

WEALTH TAXATION AND HOUSEHOLD SAVING: EVIDENCE FROM ASSESSMENT DISCONTINUITIES IN NORWAY

Marius A. K. Ring*

April 19, 2024

Abstract

Neither theory nor existing empirical evidence support the notion that wealth taxation reduces saving. Theoretically, the effect is ambiguous due to opposing income and substitution effects, and empirically, the effect may be confounded by misreporting responses. Using geographic discontinuities in the Norwegian annual net-wealth tax and third-party reported data on savings, I find that wealth taxation causes households to save more. Each additional NOK of wealth tax increases annual net financial saving by 3.76, implying that households increase saving enough to offset both current and future wealth taxes. The increase in financial saving is primarily financed by extensive-margin labor supply responses. These responses are the combination of small negative effects of increasing the marginal tax rates on wealth and larger positive effects of increasing average rates. These findings imply that income effects may dominate substitution effects in household responses to rate-of-return shocks, which has important implications for both optimal taxation and macroeconomic modeling.

Keywords: Wealth Taxes, Savings, Capital Taxation, Intertemporal Substitution

JEL: G51, D14, D15, H20, H31, E21, J22

*University of Texas at Austin, McCombs School of Business, and Statistics Norway. E-mail: mariuskallebergring@gmail.com. I thank my advisors, Dimitris Papanikolaou, Anthony DeFusco, and Matthew Notowidigdo for helpful comments and advice. I owe considerable gratitude to Andreas Fagereng and Thor O. Thoresen for their help in initiating this research project. I thank Scott Baker, Kjetil Storesletten, Liam Brunt, Jim Poterba, Espen Henriksen, Henrik Kleven, Martin B. Holm, Matthias Doepke, Krisztina Molnár, Manudeep Bhuller, Evelina Gavrilova, Enrichetta Ravina, SeHyoun Ahn, Gernot Doppelhofer, Jacopo Ponticelli, Victoria Marone, Tore Ring, Floris Zoutman, Annette Vissing-Jørgensen, and Alina Bartscher for helpful comments and discussions, and participants at the Kellogg Finance bag-lunch seminars, Statistics Norway, Skatteforum, NHH Norwegian School of Economics, London Business School, BI Norwegian Business School, Drexel, Yale, NYU, UT Dallas, WUSTL, Stanford, UCLA, MIT Sloan (Finance), Wisconsin-Madison, UT Austin, the Center for Retirement Research at Boston College, MIT Sloan (Economics), the Nordic Junior Macro Seminar, the NBER Summer Institute Public Economics workshop, the University of Oslo, Einaudi (EIEF), and the IIPF annual conference for helpful comments and questions. Support from the Research Council of Norway (grant #283315) is gratefully acknowledged.

1. Introduction

How households respond to changes in the net-of-tax rate of return is crucial to both optimal capital taxation and macroeconomic modeling. In optimal capital taxation, it determines the extent of distortionary effects on saving behavior and labor supply. Quantifying these distortions is necessary for determining the optimal tax policy ([Atkinson and Sandmo 1980](#), [Straub and Werning 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](#)). 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 net-of-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 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 in the net-of-tax rate of return comes from capital taxation in the form of an annual net-wealth

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.”

²In 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 4).

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 about 15 percent of taxpayers to the wealth tax, and primarily affects households in the top deciles of the permanent income distribution ([Halvorsen and Thoresen, 2021](#)). The main components of the tax base are financial and housing wealth. While financial wealth is 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 geographic 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 tax 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 of 2009 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 paid in wealth tax, households *increase* their yearly net financial saving by 3.76, primarily driven by increases in gross saving. 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.⁴ This adjusted saving propensity is almost four 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 that can be completely ascribed to extensive-margin responses. 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 2.37 for each additional NOK of wealth tax, enough to finance a majority of the additional saving. These findings translate into wealth effects on labor supply that are substantially larger than those found in lottery studies. Such large wealth effects are incompatible with assuming away wealth effects on labor supply by using quasi-linear or [Greenwood, Hercowitz, and Huffman \(1988\)](#) preferences.

³This includes [Chamley \(1986\)](#) and [Judd \(1985\)](#). For further discussion, see, e.g., [Bastani and Waldenström \(2018\)](#), [Scheuer and Slemrod \(2021\)](#), [Guvenen, Kambourov, Kuruscu, and Ocampo \(2022\)](#), and [Lu \(2021\)](#).

⁴References 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](#))

I find no evidence that the increase in saving crowds out illiquid pension saving. If anything, households decelerate the withdrawal of their pension wealth. In this setting with limited evasion and avoidance opportunities, I also find no evidence that households reduce their taxable wealth. The implied semi-elasticity of taxable wealth with respect to the marginal tax rate is -28, which is the opposite sign of existing findings ([Seim 2017](#), [Zoutman 2018](#), [Brülhart et al. 2019](#), [Londoño-Vélez and Ávila-Mahecha 2020a](#), [Jakobsen et al. 2020](#); [Durán-Cabré et al. 2019](#)).

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 dominant hypothesis is that risk-averse agents will respond to a wealth-tax-induced drop in lifetime consumption by allocating less 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 et al. 2019](#); [Daniel et al. 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. My point estimates are consistent with an EIS between 0.06 and 0.12. 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 are 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 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.

The main contribution of my paper is to emphasize the real effects of capital taxation, both in terms of financial saving and labor supply responses. I provide a fuller discussion of how the paper contributes to the related literatures in Appendix [A](#).

The paper proceeds as follows. Section [2](#) discusses the institutional details, the identification, and the data. Section [3](#) presents the main results. Section [4](#) uses a simple life-cycle model

to illustrate the relationship between my empirical findings and the EIS. Section 5 provides additional results. Section 6 concludes.

2. The Empirical Setting

2.1. Wealth Taxation in Norway

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

$$wtax_{i,t} = \tau_t(TNW_{i,t} - Threshold_t) \mathbb{1}[TNW_{i,t} > Threshold_t], \quad (1)$$

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 700,000 (USD 78,000) to NOK 1,200,000 (USD 208,000) during 2010–2015.⁵ Since wealth levels grew over the same period, the location of the wealth tax in the TNW distribution was virtually unchanged (see Appendix Figure B.15). The tax rate, τ , was 1.1% during 2010–2013, 1% in 2014, and 0.85% in 2015.⁶ I discuss these and other changes to the wealth tax schedule in Appendix G.

The wealth tax base, TNW , is the sum of taxable assets minus liabilities. The class of taxable assets is large, and includes most forms of marketable wealth, that is, housing wealth, securities, deposits, and other real assets, such as cars (see Appendix B.2). Pension wealth is not subject to the wealth tax. The main component of TNW for most households is housing wealth, which is assessed at a discounted fraction of estimated market value (25% for owner-occupied housing). The market value of all financial assets held through or borrowed from domestic financial institutions are third-party reported each year and enter TNW without a discount (except during the years 2005–07, see Appendix G.) 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 presence of a wealth tax threshold is a crucial ingredient in this empirical setting. It allows quasi-random variation in the assessment of the *housing* wealth component of TNW to provide variation in the marginal return on all types of taxable wealth, including *financial* wealth.

2.2. 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

⁵ Assumes the 2010 USD/NOK exchange rate of around 6.

⁶ The rates were lowered when a right-wing coalition government came to power in 2013. The rate remained at 0.85% for the duration of their tenure (2013–2021) and was subsequently increased by the next (left-wing coalition) government, first to 0.95% for 2022 and to 1.0% for 2023. See Appendix G, where I argue that households should not have anticipated a substantial weakening of the wealth tax.

multiple of the initial tax assessment, which typically corresponded to 30% of construction cost.⁷ This approach grew unpopular, because some areas experienced larger house price growth than others, which 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 saturated with geographic fixed effects.⁸ This resulted in the following formula for the (log) tax value of a household's residence:

$$\log(\widehat{\text{TaxVal}}_i) = \hat{\alpha}_{R,s} + \hat{\gamma}_{R,Z,s} + (1 + \hat{\zeta}_{R,s}^{\text{size}}) \log(\text{Size}_i) + \hat{\zeta}_{R,s,d}^{\text{Dense}} + \hat{\zeta}_{R,s,a}^{\text{Age}}. \quad (2)$$

The first two terms are *region* and *price zone* fixed effects. A region is a collection of counties or one of the largest four cities. A price zone is a within-region collection of municipalities or, in the case of the larger cities, within-city districts. The $\hat{\zeta}_{R,s}^{\text{size}}$ term accounts for the region-specific relationship between past transaction prices and the size of the house. $\hat{\zeta}_{R,s,d}^{\text{Dense}}$ is a fixed effect that applies to houses that are located in a cluster of at least 50 houses and $\hat{\zeta}_{R,s,a}^{\text{Age}}$ is a region-specific house age-bin fixed effect. The s subscripts indicate that the estimates are produced at the structure-type level, allowing the formula to vary for (i) detached and (ii) non-detached housing units and (iii) condominiums.

This formula outlines two sources of geographic variation that I use for identification. (i) Geographic discontinuities in tax assessment arise at price-zone boundaries (Z) due to cross-boundary differences in $\hat{\gamma}_{R,Z,s}$. This occurs when bordering municipalities or within-city districts are assigned to different price zones. In these cases, even if house prices move smoothly in a geographic sense, the assessment model *imposes* assessment discontinuities. (ii) Geographic discontinuities also arise across price-region boundaries (R). Across these boundaries, the discontinuity is driven by all of the estimated coefficients. The age-bin fixed effects, $\hat{\zeta}_{R,s,a}^{\text{Age}}$, for example, imply that the discontinuity may be smaller or larger at a R boundary depending on the age of the structure. This creates heterogeneous discontinuities in assessments across price-zone boundaries that provide additional variation.⁹

I collect all the data necessary to replicate the assessed house values as from Statistics Norway's estimation reports (see [Statistics Norway 2009](#), [Statistics Norway 2010](#); and Appendix [A.3](#) for an example.) In Appendix [B.3](#), I provide further details on the use of the hedonic pricing model and verify that it accurately predicts assessed tax values as observed in the tax returns. Appendix Figure [A.2](#) shows how a typical house would be assessed in different municipalities.

⁷The 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 Stănescu \(2020\)](#) 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.

⁸The housing price model used to assess house values at year t would include transactions during $t - 5, \dots, t - 1$. When households were given preliminary estimates of their assessed values during 2010, only 2004–2008 data were used in the regression. When actual tax values were assigned, 2009 data was included.

⁹Note that I only exploit the cross-boundary discontinuities for identification by controlling for the characteristics (see \mathbf{H}_i in section [2.4](#)) that determine this treatment heterogeneity. For example, I do not use building age for identification, but I exploit the fact that within a border area the assessment discontinuity depends on age.

2.3. Identification

I obtain identifying variation in household wealth tax exposure from differential tax assessments of housing wealth. The quasi-experimental variation is governed by equation (2), which says that the structure type, age, location, and size of a house are the key determinants of its assessed value. This produces variation in taxable wealth and thus wealth tax exposure as prescribed by equation (1).¹⁰ To limit the scope of selection into treatment, I assign treatment (i.e., location) based where a household lived prior to the reform I further require that households lived in their house since 2007, which is well before the hedonic pricing model was developed.

My empirical approach is to compare houses that are identical on observables such as structure type, age, and size, but differ in terms of their location. While this limits the scope for confounding, one important exclusion restriction issue persists. Households that live in higher-assessment areas also live in more expensive areas. They will thus tend to have higher incomes and wealth, both of which are positively correlated with saving behavior. Hence, there is a potential positive bias in the implied effect of wealth taxation on saving. I address this concern in two steps.

Firstly, I employ a boundary discontinuity design (BDD) approach. This exploits the fact that all of the geographic variation in tax assessments arises at geographic boundaries. Hence, I may focus on comparing households near these boundaries without sacrificing much identifying variation. This strengthens identification because I am not comparing the average household in a low-assessment municipality with the average household in a high assessment municipality, who are in fact quite different. I am rather comparing households who are geographically close—but on opposite sides of the geographic boundaries. Hence, even if assessed tax values are correlated with other determinants of saving behavior, my estimates will not necessarily be biased. The identifying assumption becomes that assessed tax values are not correlated with geographic discontinuities in these determinants. It does not pose an identification problem if, for example, households in higher-assessed areas are wealthier as long as these differences in wealth are geographically smooth.¹¹

The concern that potential determinants of the outcome variable vary discontinuously across geographic boundaries in a way that correlates with differences in tax assessments remain. Municipality-specific amenities such as elementary schools are an example of this. The concern that some confounders may vary discontinuously across boundaries is ubiquitous in the BDD literature. In the standard single-boundary BDD setting, this is essentially an omitted variables problem because one cannot control for cross-boundary differences in potential confounding

¹⁰More specifically, we see that housing assessments affect wealth tax exposure on both the intensive margin (i.e., the marginal wealth tax rate when $\mathbf{1}[TNW_{i,t} > Threshold_t]$ switches on) and the intensive margin (i.e., the wealth tax bill, $wtax_{i,t}$ or the amount of wealth subject to wealth tax, $TNW_{i,t} - Threshold_t \mathbf{1}[TNW_{i,t} > Threshold_t]$). The presence of a wealth tax threshold is an important ingredient in that it causes variation in housing assessments to have an effect on the marginal tax rates on all forms of taxable wealth through switching on $\mathbf{1}[TNW_{i,t} > Threshold_t]$.

¹¹To be precise, the empirical specification (see section 2.4) controls for smooth geographic variation in potentially unobserved covariates of saving behavior. Hence, it is not a concern if households who live on opposite in fact are different. For example, consider the incomes of four households that live equidistantly apart on a street. $1, 2 < 3, 4$ is fine. The problem arises in a setting with, e.g., $2, 2 < 3, 3$.

factors as these controls would be collinear with the treatment. A pertinent feature of my empirical setting, however, is that I obtain identifying variation from many boundaries (see Appendix Figure B.19 for a stylized example). At some of these boundaries tax assessments change considerably, but at others there is no difference at all. This allows me to control for cross-boundary differences in covariates such as wealth. Notably, I may also control for the key covariate of tax assessments, namely past transaction prices. While differences in tax assessments are significantly correlated with cross-border differences in past transaction prices (as they were constructed to be), this correlation is in fact quite modest (see Appendix Figure B.2 for a scatter plot). This weak correlation allows me to implement a second step of strengthen identification by controlling for past transaction prices without substantially reducing the amount of identifying variation.

This weak correlation between tax assessments and cross-border differences in transaction prices is driven by how municipalities are *grouped* into price zones or price regions when estimating the hedonic pricing model. In some cases, even if past transaction prices are very different, tax assessments may be identical due to bordering municipalities being allocated to the same price zone. In other cases, even if past prices are very similar, assessments may be very different. This is because many coefficients in the hedonic pricing model are estimated at a regional level (one or multiple counties), which allows geographically distant past transactions to affect a given house's assessment. I provide a fuller discussion of this in Appendix H

This multiple-boundary setting thus allows me to substantially weaken the identifying assumption to the following: The assessment discontinuities are not correlated with confounding factors that both change discontinuously at geographic boundaries and are uncorrelated with cross-border differences in past transaction prices or wealth levels. This identifying assumption is considerably weaker than in other BDD settings. It may be violated to the extent that there is geographic heterogeneity in the correlation between past transaction prices and saving behavior and this heterogeneity correlates systematically with how municipalities were allocated into price zones and price regions. I perform a range of placebo tests to investigate this. Firstly, I do not control for cross-border differences in past income levels, which allows me to examine income levels as a placebo test (Figure 1, Panel E). Secondly, I examine whether there are differences in pre-period saving behavior. Third, I check whether differences in past transaction prices predict post-period saving behavior once tax assessment is controlled for (Appendix F). All of these tests support the identifying assumption that my identifying variation is not correlated with potential confounders. In Appendices G.2, G.3 , G.6, and G.7, I discuss why municipal financing, property taxation, collateral value effects, and house price capitalization are unlikely to play confounding roles in this setting. Appendix B.8 relates my empirical approach to the existing BDD literature (e.g., Black 1999; Bayer et al. 2007; Livy 2018; Harjunen et al. 2018).

2.4. *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.¹² Boundary areas, b , are sets of households assigned to the same municipal boundary. Within a boundary area, 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.¹³ Geographic variables, such as d_i and b are all measured in 2009. Since my sample includes many border areas that are heterogeneous with respect to size and density, I normalize d_i across border areas.¹⁴

Identifying variation. I define Δ_i as the discontinuous 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. Δ_i is a border-area and structure-type-specific linear function of the vector of house characteristics used in the pricing model (2), $\mathbf{H}_i = \{\log(Size)_i; Dense_i; \mathbf{1}[Age_i \geq a]\}$, for $a = 10, 20, 35\}$, and isolates the identifying variation in model-implied tax assessment, $\widehat{\log(TaxVal_{i,t})}$, 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.

$$\Delta_i \equiv \widehat{\log(TaxVal_i)} \Big|_{d_i > 0} - \widehat{\log(TaxVal_i)} \Big|_{d_i < 0}. \quad (3)$$

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 .

$$y_{i,t} = \underbrace{\beta_1[d_i > 0]\Delta_i + \gamma^- d_i \mathbf{1}[d_i < 0]\Delta_i}_{\text{Discontinuity}} + \underbrace{\gamma^+ d_i \mathbf{1}[d_i > 0]\Delta_i + \delta'_{b,s} \mathbf{H}_i + \rho'_t \mathbf{M}_m + \Gamma'_t \mathbf{X}_i + \varepsilon_{i,t}}_{\text{Geographic controls}}. \quad (4)$$

The inclusion of border-area and structure-type-specific linear controls in housing characteristics, \mathbf{H}_i , isolates the identifying variation in $\widehat{\log(TaxVal_{i,t})}$ to $\mathbf{1}[d_i > 0]\Delta_i$. $\hat{\beta}$ 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 coefficients on $\mathbf{1}[d_i < 0]\Delta_i$ and $\mathbf{1}[d_i > 0]\Delta_i$ are meant to capture the effect of covariates that load continuously on Δ_i .

As discussed in section 2.3, to alleviate concerns that there are geographic discontinuities in other determinants of saving behavior that correlate with $\mathbf{1}[d_i > 0]\Delta_i$, I include a vector of municipality-level control variables, \mathbf{M}_m . This vector contains averages of residualized log transaction prices during 2009 as well as pre-reform TaxVal and gross financial wealth (GFW) in 2009 (see Appendix B.4). To increase precision and further limit the scope of confounding,

¹²I 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.

¹³Within 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.

¹⁴The 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 in Appendix B.6. My approach to normalization procedure is the following. I first calculate the standard deviation of d_i at the boundary-area level. I then divide d_i by the (local) standard deviation, and scale it back up by the mean (across boundary areas) standard deviation. See B.9 for additional discussion and robustness checks.

I include a vector of household-level controls, \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, $\log(\text{total taxable labor income})$, $\log(\text{GFW})$, a household college-attendance dummy, a debt dummy, $\log(\text{debt})$, the stock market share of GFW, $\log(\text{TaxVal})$, a dummy for additional real estate ownership and the log of its tax value, and finally a dummy for non-listed stock ownership. To limit the influence of outliers and accommodate zeros when taking logs, the arguments are shifted by NOK 10,000 (about USD 1,700; see Appendix B.7)

2.5. 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. The main data sources on household savings, labor supply, and taxable wealth come from tax returns. I use real-estate ownership registers link the tax-return data to information on housing characteristics, geographic location, and past transaction prices. I further supplement with demographic data from the National Population Register and employer-employee registers. I describe the data in more detail in Appendix B.1 and provide summary statistics in Panel A of Table A.1.

Sample restrictions. I only keep households with an average age of 25 in 2009, who lived in the same home, exceeding $50m^2$ in size, during 2007–2009, directly owned at least 90% of their primary residence, and had a positive assessed tax value on their house in 2009, and total labor income (incl. pensions) above NOK 150,000 (approx. USD 25,000) in 2009. I then only keep households with taxable net wealth (per adult) in 2009 strictly above 0 and below 6 MNOK (99th percentile). 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 primary reason for incorporating the upper bounds on TNW_{2009} is that these households will contribute very little to the identifying variation due to housing wealth being a small share of their TNW . I further restrict my sample to households within 10 km of the boundary, which retains about 80% of my sample.

An immediate consequence of focusing on households with initial positive TNW is that the resulting sample has a fairly high median age of 61 (see Table A.1), and is thus fairly close to retirement. This is the same as the average age of 61 in Jakobsen et al. (2020). 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. Another consequence is that sample participants have considerable wealth. It is therefore unlikely that my identifying variation in wealth tax exposure affects behavior through liquidity as opposed to income and substitution effects.¹⁵ I discuss the potential role of liquidity effects in more detail in Appendix G.5

¹⁵It may be useful to contrast progressive wealth taxation in Norway with property taxation. Since primary housing wealth enters at a 75% discount but debt and liquid financial assets enter one-for-one, households with only illiquid housing wealth and a large mortgage have *negative* TNW are shielded from wealth taxation. In most property tax regimes, however, they would not be shielded from property taxation, implying that liquidity effects are likely much more important in understanding responses to property taxes as opposed to wealth taxes.

3. Empirical Results

3.1. A Graphical Overview

FIGURE 1: VERIFYING THAT ASSESSED TAX VALUES JUMP AT PRICING BOUNDARIES WHILE OBSERVABLE CHARACTERISTICS DO NOT

This figure shows how tax 2010 tax assessment and pre-period observables vary across the price-zone boundaries. Panel A considers tax assessment in 2010. Panel B considers past transaction prices (2005–09). Panel C considers past tax assessment (2009). Panel D considers taxable net wealth in 2009. Panel E considers total labor income (including pensions and other labor-related transfers). Finally, Panel F considers gross financial wealth in 2009. All panels except B consider households in the main analysis sample. To improve precision, panel B includes houses purchased by households not in the main sample: e.g., households with $TNW_{2009} < 0$. The scatter points stem from estimating coefficients on Δ_i in equation (4) (without including the vector of household-level controls \mathbf{X}_i) separately for distance (d_i) bins. The solid line proves fitted geographic slopes, and the dashed curved lines provide the associated confidence intervals. The estimated discontinuities equal the jump from the left-hand-side to the right-hand-side solid lines. See also Appendix Figure B.8 that considers the change in tax assessments between 2009 and 2010 and Appendix Figure B.1 that considers past transaction prices during narrower time windows (2008–09 and 2009).

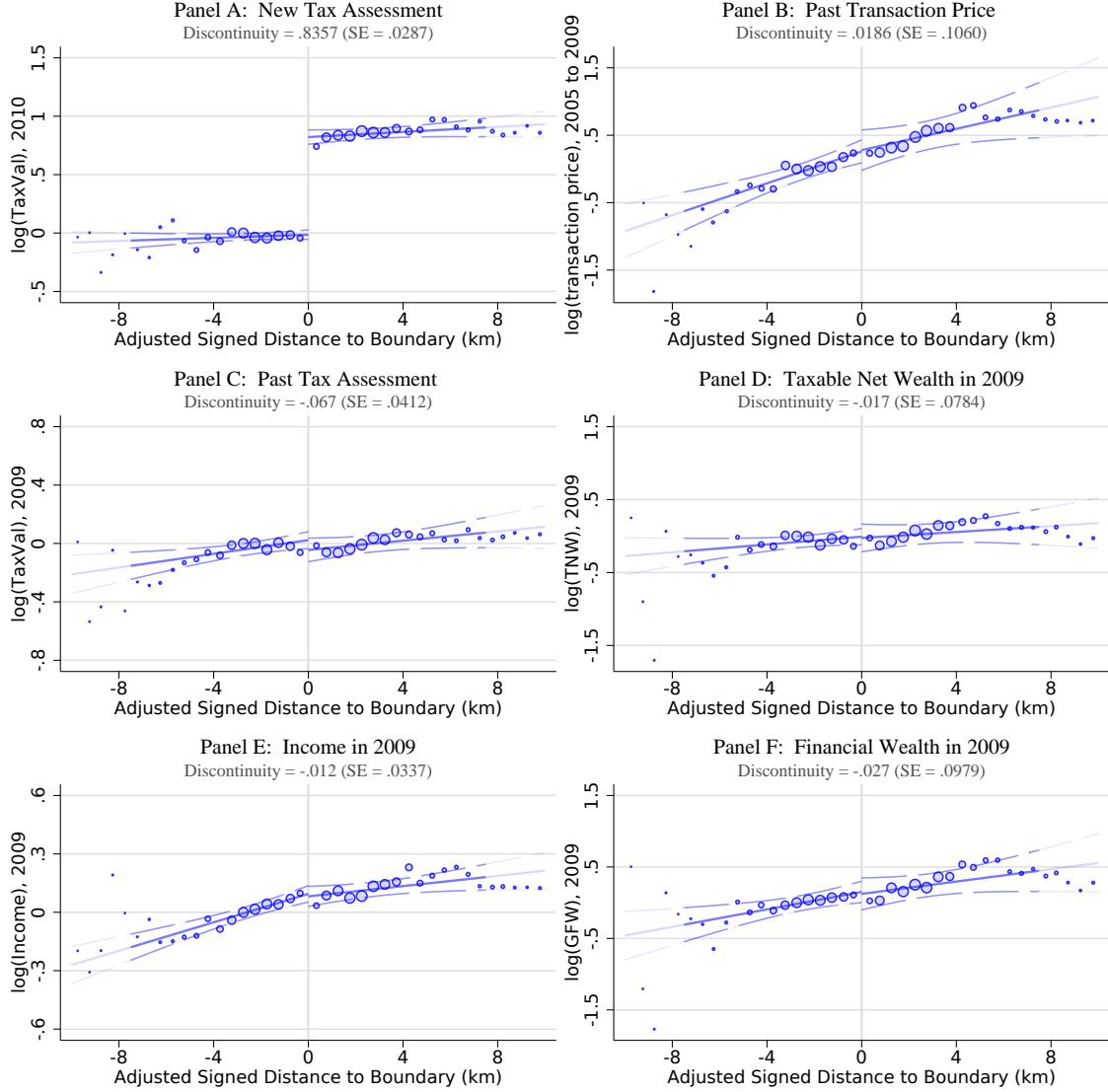


Figure 1 provides a graphical overview of my empirical setting. Panel A shows that for a given model-implied treatment discontinuity, Δ_i , assessed housing wealth does indeed rise by

close to Δ_i log-points. Appendix Figure B.8 shows that there is a corresponding *change* in tax assessments between 2009 and 2010. This verifies that the tax authorities do use the model-implied tax assessment, $\widehat{\text{TaxVal}}$ to assess housing wealth, TaxVal , for wealth tax purposes.

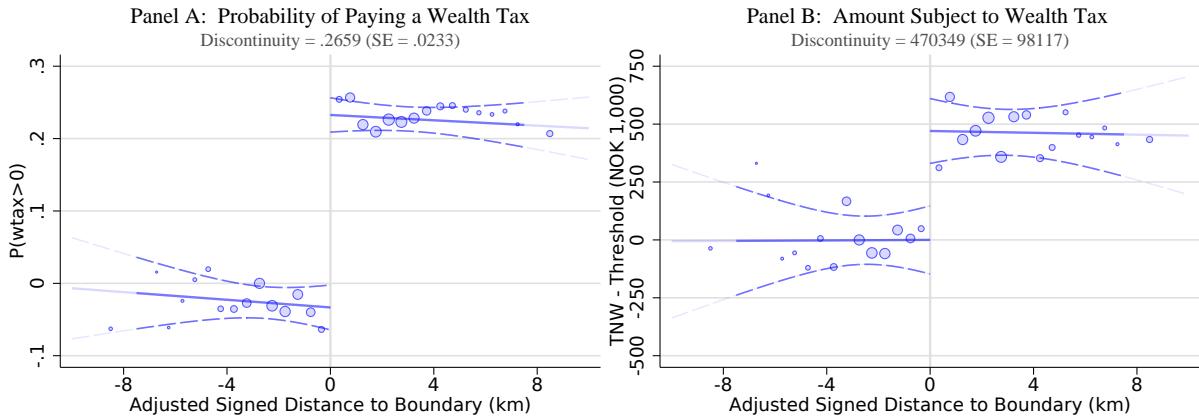
Panel B verifies that my identifying variation is not correlated with transaction prices during 2005–09. In Appendix Figure B.1, I show the same for transaction prices during more recent time periods (2008–09 and 2009). Similarly, in Panels C and D, I find no evidence that the discontinuities are correlated with past wealth assessments. In Panel E, I consider total labor income. While I find that households in higher-assessed areas have higher incomes, there is no indication that this relationship occurs discontinuously at the geographic boundaries. Similarly, I find no evidence of residual discontinuities in gross financial wealth in Panel F. These results show that the identifying variation is uncorrelated with a range of potential confounders that are associated with saving behavior.

3.2. First-Stage Effects: Assessment Discontinuities and Wealth Taxation

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.

FIGURE 2: DISCONTINUITIES IN WEALTH TAX EXPOSURE

These graphs illustrate how geographic discontinuities in tax assessment, $\widehat{\text{TaxVal}}$, affect intensive- and extensive-margin wealth tax exposure during 2010–2015. Panel A considers the extensive-margin effect on whether households are above the threshold and thereby face wealth tax of about 1% of marginal savings. Panel B considers the effect on the amount of wealth above the threshold and thereby subject to the wealth tax. 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 left-hand-side to the right-hand-side solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{\text{TaxVal}}$. One negative-distance bin is normalized to be zero. The size of each circle corresponds to the relative number of observations in that bin. Standard errors are clustered at the municipality level.



I show the first-stage effects graphically in Figure 2. There is clear evidence of discontinuous

wealth tax exposure at the geographic boundaries. In Panel A, I find that a one log-point increase in tax assessment increases the probability of paying a wealth tax by about 27 percentage points. In Table 1, I translate this estimate into an average effect on the marginal tax rate of about 0.28 percentage points. Panel B shows that the intensive-margin effect is also sizable. A one log-point increase in tax assessment increases the amount subject to a wealth tax by NOK 470,000 (USD 78,000). Table 1, column (4) shows that this estimate maps into an average increase in the annual wealth tax bill of NOK 4,946.

This intensive-margin effect on the amount paid in wealth tax will affect households' *average* tax rate on wealth (ATR). I define the ATR in two ways: one with respect to gross financial wealth, GFW , and one with respect to marketable net wealth, W , which includes housing wealth and is net of debt. Table 1 shows a large effect on the ATR with respect to GFW of almost 0.43 percentage points. Since marketable wealth is generally much higher than financial wealth, the effect on the ATR with respect to W is smaller at about 0.05 percentage points.

Quantifying the first-stage effect the average tax rate is useful to form priors about the behavioral responses in terms of saving behavior. For example, the classical ambiguity in whether households save more or less in response to capital taxation refers to a linear (proportional) tax on all marketable wealth in which the MTR and ATR are the same. When, e.g., the ATR exceeds the MTR, positive saving responses are more likely since income effects will be disproportionately larger than substitution effects.¹⁶ In my setting, however, the MTR exceeds the ATR on marketable wealth, causing positive saving responses to be less obvious.¹⁷

TABLE 1: FIRST STAGE EFFECTS ON WEALTH TAX OUTCOMES

Columns (1) and (3) provide the estimated discontinuities in Figure 2, based on equation (4). Columns (2), (4)-(6) provide the discontinuities with respect to the average marginal tax rate, the amount accrued in wealth taxes, the average tax rate (ATR) with respect to gross financial wealth (GFW), and the ATR with respect to marketable wealth. Marketable wealth equals GFW plus housing wealth minus debt. Standard errors are in parentheses and are clustered at the municipality level. The F -statistic is the square of the t -statistic.

	Extensive margin		Intensive margin			
	$1[wtax > 0]$	MTR (pp.)	Amount Above (NOK)	$wtax$ (NOK)	ATR wrt. GFW (pp.)	ATR wrt. W (pp.)
	(1)	(2)	(3)	(4)	(5)	(6)
$1[d_i > 0] \times \Delta_i$	0.2659*** (0.0233)	0.2821*** (0.0247)	470349*** (98117)	4946*** (1042)	0.4291*** (0.0524)	0.0501*** (0.0072)
F -statistic	129.44	130.83	22.98	22.52	66.99	49.13
N	1433843	1432811	1433843	1432811	1425658	1411461
R ²	0.4550	0.4659	0.3358	0.3455	0.4738	0.4610

In interpreting my findings, one should be aware of the several changes to the wealth tax schedule that occurred during 2005–2015. These changes may have lead some households to perceive shocks to their wealth tax exposure as being transitory. This is not because the new

¹⁶See, e.g., [Gruber and Saez 2002](#) who formalize this in a static model of labor earnings

¹⁷This holds even if we account for the fact that the MTR on housing wealth is in fact lower than the nominal MTR. Since housing wealth enters at a discount of up to 75%, the effect on the MTR on housing wealth is about 0.07 percentage points, which is still larger than the effect on the ATR with respect to marketable wealth.

assessment methodology was challenged, but because households may have expected the wealth tax thresholds to rise enough for the housing valuation to be irrelevant. However, while many households became exempt from the wealth tax during 2005–09, the increases in the threshold during 2010–2015 were only enough to keep up with rising over-all wealth levels. Overall, there was no broad political consensus in favor of materially weakening the wealth tax. I discuss this in more detail in Appendix G, where I argue that quasi-experimental variation that I exploit therefore should be perceived as fairly persistent, implying that the magnitude of the responses I find should be close to the responses to a perfectly permanent shock.

3.3. The Effect on Saving Behavior

In terms of the behavioral responses to wealth taxation, I begin by examining the effect on gross financial saving. My outcome variable is the relative change in gross financial wealth, 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.¹⁸ I 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 gross financial saving at time t .

$$\text{Adjusted } GFS_{i,t} \equiv \frac{\Delta GFW_{i,t} + wtax_{i,t-1}}{GFW_{i,t-1}}. \quad (5)$$

This approach uses a simple approximation of the mechanical effect. Effectively, it assumes a zero counterfactual return on wealth lost due to wealth taxes. In Appendix Figure B.3, I show that this simplification only leads to a slight understatement of the behavioral response.

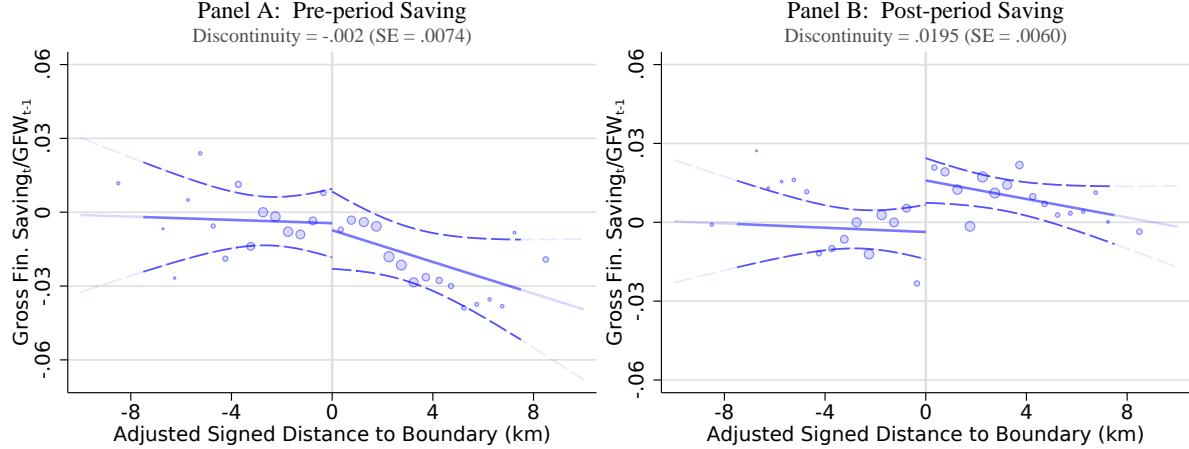
I provide my empirical findings in Figure 3. Panel B shows that there is no differences in past saving behavior. Panel B, however, shows a clear jump in post-period saving rates for households who face discontinuously higher tax assessment. A one log-point higher tax assessment increases the saving rate by about 2 percentage points.

I proceed by considering alternative measures of saving in Figure 4. Panel A considers the effect on debt. Visually, there appears to be a negative effect, suggesting that households also save more by paying off their debts. While this effect is statistically insignificant, it is consistent with wealth taxation causing more net saving. Panel B provides the effect on net financial saving. The numerator is simply (adjusted) changes to GFW minus any changes in debt. To avoid dividing by zeros or negative numbers, I use *gross* financial wealth in the numerator. This measure can thus be thought of as saving out of gross financial wealth, where debt payments are counted as saving.

¹⁸Foreign 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 Appendix B.

FIGURE 3: THE EFFECT ON GROSS FINANCIAL SAVING

These graphs consider the effect on gross financial saving, which is adjusted for the mechanical effects of higher wealth tax payments. 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 in the solid blue line at zero is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{\text{TaxVal}}$. Scatter-points stem from estimating a coefficient on Δ_i using equation (4) separately for d_i bins. 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 municipality level.



To grasp the economic significance of the effects on financial saving, it is useful to cast these findings in terms of saving propensities. I define the implied saving propensity out of annual wealth taxes, as

$$\text{Saving Propensity} = \frac{\text{BDD Estimate} \times \overline{GFW}}{\text{First-stage coefficient on } wtax}. \quad (6)$$

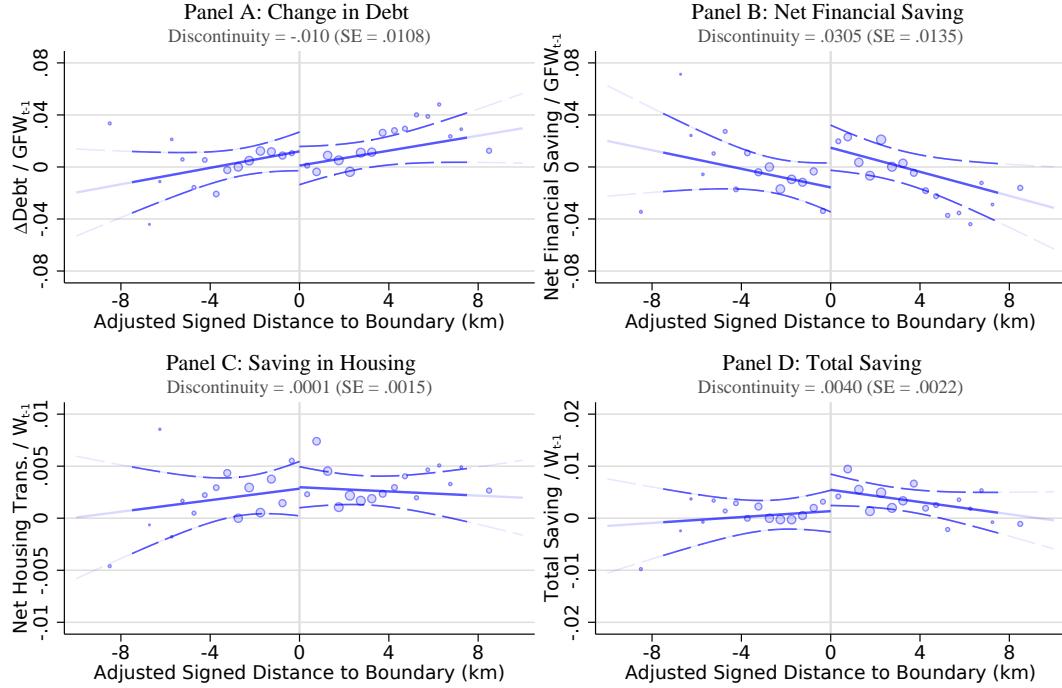
This approximates the change in the amount of saving by multiplying the saving rate (out of GFW) by median GFW. From Table A.1, we see that median GFW in the sample is 0.61 MNOK. Combining this with the first-stage coefficient of 4946 MNOK in column (4) of Table 1, we obtain a gross financial saving propensity of 2.40. This propensity says that for each additional NOK of wealth taxes, households increase their gross financial saving by 2.4 NOK. Similarly, the net financial saving propensity is found to be 3.76. These numbers are larger than unity, which implies that households save more than they need to maintain their current level of financial wealth. This is reasonable in my sample where the average household age is 61.5 years old and thus face declining incomes due to retirement. Consumption smoothing implies that treated household may wish to effectively pre-pay future wealth taxes prior to retirement in order to offset the negative effect on future net-of-wealth-tax capital incomes.

Panels C and D of Figure 4 consider saving in housing and total saving. Saving in housing equals net transactions in real estate markets plus capital gains from previous-years' net transactions. In Panel C, this measure is divided by marketable wealth, $W_{i,t-1}$, which equals housing wealth plus net financial wealth (see Appendix B.4 for details). The saving measure in Panel D equals saving in housing plus gross financial saving minus changes in debt, scaled by $W_{i,t-1}$. I don't find any effect on saving in housing. Consequently, since marketable wealth is generally

much larger than financial wealth, the effect on the total saving rate is small relative to the effect on the net financial saving rate. However, the implied *propensity* to save is very similar at 4.24.¹⁹

FIGURE 4: DEBT, NET FINANCIAL SAVING, AND TOTAL MARKETABLE WEALTH

I repeat the analysis in Panel B of Figure 3 for different measures of saving. Panel A considers the effect on debt ($\Delta\text{Debt}/GFW_{t-1}$). Panel B considers the effect on net financial saving, which equals gross financial saving minus $\Delta\text{Debt}/GFW_{t-1}$. Panel C considers the effect on saving in housing. The numerator equals net transactions in real estate markets and any value-increases from net transactions occurring during 2010–2014. The denominator equals marketable wealth, W , which equals GFW plus housing wealth minus debt. Panel D considers the effect on Net Financial Saving (divided by W_{t-1}) plus saving in housing. See Appendix B.4 for details on the variable construction. The discontinuities equal the vertical distances between the solid blue lines, and are estimated using equation (4). Standard errors are in parenthesis and are clustered at the municipality level.



In the Appendix, I provide supplementary analyses. Appendix Figure B.7 considers on saving rates out of income. Panel D shows that there is a clear jump in the saving rate of about 3.51 percentage points at the boundary. In Appendix E, I find no evidence that there is any crowd-out in terms of pension savings. Households instead increase their pension wealth by decelerating withdrawals, which is likely caused by pension wealth being exempt from wealth taxation. In Appendix Figure B.9, I consider the dynamic effect on savings and taxable net wealth during 2010–2015. This shows that net financial and marketable wealth accumulation is gradual. It further shows that that households do not respond by lowering their taxable net wealth, which would be the case if households were able to evade wealth taxation by misreporting components of TNW not included in financial or marketable wealth. Figure B.9 shows that TNW increases by about 8% over a six-year period. Since this is in response to a 0.28 pp. increase in the marginal tax rate, the implied elasticity of TNW with respect to one minus the tax rate is about -28. This is comparable in magnitude to the existing differences-in-differences literature

¹⁹I replace median GFW with the median W of 5.241 MNOK in equation (6) and obtain a total saving propensity of $0.0040 * (5.241 / (4946 / 1000000)) = 4.2386$

(see Table 1 in [Advani and Tarrant 2021](#)), but of an opposite sign, which is consistent with fewer evasion opportunities.²⁰

3.4. The Effect on Labor Earnings

Understanding labor supply responses to capital taxation is important due to potential spillovers to labor income taxation ([Atkinson and Sandmo, 1980](#)), but there is scarce empirical evidence. The effects I document on savings indicate that income effects dominate substitution effects. However, whether income effects play a role in determining labor supply is subject to debate ([Auclert, Bardóczy, and Rognlie, 2023](#)). In both public finance and macroeconomics, it is common to assume away income or wealth effects on labor supply by choosing quasi-linear or [Greenwood et al. \(1988\)](#) preferences. Documenting labor supply responses to wealth taxation informs this debate. Beyond this, studying labor supply responses is useful because it is not subject to the same kinds of measurement issues that affect estimates of savings elasticities. During my sample period, there is no “tax ceiling” limiting wealth taxes to a fraction of taxable income, hence there is no direct incentive for wealth-tax payers to underreport labor earnings.

My measure of labor earnings is the following:

$$\text{Labor Earnings}_{i,t} = \text{Salary and Wage Earnings}_{i,t} + \max(\text{Self-Employment Income}_{i,t}, 0). \quad (7)$$

I focus on pre-tax labor earnings in the form of salary, wages, and self-employment income. Under the reasonable assumption of interconnected municipal labor markets, wages are unaffected and labor earnings proxy for labor supply.²¹

Figure 5, Panel A, shows that a one log-point increase in tax assessment increases labor earnings growth by 0.0198 log points. In order to relate this point estimate to the effect on saving behavior, I define an earning propensity, similar to the saving propensity in equation (6). Since labor earnings is a flow variable, and I am considering the effect on its growth rate, I cumulate the growth over the 6-year period, and divide again by 6 to obtain an average-earning propensity.

$$\text{Earning Propensity} = \text{BDD Estimate} \times \frac{\frac{1}{6} \sum_{t=1}^6 t \times \overline{\text{Labor Earnings}}}{\text{First-stage coefficient on } wtax_{i,t}}. \quad (8)$$

Table A.1 shows that the median amount of labor earnings is 0.242 MNOK. With a first-stage coefficient on the annual amount of wealth taxes of 4946, this implies a multiplier of 171.25 on the BDD estimates and thus a pre-tax earning propensity of 3.39. If we assume a marginal tax rate of 30%, the post-tax propensity becomes 2.37, which is more than half as large as the net financial saving propensity of 3.76. This comparison shows that more than half of the net saving response appears to be financed by increased labor earnings.²²

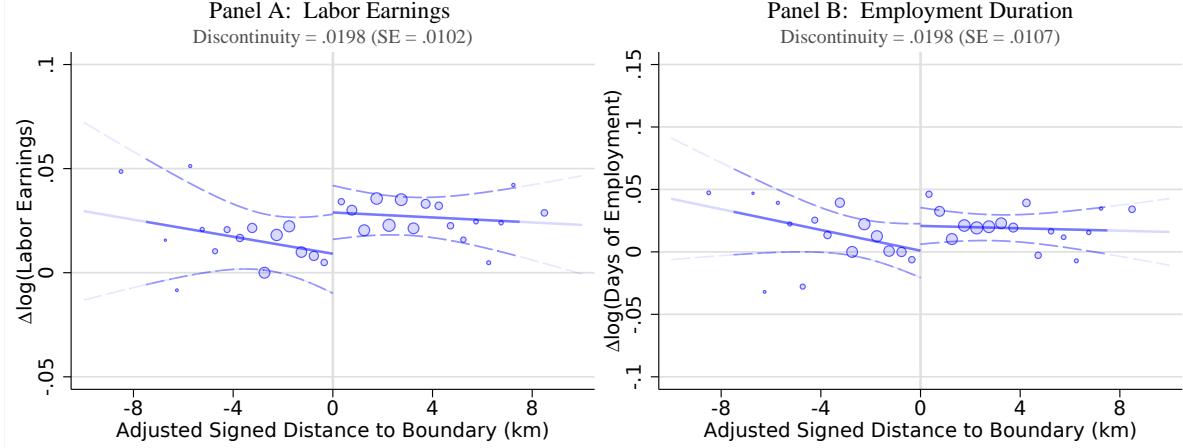
²⁰Not only do evasion opportunities differ across countries and samples, but the relationship between average and marginal tax rates may differ as well due to varying degrees of progressivity.

²¹The caveat is that there may be some form of income-shifting among business owners, as, e.g., in subsequent work by [Lefebvre et al. \(2023\)](#) who find a negative effect of capital taxation on labor income in France.

²²This comes with the caveat that there will be estimation error both above and below the numerator which precludes me from putting narrow intervals this ratio of propensities.

FIGURE 5: THE EFFECT OF WEALTH TAXATION ON HOUSEHOLD LABOR EARNINGS

These graphs consider the effect on labor earnings growth (A) and the number of days employed (B). Days Employed for an individual is the number of days during the year in which the individual is in an employment relationship. For households with two adults, I divide total the number of employment days by 2. To accommodate zeros, one week (7 days) is added to employment duration prior to taking logs. Discontinuities are estimated using equation (4). Standard errors are in parenthesis and are clustered at the municipality level.



Labor earnings effect relative to other work. In order to compare my findings to existing estimates of wealth effects on labor supply, I need to cast this propensity in terms of the present-value as opposed to the annual flow of wealth tax paid. The present value of an annual \$1 wealth tax discounted by 3% over 25 years equals 17.41. Hence, the pre-tax earnings propensity of 3.39 maps into a marginal propensity to earn out of wealth of -0.19. This is considerably larger than most existing estimates from lottery studies. In terms of the average annual effect over five to six years of one additional dollar of lottery winnings, findings range from -0.011 to -0.1 ([Cesarini et al. 2017](#), [Picchio et al. 2018](#), [Golosov et al. 2024](#), [Imbens et al. 2001](#)). However, [Zator \(2020\)](#) documents that responses to negative shocks are much larger. More similar to my findings are therefore those by [Giupponi \(2019\)](#) and [Deshpande \(2016\)](#) who study responses to reductions in welfare transfers and find that wealth losses are fully offset by higher labor earnings.

Another way to relate my results to existing work is to via Marshallian labor supply elasticity. To obtain a lifetime MPE, I assume that the six-year effect comprises the total effect over the remainder of their lifetime (assumed to be 25 years), which is similar to the structural assumptions in [Cesarini et al. \(2017\)](#).²³ This returns a lifetime MPE of $2.37 \times \frac{6}{25} = 0.57$., which implies a substantial wedge between the Hicksian and Marshallian elasticities unlike, e.g., early work by [Gruber and Saez \(2002\)](#) that find a wedge of zero.²⁴

Intensive versus extensive-margin responses. The average household in my sample is close to retirement age. One likely margin of adjustment is therefore marginally delaying retirement. While it is hard to formally define retirement due to households potentially exiting and re-entering the labor market multiple times within a year, I make some headway by employing

²³To simulate life-time MPEs, [Cesarini et al. \(2017\)](#) use reduced-form moments to calibrate a model in which there is a binding retirement age after which there simulated earnings responses must be zero. In my setting, households are 62 years old on average and thus close to the typical retirement age.

²⁴Note that while the point estimates are large in my setting, I have considerably less identifying variation in wealth than most lottery studies causing the implied MPE to be less precisely measured

the following decomposition of labor earnings.

$$\log(\text{Labor Earnings}_{i,t}) = \log(\text{Wage}_{i,t}) + \underbrace{\log\left(\frac{\text{Hours}_{i,t}}{\text{Days Employed}_{i,t}}\right)}_{\text{Intensive margin}} + \underbrace{\log\left(\text{Days Employed}_{i,t}\right)}_{\text{Extensive margin}}, \quad (9)$$

where the *Days Employed* term is observable in employer-employee registers. I measure days of employment as the number of days within a year that an individual is in any paid employment relationship.²⁵ If someone works longer hours or an additional day per week, this effect counts towards the intensive margin (hours/days employed), but if they if they retire later or re-enter the labor market following retirement, the response contributes towards the extensive margin (number of days employed). I provide the results in Panel B of Figure 5. This reveals a point estimate that is virtually identical to the one for labor earnings, suggesting that the entire labor earnings effect is driven by extensive-margin responses, such as marginally postponing retirement.

3.5. Portfolio Allocation

3.5.1. 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](#)); is important in understanding why wealthier households achieve higher returns ([Bach, Calvet, and Sodini, 2020b](#)); 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, I 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 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 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](#)).

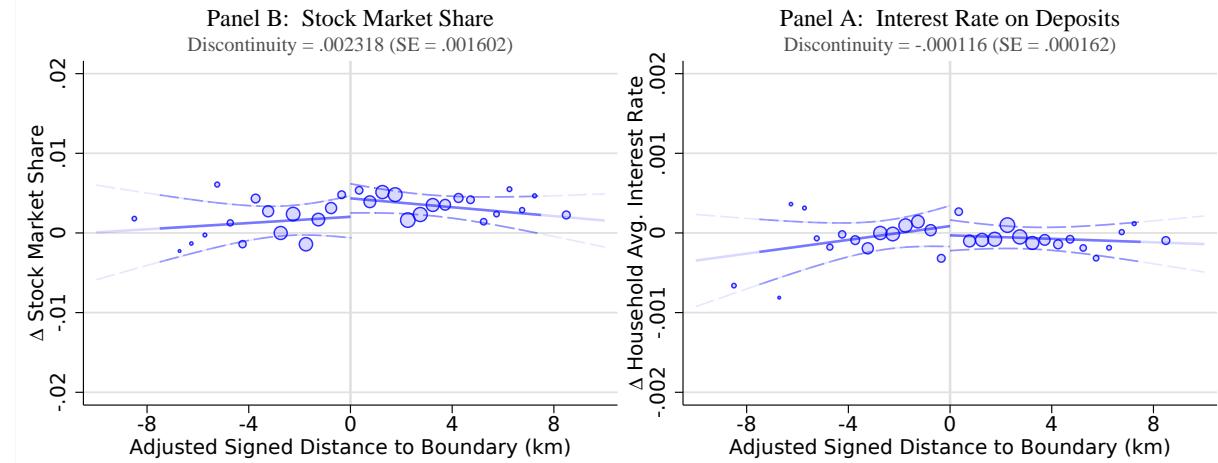
I present my empirical results in Panel A of Figure 6. This plot reveals no change in the

²⁵To accommodate observations that change their days of employment from or to zero, I shift the log argument by one week (7 days).

stock market share for households more exposed to wealth taxation. A back-of-the-envelope calculation suggests that we can rule out a yearly increase in capital incomes above NOK 275 (USD 46).²⁶ There is little evidence with which to compare these findings. While, for example, Alan et al. (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 6: THE EFFECTS ON PORTFOLIO ALLOCATION:
STOCK MARKET SHARE AND REALIZED PRE-TAX RETURNS ON SAFE ASSETS

Panel A consider the effect on changes in the stock market share (SMS), which is the ratio of stock market wealth (SMW) to gross financial wealth (GFW). Stock market wealth includes listed stocks and mutual fund holdings. Panel B considers the effect on changes in the realized interest rates on deposits. The discontinuity equals the vertical distance between the solid blue lines, which is estimated using equation (4). Standard errors are in parenthesis and are clustered at the municipality level.



While I can rule out a meaningful increase in the stock market *share*, this does not imply that stock market wealth does not increase. Appendix Figure B.4 shows a noticeable jump in the level of stock market wealth at the boundary, which is very similar to the overall effect on gross financial wealth.

3.5.2. The effect on realized returns on safe assets

I further 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, which may cause wealth taxation to increase (pre-tax) return heterogeneity,²⁷ and through it, wealth inequality (Bach, Calvet, and Sodini, 2017). 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

²⁶The confidence interval is [-0.0008, 0.0055]. $110 = 0.0055 * 1\text{MNOK} * 5\%$. I multiply the upper bound of the confidence interval by the mean amount of GFW (1MNOK) and by an assumed risk premium of 5% to obtain an estimate of the effect on capital income.

²⁷See, e.g., Bach, Calvet, and Sodini (2020b) and Fagereng, Guiso, Malacriño, and Pistaferri (2020a) who document the extent of return heterogeneity across the Swedish and Norwegian wealth distributions.

choose to suffer these non-pecuniary costs, i.e., supply more effort, in order to offset the adverse effects of more aggressive capital taxation. I test this by considering the average realized returns on bank deposits,

$$\text{Interest Rate on Deposits}_{i,t} = \frac{\text{Total Taxable Interest Income}_{i,t}}{0.5 \cdot \text{Deposits}_{t-1} + 0.5 \cdot \text{Deposits}_{i,t}}. \quad (10)$$

I report the main result in Panel B of Figure 6. The evidence is inconsistent with my initial hypothesis. Households' realized returns appear quite unaffected by the wealth tax treatment.²⁸ 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 small to trigger an increasing-returns-to-scale effect.

4. The Implied EIS in a Simple Life-Cycle Model

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, 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).²⁹ 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.

4.1. A simple life-cycle model

The model environment is simple: It only contains the core elements necessary to replicate my empirical results and the shock to wealth tax exposure. Agents choose both how much to save and how much to work, and 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.

²⁸As 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 0.0016 (0.61 p.p. times the first-stage effect on $1[w_{\text{tax}} > 0]$ of 0.2659). This hypothetical effect is eight larger than the upper bound of the 95% confidence interval in Panel A.

²⁹Of course, multiple studies outline settings in which capital taxation is indeed optimal. See, for example, Diamond and Spinnewijn 2011 and Conesa et al. 2009.

I follow Jakobsen et al. (2020) in modeling the responses of a representative agent. I use a simple life-cycle model with perfect foresight. The model features additively separable preferences with a constant EIS, $\frac{1}{\gamma}$, and Frisch elasticity of labor supply, $\frac{1}{\nu}$.

In this representative agent setting, we should think of $\frac{1}{\nu}$ as governing the both the intensive and extensive-margin

$$\max_{\{c_t, s_{t+1}, l_t\}_{t=0}^T} \sum_{t=0}^T \beta^t \left(\frac{1}{1-\gamma} c_t^{1-\gamma} - \psi \frac{l_t^{1+\nu}}{1+\nu} \right), \quad (11)$$

$$\begin{aligned} \text{s.t. } c_t + s_{t+1} &= y_t + l_t w_t \\ &+ s_t R - wtax_t(s_t). \end{aligned} \quad (12)$$

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.

$$wtax(s_t) = (s_t - \bar{s}) \mathbb{1}[s_t > \bar{s}] \tau. \quad (13)$$

Rewritten budget constraint. I 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

$$c_t + s_{t+1} = y_t + l_t w_t + \underbrace{s_t(R - MTR_t)}_{\text{Linearized Gross Capital Income}} + \underbrace{s_t(MTR_t - ATR_t)}_{\text{Virtual Income}}, \quad (14)$$

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.

4.2. Calibration

I set $R = 1.03$.³⁰ 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 , $ATR^{GFW} = wtax_{i,t}/GFW_{i,t}$. The virtual income shock is set to $s'_t(dMTR - dATR)$, where s'_t is the savings path of the

³⁰Fagereng et al. (2020a) show that the returns on financial wealth for households in the top 80% to 95% of the wealth distribution is around two to three percentage points.

unshocked agent.

I simulate the responses in terms of their saving behavior and labor supply for different values of the EIS ($\frac{1}{\gamma}$). I set $\beta = 0.96$ and the Frisch elasticity to 1. In this representative agent setting, $\frac{1}{\nu}$ governs the overall elasticity of labor supply, including both intensive and extensive-margin adjustments. 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%.³¹

y_t reflects pension income. I assume that agents gradually retire between ages 65 and 70. For agents below 65, I set y_t equal to the difference between mean total taxable labor income and labor earnings observed in the data. Once agents turn 65, 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 gradually stop working 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.³²

4.3. Simulated versus Empirical Treatment Effects

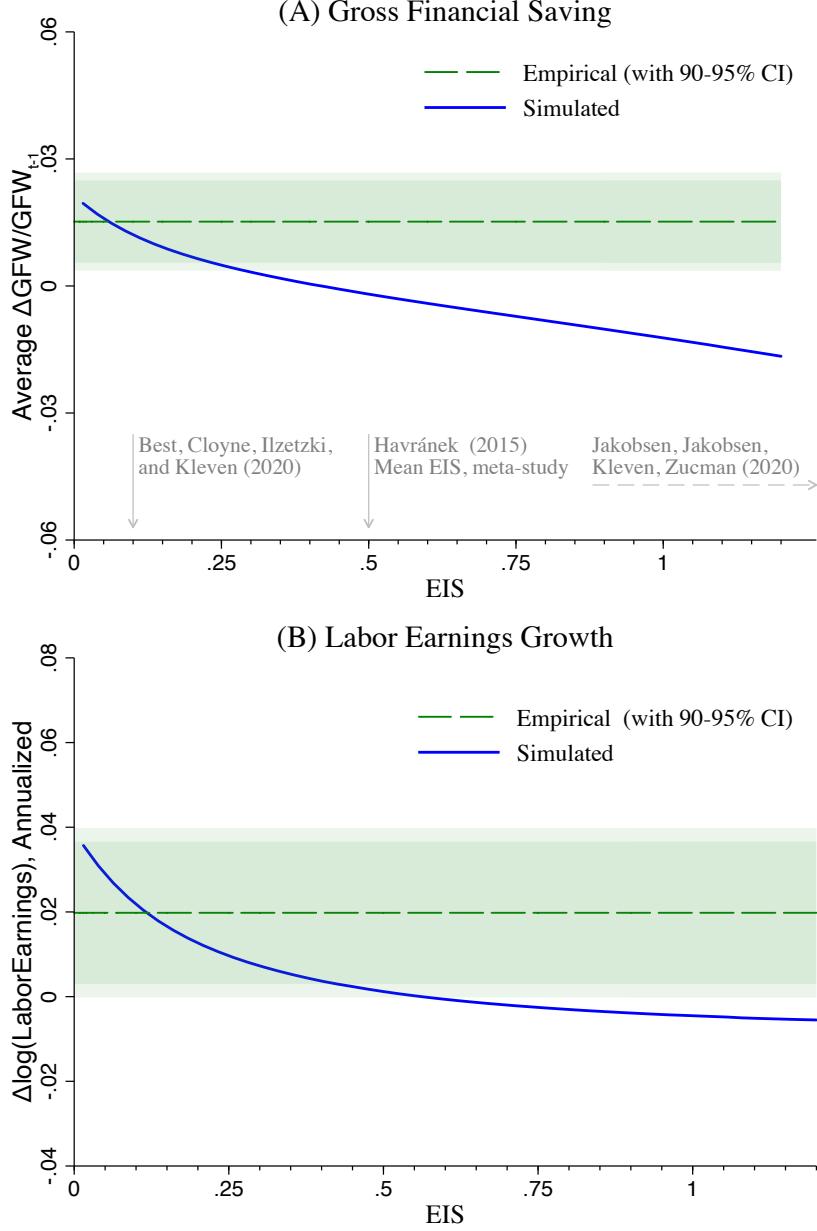
Figure 7 shows simulated treatment effects for different values of the EIS. Panel A considers the effect on gross financial saving absent the wealth tax adjustment (corresponding to the empirical findings in Panel A of Figure B.3). We see that the cut-off for when we see a change in the sign of the saving response is around 0.40. 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 0.06 can replicate my empirical findings.

³¹Choosing 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. More formally, $\log \psi = -\gamma \log(0.8 \times 0.456) + \log(0.456)$, where 0.456 is mean labor earnings. Note that labor supply, l is normalized to 1.

³²Absent 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 1. Panel A considers the effect on gross financial saving, without the wealth tax adjustment, where the empirical point estimate comes from Panel A of Figure B.3. Panel B considers labor earnings growth, where the point estimate comes from Panel A of Figure 5. 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. Simulated effects are smoothed by using a local 5th-order polynomial fit.



This figure also shows that positive saving responses to wealth taxation follows from a subset of recent EIS estimates. 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, Japan, and the U.S., where the EIS is found to be 0.022 (Agarwal, Chua, Ghosh, and Song, 2020) and 0.21 (Cashin and Unayama, 2016) and 0.19 (Baker, Johnson, and Kueng, 2021). My empirical evidence is

further largely consistent with values of the EIS used in recent research using quantitative macro models to consider the effects of wealth taxes: e.g., [Broer et al. \(2021\)](#) who use an (implied) EIS of 0.2 and [Rotberg and Steinberg \(2021\)](#) who use 0.25. However, [Havránek \(2015\)](#) reviews existing estimates of the EIS more broadly and finds a mean of 0.5, but considerable dispersion, with the mean estimate in Top-5 journal articles being close to 1.

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. As noted by [citetjakobsen2020wealth](#), this discrepancy may be explained by evasion or avoidance responses inflating the EIS in their setting.

Panel B considers the effect on labor earnings. To map the simulated responses to those found in Panel B of Figure 5, I consider the cumulative labor earnings response, which I average over time.³³ Interestingly, labor earnings responses are almost as sensitive to the EIS as the savings responses. The EIS cut-off below which we see positive earnings responses is about 0.55. In order to replicate the empirical treatment effect, I need an EIS of about 0.12, which is very close to the one needed to replicate the financial saving responses.

Appendix Figure B.12 shows that the exact choice of the Frisch elasticity does not have a qualitative effect on the implied EIS. A smaller Frisch elasticity causes labor supply increases (and thus increased saving) to be more costly in a utility sense. In order to match the empirical treatment effect with a smaller Frisch elasticity of 0.25, we need an even lower EIS of 0.02.

Frictions and the implied EIS. Binding credit constraints may mute the responses to wealth taxation as households' saving would be at a corner solution. Nevertheless, as I discuss in Appendix G.5, this is unlikely to play a material role in my setting where households have ample liquidity. Similarly, consumption adjustment frictions (see, e.g., [Chetty and Szeidl 2007](#)) would also mute saving responses to wealth taxation. An inability to consume in the short-run would make it more costlier, in a utility sense, to save more. This may partially explain why labor earnings responses account for a large part of the saving effect but cannot qualitatively explain why households save more as opposed to less, and would thus not bias the EIS downward.

5. Additional results

5.1. Disentangling The Effects of Changing Marginal and Average Tax Rates

Section 4 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 responses is a built-in feature in standard life-cycle models. This is because the EIS determines the strength

³³In 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.

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 marginal wealth tax rate of return, while income effects are driven by changes in the average wealth tax rate.

TABLE 2: THE EFFECTS OF CHANGING MARGINAL VERSUS AVERAGE WEALTH TAX RATES

This table provides the effect of changing marginal and average tax rates on saving and wealth accumulation behavior. I obtain differential MTR and ATR variation by allowing assessment discontinuity term ($1[d_i > 0]\Delta_i$) to have differential effects for households with different $\widehat{TNW}_{i,2009}$, which is the TNW that household i would have had in 2009 if their house had been assessed with the average assessment rules in their border area: I assess their house as if it were on the low-assessment side and again as if it were on the high assessment side and then take the average. T_t is the threshold in year t . n_i is the number of (married) adults in the households. The idea is that households with, e.g., $(\widehat{TNW}_{i,2009}/n_i - T_t)$ near 0 will be close to the threshold in year t , and thus the assessment discontinuity will have a large effect on their marginal tax rate (MTR). The outcome variables in columns (1) and (2) adjust for the mechanical effects of paying more in wealth taxes. ATR^{GFW} is the average tax rate with respect to GFW and ATR^W is the average tax rate with respect to (total) marketable wealth, which includes GFW and housing wealth, net of any debt. See Appendix Table B.1 for the underlying first-stage and reduced-form coefficients.

	Net Financial Saving	Total Net Saving	Labor Earnings
	$\frac{ANFS_t}{GFW_{t-1}}$	$\frac{ATNS_t}{W_{t-1}}$	$\Delta \log(LaborEarnings_t)$
	(1)	(2)	(3)
MTR	-2.2548 (4.7364)	0.7478 (0.5680)	-2.7466 (3.3506)
ATR^{GFW}	5.9364* (3.1937)		
ATR^W		4.9303* (2.8386)	21.0731* (11.9014)
N	1669285	1669285	1669285
rk-F-statistic	6.60	54.34	54.34

To decompose the effect of changing marginal and average tax rates, I follow Gruber and Saez (2002) in allowing the first-stage effects of the tax instrument (in this setting, the assessment discontinuities) to have differential effects based on a proxy for the counterfactual position in the progressive tax schedule.³⁴ The idea is that households initially below the threshold will largely see MTR effects while households initially above the threshold will primarily see intensive-margin ATR effects. My measure of counterfactual TNW is $\widehat{TNW}_{i,2009}$, which is the TNW that household i would have had in 2009 if their house had been assessed with the average assessment rules in their border area.³⁵ I then allow the first stage effects (as well as the geographic controls, see Appendix B.10) to vary by $\widehat{TNW}_{i,2009}$ bins. This essentially transforms a single instrument (the wealth tax discontinuity) into several, allowing me to instrument for both the ATR and MTR. To increase precision in estimating MTR effects, I now include households with $TNW_{i,2009} > -0.25M$ as opposed to only $TNW_{i,2009} > 0$ in my main sample.

I provide the reduced-form and first-stage coefficients in Appendix Table B.1 and Table

³⁴Gruber and Saez (2002) do this in the context of income taxation: A reduction in income-tax thresholds affect the marginal income tax rate primarily for those ex-ante below the threshold, while those above see a reduction in their average tax rates.

³⁵That is, I use the 2010 hedonic pricing model to assess their house as if it were on the low-assessment side and again as if it were on the high assessment side and then take the average.

[2](#) provides the IV estimates. I find no effects of changing the marginal tax rate on wealth, consistent with a low degree of intertemporal substitution.

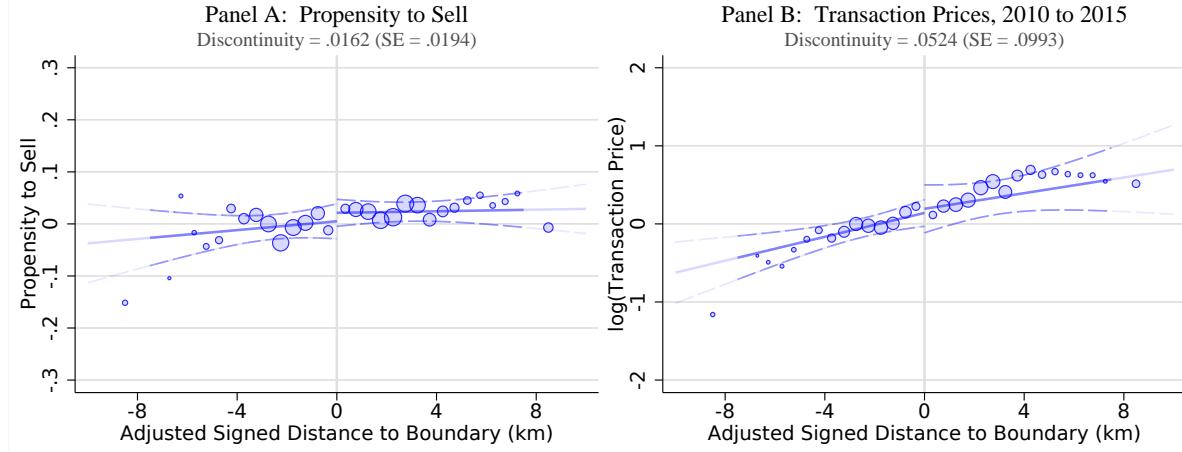
In terms of external validity, the MTR and ATR effects are largely identified by close-to and above threshold households, respectively. Given the qualitative finding of weak intertemporal substitution effects, this presents an external validity issue to the extent that ultra wealthy households have more intertemporally elastic consumption preferences than moderately wealthy households. In terms of internal validity, there may be a bias from the fact that the first-stage estimates are affected by reduced-form responses. That is, if households respond to wealth taxation by substantially reducing their savings, then the first-stage coefficient becomes downward biased and the magnitude of the IV coefficient becomes upward biased. However, given that the reduced-form coefficients are small positive, this unlikely plays a role in my setting.

5.2. The Effect on House Prices and the Propensity to Sell

I now consider the effect on whether households subject to higher wealth tax exposure sell their home, and I explore whether higher-assessed houses sell for less. The results are provided in Figure 8.

FIGURE 8: EFFECT ON HOUSE PRICES AND MOBILITY

Panel A considers the effect on whether the house (owned during 2007–09) is sold during 2010–2015. Panel B considers the effect on subsequent transaction prices. To increase the sample size (which now requires a sale during 2010–2015), I use no sample restrictions on household's demographics or financials. The discontinuity equals the vertical distance between the solid blue lines, which is estimated using equation (4). Standard errors are in parenthesis and are clustered at the municipality level.



Propensity to sell. A growing literature shows that household location choices are sensitive to taxation ([Agrawal and Foremny 2019](#); [Agrawal, Foremny, and Martínez-Toledano 2020](#); [Martinez 2017](#); [Muñoz 2023](#), [Jakobsen et al. 2023](#)). My setting has quite granular geographic variation in tax exposure, implying that households could relocate and lower their tax burden without having to switch jobs. Accordingly, I investigate whether households more exposed to wealth taxes sell their home, which would be the first main step in undoing the treatment of higher tax assessments. These results are provided in Panel B of Figure 8. There is no clear evidence that households sell their homes. The point estimate implies that the probability of moving increases by 1.62 percentage points, but this effect is statistically insignificant.

Subsequent transaction prices. Panel B of Figure 8 shows no effect on conditional sales prices. The effect of increased tax assessment (which follows the house) on prices likely depends on the propensity of potential buyers to be subject to a wealth tax. Since housing wealth enters at a 75% discount into TNW but debt enters one-for-one, most *new* homeowners will be shielded from the assessment discontinuities, which is why finding no effect on house prices is unsurprising. I discuss this further in Appendix G.6.

It is important to note that even if there is some degree house price capitalization, this would not produce income or wealth effects on top of the income effects associated with facing higher future wealth taxes. This is because the housing wealth effect of price capitalization would only materialize conditional on selling, in which case the standard wealth-tax income effect seizes. Thus, any potential house price capitalization effect simply renders a sale less effective at undoing the tax treatment.

6. Discussion

In this paper, I address an important and long-standing question in economics, namely, how household saving responds 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. My key contribution is to use 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. 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.³⁶ To account for these and other effects, researchers may need to employ a macroeconomic model as in Rotberg and Steinberg (2021), Broer et al. (2021), or Guvenen et al. (2019), or estimate effects at a less-disaggregated (e.g., state) level as Agersnap and Zidar (2020) and Krapf and Staubli (2020) do, and account for effects on asset prices (Mason and Utke 2021; Bjerksund and Schjelderup 2021; Kessel, Tyrefors, and Vestman 2019) and migration (Agrawal et al., 2020).

³⁶Interestingly, however, Boissel and Matray (2021) find evidence consistent with income effects dominating substitution effects in how owner-managers respond to more aggressive dividend taxation, and Bjørneby et al. (2020) find a *positive* effect on employment in firms whose owners are more exposed to the wealth tax.

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.³⁷ While [Jakobsen et al. \(2020\)](#), for example, find strong negative effects on taxable wealth, their wealth measure presumably consists largely of self-reported wealth,³⁸ which facilitates evasion through third-party reporting. In addition, their sample consists of very wealthy households (in the top 1% to 2% of the wealth distribution) who likely have access to better evasion or avoidance technology than the households that provide identifying variation in my setting (i.e., households around the 85th to 90th percentiles). It is further possible that differences in the timing of the reforms we study can explain the different findings: the opportunities to evade wealth taxation has likely declined substantially between the Danish 1988 reform and the 2010 reform that I study.

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 4, 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, 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 et al. \(2018\)](#) in explaining aggregate responses to monetary policy. In addition, my results are driven by older, wealthier households, which suggests that these households may respond in the opposite way to that of a representative agent, highlighting the need to study the behavior of younger,

³⁷This offers an interesting analogy to [Martinez et al. 2021](#) who find a near-zero intertemporal labor supply elasticity for individuals with fewer avoidance opportunities.

³⁸In Denmark, only households in the top 1% to 2% of the wealth distribution paid a wealth tax. Half of these households are business owners and business wealth is self-reported.

constrained households, as in Wong (2019), for whom the cash-flow and housing channels are likely important (Flodén et al. 2019; Hedlund et al. 2017).

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Online Appendices

A. Literature review

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; Nekoei and Seim 2018; Arefeva, Davis, Ghent, and Park 2021; Glogowsky 2021; Lavecchia and Tazhitdinova 2021; Martínez-Toledano 2020; Bjørneby, Markussen, and Røed 2020; Bach, Bozio, Guillouzouic, and Malgouyres 2020a; Tsoutsoura 2015; Korevaar and Koudijs 2023; Micó-Millán 2023; Lefebvre, Lehmann, and Sicsic 2023). Starting with Seim (2017), a central finding is that wealth taxation reduces the amount of taxable wealth that households report (see, e.g., Seim 2017; Zoutman 2018; Brülhart, Gruber, Krapf, and Schmidheiny 2019; 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; Berg and Hebous 2021; and the review by Advani and Tarrant (2021)). However, these findings do not necessarily imply that wealth taxes cause households to save less, as evasion responses may dominate (real) saving responses. Seim (2017) notes that evasion and avoidance responses contaminate the link between behavioral responses and the structural parameters that govern real responses (i.e., the elasticity of intertemporal substitution). Using a bunching design, Seim (2017) documents behavioral responses that are fully attributable to misreporting.³⁹ Zoutman (2018) argues that the immediate responses he observes are unlikely to indicate real adjustments, and Jakobsen et al. (2020) note that their estimated elasticities from Denmark may be a combination of real, avoidance, and evasion responses. While Brülhart, Gruber, Krapf, and Schmidheiny (2021) find a large elasticity of taxable wealth, they also use supplemental survey data that covers a subset of their sample and find no indication that their effects are driven by saving responses. Accordingly, they fully attribute the responses to misreporting (i.e., evasion).

Consistent with the notion that the presence of evasion or avoidance opportunities mask real saving responses, I find strikingly different effects when limiting the role for evasion by (i) using third-party reported data on wealth to infer saving behavior, and by (ii) obtaining identifying variation in wealth tax exposure from below the top 1%, where evasion is less prominent.⁴⁰ In Denmark (Jakobsen et al., 2020), for example, the wealth tax mostly affected households in the top 1% to 2% of the wealth distribution. In Sweden, the wealth tax affected households in the top 3% of the wealth distribution before it was abolished in 2007. Beyond differences in evasion opportunities, it is possible that differences in the timing of the reforms studied in the wealth tax literature can explain the different findings: the opportunities to evade wealth taxation have likely declined substantially between, e.g., the Danish 1988 reform and the 2010 reform that I

³⁹As I elaborate on in Appendix I, the main driver of the bunching responses in Sweden was that households could self-report car values. In Norway, car values are third-party reported, which explains the lower bunching elasticities presented in Appendix I. A complementary DiD design reveals no behavioral effects but does not offer (confidence intervals on the) implied savings elasticities comparable to my results.

⁴⁰Wealth 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., 2020a). Alstadsæter et al. (2019a) show that wealth tax evasion primarily occurs above the 99th percentile of the wealth distribution.

study.

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 income and substitution effects, which is done by estimating both ATR and MTR effects.

By examining real saving responses to (net-of-tax) rate-of-return shocks, I also contribute to the 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](#); [Kinnerud 2021](#)) to the net-of-tax interest rate. Notably, however, [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](#); [Calvet, Campbell, Gomes, and Sodini 2021](#)). 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 \(2020\)](#).

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 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 is also 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 treatment intensity. Yet, there is no established approach that facilitates a graphical representation of the resulting BDD estimates. 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.

B. Data and Empirical Appendix

B.1. Data

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, 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.

A detailed description of the financial data sources can be found in [Fagereng et al. \(2020a\)](#). I provide additional detail regarding the (taxable) wealth variables in section [B.2](#) below.

Employer-Employee data. In order to study the length of employment spells, I use the employer-employee registers (two separate registers for the 2010–2014 and 2015 due to the switch to a new reporting scheme, “Ameldingen”, in 2015).

B.2. *Wealth tax base*

For most households, the key components of taxable net wealth is financial wealth (mostly bank deposits) plus their assessed housing wealth minus debt. Other real assets are included in the wealth tax base as well. For example, the imputed value of automobiles enters automatically as the tax authorities employ the national vehicle register. The value of boats must be self-reported but the values are supposed to correspond to 75% of the insured value. Art typically counts as “housing inventory”, which is self-reported and is subject to its own NOK 100,000 exemption threshold.⁴¹

- 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 (foreign assets) is largely self-reported and includes foreign financial securities (excl. deposits in foreign banks). The average share of foreign assets to GFW is 0.008 (see Table [A.1](#)).
- 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). The mean share of self-reported items to total GFW is 0.027 (see Table [A.1](#)).
- 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.

⁴¹Above this threshold, inventory should be valued at up to 40% of its value, but this is subject to self-reporting. Art that is considered a pure investment should be valued at 100% of its value, but this would also be subject to self-reporting.

- 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), private business assets (TR 4.4.4, 4.6.1)
 - home contents/moveable property typically includes assets such as artwork. This is self-reported, but should equal a fraction (0.1 to 0.4) of the underlying insurance value (if the items are insured). There is an exemption threshold of 1 MNOK on the underlying insurance value).
- TNW (TR 4.9) is TGW minus debt (TR 4.8).
- 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 $(\text{deposits in foreign banks plus other taxable securities abroad})/GFW$.
- The deposit share (Deposits/GFW) is the ratio of deposits to GFW.

B.3. The Hedonic Pricing Model

Using a large national dataset on property transactions during 2004–2009, the hedonic pricing model was estimated according to equation (15) below.⁴²

$$\log \left(\frac{\text{Price}_i}{\text{Size}_i} \right) = \alpha_{R,s} + \gamma_{Z,s} + \zeta_{R,s}^{\text{size}} \log(\text{Size}_i) + \zeta_{R,s}^{\text{Dense}} \text{DenseArea}_i + \zeta_{R,s,a}^{\text{Age}} + \varepsilon_i,$$

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. $\zeta_{R,s,a}^{\text{Age}}$ is an age-bin fixe effect, $a \in \{10-19, 20-34, 45+\}$. As the subscripts indicate, the equation is estimated separately for each of the three structure types, $s \in \{\text{Detached}, \text{Non-detached}, \text{Condominium}\}$, and for each region, R . A region consists either of one or multiple counties or of one of the largest four cities.⁴³ 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.⁴⁴ A price zone may consist of several, potentially disconnected, municipalities. All

⁴²The housing price model used to assess house values at year t would include transactions during $t - 5, \dots, t - 1$. 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.

⁴³For non-detached housing and condominiums, some counties were combined, e.g., Aust-Agder, Vest-Agder and Rogaland. The rationale for this was likely to increase sample size in each regression. The four largest cities are Oslo, Bergen, Trondheim, and Stavanger.

⁴⁴Municipalities 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 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.)

of the estimated coefficients from a total of 44 regressions are provided in regression output form (see Figure A.3 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 (log) tax value of a given residence:

$$\begin{aligned}\log(\widehat{\text{TaxVal}}_i) &= \log(0.25) + \hat{\alpha}_{R,s} + \hat{\gamma}_{R,Z,s} + (1 + \hat{\zeta}_{R,s}^{\text{size}}) \log(\text{Size}_i) \\ &\quad + \hat{\zeta}_{R,s}^{\text{Dense}} \text{DenseArea}_i + \hat{\zeta}_{R,s,a}^{\text{Age}} + 0.5\hat{\sigma}_{R,s}.\end{aligned}\quad (15)$$

where $\exp(0.5\hat{\sigma}_R^2)$ is a concavity adjustment term, with $\hat{\sigma}_{R,s}^2$ being the mean squared error of the regression, and $\log(0.25)$ implements the housing wealth discount in the wealth tax base. This formula outlines two sources of geographic variation that I use for identification. (i) Geographic discontinuities in tax assessment arise at price-zone boundaries, Z , due to cross-boundary differences in $\hat{\gamma}_{R,Z,s}$. This occurs across municipal or district boundaries whenever these coincide with price-zone boundaries. In these cases, even if house prices move smoothly in a geographic sense, the assessment model *imposes* assessment discontinuities. (ii) Geographic discontinuities also arise across price-region boundaries (R). Across these boundaries, the discontinuity is driven by all of the estimated coefficients. The age-bin fixed effects, $\hat{\zeta}_{R,s,a}^{\text{Age}}$, for example, imply that the discontinuity may be smaller or larger at a R boundary depending on the age of the structure. This creates heterogeneous discontinuities in assessments across price-zone boundaries.

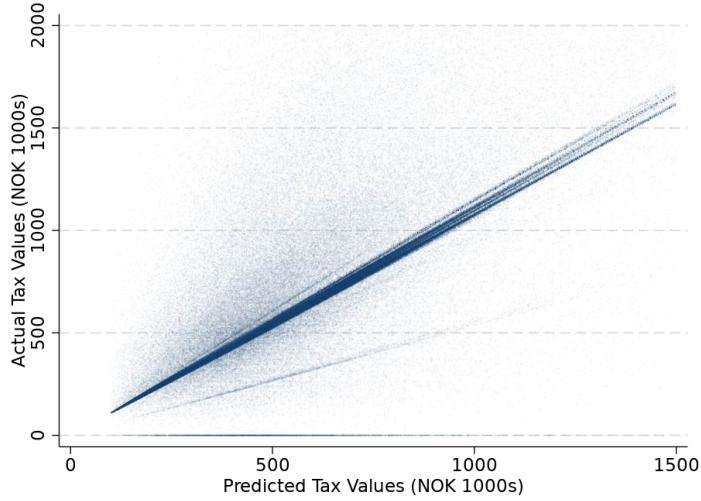
Note that in equation (2) in the main text, the $0.5\hat{\sigma}_{R,s}$ and $\log(0.25)$ terms are omitted for brevity. This should not matter for understanding the empirical variation since we may think of this term as being absorbed by, e.g., the region fixed effects, $\hat{\alpha}_{R,s}$. The fact that estimated housing wealth enters at a discounted rate of 25% is stated in the preceding section 2.1

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 Appendix G.4.

In Figure A.1 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.

FIGURE A.1: VERIFYING THE HOUSE PRICE MODEL COEFFICIENTS

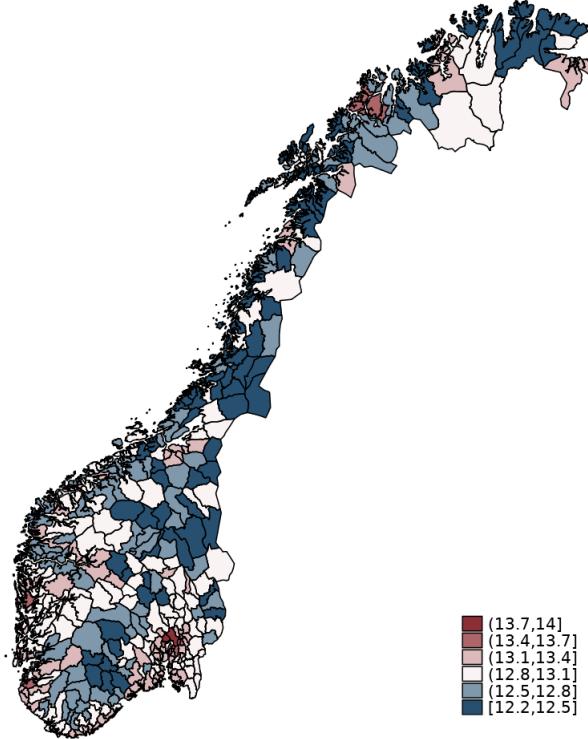
This figure plots actual assessed tax values against tax values predicted using the real estate data and coefficients from the hedonic pricing model. The Y-axis has the actual tax values that are retrieved from individuals' tax returns for 2010, presumably based on the coefficients from the model estimated 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.



The actual tax values may differ from predicted tax values for a few reasons. First, the coefficients I use are based on estimating equation (15) on 2004–2008 data. I use these coefficient for all years of my sample (and hence $\widehat{\text{TaxVal}}_i$ does not have a t subscript.) 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 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, $\widehat{\text{TaxVal}}$, only considering primary residences. This leads to a few cases in which $\text{TaxVal} > \widehat{\text{TaxVal}}$. 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 $\widehat{\text{TaxVal}}$ 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: MODEL-IMPLIED GEOGRAPHIC VARIATION IN TAX ASSESSMENT FOR A STANDARD HOUSE

This figure shows the logarithm of the 2010 assessed tax value of a detached 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 (2). Each distinct (shade of) color corresponds to a bin of $\widehat{\text{TaxVal}}$ 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 resulting variation may be very different for other housing types (e.g., townhomes or apartments) due to larger (multiple-municipality) price zones and (multiple-county) price regions.



B.4. Definition of variables

B.4.1. Municipality-level control variables

\mathbf{M}_m , used in equation (4), contains variables that vary at the municipality–district–border area level. The district distinction only matters for the largest four cities (Oslo, Bergen, Trondheim, Stavanger).

Residualized log transaction prices. These enter as a control variable inside the \mathbf{M}_m vector in the main reduced-form regression equation (4). I first estimate the regression equation

$$\log(\text{TransactionPrice}_{i,t}) = \tilde{\delta}'_b \mathbf{H}_i + \epsilon_{i,t} \quad \text{for } t = 2009, \quad (16)$$

and then average the residuals, $\hat{\epsilon}_{i,t}$ at the municipality \times district \times border area level (b). I *residualize* the past transaction prices to remove any variation driven by the characteristics of the housing stock (such as age and size) contained in the vector \mathbf{H}_i (which is also used in the hedonic pricing model). This approach thus measures average *prices* in a similar way to what

the hedonic pricing model does, but without the peculiarities inherent in the hedonic pricing models grouping of municipalities into price zones for the purpose of estimating fixed price-zone as opposed to municipality-level fixed effects (see discussion in section 2.3).

Pre-form TaxVal. This variable, measured in 2009, is averaged at the municipality level, m , after taking logs. This is included as a control variable due to likely municipality-level discretion in adjusting pre-reform tax assessment. While, in principle, pre-reform tax assessments should depend only on historical construction cost, these values were recorded at municipality-specific tax offices, which could allow for differences in assessments across boundaries.

Pre-reform GFW. Gross financial wealth in 2009 is similarly averaged at the municipality level after taking logs.

B.4.2. Defining outcome variables.

Total Saving and Marketable Wealth.

$$\text{Saving in Housing}_{i,t} = \sum_{s=2010}^t \text{Net Housing Transactions}_{i,s} \cdot \Pi_{q=s}^t (1 + \pi_{i,q}), \quad (17)$$

where $\text{Net Housing Transactions}_{i,s}$ is net transactions in real-estate during year s (that is, if you sold a house for 1MNOK and bought another for 0.5MNOK, net transactions is 0.5 MNOK) and $\pi_{i,q}$ is the local house-price appreciation in year q (Fagereng, Holm, and Torstensen, 2020b). To measure local house-price appreciation, I use the same index as in Fagereng, Holm, Moll, and Natvik. The saving measure is divided by $W_{i,t-1}$, which is marketable net wealth:

$$W_{i,t} = GFW_{i,t} - Debt_{i,t} + Housing_{i,t}, \quad (18)$$

where GFW is the standard measure of gross financial wealth used in the paper. $Housing_{i,t}$ is measured using the valuations from the OLS methodology in Fagereng, Holm, and Torstensen (2020b). To ensure that $W_{i,t}$ is itself not affected by any assessment discontinuities (I want the saving effect to be caused by behavioral responses in the numerator, not assessment discontinuities in the denominator), I take out border area plus “side of border” fixed effects, and add only the border area fixed effects back in.

$$\text{Total Saving}_{i,t} = \text{Saving in Housing}_{i,t} + \Delta GFW_{i,t} - \Delta Debt + wtax_{i,t-1}, \quad (19)$$

which is typically also scaled by $W_{i,t-1}$. Following the scaling, the variable is bounded (not trimmed) to lie inside $[-1, 1]$

Ratios of Saving to Income. Consider some saving measure, ΔX , for example equal to ΔGFW (gross financial saving). To ensure that the ratio of saving to income is always well defined and does not tend to infinity as income approaches zero, I make a two adjustments in the denominator:

$$\text{Ratio of } \Delta X \text{ to Income} \equiv \frac{\Delta X}{\max(Income(1 - \min(\max(\frac{Taxes}{Income}, 0), 0.5), 10000 \cdot 1.02^{t-2010} + abs(\Delta X)), 1)}, \quad (20)$$

where *Income* is a broad measure of labor income (labor earning from salaried or wage employment, plus positive self-employment income, social transfers, unemployment or disability benefits, and pension income.) 1.02 is used to inflate the minimum imposed income level of NOK 10,000 for years after 2010. The definition also limits total tax payments to 50% of income. The measure is strictly bounded by ± 1 .

Adjusted Net Financial Saving. is the sum of gross financial saving and (the negative of) changes in debt, both divided by lagged gross financial wealth.

$$\frac{ANFS_{i,t}}{GFW_{i,t-1}} = \frac{\Delta GFW_{i,t}}{GFW_{i,t-1} + wtax_{i,t-1}} - \frac{\Delta Debt_{i,t}}{GFW_{i,t-1}}, \quad (21)$$

where both fractions are bounded to lie inside $[-1, 1]$.

Adjusting for mechanical effect of paying more in wealth taxes. My main approach is to treat wealth tax payments as a saving flow. Since these payments occur one year after they're accrued, I use $wtax_{i,t-1}$. A secondary approach is to also adjust for lost returns on past wealth tax payments. To calculate the lost returns, I assume that households earn a 2% return on deposits and a 7% return on stocks (i.e., I assume that the CAPM applies, and the portfolio beta is 1 and the equity risk premium is 5%). Hence, the return for some household i during year t is set to equal $0.02 + \frac{SMW_{i,t-1}}{GFW_{i,t-1}} \cdot 0.05$. In Figure B.3, I contrast the effect on gross financial saving when making (A) no adjustment; (2) the main adjustment; and (C) the adjustment that accounts for lost returns on past wealth taxes. When considering cumulative effects in Figure B.9, I account for lost returns on past payments.

B.5. Variable Definitions for Cumulative Effects on Saving and TNW

More specifically, for some outcome, y , I consider the effect on

$$\log(\tilde{y}_t) - \log(y_{2009}).$$

For net financial saving, $y_{2009} = GFW_{2009}$. \tilde{y}_t contains y_{2009} , cumulative increases in GFW , the negative of cumulative debt increases, and cumulative wealth tax payments (and any returns these would have had). Cumulative increases in debt are bounded to GFW_{2009} . For total saving, $y_{2009} = W_{2009}$. \tilde{y}_t contains y_{2009} , cumulative increases in GFW , the negative of cumulative increases in debt, cumulative wealth tax payments (incl. returns), and also contains cumulative net housing transactions and associated capital gains. Cumulative increases in debt are bounded to W_{2009} . For TNW , $\tilde{y}_t = y_t = TNW_t$. All log arguments are shifted by an inflation adjusted NOK 10,000 ($10,000 \cdot 1.02^{t-2010}$).

B.6. 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 urban 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, where the slope of house prices will be steeper. I provide a graphical example of the issue in Figure A.4 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.

Adjusted border distance. To account for heterogeneity in density, I implement the following approach: I first calculate the standard deviation of border distances at the boundary level, b . Then each distance in km, d_i^{KM} is scaled by this standard deviation, SD_b . Then, I calculate the mean standard deviation (SD_b) at the individual level, \overline{SD}_b . This effectively weights SD_b s by the number of observations in that boundary area when taking means. I use \bar{SD}_b to scale distances back up, so that they, roughly, on average correspond to a distance in kilometers:

$$\text{Adjusted } d_i = \frac{d_i}{SD_b} \cdot \overline{SD}_b. \quad (22)$$

B.7. Log shifting

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). 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.

B.8. 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 $\mathbf{1}[d_i > 0]\Delta_i$ in equation (4) could be replaced with $\log(\widehat{\text{TaxVal}})$ 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 representation of estimates.⁴⁵

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 (4) 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_i 's 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 B, I provide examples of how geographic heterogeneity in residential density may invite the false detection of discontinuities in observable characteristics.

B.9. Boundary Discontinuity Design Robustness Checks

Bandwidth. My main bandwidth choice in the BDD regressions is 10 km. The extent to which the estimates of the discontinuity depends on the choice of bandwidth can largely be gleaned from the figures. For the labor earnings results, for example, we see that if we substantially reduce the bandwidth, then the estimated discontinuities are likely to be larger. I verify this graphical intuition in Appendix Table B.2 by reporting discontinuity estimates for different bandwidths. Indeed, the estimated effects on net financial saving, total net saving, and labor earnings all become larger as the bandwidth is reduced. This table, however, shows that this comes with a cost of a considerable loss of precision: The standard errors roughly double when reducing the bandwidth from 10 km to 2 km. Therefore, the confidence intervals of the 2

⁴⁵Prior 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.

km estimates contain the baseline point estimates.

Using unadjusted distance in km. My main distance measure is effectively normalized across boundary areas. The raw distance measure is divided by the border area's standard deviation of distance measures, and then it is scaled up by the mean standard deviation. The purpose of this is to allow us to more reasonably assume linear geographic trends which aids precision. In Panel B of Appendix Table B.2, I report the main point estimates when using unadjusted distance in kilometers. We see that the qualitative findings are robust. For financial saving, the standard errors are somewhat larger (for the 10 km bandwidth, they are about 33% larger) and the point estimates slightly smaller (about 17% when the bandwidth is 10 km). For labor earnings growth, both standard errors and point estimates are larger. For total saving, there is an indication of a positive effect in my preferred specification, but we do not see this when using unadjusted distance for most bandwidth choices. However, when we zoom in on households close to the boundary, we still find a significant positive effect on total saving using either adjusted or unadjusted distance.

B.10. IV methodology

The reduced-form regression equation presented in the main text, and used for the reduced-form graphical evidence, is the following:

$$y_{i,t} = \underbrace{\beta 1[d_i > 0]\Delta_i}_{\text{Discontinuity}} + \underbrace{\gamma^- d_i 1[d_i < 0]\Delta_i + \gamma^+ d_i 1[d_i > 0]\Delta_i}_{\text{Geographic controls}} + \delta'_{b,s} \mathbf{H}_i + \rho'_t \mathbf{M}_m + \Gamma'_t \mathbf{X}_i + \varepsilon_{i,t}. \quad (23)$$

I exploit first stage heterogeneity in the IV approach by estimating the following first-stage regression.

$$x_{i,t} = \sum_f D_{i,f} [\beta_f 1[d_i > 0]\Delta_i + \gamma_f^- d_i 1[d_i < 0]\Delta_i + \gamma_f^+ d_i 1[d_i > 0]\Delta_i + \xi_f] \quad (24)$$

$$+ \delta'_{b,s} \mathbf{H}_i + \rho'_t \mathbf{M}_m + \Gamma'_t \mathbf{X}_i + \varepsilon_{i,t}, \quad (25)$$

for $x_{i,t} \in \{MTR_{i,t}, ATR_{i,t}\}$. $D_{i,f}$ takes the value 1 if i is in wealth bin f (measured as of 2009 and defined in Table B.1). Note that the estimated discontinuities and geographic slopes can now vary with the wealth bin, f . The second stage equation then becomes

$$y_{i,t} = \beta^{ATR} \widehat{ATR}_{i,t} + \beta^{MTR} \widehat{MTR}_{i,t} + \sum_f D_{i,f} [\tilde{\gamma}_f^- d_i 1[d_i < 0]\Delta_i + \tilde{\gamma}_f^+ d_i 1[d_i > 0]\Delta_i + \tilde{\xi}_f] \quad (26)$$

$$+ \tilde{\delta}'_{b,s} \mathbf{H}_i + \tilde{\rho}'_t \mathbf{M}_m + \tilde{\Gamma}'_t \mathbf{X}_i + \tilde{\varepsilon}_{i,t}. \quad (27)$$

Note that wealth bin fixed effects, ξ_f , are always included. Hence, this methodology exploits variation from differential first-stage and reduced-form effects across wealth bins, but does not exploit wealth by itself as a source of identifying variation.

B.11. Additional figures

FIGURE A.3: 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 $\sigma_R = 0.28800$.

Notater 39/2010		Reestimering av modell for beregning av boligformue					
Eneboliger i Aust-Agder 170							
The REG Procedure Model: MODELL1 Dependent Variable: lnkvmp pris							
Number of Observations Read		4196					
Number of Observations Used		4196					
<i>Analysis of Variance</i>							
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						
<i>Parameter Estimates</i>							
Variable	DF	Parameter Estimate	Standard Error	t Value			
				Pr > t			
Intercept	1	11.83711	0.07555	156.68			
lnareal	1	-0.38555	0.01447	-26.65			
tett	1	0.06373	0.01197	5.33			
aar1	1	-0.43694	0.01599	-27.33			
aar2	1	-0.36403	0.01544	-23.57			
aar3	1	-0.26966	0.01494	-18.05			
aar4	1	-0.09556	0.01496	-6.39			
aar5	1	-0.05457	0.01537	-3.55			
alder2	1	-0.09434	0.01954	-4.83			
alder3	1	-0.21329	0.01615	-13.21			
alder4	1	-0.21287	0.01475	-14.43			
sone2	1	-0.15054	0.01850	-8.14			
sone3	1	-0.26613	0.02581	-10.31			
sone4	1	-0.24332	0.01699	-14.32			
sone5	1	-0.36361	0.02706	-13.44			
sone6	1	-0.43013	0.02127	-20.23			
sone7	1	-0.72255	0.02323	-31.10			

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FIGURE A.4: EXAMPLE OF NONLINEARITIES WHEN POOLING
BOUNDARY AREAS

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 $200 \cdot b \cdot \frac{\sqrt{12}}{2}$ (in meters). Each b has 1000 households, equidistantly populated. The standard deviation of border distances is equal to $100b$. Within each b , house prices move linearly according to their border distance in km, k . Price is defined as $p = \frac{\Delta}{100}$. 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 k , separately for $b = 10, 25, 50, 75, 100$. In the second plot, I provide 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.

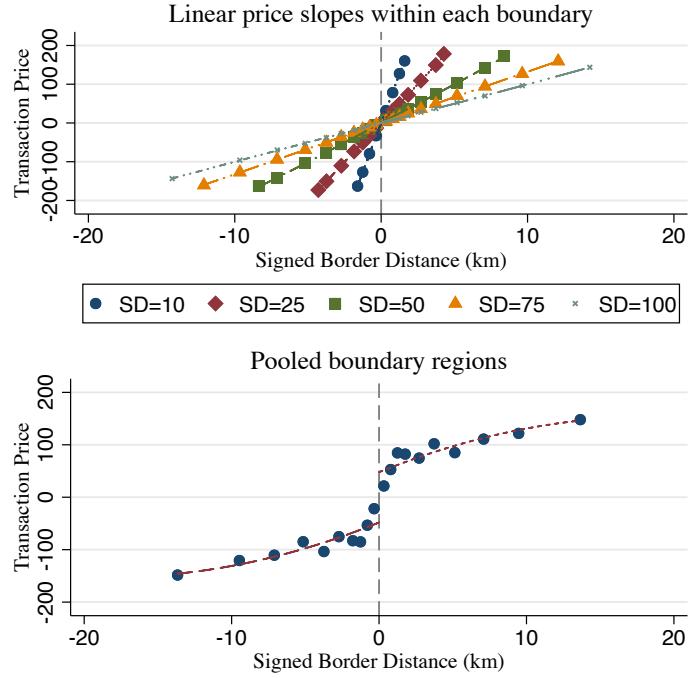


TABLE A.1: SUMMARY STATISTICS

This table provides summary statistics for the households in the main sample (i.e., $TNW_{i,2009} > 0$). GFW is Gross Financial Wealth. GFW , *censored* indicates that GFW or W is winsorized (*censored*) at the 95th percentile. W is marketable wealth: GFW plus (undiscounted) housing wealth minus debt. 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. Average tax rates (ATR) equal wealth taxes divided by either W , TNW , or GFW . When divided by GFW , the ratio is censored at 10 times the wealth tax rate. 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 B.2.

	N	mean	sd	p25	p50	p75
2010–2015, NOK 1000s						
GFW, censored at 95%	1470536	1044	1155	239	605	1375
W, censored at 95%	1470536	6650	4306	3474	5321	8531
GFW	1470536	1227	2405	239	605	1375
Debt	1470536	537	1087	0	158	650
W	1444073	6867	6055	3444	5241	8275
TGW	1440206	2359	2891	1018	1615	2706
TNW	1440206	1814	2665	622	1230	2237
T. Taxable L. Income	1472161	723	534	395	602	920
Labor Earnings	1472161	456	618	0	242	779
SMW	1470536	163	594	0	0	92
Deposits	1470536	745	1017	162	421	940
TaxVal	1443962	870	729	498	691	1010
wtax	1466523	9	24	0	0	8
2004–2009, NOK 1000s						
TaxVal	1593493	416	227	263	377	525
2010–2015						
SMW/GFW	1463891	0.111	0.204	0.000	0.000	0.127
RiskyShare	1463891	0.169	0.269	0.000	0.010	0.238
Deposits/GFW	1463891	0.797	0.294	0.673	0.972	1.000
Foreign/GFW	1463891	0.008	0.056	0.000	0.000	0.000
Self-rep/GFW	1463891	0.027	0.109	0.000	0.000	0.000
wtax>0	1466523	0.471	0.499	0.000	0.000	1.000
ATR ^{TNW}	1440191	0.0022	0.0030	0.0000	0.0000	0.0044
MTR	1466523	0.0050	0.0053	0.0000	0.0000	0.0110
ATR ^W	1442773	0.00087	0.001426	0.0000	0.0000	0.001318
ATR ^{GFW}	1459292	0.0041	0.0058	0.0000	0.0000	0.0076
r, Deposits (pp.)	1267035	1.97	1.06	1.20	2.06	2.66
r, Debt (pp.)	773825	4.00	1.53	3.37	4.04	4.74
Avg. age in 2009	1474110	61.16	12	52	61	70
log(GFW)	1470536	13.23	1.35	12.43	13.33	14.14
log(Labor Earnings)	1472161	11.66	2.04	9.31	12.44	13.58
1[GFW<50,000]	1474110	0.0659				
1[wtax>0.25*GFW]	1466523	0.0020				

C. Supplementary Figures

FIGURE B.1: TRANSACTION PRICES DURING 2008–09

This figure examines whether there are discontinuities in past transaction prices during 2008 and 2009. See main text Figure 1 for additional details and results on pooled 2005–2009 transactions.

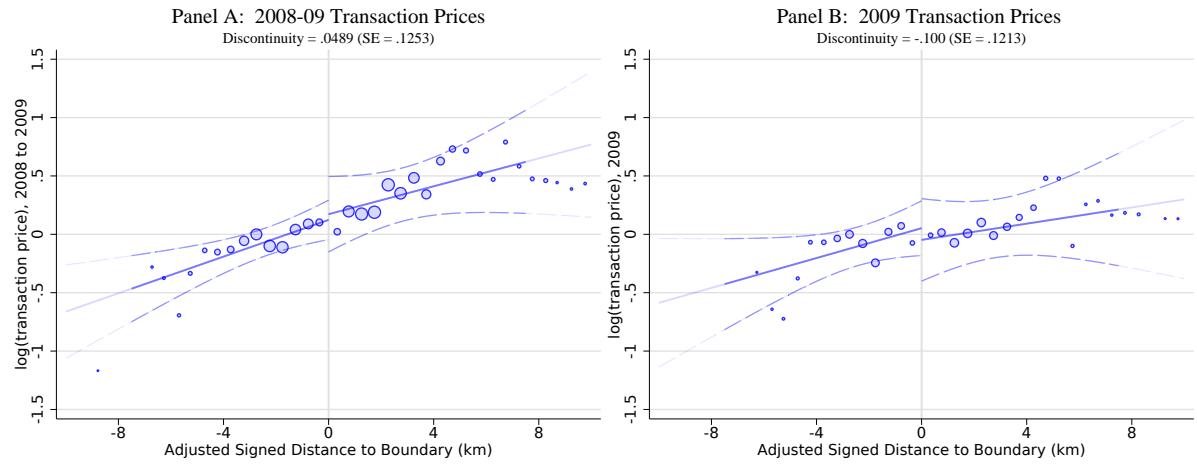


FIGURE B.2: THE (WEAK) CORRELATION BETWEEN CROSS-BORDER DIFFERENCES IN PAST TRANSACTION PRICES AND ASSESSMENT DISCONTINUITIES

This scatter plot shows the relationship between tax assessment discontinuities (Δ_i) on the y-axis and cross-border differences in past mean transaction prices on the x-axis. Past transaction prices are first residualized with respect to \mathbf{H}_i (from the hedonic pricing model), allowing the coefficients to vary at the border-area level. They are then averaged separately on either side of the geographic boundary. A given datapoint contains the cross-border difference of these averaged residuals on the x-axis and that household's Δ_i on the y-axis. The plot is limited to one randomly-selected household at the border-area (b) \times structure type (s) level. To preserve anonymity, the y-axis values have an error term $\tilde{N}(0, 2\%)$ added when $\Delta_i \neq 0$. The gray line is a linear fit on the underlying (no-noise) datapoints.

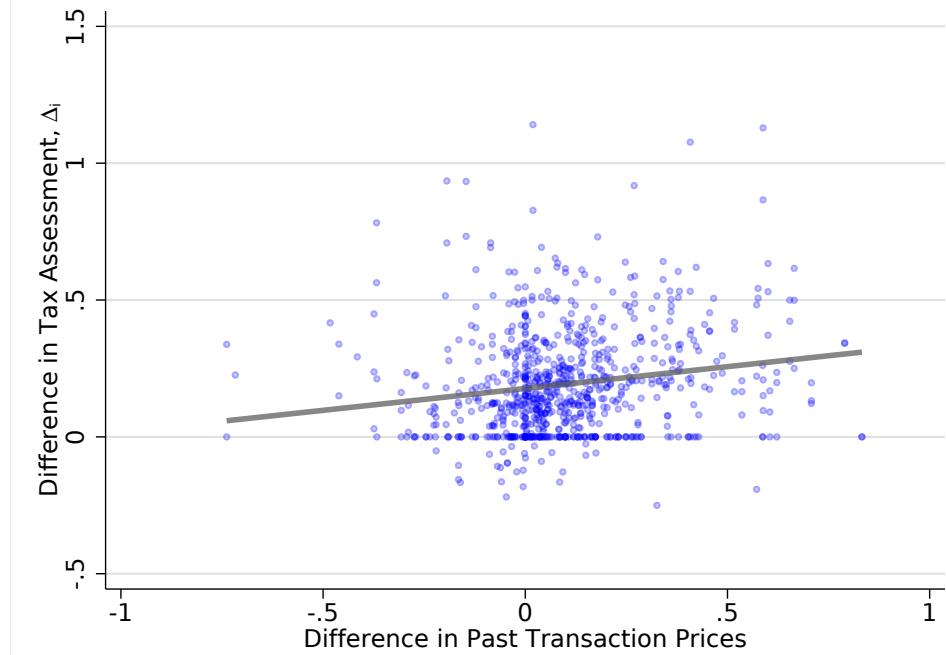


FIGURE B.3: FINANCIAL SAVING AND ADJUSTMENTS FOR MECHANICAL EFFECT OF WEALTH TAXES

This figure shows how the main results on gross financial saving depend on how the mechanical effects of wealth taxation are adjusted for. My main approach (middle panel) is to treat wealth tax payments as a saving flow. Since these payments occur one year after they're accrued, I use $wtax_{i,t-1}$. A secondary approach (third panel) is to also adjust for lost returns on past wealth tax payments. To calculate the lost returns, I assume that households earn a 2% return on deposits and a 7% return on stocks (i.e., I assume that the CAPM applies, and the portfolio beta is 1 and the equity risk premium is 5%). Hence, the return for some household i during year t is set to equal $0.02 + (SMW_{i,t-1}/GFW_{i,t-1}) \cdot 0.05$.

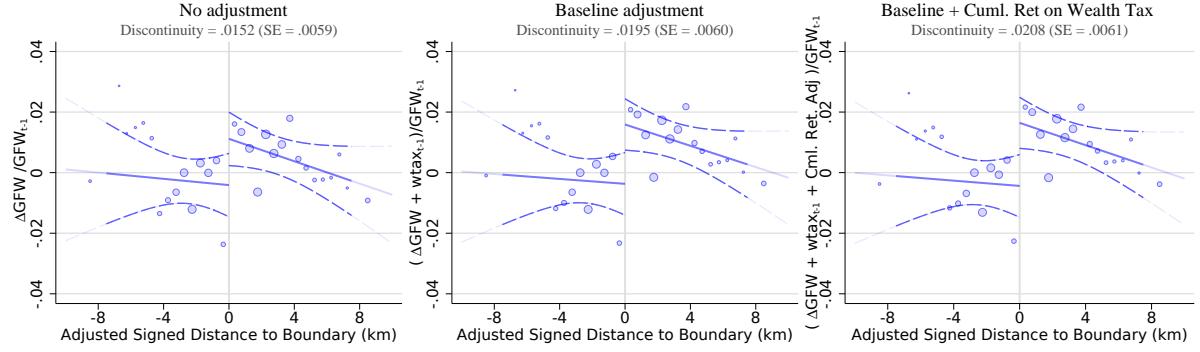


FIGURE B.4: STOCK MARKET WEALTH

The outcome variable is log-differenced stock market wealth (listed stocks plus mutual funds). The log-argument is shifted to accommodate zeros (discussed in manuscript).

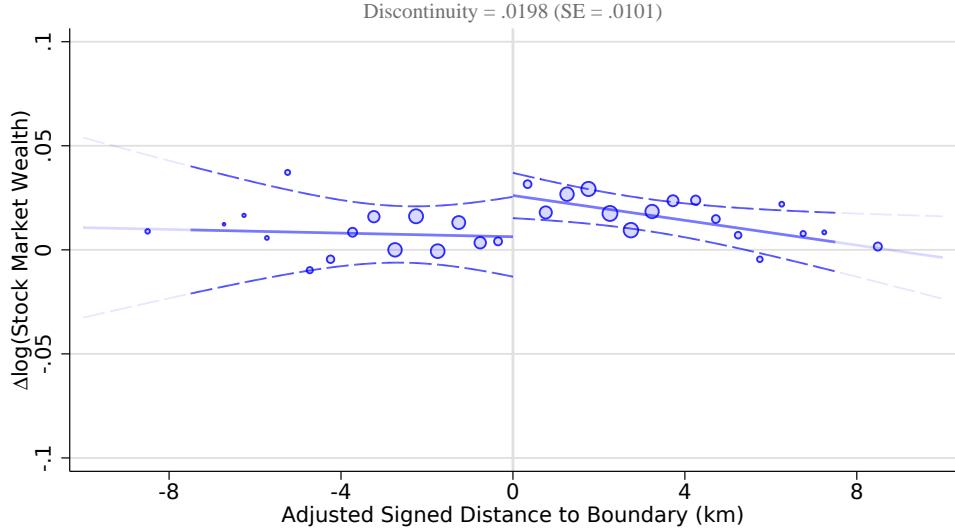


FIGURE B.5: EFFECT ON DEBT: PRE-PERIOD AND POST-PERIOD

This panel considers log-differenced debt as the outcome variable. Panel A considers the pre-period (2005–2009) and Panel B considers the post-period (2010–2015).

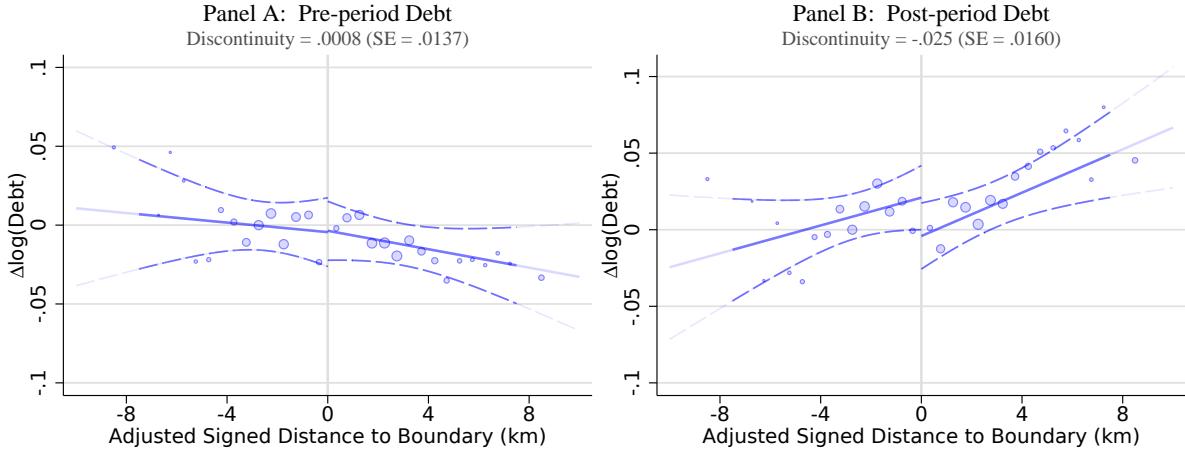


FIGURE B.6: LABOR EARNINGS GROWTH IN PRE-PERIOD

This panel considers labor earnings and employment duration during the pre-period (2005–2009) as opposed to the post-period (2010–2015). The post-period results are reported in main text Figure 5.

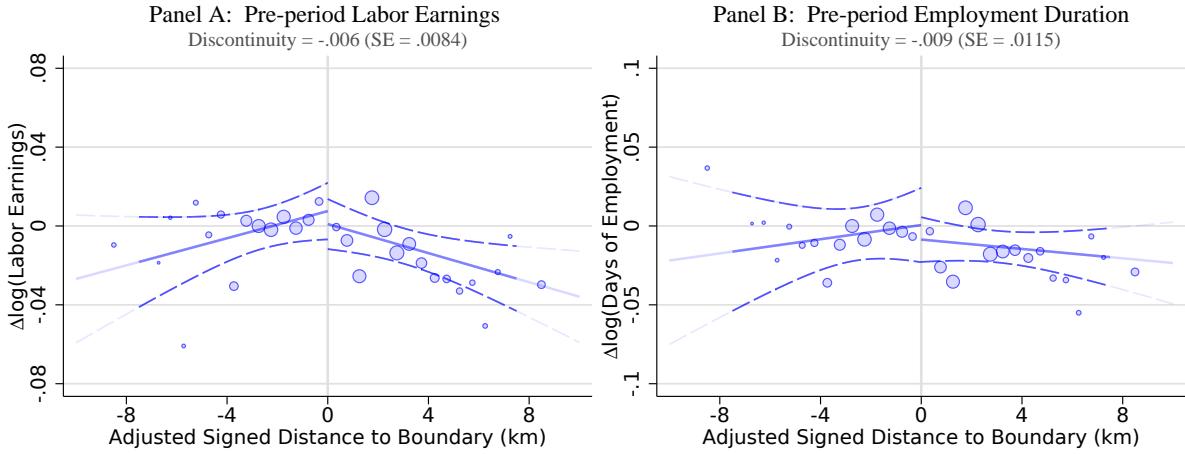


FIGURE B.7: FINANCIAL SAVING SCALED BY INCOME

This figure provides estimated effect on various saving when changes in saving are scaled by income instead of being measured as growth rates. See Appendix B.4 for variable definitions.

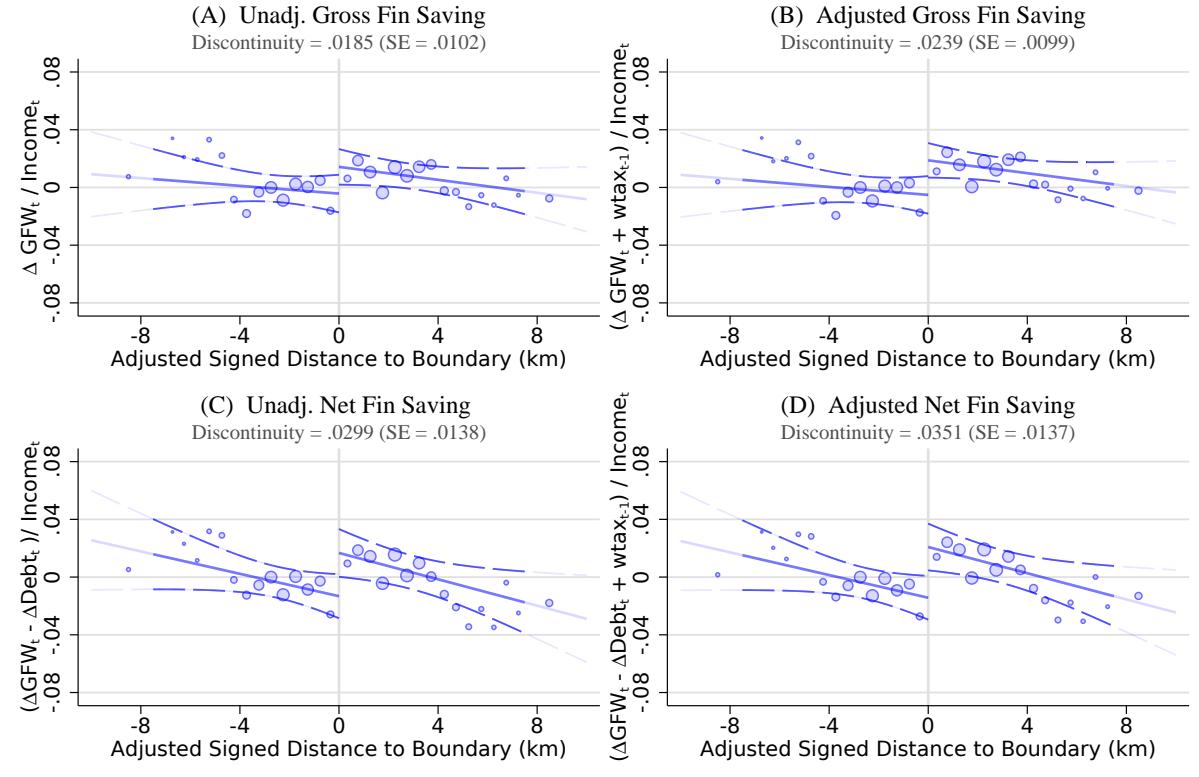


FIGURE B.8: CHANGE IN TAX ASSESSMENTS FROM 2009 TO 2010

The main-text Figure 1 shows how pre-reform (2009) and post-reform (2010) tax assessments vary across the boundary. This figure plots the *change* in tax assessments ($\log(TaxVal_{i,2010}) - \log(TaxVal_{i,2009})$). See Figure 1 for the methodology.

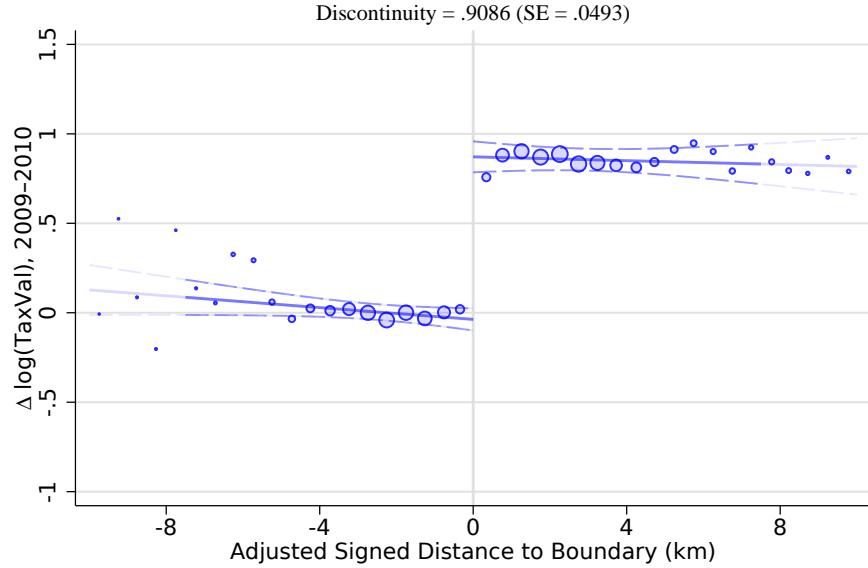


FIGURE B.9: CUMULATIVE EFFECTS ON NET FINANCIAL SAVING, MARKETABLE WEALTH, AND TAXABLE NET WEALTH

This figure provides the estimated cumulative effects on net financial saving (blue squares), total saving (green circles), and taxable net wealth (hollow black circles). To limit the influence of outliers in cumulative saving measures, I use a log-differencing approach. The blue points tell us the cumulative log increase in gross financial wealth when (i) adjusting for the mechanical effects of wealth taxes and (ii) treating reductions in debt as increases in GFW . The green points tell us the cumulative log increase in marketable wealth, W , when allowing increases to come from increases in GFW , reductions in $Debt$, and net housing transactions. Here, I also adjust for the mechanical effects of wealth taxes. For these analyses, the mechanical effects adjustment accounts for lost returns on past wealth tax payments. For taxable net wealth (TNW), I consider the log difference and do not adjust for the mechanical effects. Appendix B.5 further describes the variable construction. In estimating the cumulative effects, I use a more flexible version of equation (4) to estimate the discontinuities (plotted in the figure). Coefficients on $1[d_i > 0]\Delta_i$ and \mathbf{H} can now vary by year, and I allow for a linear time trend in the geographic slopes by including the terms $\gamma^{-*}d_i1[d_i < 0]\Delta_i(t - 2010) + \gamma^{+*}d_i1[d_i > 0]\Delta_i(t - 2010)$.

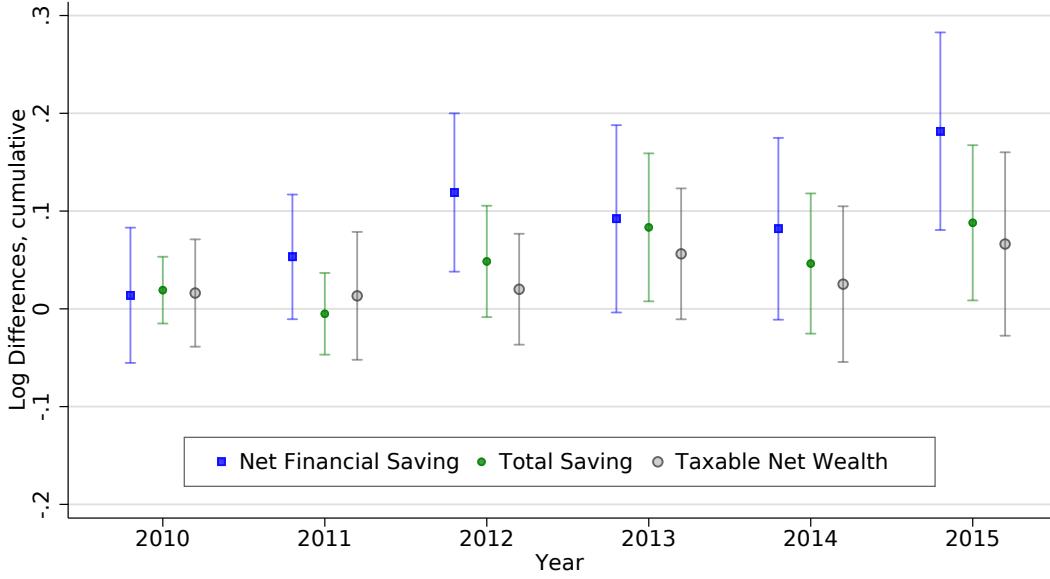


FIGURE B.10: DIFFERENCES-IN-DIFFERENCES REGRESSION DISCONTINUITY DESIGN

This figure provides the estimated effect on gross financial saving when using a differences-in-differences (DID) RDD setup. That is, I now also include the years 2005–2009 in the sample and allow all estimated coefficients to vary by period, $p \in \{2005–2009, 2010–2015\}$. I then report the results on the interaction between the geographic variables ($\mathbb{1}[d_i > 0], d_i \mathbb{1}[d_i < 0], d_i \mathbb{1}[d_i > 0]\}$) and a dummy for whether $t \geq 2010$. From the regression equation below, the discontinuity in saving behavior is given by β .

$$\begin{aligned} y_{i,t} = & \beta_1 [t \geq 2010] \mathbb{1}[d_i > 0] \Delta_i + \beta_* \mathbb{1}[d_i > 0] \Delta_i \\ & + \gamma^-_* d_i \mathbb{1}[t \geq 2010] \mathbb{1}[d_i < 0] \Delta_i + \gamma^+_* d_i \mathbb{1}[t \geq 2010] \mathbb{1}[d_i > 0] \Delta_i \\ & + \gamma^-_* d_i \mathbb{1}[d_i < 0] \Delta_i + \gamma^+_* d_i \mathbb{1}[d_i > 0] \Delta_i + \delta'_{b,s,p} \mathbf{H}_i + \rho'_t \mathbf{M}_m + \Gamma'_t \mathbf{X}_i + \varepsilon_{i,t}. \end{aligned}$$

See main-text Figure 3 for the baseline results that consider pre-period and post-period discontinuities separately.

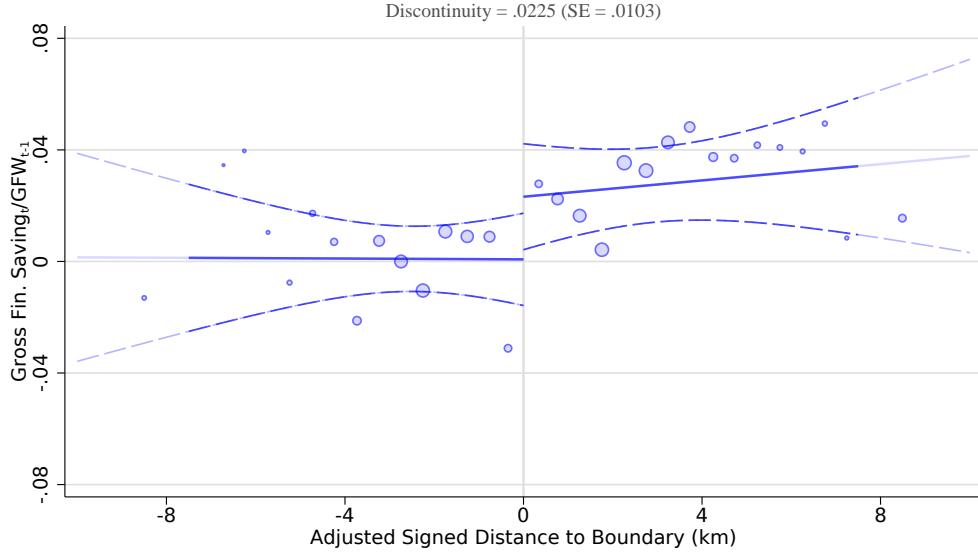


FIGURE B.11: ROBUSTNESS TEST: GROSS FINANCIAL SAVING AMONG NONSELLERS

This figure considers gross financial saving during 2010–2015 (as in Panel B of main-text Figure 3) in the subset of households who do not sell their house during 2010–2015. See main-text Figure 3 for other details.

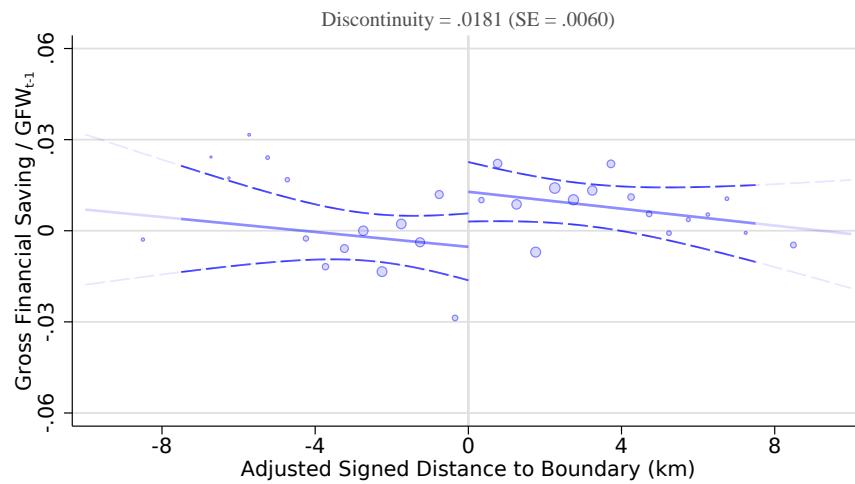
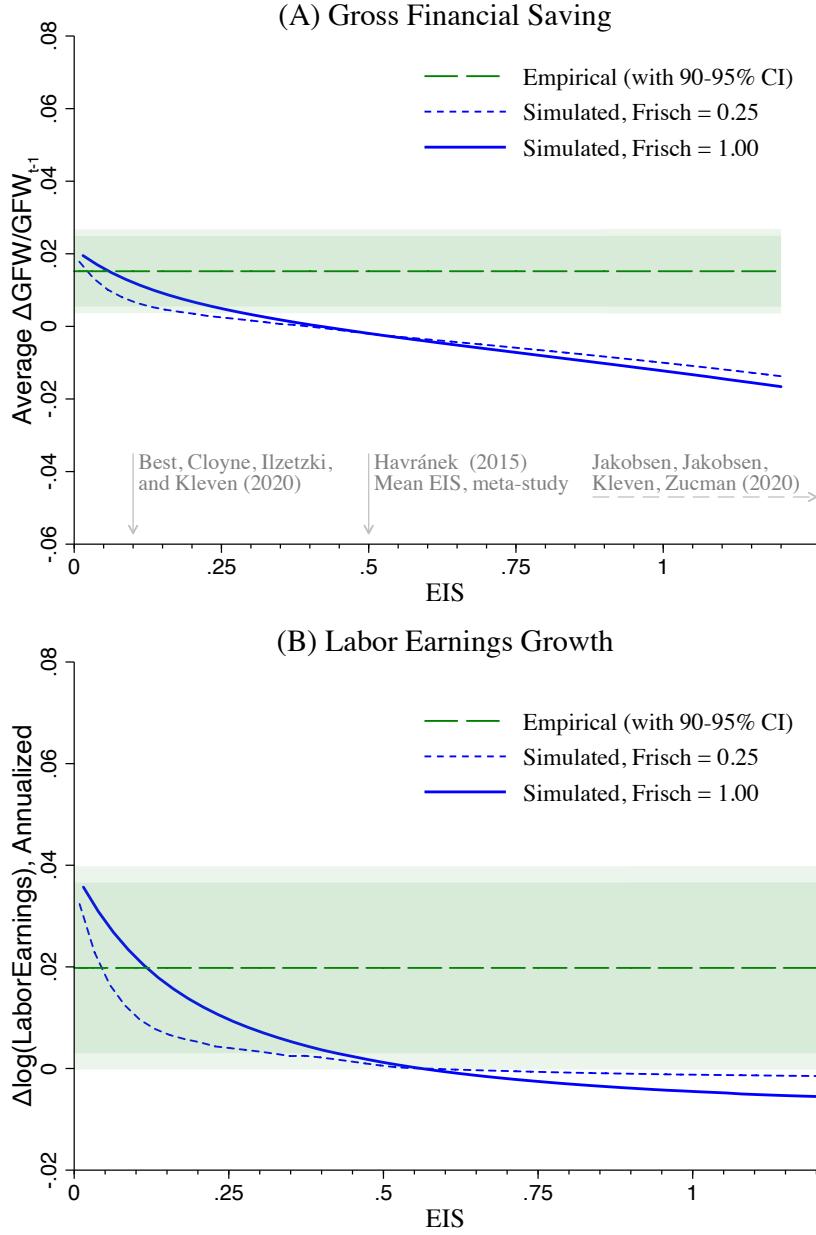


FIGURE B.12: SIMULATED TREATMENT EFFECTS AS A FUNCTION OF THE EIS FOR DIFFERENT VALUES OF THE FRISCH ELASTICITY

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 1. Panel A considers the effect on gross financial saving, without the wealth tax adjustment, where the empirical point estimate comes from Panel A of Figure B.3. Panel B considers labor earnings growth, where the point estimate comes from Panel A of Figure 5. 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. Simulated effects are smoothed by using a local 5th-order polynomial fit.



D. Appendix Tables

TABLE B.1: UNDERLYING FIRST-STAGE AND REDUCED-FORM
REGRESSION COEFFICIENTS FOR THE ATR–MTR EFFECT DECOMPOSITION

This table reports the underlying first-stage and reduced-form coefficients from our instrumental variables analysis. Each coefficient provides the estimated effect of the assessment discontinuity ($1[d_i > 0]\Delta_i$) for a subset of households. $\widehat{TNW}_{i,2009}$ is the TNW that household i would have had in 2009 if their house had been assessed with the average assessment rules in their border area: I assess their house as if it were on the low-assessment side and again as if it were on the high assessment side and then take the average. T_t is the threshold in year t . n_i is the number of (married) adults in the households. The idea is that households with, e.g., $(\widehat{TNW}_{i,2009}/n_i - T_t)$ near 0 will be close to the threshold in year t , and thus the assessment discontinuity will have a large effect on their marginal tax rate (MTR). Standard errors are provided in parentheses.

	First-stage			Reduced-form		
	ATR^{GFW} (1)	ATR^W (2)	MTR (3)	$\frac{ANFS}{GFW_{t-1}}$	$\frac{ATNS}{W_{t-1}}$	$\Delta \log(LaborEarnings)$
$1[d_i > 0]\Delta_i$						
$\times(\widehat{TNW}_{i,2009}/n_i - T_t)$						
$\in [-1.00M, -0.50M]$	0.0027 (0.0006)	0.0001 (0.0001)	0.0012 (0.0002)	0.0055 (0.0171)	0.0055 (0.0171)	-0.0011 (0.0031)
$\in [-0.50M, -0.25M]$	0.0034 (0.0006)	0.0001 (0.0001)	0.0022 (0.0004)	0.0122 (0.0233)	0.0122 (0.0233)	0.0015 (0.0029)
$\in [-0.25M, 0)$	0.0075 (0.0012)	0.0005 (0.0001)	0.0051 (0.0004)	0.0322 (0.0226)	0.0322 (0.0226)	0.0058 (0.0030)
$\in [0, 0.25M)$	0.0070 (0.0014)	0.0008 (0.0001)	0.0025 (0.0002)	0.0243 (0.0231)	0.0243 (0.0231)	0.0029 (0.0035)
$\in [0.25M, 0.50M)$	0.0064 (0.0020)	0.0008 (0.0001)	0.0018 (0.0003)	0.0355 (0.0206)	0.0355 (0.0206)	0.0034 (0.0041)
$\in [0.50M, 1.00M)$	0.0038 (0.0009)	0.0010 (0.0002)	0.0012 (0.0003)	0.0077 (0.0171)	0.0077 (0.0171)	0.0073 (0.0046)
$\in [1.00M, 6.0M]$	0.0053 (0.0008)	0.0011 (0.0002)	0.0011 (0.0003)	0.0393 (0.0233)	0.0393 (0.0233)	0.0042 (0.0045)
N	1669285	1669285	1669285	1669285	1669285	1669285

TABLE B.2: BANDWIDTH AND DISTANCE MEASURE ROBUSTNESS

This table estimates the reduced-form effect of higher tax assessments on saving using different bandwidths. A bandwidth of, e.g., 6 km, implies that only observations with $d_i \in [-6, 6]$ is included in the sample. Columns (1)-(3) uses the baseline normalized distance measure: distance in kilometers divided by the boundary-area standard deviation of distances and then scaled up again by the cross-boundary area mean of these standard deviations.

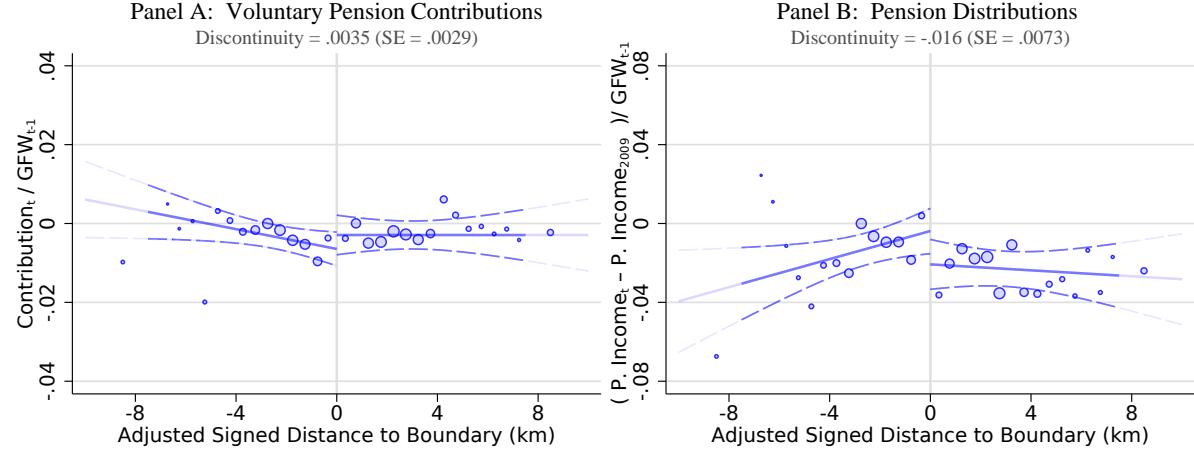
	(A) Adjusted Distance in KM			(B) Distance in KM, unscaled		
	$\frac{ANFS}{GFW_{t-1}}$ (1)	$\frac{ATNS}{W_{t-1}}$ (2)	$\Delta \log(LaborEarnings)$ (3)	$\frac{ANFS}{GFW_{t-1}}$ (4)	$\frac{ATNS}{W_{t-1}}$ (5)	$\Delta \log(LaborEarnings)$ (6)
Bandwidth = 14 km	0.0204 (0.0134)	0.0036 (0.0023)	0.0177* (0.0101)	0.0252 (0.0156)	0.0016 (0.0024)	0.0299*** (0.0109)
Bandwidth = 12 km	0.0245* (0.0134)	0.0034 (0.0022)	0.0203** (0.0100)	0.0180 (0.0169)	0.0000 (0.0025)	0.0285** (0.0112)
Bandwidth = 10 km	0.0305** (0.0135)	0.0041* (0.0023)	0.0199* (0.0103)	0.0253 (0.0179)	-0.0005 (0.0025)	0.0295** (0.0117)
Bandwidth = 8 km	0.0381*** (0.0142)	0.0041* (0.0022)	0.0169 (0.0108)	0.0271 (0.0191)	-0.0020 (0.0027)	0.0231* (0.0127)
Bandwidth = 6 km	0.0417** (0.0166)	0.0046* (0.0024)	0.0204* (0.0114)	0.0240 (0.0200)	-0.0013 (0.0028)	0.0214 (0.0149)
Bandwidth = 4 km	0.0371** (0.0161)	0.0029 (0.0025)	0.0185 (0.0138)	0.0300 (0.0230)	0.0019 (0.0032)	0.0208 (0.0173)
Bandwidth = 2 km	0.0764*** (0.0247)	0.0076* (0.0039)	0.0267 (0.0232)	0.0717** (0.0356)	0.0075** (0.0037)	0.0361* (0.0204)

E. Pension Wealth

In Panel A of Figure B.13, I consider the effect on voluntary pension contributions. Voluntary pre-tax retirement saving in Norway is limited. Individual pension plans only account for about 0.3% of aggregate pension wealth (Ozkan, Hubmer, Salgado, and Halvorsen, 2023). Individuals may contribute up to NOK 15,000 (USD 2,500) using pre-tax earnings. Accumulated savings are not subject to the wealth tax, but subsequent withdrawals during retirement are taxed as labor income. In Panel B, I consider the effect on pension distributions. Households can elect to start disbursement of pension wealth at age 62. Once they elect to start disbursements, their accumulated pension wealth is turned into a life-long annuity amount. The sources of this pension wealth are both public (roughly, a fraction of your average previous labor earnings up to a cap) and private where the employer (but not the employee) contributes an amount equal to a fraction of your pre-tax earnings.

FIGURE B.13: EFFECT ON PENSION SAVING

Panel A considers the effect on voluntary contributions to tax-favored pension savings vehicles. Panel B considers the effect on pension income, where any discontinuities in pension income may be driven by households choosing to accelerate or delay distributions from public or private pension schemes. Accumulated pension wealth is not subject to wealth or other types of capital taxation but disbursements are taxed as labor income.



F. Placebo test: House Prices and Saving Behavior

The inclusion of controls for average past transaction prices in \mathbf{M}_m in my main specification means that my causality argument is robust to the existence of underlying discontinuities in house prices. However, one may be concerned that these discontinuities are not fully accounted (e.g., by including a linear control). Hence, it is useful to assess whether there is any underlying relationship between saving behavior and cross-border differences in average past transaction prices once assessment discontinuities (and thus wealth tax exposure) is controlled for. To this end, I perform the following placebo test. In my main reduced-form regression specification, I replace the border discontinuity measure Δ_i with $\Delta_b^{placebo}$ in equation (4), where $\Delta_i^{placebo}$ is obtained the following way: I first regress past transaction prices (2005–09) on \mathbf{H}_i (the housing characteristics used in the hedonic pricing model, defined in the empirical specification section) and allow the coefficients to vary at the boundary area level. I then average the residuals at the $\mathbb{1}[d_i > 0] \times$ boundary area level, including only the 50% of observations closest to the boundary on either side. I calculate the cross-border difference, which becomes $\Delta_b^{placebo}$. This quantity provides a measure of cross-border differences in past transaction prices that cannot be explained by differences in the housing characteristics, \mathbf{H}_i . It is however, not created the exact same way as Δ_i , largely because (i) Δ_i can be zero for boundaries whenever the two municipalities are assigned to the same price zone, or it can be influenced by other municipalities far away (see discussion in main text section 2.3). I then include $\tilde{\beta}1[d_i > 0]\Delta_i + \tilde{\gamma}^-d_i1[d_i < 0]\Delta_i + \tilde{\gamma}^+d_i1[d_i > 0]\Delta_i$ as *control* terms to remove variation implied by the hedonic pricing model itself. That is, I condition on tax assessments. I also remove average residualized past transaction prices from \mathbf{M}_m .⁴⁶

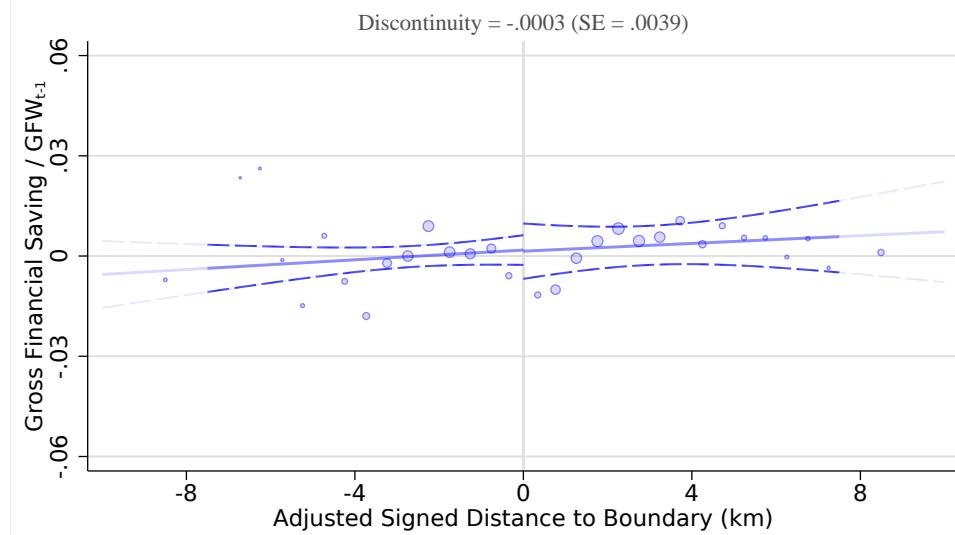
The resulting discontinuity estimate tells us the following: To what extent do households in higher-priced municipalities save more once we control for any discontinuities in tax assessments?

⁴⁶If I had not, this would have absorbed most of the variation in $\Delta_b^{placebo}$, rendering the placebo test less useful.

If it were the case that the tax assessments were irrelevant for saving behavior, and the results were driven by wealthier households in higher-priced areas saving more for other reasons, this placebo analysis should show a positive jump in saving behavior at the boundary. Reassuringly, as I show in Figure B.14, this is not the case. In fact, the discontinuity is precisely estimated to be very close to zero. The point estimate is negative and two orders of magnitude below my main estimate, and the associated confidence intervals do not include (even half of) the main estimate in Panel B of Figure 3.

FIGURE B.14: PLACEBO TEST: DISCONTINUITIES IN SAVING ACROSS BOUNDARIES WHEN CONTROLLING FOR TAX ASSESSMENT DISCONTINUITIES

I construct a measure of cross-border differences in house prices by regressing 2005–09 transaction prices on the same set of variables as in the hedonic pricing model. I then calculate the cross-border difference in mean residuals among the 50% of households nearest the boundary. I use this as a placebo version of Δ_i —the tax assessment model’s implied assessment discontinuity. Due to, e.g., several bordering municipalities being allocated into the same price zone, this $\Delta_b^{placebo}$ is correlated but not collinear with Δ_i . The placebo test consists estimating discontinuities in saving with respect to $\Delta_b^{placebo}$, while controlling for discontinuities with respect to Δ_i .



G. Further Institutional Details

G.1. (*Expected*) changes to the overall wealth tax schedule.

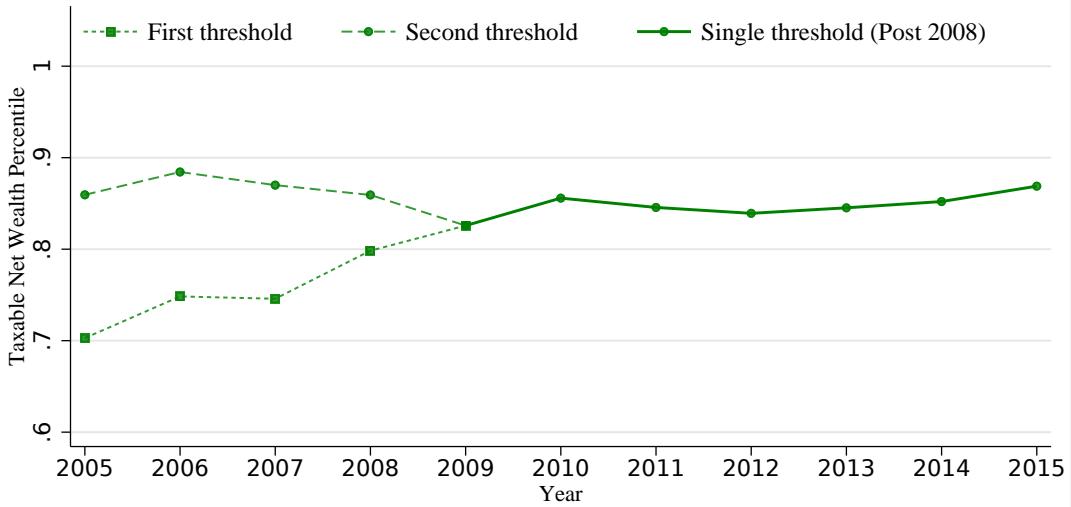
This section discusses the changes in the wealth tax schedule during my sample period and the political process behind these changes. This is useful for understanding how households anticipated that the tax assessment on their house would affect their long-run financial situation.

During 2005–09, the left-wing coalition government increased the progressivity of the tax schedule by essentially removing the first (lowest) threshold and introducing a single threshold. This increased progressivity was a stated goal following their election in 2005. During 2010–2011, this reform was evaluated by the government and they concluded that the progressivity increase was a success (GOVT 2011). No indication of intent to further increase the progressivity was given. In accordance with this, the progressivity of the tax threshold in real terms did not materially change for the remainder of this governments term (which ended in 2013). This

is evident from Figure B.15, which shows that the location of the wealth tax threshold in the taxable net wealth distribution was stable at around the 85th percentile during 2010–2015.

FIGURE B.15: THE WEALTH TAX THRESHOLD'S LOCATION IN THE TAXABLE NET WEALTH DISTRIBUTION.

The location was 85.58%, 84.56%, 83.92%, 84.52%, 85.21%, 86.89% in the years 2010–2015. These numbers are calculated as one minus the fraction of households who have taxable net wealth above their relevant wealth tax threshold. The underlying sample includes all Norwegian households for whom TNW is observed. The relevant tax threshold is twice the nominal threshold for married households starting in 2006. In 2005, the relevant threshold was NOK 40,000 higher for some households, e.g., due to being a single parent.



This background information is useful for assessing whether households would consider the effect of the assessment discontinuity on their wealth tax exposure as being a transitory versus a permanent shock. At least for the 2010–2013 period, when the left-wing government was in charge, it seems most reasonable to assume that households considered the effect permanent, given that there was no indication that the progressivity of the wealth tax would change further (nor did it, in real terms). The caveat is that a right-wing coalition government was elected in the fall of 2013. This coalition government had the stated intent of increasing the wealth tax threshold and reducing marginal rates ([GOVT 2011](#)). While they did lower the marginal rate from 1.1% to 1.0% in 2014 and then 0.85% in 2015 and beyond, they did not make any substantial changes to the overall tax progressivity. Following 2015, they only modestly increased from 1.2MNOK to 1.48MNOK in 2017, and left it virtually unchanged until 2020. The left-wing parties (in the previous coalition government) were in disagreement with these changes ([GOVT, 2016](#)). Hence, households who correctly anticipated a switch back to a left-wing coalition government (which occurred in 2021) would likely anticipate a reversal of policies back to the 2010–2013 baseline.

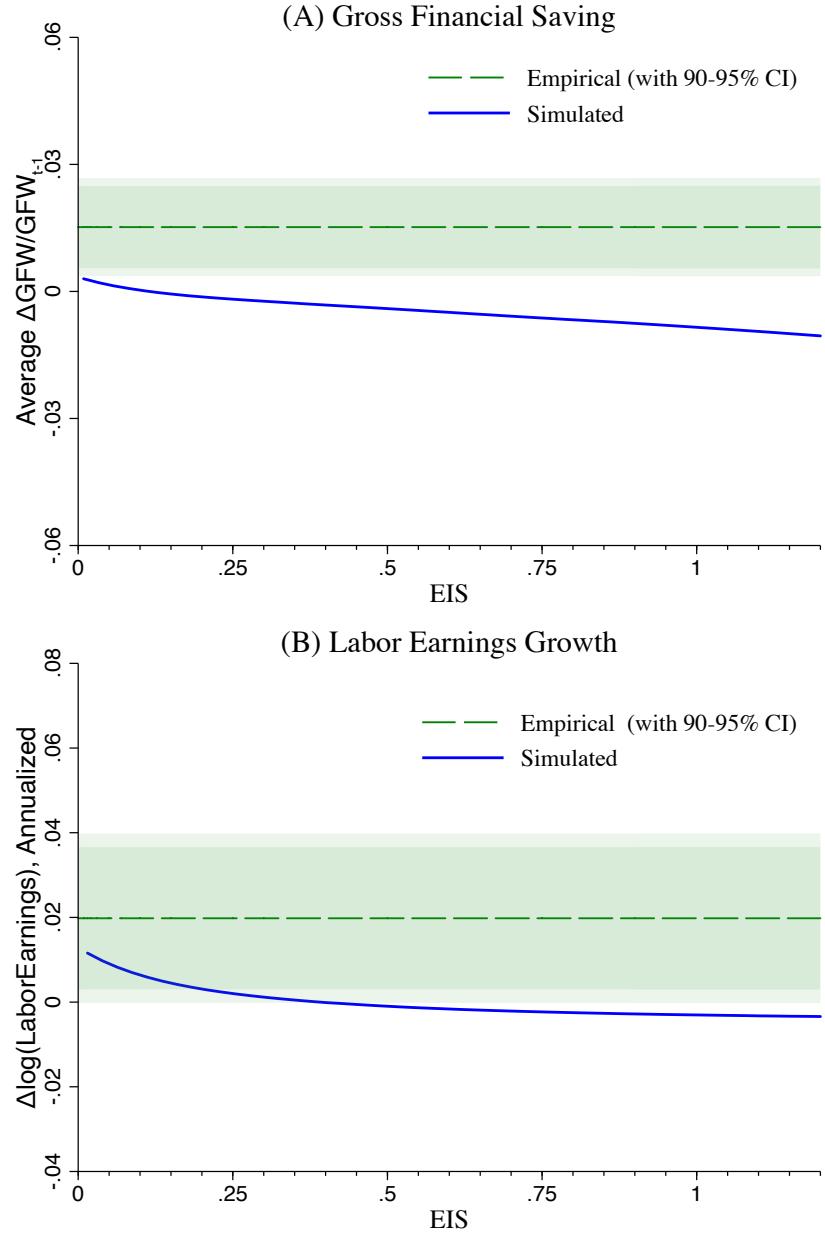
Expected long-term marginal tax rate. Households may have operated with an expected long-term nominal tax rate below the 2010 rate of 1.1% given a positive probability of a right-wing coalition government forming in 2013. However, older households may also recall that rate decreases may be followed by rate hikes. For example, during 1970–1971, the top marginal rate was 1%, whereas during 1972–1991 it exceeded 2%. It was then reduced again to 1.3% during 1992–1993, but saw an increase to 1.5% during 1994–1997. Hence, it is plausible

that households may also have operated with an expected long-term marginal rate exceeding 1.1%, even if they thought it was likely to decrease during 2014–2015.

In sum, given the political process behind the changes to the wealth tax, and the alternating between left-wing and right-wing coalition governments, it seems reasonable to assume that treated households in my setting would anticipated that the wealth tax schedule remained stable. If so, they would anticipate that their higher taxable wealth due to higher tax assessments would continue to affect their financial situation (i.e., whether they paid a wealth tax and how much they paid). If this were not the case, the empirical treatment effects would be much harder to rationalize. In Appendix Figure B.16, I model the responses to a 10-year shock to average and marginal tax rates (as opposed to a permanent shock in the main calibration in section 4.2). This shows that the simulated treatment effects are too small to rationalize the empirical findings, even when the EIS becomes very small. What happens when the shock is anticipated to be short-lived is that the anticipated effect on future disposable income becomes too small to warrant dissaving of the magnitude that I find empirically. Importantly, however, that the shock is transitory does not make it more likely to observe positive responses to wealth taxation. In fact, when the shock is transitory, we require an even smaller EIS to simulate positively-signed saving responses. In, e.g., Panel A of Figure B.16, a positive saving response to a transitory shock requires an EIS below approximately 0.05. In the main simulation (Figure 7), the EIS only needed be below about 0.4. Hence, the possibility that agents view the shock as short-lived only strengthens the case that the underlying EIS is low.

FIGURE B.16: SIMULATED TREATMENT EFFECTS WHEN THE WEALTH TAX EFFECT IS EXPECTED TO BE TRANSITORY

This figure shows the relationship between simulated saving and labor earnings responses and the Elasticity of Intertemporal Substitution (EIS). This figure assumes that the first-stage effect on marginal and average tax rates only last for 10 years (as opposed to permanently in the main calibration). 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 1. Panel A considers the effect on gross financial saving, without the wealth tax adjustment, where the empirical point estimate comes from Panel A of Figure B.3. Panel B considers labor earnings growth, where the point estimate comes from Panel A of Figure 5.



2005–07 Stock Discount. During 2005–07 risky assets (e.g., stocks) were favored by the wealth tax scheme. One question is whether households were still adjusting to the abolishment of this tax incentive to hold risky assets when the 2010 housing assessment methodology change occurs.

During 2001–2005, a right-wing coalition government was in charge. In 2004, they decided

to lessen the taxation of what they called “working capital”. This was implemented by having stocks (listed, unlisted, and mutual funds) enter the wealth tax base at a discount. For 2005, this discount rate was set to 35%. This implies that the marginal tax rate on stocks was $1.1\% \times (1 - 35\%) = 0.715\%$ instead of 1.1%. However, the right-wing coalition government did not obtain enough seats during the 2005 election to remain in power. The left-wing coalition government (who argued that these discounts were egressive) immediately announced a reversal to pre-2005 tax policy ([SoriaMoria, 2005](#)). This manifested as a gradual removal of the discount from 35% in 2005, to 20% in 2006, to 15% in 2007, and finally to 0% in 2008.

This series of events indicates that almost as soon as the stock discount was introduced, it was announced that it would be removed. The 2008 removal would have been expected as of the fall of 2005. Hence, I find it unlikely that the responses to the 2010-reform that I study are materially affected by the past presence of this short-lived stock discount..

The stock discount, similarly to the change in tax assessments on housing, created variation in taxable net wealth (TNW). In my sample, the mean mechanical effect in 2005 ($SMW_{i,2005} * 0.35$) was NOK 38,977 (p90–p10 = 85,067). The effect of the change in assessment methodology ($TaxVal_{2010} - TaxVal_{2009}$) was much larger: The mean absolute change was NOK 267,656. Isolating the variation coming from the geographic assessment discontinuities ($TaxVal_{i,2010} \cdot e^{\Delta_i} - 1$), I find a mean absolute effect of NOK 148,569 (p90–p10 = 337,616).

G.2. *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 et al. 2020a](#)). 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.⁴⁷ Thus any reasonable bounds on household sensitivity to municipal

⁴⁷I 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

finances suggest that the effect will be negligible.

G.3. *Property Taxation*

In 2015, 242 out 428 municipalities levied property taxes on residential homes.⁴⁸ Starting in 2015, a subset (49) of municipalities began using the tax authorities' assessments ($\widehat{\text{TaxVal}}$) 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 $\widehat{\text{TaxVal}}$, which would limit municipal property tax revenues. This disincentive was partially reduced in 2015, when $\widehat{\text{TaxVal}}$ only needed to be reduced by 20%. A continuing disincentive is that the tax authorities do not allow municipalities to use their own information to adjust or fine tune $\widehat{\text{TaxVal}}$. 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 $\widehat{\text{TaxVal}}$.

The potential use of $\widehat{\text{TaxVal}}$ for property-tax purposes implies some scope for the exclusion restriction to be violated: Border discontinuities in $\widehat{\text{TaxVal}}$ 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, my sample omits municipality-year observations in which $\widehat{\text{TaxVal}}$ is reported to be used for property taxation purposes. 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. Appendix Figure B.9 shows that responses are gradual and occur even during years in which there were no opportunity for municipalities to base their property taxes on $\widehat{\text{TaxVal}}$. While it's 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.

G.4. *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

difference will pass through after applying the government revenue equalization scheme, leaving only 0.26% more revenue for the high-assessment side municipality.

⁴⁸Source: 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).

for the calculation of new tax values for residential properties,”⁴⁹ and provided registered information about the house, namely structure type, construction year and size. Home-owners were asked to verify and possibly correct this information, either by 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 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.” See Figure [B.17](#).

⁴⁹My own translation.

FIGURE B.17: TAX AUTHORITY'S EXPLANATION OF HOW TAX VALUES ARE ASSIGNED

This figure provides a screenshot of the tax authority's webpage that describes how tax values are assigned. Screenshot is taken April 9th 2024. URL is <https://www.skatteetaten.no/person/skatt/hjelp-til-riktig-skatt/bolig-og-eiendeler/bolig-eiendom-tomt/formuesverdi/egen-bolig-primærbolig/slik-beregnes-formuesverdien/>.

[Person / Skatt / Hjelp til å få riktig skatt / Bolig og eiendeler](#)
[/ Bolig, eiendom og tomt / Formuesverdi / Egen bolig – primærbolig](#)

Slik beregnes formuesverdien av egen bolig (primærbolig)

Skatteetaten beregner en boligverdi (beregnet markedsverdi for bolig) basert på SSBs statistiske opplysninger om omsatte boliger. I beregningen tas det hensyn til din boligs beliggenhet, areal, byggeår og type bolig.

Formuesverdien er en prosentandel del av denne boligverdien.

Boligens verdi

Boligens boligverdi er lik boligens areal multiplisert med kvadratmeterpris basert på statistikk over omsatte boliger.

Beregningen bygger på opplysninger om boligens areal, beliggenhet, alder og type.

Formuesverdien er en gitt prosentdel av denne boligverdien.

Formuesverdi for primærbolig

Formuesverdien for primærbolig er **25 prosent** av boligens verdi opp til 10 millioner kroner, og deretter 70 prosent av det som overstiger 10 millioner kroner.

Opplysningene står i skattemeldingen din

Normalt vil opplysninger om boligens areal, byggeår og type bolig, fremgå av [skattemeldingen din](#).

Du må likevel sjekke i skattemeldingen at forhåndsfylte opplysninger om eiendommen din er korrekte. Skulle noen opplysninger være feil eller ufullstendige, må du endre dette i skattemeldingen.

Regn ut formuesverdien selv

Du kan selv [regne ut formuesverdien ved å bruke boligkalkulatoren vår](#).

Attest på innmeldte verdier

Ønsker du attest på allerede innmeldte verdier kan du [logge inn og bestille en attest for formuesverdi eiendom](#). Denne viser total formuesverdi for hele eiendommen.

G.5. Potential role for liquidity effects

The summary statistics (Table A.1) show that average household has about NOK 745,000 (\$124,000) in deposits alone and that only 6.6% of households have less than NOK 50,000 (\$8,300) in financial wealth (which includes stocks and bonds). For only 0.2% of households do their annual wealth tax bill exceed one quarter of their gross financial wealth. In other words, most households in the sample have significant liquidity—especially in comparison to their annual wealth tax bill. In addition, the households in the sample have particularly low LTV ratios, suggesting that extracting home equity is possible (see Table B.3).

The reason for this high level of liquidity in the sample is partly a mechanical consequence

of the design of the Norwegian wealth tax. While financial wealth enters TNW one-for-one, the main illiquid assets such as primary housing wealth enters at a significant discount of 75%. Households near or above the wealth tax threshold thus tend to have considerable liquid wealth. This effective targeting of more liquid households stands in contrast to other taxes on capital, such as property taxes, where taxes accrue based on someone's estimated housing wealth alone (potentially leading to considerable adverse liquidity effects, as in [Wong 2020](#))

Nevertheless, it is useful to consider potential liquidity effects in light of the empirical findings. One way to assess their importance is to consider the size of the wealth tax effect relative to a household's gross financial wealth. I provide evidence on this in Table 2, which shows the underlying first-stage coefficients for the MTR-ATR effect decomposition. Here, we see that a one log point increase in tax assessment is associated with an increase in the ATR with respect to gross financial wealth (i.e., wealth tax bill divided by GFW). These effects range from 0.27% to 0.75%. While these effects are large in terms of the after-tax return households achieve on their savings, they are not large enough to suggest a sizable liquidity effect.

Furthermore, if liquidity effects are important, we might expect households to respond by moving and selling their house. While I find no evidence of this (see Figure 8), it is possible that a non-trivial proportion of households do in fact respond by selling (the 95% confidence interval does e.g., include a 2% effect on whether someone sells). This is worrying to the extent that these households downsize and increase their financial savings. To address this, I examine the effect on saving among the (majority) subset of households that do not move in Figure B.11. Reassuringly, this provides a very similar and statistically indistinguishable estimate relative to the full sample (0.0180 versus 0.0195). In addition, Panel C of main-text Figure 4 shows that there is no effect on net saving in housing (that is, the sum of sales proceeds minus purchase costs), which is what we would expect if many households responded by selling their homes to free up housing wealth in response to higher tax assessments. Beyond this, I would also expect to find that households either defund illiquid pension savings faster or reduce contributions. I find no evidence of this either (see Appendix E).

Importantly, the wealth tax is not a sudden one-time shock. As I argue in Appendix G.1, the wealth tax treatment should be considered by households to be persistent. Most of the tax burden thus falls in the future, arguably giving households considerable time to adjust, through, e.g., liquidating illiquid assets. If liquidity effects are so severe that households are unable to furnish the liquidity to pay near-term wealth taxes, I would expect to find that households increase their labor supply (which I find) but I would not expect to also find that they *increase* their gross and net financial wealth. This positive effect on financial saving is consistent with a type of forward-looking behavior that I would not expect from severely liquidity constrained households.

I caveat this discussion by saying that there is likely *some* role for liquidity constraints in determining how households respond to wealth taxation in my setting. In particular, if households are risk averse and still face remaining unrealized income risk—even when they are close to retirement, they may be liquidity constrained while still having non-negligible liquid assets due to precautionary savings motives. I do, however, think that this role is limited due

to, e.g., the fact that I do not find any effect on whether households more or any evidence that households dissave from illiquid pension wealth.

Financial frictions and the implied EIS. 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 mute substitution effects, as households are 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 may bias my calibration in favor of a low EIS if there is considerable EIS heterogeneity in the population, in which case, credit constraints would mute the responses of high-EIS households and only allow us to observe partially-muted responses of low-EIS households. However, this is hard to square with my empirical setting where virtually all households in the sample have considerable liquidity. In addition, existing quasi-experimental work by [Best et al. 2020](#) finds no evidence of material heterogeneity in the EIS.

G.6. *House price effects*

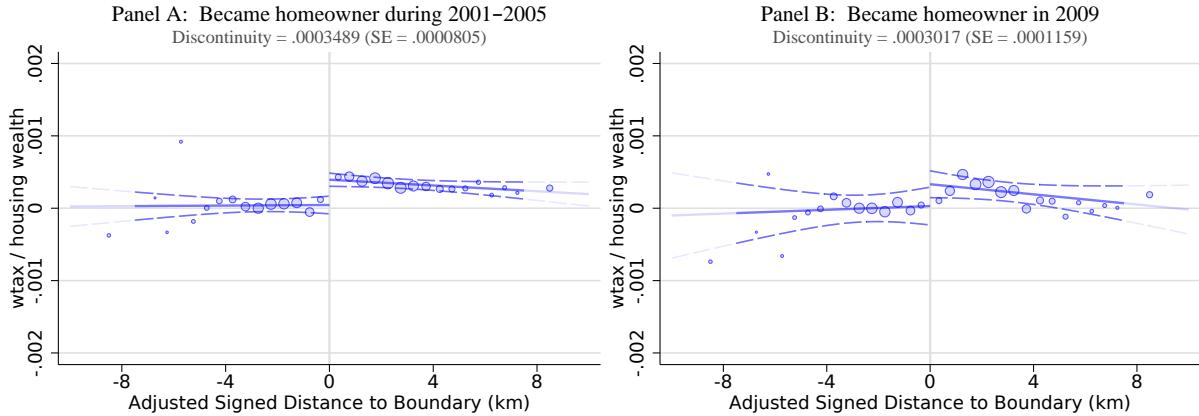
Given the modest number of housing transaction that I observe in my data during the post-period of 2010–2015, the standard errors on the house price effect of higher tax assessments are quite large. Given this empirical ambiguity, it is useful to consider what the price effect should be if the wealth tax implications of buying a higher-assessed house is fully capitalized into house prices. It is important to note, however, that whether there is an effect on house prices does not necessarily bias my empirical estimates. Any house price capitalization simply renders moving (and selling your house) a less feasible way of undoing the wealth-tax implications of the assessment discontinuities. Any house price capitalization does not cause any pure wealth effect above and beyond the income effect associated with paying more in wealth taxes since they are never at play at the same time: wealth effects from house price capitalization matters only conditional on selling and the income effects only matter conditional on not selling. 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’ near-term wealth tax liabilities, lowering the demand side’s sensitivity to the tax assessments. Consistent with this, there is no clear visual evidence of a drop in house prices at the boundary.

In this appendix section, I provide evidence consistent with the notion that new homeowners are largely shielded from the assessment discontinuities. I measure wealth tax exposure as the amount of wealth taxes accrued in a given year divided by estimated housing wealth. Any discontinuities in this measure may be informative of the extent to which assessment discontinuities should be capitalized into house prices. I show the results in Figure [B.18](#). Panel A considers a sample of relatively new homeowners who began their tenure in their current home during

2001–2005. Thus, during the sample period of 2010–2015, their tenure as homeowners will range from 5 to 15 years. For this sample, we identify a discontinuity of 3.5 basis points. If we take the present value of this number over 25 years, using a discount rate of 3%, get a present-value of $3.5 \text{ basis points} \times 17.41$ which equals 0.6 percentage points. Hence, if the average effect on relatively new homeowners' wealth tax exposure is fully capitalized into housing prices, we would expect that a one log-point increase in tax assessment causes a reduction in house prices of 0.6%.

FIGURE B.18: WEALTH TAX EXPOSURE RELATIVE TO HOUSING WEALTH

These graphs illustrate how geographic discontinuities in tax assessment, $\widehat{\text{TaxVal}}$, affect the amount of wealth taxes household pay ($wtax$) in units of estimated housing wealth. The y-axis thus provides the ratio of $wtax_{i,t}$ to an estimate of housing wealth ($Housing_{i,t}$, defined in Appendix B.4.2) that is purged of assessment discontinuities. The denominator, $wtax_{i,t}$, is limited to $\tau_t \times \widehat{\text{TaxVal}}_{i,t}$ since this is the maximal impact tax assessments can have on the wealth tax bill. Panel A considers a sample of households who became homeowners in their 2009 home during 2001–2009 (i.e., they did not change their address since this time period and their house was most recently transacted during this time period). Panel B considers a sample of households who became homeowners in 2009. 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 left-hand-side to the right-hand-side solid line, is the estimated effect of a 1-log point increase in (model-implied) tax assessment, $\widehat{\text{TaxVal}}$. One negative-distance bin is normalized to be zero. The size of each circle corresponds to the relative number of observations in that bin. Standard errors are clustered at the municipality level.



Panel B considers a sample of even newer homeowners—those who began their tenure in 2009. For this sample we find an even lower effect on the ratio of wealth taxes to housing wealth, consistent with shorter tenure correlates negatively with age and taxable wealth.

Note that this discussion only considers wealth effects from house price capitalization. I consider potential collateral effects in Appendix G.5

G.7. Collateral effects

One potential concern is that some banks use a hedonic pricing model similar to that used by the tax authorities when assessing collateral values. This may be particularly important if many of the households are borrowing constrained and also have relatively high loan-to-value ratios, in which case changes in the (estimated) housing value could affect how much they borrow or are able to borrow.

This concern is important to address since it is quite likely that any banks that use a hedonic pricing model would include municipal fixed effects. This by itself does not imply that the identifying variation in this paper is correlated with bank-assessed collateral values since I

include several controls that would capture such cross-municipality differences in average house prices (see discussion in main text section 2.3). However, since the data does not directly reveal banks' assessments of collateral values this is impossible to test. What we can empirically assess, however, is whether many of the households have high LTV ratios, which is when one would expect any collateral assessment discontinuities to be important. I provide some evidence on this in Table B.3, which shows that the median household in my sample has an (estimated) LTV ratio of only 3.2%. Even at the 99th percentile, the LTV ratio is only about 66%.

These low LTV ratios are not too surprising given the structure of the Norwegian wealth tax and my sample restrictions. Housing wealth enters at a highly discounted rate into taxable net wealth (TNW) but debt enters one-for-one. My sample restriction is that households have positive TNW as of 2009. Hence, I naturally select households who have (as of 2009) very little debt—otherwise, their TNW would most likely be very low (and negative).

TABLE B.3: LOAN TO (ESTIMATED) VALUE RATIOS

This table provides the distribution of loan to value ratios for households in the analysis sample during the years 2010–2015. The loan to value ratio is defined as total household debt to their estimated housing wealth ($Housing_{i,t}$).

p1	p5	p10	p25	p50	p75	p90	p95	p99
0	0	0	0	3.2%	13.0%	25.34%	35.69%	65.82%

Another reason why any hypothetical collateral discontinuities are unlikely to play an important role in my sample is that the households do not appear to be liquidity constrained. I provide a fuller discussion of this (including caveats) in Appendix G.5.

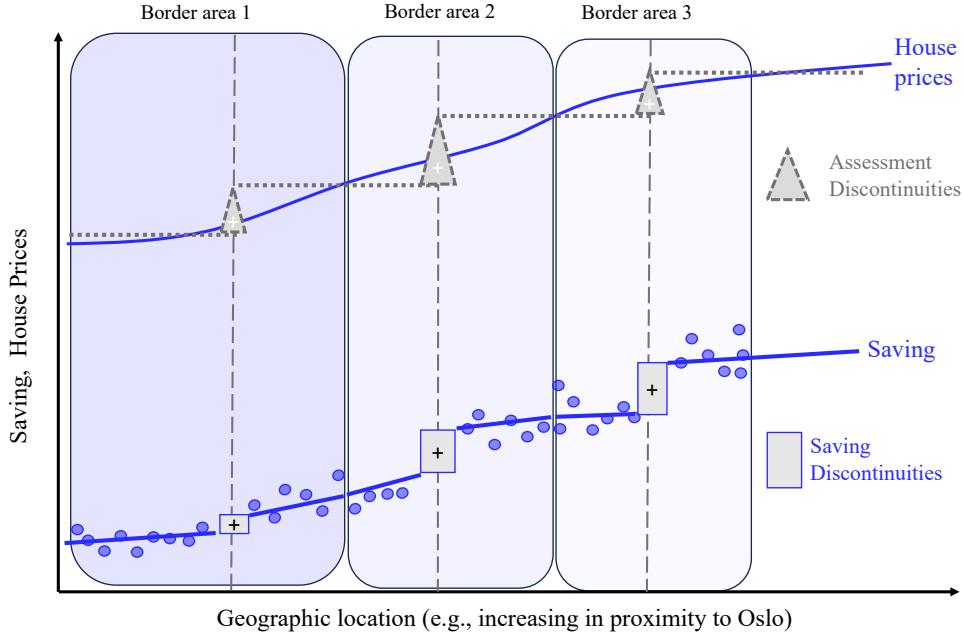
Finally, there is no evidence that treated households take on more debt. I instead find that when considering *net* as opposed to *gross* financial saving, the effect on saving is larger. I also verify in Figure B.5 that there are no discontinuities in *pre-period* debt accumulation.

H. Additional discussion of identification

A key feature of my boundary discontinuity design approach is that I exploit treatment discontinuities across *many* boundaries. I provide a stylized example in Figure B.19, which considers three boundaries inside three different border areas. As we move to the right on the x-axis, house prices increase smoothly, but tax assessments only increase at the boundaries. By also considering how saving increases discontinuously at these boundaries, I am effectively correlating the assessment discontinuities across boundaries with the saving discontinuities. In a standard single-boundary discontinuity design, one simply divide the saving discontinuity by the assessment discontinuity to get a first-stage estimate on how assessment discontinuities affect saving behavior. In such a single-boundary setting, it would not be possible to control for other covariates that vary at the municipality level (in the figure, a municipality would be contained inside one region of a border area). In my setting, given the large number of boundary areas between price zones (typically municipalities), I can control for a wide range of such covariates, as discussed in main-text section 2.3.

FIGURE B.19: GRAPHICAL EXAMPLE OF A
MULTIPLE-BOUNDARY DISCONTINUITY DESIGN

Consider a simple setting with four municipalities, where everyone lives on the same street. As the street approaches the capital, Oslo, house prices rise (blue solid line). The assessment methodology (using municipal price zone fixed effects), imposes discontinuous tax assessments (dotted gray line; and assessment discontinuities in triangles). The blue circles indicate the saving behavior of households on the street. The blue solid lines are linear fits for each side of each of the three boundary areas, and the squares indicate the estimated discontinuities in saving behavior. The empirical methodology in this paper essentially provides reduced-form estimates that are the correlations between the assessment discontinuities (triangles) and the discontinuities in saving behavior (squares).



Note that Figure B.19 is simplified such that the assessment discontinuities correspond to actual differences in mean house prices (blue line) across municipalities. This is not the case empirically (see Figure B.2), due to several “quirks” in the hedonic pricing model. Below, I discuss these quirks and how they allow me to control for past municipality-wide transaction prices to further strengthen identification.

The quirks in the hedonic pricing model. If the hedonic pricing model had consistently estimated fixed effects at the price-zone (primarily municipalities) level, then the pricing model fixed effects could have been collinear with cross-border differences in past transaction prices. If that were the case, including past transaction prices in \mathbf{M}_m would have absorbed most of my identifying variation. This is the case in the stylized example in Figure B.19, but (fortunately) not in practice.

Firstly, fixed effects are estimated at the *price-zone* level. This means that, in many cases, even if past transaction prices are very different across the boundaries, tax assessments may be identical due to many bordering municipalities being allocated to the same price zone. In other cases, even if past prices are very similar, assessments may be very different: Many coefficients in the hedonic pricing model are estimated at a regional level (one or multiple counties), which allows far-away past transactions to affect a given house’s assessment.

For example, apartments and row houses (houses that are connected by a shared wall) in the counties of Aust-Agder, Vest-Agder, and Rogaland (the entire south/south-western tip of Norway) are all assigned to the same pricing region ([Statistics Norway, 2009](#)). This implies that if older row houses in the south east of Norway sell at a large discount, older row houses in the far west of Norway would obtain a lower assessment. In addition, a connected house or apartment in, e.g., my coastal home town of Tvedstrand would also be assigned to the same price zone as Sauda, which is *a six to seven hour drive away* (at the time of writing in 2024). Hence, the transactions of relatively cheap apartments in rural Sauda would cause the tax values of more expensive ocean-front apartments in Tvedstrand to decrease.

In the end, we should consider the residual identifying variation in tax assessments ($TaxVal_{i,2009}$) to be orthogonal to cross-boundary differences in household observables such as wealth and past transaction prices. Instead the residual variation comes from the quirks in the hedonic pricing model. Presumably, many of these choices were made to increase the statistical precision of the model's parameters as opposed to make it accurately reflect geographic variation in house prices. While only exploiting this residual variation alleviates a host of potential identification concerns, it does not guarantee the absence of any bias. Accordingly, I perform a range of tests to investigate whether the residual variation appears to be correlated with potential confounders. Firstly, Figure [Figure 1](#) shows that there are no discontinuities in past income levels (even though municipality-wide income levels is not included as a control variable in M_m). Secondly, I find no evidence of differences in pre-period saving behavior (i.e, before the new valuation methodology was implemented in 2010) saving behavior and I find virtually the same estimate on saving behavior when doing a DID-RDD setup in Figure [B.10](#).

Finally, I also examine whether cross-boundary differences in past transaction prices is associated with higher post-period saving behavior *once I control for the tax assessments*. I discuss these results in Appendix [Appendix F](#).

I. 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 for evasion.

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 (easier-to-evade) asset classes, such as art. This would imply that my saving measure, based on changes in largely third-party reported financial wealth, would underestimate the true effect on saving. I shed some light on this question following the approach in [Seim \(2017\)](#); that is, by considering the extent to which households bunch at the tax deduction threshold.

I show my results in Figure [B.20](#) below. Panel A shows the results for my full sample of households during 2010–2015. Visually, there is little indication that households bunch at the

wealth tax threshold. However, my approach does suggest some excess mass to the left of the threshold, and the implied elasticity of taxable wealth with respect to one minus the wealth tax rate is found to be positive at 0.0308 and borderline statistically different from zero (t -statistic = 1.90). As a comparison, [Seim \(2017\)](#) finds elasticities in the range of 0.09 to 0.27 in Sweden. In Panel B of Figure [B.20](#), I zoom in on the threshold to see whether there is any visual indication of bunching. Visually, we clearly see that there is excess mass right at the threshold. However, the excess mass is economically small and not statistically different from zero.

This lack of meaningful bunching may be driven by most households not possessing the technology or resources to bunch. A group that is typically thought to have the necessary technology is the self employed. I therefore consider heterogeneity with respect to whether a household owns unlisted stocks (i.e., they're private business owners) or receives sole-proprietor income in Figure [B.21](#). I also examine whether there are changes to the propensity to bunch over time by comparing the 1993–1998 period (the earliest I observe) with the 2010–2015 period. My hypothesis is that as the tax-authorities capabilities to limit evasion have improved over time, the opportunities to evade have been limited as well, leading to reduced bunching. Panels A and B consider bunching during 1993–1998 and 2010–2015 for non-self-employed households. Here, I find a much larger implied elasticity during the earlier 1993–1998 time period. While the bunching is not as sharp (i.e., visually clear) as in [Seim \(2017\)](#), the implied behavioral elasticity of 0.20 is within the range of the estimates found in the Swedish context.

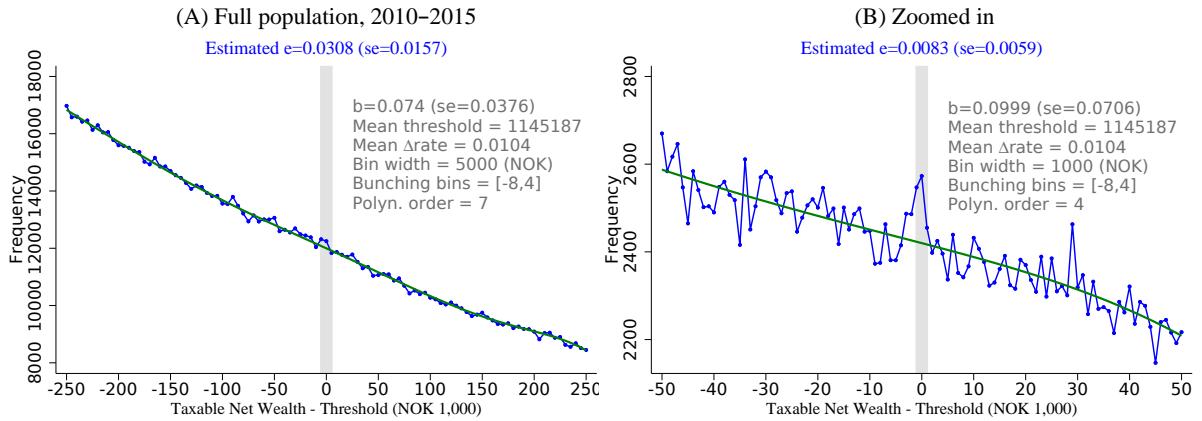
Panels (C) and (D) consider self-employed households. I find no evidence of bunching during 1993–1998. This is a bit puzzling, given the appearance of significant bunching among the non-self-employed households during the same period. I do, however, find evidence of bunching during the 2010–2015 period. There is fairly clear evidence of excess mass around the threshold, and the implied elasticity is about 0.1, which is in the lower range of the implied estimate for the entire Swedish population in [Seim \(2017\)](#). Interestingly, [Seim \(2017\)](#) finds that one third of the bunching is driven by households underreporting car values. He documents this by using a car registry (that the Swedish tax authorities did not use) to obtain estimated car values. In Norway, however, the tax authorities do use a similar registry that Seim (but not the Swedish tax authorities) used. This allows them to pre-populate tax returns with car values, which likely eliminates bunching due to underreporting these asset values. Another reason why there is less over all bunching in Norway relative to Sweden may be the clear guidelines offered by the Norwegian tax authorities on how to value other self-reported assets, such as boats, where the rule is that these assets shall be valued at some fraction of their insurance value. While misreporting is still possible, the asset's taxable value becomes considerably less subjective.

In sum, there is modest evidence that households respond to the wealth tax by bunching. Both visually and statistically, the evidence is considerably weaker than in, e.g., [Seim \(2017\)](#), [Jakobsen, Jakobsen, Kleven, and Zucman \(2020\)](#), [Brülhart, Gruber, Krapf, and Schmidheiny \(2021\)](#), [Londoño-Vélez and Ávila-Mahecha \(2020a\)](#). However, there appears to be some responsiveness. These responses translate into meager economic elasticities. For example, the highest elasticity of 0.20 is two orders of magnitude below the long-run wealth elasticity found by [Jakobsen, Jakobsen, Kleven, and Zucman \(2020\)](#). They find an implied long-run semi-elasticity

of wealth with respect to the rate-of-return of about 17.5.⁵⁰

FIGURE B.20: MEASURING THE RESPONSIVENESS TO WEALTH TAXATION BY EXAMINING BUNCHING AT 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 all households with TNW within 250,000 of the threshold, where thresholds are multiplied by two for married couples. Panel B “zooms in” on households within 50,000 of the threshold, and plots the number of households within NOK 1,000 bins. Plots and estimates are partially produced using the .ado file provided by Chetty, Friedman, Olsen, and Pistaferri (2011). The counterfactual distribution (green line) is constructed by fitting a 7th or 4th order polynomial on all bins outside the bunching region [-40,000, 20,000] in Panel A and outside [-8,000, 4,000] in Panel B. e is the implied estimated sensitivity of TNW to (one minus) the wealth-tax rate. 1,000-repetition bootstrapped standard errors are in parenthesis. The analyses use pooled data for 2010–2015. The statistics that are used to calculate the implied elasticity is provided in gray. b is the relative excess mass of households at the threshold. This is calculated as the (net) integral of the difference between the dotted blue line and the solid green line over the bunching region and is divided by the counterfactual number of households (green line) located at the threshold. See, e.g., Seim (2017) for further details, as I use the same notation.



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 underestimate 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, Johannessen, and Zucman (2019a). One example is the ease of placing wealth in foreign financial assets. Alstadsæter, Johannessen, and Zucman (2018) find that only 0.03% of households in the bottom 99% of the wealth distribution 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

⁵⁰They present the finding as an elasticity of wealth with respect to the after-tax rate of return. This takes a value of 0.77. To make this comparable the implied semi-elasticities from bunching, we need to divide this elasticity by the assumed average rate of return (in their sample, 4.4%).

setting that likely limit such responses for the households that I study.

FIGURE B.21: WEALTH TAX THRESHOLD BUNCHING HETEROGENEITY

This figure considers heterogeneity in bunching at the wealth tax threshold. I use the same methodology as in Figure B.20. Panels A and B consider households who are neither sole-proprietors nor owners of unlisted stocks (i.e., private business owners). Panels C and D consider sole-proprietors and unlisted-stock owners. The left-hand side panels, A and C, examine bunching at the first wealth tax threshold during 1993–1998. The right-hand-side panels, B and D, consider bunching at the wealth tax threshold during 2010–2015. During 1993–1998, married households are subject to a single tax threshold (i.e., a marriage penalty) but may qualify for a slightly lower tax threshold (by about NOK 25,000) under some conditions. I account for this in constructing their relevant tax threshold. In panels A and C, I use a lower polynomial order and a smaller bunching region. This is because the threshold was low (NOK 120,000) and thus close to zero where there is a spike in the number of households, and it is thus preferable to avoid the region around 0 to estimate the counterfactual density.

