attention to households near the boundary, or when we focus on households positioned to experience greater MTR *or* ATR effects.

Table B.11: IV Table: Saving Behavior and Labor Earnings
Results when omitting households with low-moderate initial financial wealth

This table provides results when altering the sample selection underlying the main findings in Table 3 to only include households with GFW_{2009} >100,000. The table also provides unconditional sample statistics for GFW.

	(1)	(2)	(3)
	Adj. $\Delta \log(GFW)$	$\Delta \log(GFW)$	$\Delta \log(L.\ Earnings)$
MTR	-5.28*	-5.45*	-1.11
	(3.18)	(3.25)	(4.00)
ATR	11.96**	8.71	16.28**
	(5.58)	(5.85)	(8.12)
ATR+MTR	6.67	3.26	15.17
s.e. (ATR+MTR)	3.70	3.94	5.84
First-stage F-statistic	65.30	65.30	65.29
N	1725306	1725324	1725325
Unconditional GFW statistics			
unweighted			
p10	126,852	126,853	126,852
p50	556,410	556,410	556,410
mean	1,133,516	1,133,513	1,133,513
MTC-weighted			
p10	152,908	152,908	152,908
p50	547,982	547,982	547,982
mean	841,928	841,930	841,929
ATC-weighted			
p10	203,257	203,257	203,257
p50	843,101	843,101	843,101
mean	1,426,977	1,426,982	1,426,981

E.3 Data for calibration

Table B.12: Data for Calibration Exercise

Columns (1) and (3) provide first-stage coefficients on the marginal tax rate MTR. Columns (2) and (4) provide the effects on the average tax rate relative to GFW. $ATR^{GFW} = wtax_{i,t}/GFW_{i,t}$, and is censored to lie below $2\tau_t$. First-stage regression results for subsamples are obtained by estimating a modified version of equation (6): Coefficients on $\log(\widehat{TaxVal})$, $\mathbb{1}[d_i > 0]d_i^{scaled}\Delta_i$, $\mathbb{1}[d_i < 0]d_i^{scaled}\Delta_i$, and Δ_i are estimated separately for different TNW_{2009} quartiles. I also include quartile-bin fixed effects. The resulting simulation results based on columns (1) and (2) are provided in Figure 7. Figure B.9 provides the simulation results that incorporate the heterogeneity in columns (3)-(9).

	First-stage regression results				Subsample means				
					NOK 1000s				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	MTR (pp)	ATR^{GFW} (pp)	MTR (pp)	ATR ^{GFW} (pp)	TNW	GFW	Labor Earnings	TTLI	Age$_{2009}$
$\log(\widehat{TaxVal})$	0.2416	0.3648							
	(0.0146)	(0.0196)							
	First-stage heterogeneity, coefficients on $\log(\widehat{TaxVal})$								
TNW_{2009} Q1			0.2361	0.2688	515	380	453	637	57
			(0.0141)	0.0193					
TNW_{2009} Q2			0.3295	0.4141	939	517	358	601	63
			(0.0140)	(0.0194)					
TNW_{2009} Q3			0.2882	0.4471	1542	900	368	659	64
			(0.0142)	(0.0195)					
TNW_{2009} Q4			0.0470	0.3067	3809	2828	523	848	62
			0.0141	(0.0192)					
N	1441985	1433544	1440439	1433539					