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

Marius A. K. Ring*

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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 masked 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. This positive effect on saving is primarily financed by increases in labor earnings. These responses are the combination of small negative effects of increasing the marginal tax rates and larger positive effects of increasing average rates. My findings imply that income effects may dominate substitution effects in household responses to rate-of-return shocks, which has implications for both optimal taxation and macroeconomic modeling.

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

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*University of Texas at Austin, McCombs School of Business. E-mail: mariuskallebergring@gmail.com. I thank my advisors, Dimitris Papanikolaou, Anthony DeFusco, and Matthew Notowidigdo for helpful comments and advice. I am grateful 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 seminar participants at Kellogg, Statistics Norway, Skatteforum, NHH, LBS, BI, Drexel, Yale, NYU, UT Dallas, WUSTL, Stanford, UCLA, MIT Sloan (Finance), Wisconsin-Madison, UT Austin, the Center for Retirement Research at BC, MIT Sloan (Economics), the Nordic Junior Macro Seminar, the NBER Summer Institute Public Economics workshop, the University of Oslo, EIEF, and the IIPF annual congress for helpful comments and questions. Support from the Research Council of Norway (grant 283315) is gratefully acknowledged.

1. Introduction

It is widely believed that lowering the return on savings, either through taxation or monetary policy, causes households to save less. This belief pervades economic models and policy. Economic theory, however, says that the effect is ambiguous. Lowering the return on savings has a negative substitution effect but also a positive income effect. Since future consumption becomes more expensive, households need to save more if they want to smooth consumption. In a standard life-cycle model, this income effect dominates when the elasticity of intertemporal substitution (EIS) is sufficiently small. However, there is considerable disagreement regarding the value of the EIS.

Existing empirical studies likewise has no clear guidance. Inferring causal effects from interest rate changes is challenging, in part due to equilibrium effects. One solution is to exploit variation in the return on savings from capital taxation. A challenge with this approach is that it typically requires comparing households who differ on tax-relevant characteristics, such as income or wealth, that are also determinants of saving behavior. Using tax reforms for identification also invites measurement issues. While several studies show that wealth taxation causes households to report less wealth, it is unclear whether this is driven by dissaving or tax evasion. Distinguishing between saving and evasion responses is necessary to inform key modeling parameters such as the EIS. It is also important to inform behavioral responses to capital taxation in settings with limited evasion opportunities.

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 after-tax return on savings comes from capital taxation in the form of an annual wealth tax. Importantly, wealth taxation requires regular assessments of the stock of capital. The steps the Norwegian government has taken to make such assessments provide promising quasi-experimental variation that I exploit together with third-party-reported data on savings.

Norwegian households pay 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. 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 average and marginal after-tax return on savings that is uncorrelated with characteristics such as ex-ante income, wealth, and transaction prices. I use data on structure-level ownership and location as of 2009 to implement this identifying variation in a novel Boundary Discontinuity Design (BDD) approach.

I first consider the effect on financial saving. My estimates imply that for each additional NOK of wealth tax, households *increase* their yearly gross financial saving by 2.40 and their net financial saving by 3.76. 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 (see, e.g., Saez and Stantcheva 2018). These adjusted saving propensities 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 is close to retirement and thus faces declining incomes.

I show that the additional saving is primarily financed by increased labor supply. Corresponding to the geographic discontinuities in wealth tax exposure, I find clear discontinuities in household labor earnings following the 2010 reform. These discontinuities constitute novel evidence of a meaningful cross-elasticity between labor supply and the after-tax return on capital. For each additional NOK of wealth tax, households increase their after-tax earnings by 2.37, enough to finance a majority of the additional saving. This estimate translates into a wealth effect on labor supply that is substantially larger than those found in lottery studies. The substantial labor supply effect is incompatible with the common assumption of no wealth effects on labor supply. I further exploit employer-employee registers to document that the labor earnings effect is entirely driven by extensive-margin responses, such as delayed retirement.

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

I further study whether households adjust their portfolio allocation. I first consider the stock market share of financial wealth. One hypothesis is that the adverse wealth effect induces risk-averse agents to lower their stock market share. The alternative view is that households "reach for yield" by substituting low-risk bank savings with stocks (Lian et al. 2019; Daniel et al. 2021; Campbell and Sigalov 2021). Consistent with this ambiguity, I find no effect on the stock market share. I further present the hypothesis that the adverse wealth effect induces households to exert more effort towards improving the returns they receive on their low-risk bank savings. My findings, however, lend no support to this hypothesis.

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 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, some studies find that consumers suboptimally confuse marginal and average prices (Ito, 2014). I therefore test whether households respond to marginal and average tax rates as theory would prescribe. I use an instrumentalvariables 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:

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

where $wtax_{i,t}$ is the amount of wealth taxes incurred during year t and is due the following year. τ_t is the tax rate applied to any Taxable Net Wealth (TNW) in excess of a time-varying threshold. This threshold gradually rose from NOK 700,000 (USD 78,000) to NOK 1,200,000 (USD 208,000) during 2010–2015.[1] 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.[2] I discuss these and other changes to the wealth tax schedule in Appendix G. Appendix I shows that there is virtually no bunching at the wealth tax thresholds.

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 G). 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 in my sample period. The tax value of unlisted stocks is reported directly by the stock issuer as part of their financial reporting to the tax authorities. 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

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

[2] 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.

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 multiple of the initial tax assessment, which typically corresponded to 30% of construction cost.[3] 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.[4] This resulted in the following formula for the (log) tax value of a household's residence:

$$\log(\widehat{TaxVal_i}) = \hat{\alpha}_{R,s} + \hat{\gamma}_{R,Z,s} + (1 + \hat{\zeta}^{size}_{R,s})\log(Size_i) + \hat{\zeta}^{Dense}_{R,s,d} + \hat{\zeta}^{Age}_{R,s,a}. \tag{2}$$

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 $\hat{\zeta}^{size}_{R,s}$ term accounts for region-specific relationships between past transaction prices and size. $\hat{\zeta}^{Dense}_{R,s,d}$ is a fixed effect that applies to houses that are located in a cluster of at least 50 houses and $\hat{\zeta}^{Age}_{R,s,a}$ 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, $\hat{\zeta}^{Age}_{R,s,a}$, 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.[5]

[3] The tax value of a house would first enter at construction cost. Then each year the tax value is changed by some percentage; e.g., -5%, 0%, 10%. The practice of using initial construction cost is described in the government budget of 2010 (FINDEP, 2009).

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

[5] Note 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

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.

2.3. *Identification*

I obtain identifying variation in 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).[6] 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 is 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.[7]

The concern that potential determinants of the outcome variable vary discontinuously across

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identification, but I exploit the fact that within a border area the assessment discontinuity depends on age.

[6]More specifically, we see that housing assessments affect wealth tax exposure on both extensive margin (i.e., when $\mathbb{1}[TNW_{i,t} > Threshold_t]$ switches on) and the intensive margin (i.e., the wealth tax bill, $wtax_{i,t}$ or the amount of wealth subject tot wealth tax, $TNW_{i,t} - Threshold_t)\mathbb{1}[TNW_{i,t} > Threshold_t]$).

[7]To 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. $1, \quad 2 < 3, \quad 4$ is fine. The problem arises in a setting with, e.g., $2, \quad 2 < 3, \quad 3$.

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 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). Across some of these boundaries tax assessments differ considerably. Across others there is no difference at all. This heterogeneity lets me 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 strengthening identification by controlling for past transaction prices without substantially restricting the 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).

Distance and Boundary Areas. I define the key geographic measure, d_i, as the signed distance, in kilometers, to the closest price zone boundary. Households on the low-assessment side of the borders receive a negative distance, and households on the high-assessment side receive a positive distance.[8] Boundary areas, b, are sets of households assigned to the same boundary. Within a boundary area, households are defined as being on the high-assessment side if the average household in that boundary area would see a higher tax assessment on that side.[9] 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.[10]

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 a border-area and structure-type-specific linear function of the vector of house characteristics used in the pricing model (2), $\mathbf{H}_i = \{log(Size)_i;\ Dense_i;\ \mathbb{1}[Age_i \geq a]$, for a =10, 20, 35$\}$, and isolates the identifying variation in model-implied tax assessment, $\log(\widehat{TaxVal}_{i,t})$, to come from cross-border (but within border area) differences in pricing model coefficients, and allows this effect to vary with $\mathbf{H}_i$, measured as of 2009.

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

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.

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

The inclusion of border-area and structure-type-specific linear controls in housing characteristics, $\mathbf{H}_i$, isolates the identifying variation in $\log(\widehat{TaxVal}_{i,t})$ to $\mathbb{1}[d_i > 0]\Delta_i$. $\hat{\beta}$ thus identifies the effect on households on the high assessment side of the boundary ($d_i > 0$) of seeing a Δ_i log-point increase in $\widehat{TaxVal}$. While the estimator for β identifies the effect of a *discontinuous loading* on Δ_i, the estimated coefficients on $d_i 1[d_i < 0]\Delta_i$ and $d_i 1[d_i > 0]\Delta_i$ are meant to capture the effect of covariates that load continuously on Δ_i.

To alleviate concerns that there are geographic discontinuities in other determinants of sav-

[8] I calculate d_i by minimizing the distance to the nearest residence in a different price zone (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.

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

[10] The extent to which confounding variables change more rapidly, in a geographic sense, in denser urban areas is problematic. I provide a fuller discussion of this issue 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 Appendix B.9 for additional discussion and robustness checks.

ing behavior that correlate with $\mathbb{1}[d_i > 0]\Delta_i$, I include a vector of municipality-level control variables, $\mathbf{M}_m$.[11] 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, I include 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(total taxable labor income), log(GFW), a household college-attendance dummy, a debt dummy, log(debt), the stock market share of GFW, log(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 several 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 to link tax-return data to information on housing characteristics, geographic location, and past transaction prices. I supplement with demographic data from the National Population Register and data from employer-employee registers. I describe the data in more detail in Appendix B.1 and provide summary statistics in Panel A of Appendix 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 Appendix 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). This does not necessarily pose external validity concerns, since savings tend to be concentrated in older households. Another consequence is that sample participants have considerable liquid wealth. It is therefore unlikely that my identifying variation in wealth tax exposure affects behavior through liquidity as opposed to income and substitution effects.[12] I discuss the potential for

[11] When there are several *price zones* in a municipality, this vector is defined at district level.

[12] It 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

liquidity effects in more detail in Appendix G.5.

3. Empirical Results

3.1. *A Graphical Overview*

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, $\widehat{TaxVal}$ 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*

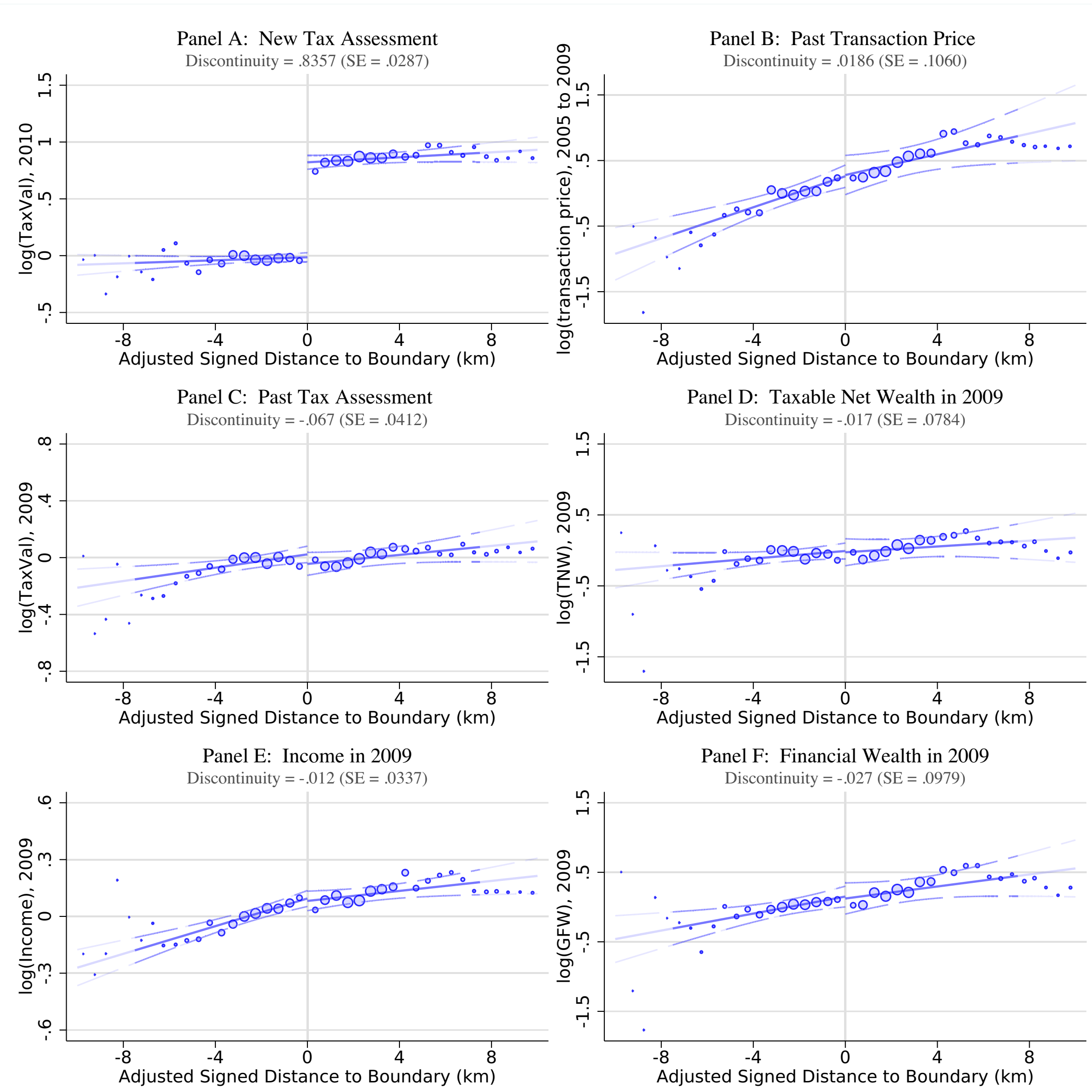
The assessment discontinuities create variation in assessed housing wealth, and thereby overall TNW. This affects both whether households have to pay a wealth tax and how much they pay. Quantifying these two exposure effects is necessary to map the reduced-form estimates on, e.g., saving behavior into elasticities or saving propensities.

I show the first-stage effects graphically in Figure 2. There is clear evidence of discontinuous wealth tax exposure at the geographic boundaries. Panel A shows that a one log-point increase in tax assessment increases the probability of paying a wealth tax by 27 percentage points. Table 1 translates 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.

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.

Figure 1: Verifying That Assessed Tax Values Jump at Pricing Boundaries While Observable Characteristics Do Not

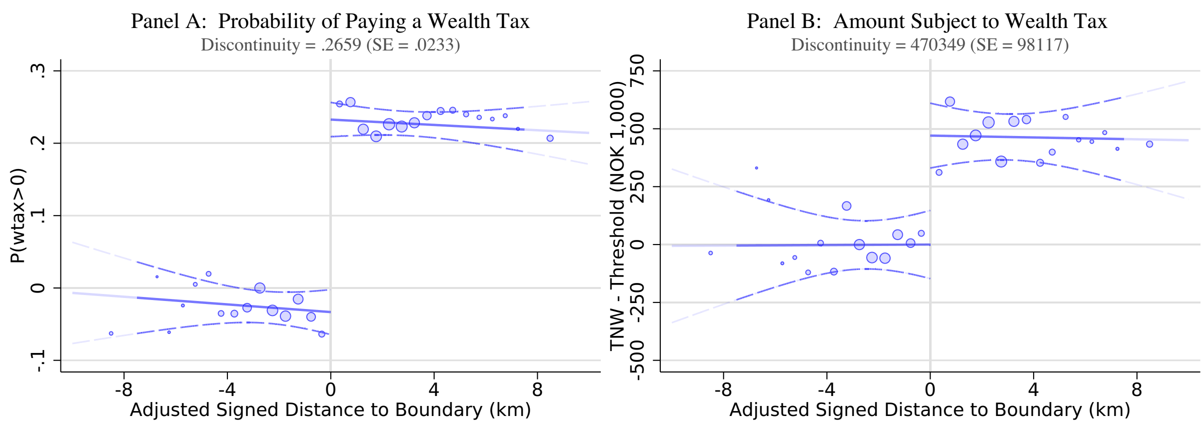
This figure shows how tax 2010 tax assessment and pre-period observables vary across the price-zone boundaries. Panel A considers tax assessment in 2010. Panel B considers past transaction prices (2005–09). Panel C considers past tax assessment (2009). Panel D considers taxable net wealth in 2009. Panel 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).



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.

Figure 2: Discontinuities in Wealth Tax Exposure

These graphs illustrate how geographic discontinuities in tax assessment, $\widehat{TaxVal}$, affect intensive- and extensive-margin wealth tax exposure during 2010–2015. Panel A considers the 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, $\widehat{TaxVal}$. ◦ 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.



Quantifying the first-stage effect on the average tax rate is useful to form priors about behavioral responses. 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.[13] In my setting, however, the MTR exceeds the ATR on marketable wealth, causing positive saving responses to be less obvious.[14]

While there were several changes to the wealth tax schedule during my sample period, I argue in Appendix G that the effect on wealth tax exposure should be considered a permanent shock. While the thresholds were increasing, they only increased enough to keep up with rising wealth levels. Beyond this, there was no political consensus in favor of removing the wealth tax and no discussion of making material changes to the hedonic pricing model.

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.[15] I follow

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

[14] This 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.

[15] Foreign deposits and foreign financial assets are self-reported. Outstanding claims are primarily self-reported. Third-party reported components include unpaid wages. For the average household, the potentially self-reported

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)
$\mathbb{1}[d_i > 0] \times \Delta_i$	0.2659***	0.2821***	470349***	4946***	0.4291***	0.0501***
	(0.0233)	(0.0247)	(98117)	(1042)	(0.0524)	(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

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.[16]

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

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 1.95 percentage points.

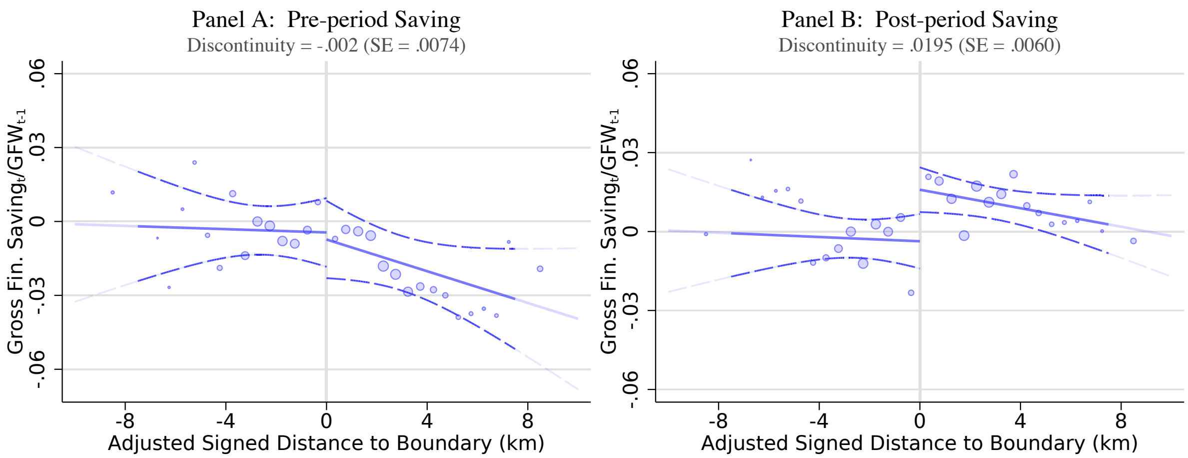
I proceed by considering alternative measures of saving in Figure 4. Panel A considers the effect on debt. Visually, there appears to be a negative effect, suggesting that households also save more by paying off their debts. While this effect is statistically insignificant, it is consistent with wealth 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.

components of GFW account for less than 3%. For a detailed description of wealth variables see Appendix B.

[16] 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.

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, $\widehat{TaxVal}$. 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

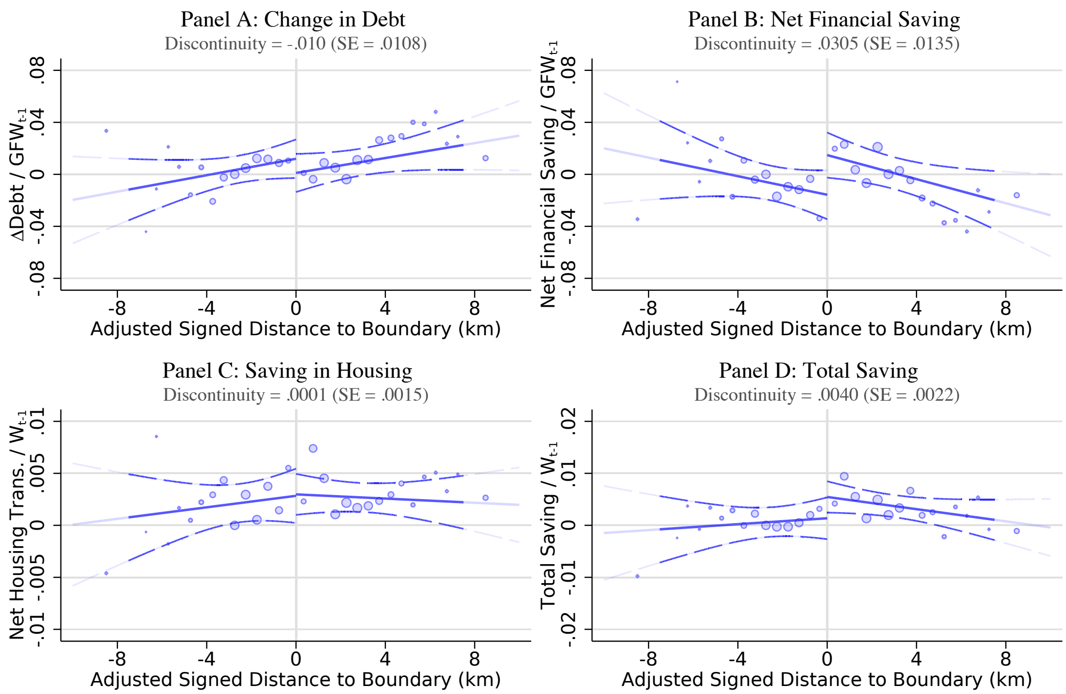
$$\text{Saving Propensity} = \frac{\text{BDD Estimate } \times \overline{GFW}}{\text{First-stage coefficient on } wtax}. \tag{6}$$

This approximates the change in the amount of saving by multiplying the saving rate (out of GFW) by median GFW. From Appendix 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 faces 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 do not 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.[17]

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

I repeat the analysis in Panel B of Figure 3 for different measures of saving. Panel A considers the effect on debt ($\Delta Debt/GFW_{t-1}$). Panel B considers the effect on net financial saving, which equals gross financial saving *minus* $\Delta Debt/GFW_{t-1}$. 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 saving rates out of income. At the boundary, net financial saving (out of income) increases by 3.51 percentage points at the boundary. Appendix E shows no evidence of 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.

Appendix Figure B.9 shows 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. Appendix 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

[17]I 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.[18]

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 et al., 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.

I focus on pre-tax labor earnings: salary, wages, and self-employment income.

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

Under the reasonable assumption of interconnected municipal labor markets, wages are unaffected, and thus labor earnings proxy for labor supply.

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.

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

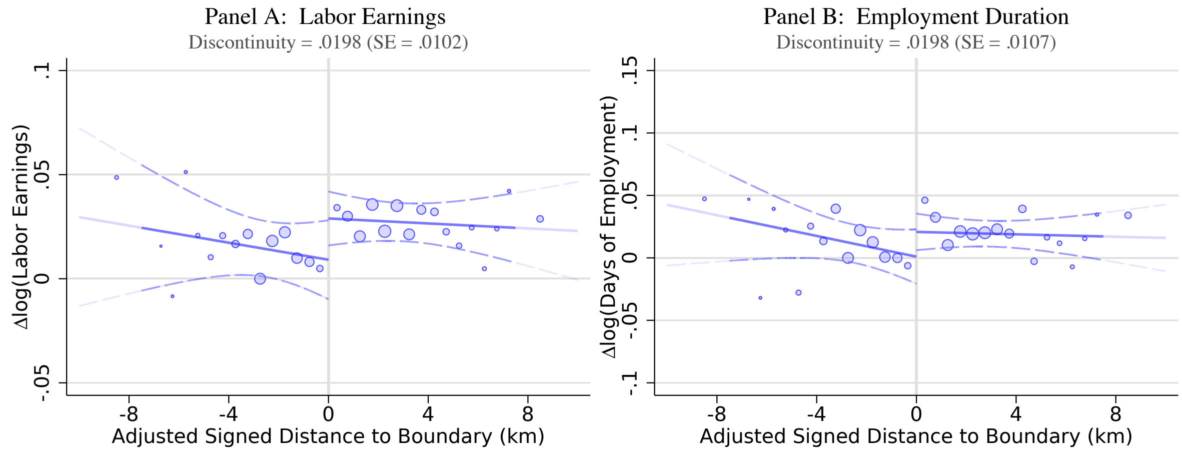
Appendix Table A.1 shows that the median amount of labor earnings is 0.242 MNOK. With a first-stage coefficient on the annual amount of wealth taxes of 4946, this implies a multiplier of 171.25 on the BDD estimates and thus a pre-tax earning propensity of 3.39. If we assume a marginal tax rate of 30%, 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.[19]

[18] Not 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.

[19] This 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 (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.

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 made by Cesarini et al. (2017) to account for retirement.[20] This returns a lifetime MPE of $2.37 \times \frac{6}{25} = 0.57$. Per the Slutsky equation, this constitutes a substantial wedge between the Hicksian and Marshallian elasticities. In contrast, early work by Gruber and Saez (2002) finds a wedge of zero.[21] My findings are qualitatively similar to 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.

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

[20] To 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.

[21] Note 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.

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

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.[22] 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, 2020); and both theory and evidence from the household finance literature suggest that the risky share of financial wealth may be affected by a wealth-tax induced reduction in the rate of return.

One hypothesis is that the stock market share goes down. Risk averse agents respond to the the tax-induced drop in life-time consumption by allocating less wealth to risky assets. The alternative hypothesis is that households 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).

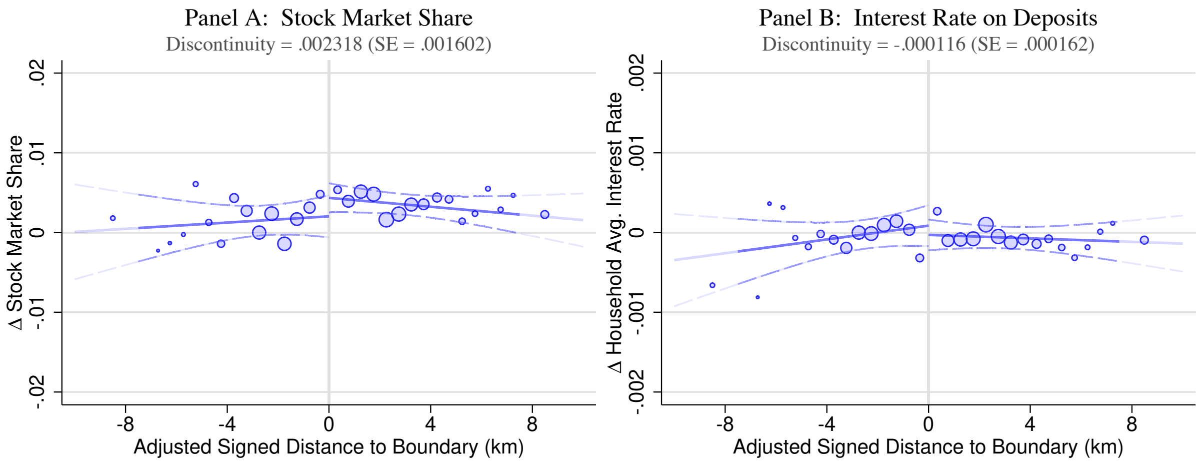
I present my empirical results in Panel A of Figure 6. This plot reveals no change in the stock market share for households more exposed to wealth taxation. A back-of-the-envelope calculation suggests that we can rule out a yearly increase in capital incomes above NOK 167 (USD 28).[23] There is little 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 differential effect on the returns on safe versus risky assets.

[22] To accommodate zero days of employment, I shift the log argument by one week (7 days).

[23] $\approx$ (0.002318+1.96*0.001602)*610000*5%, where 0.61 MNOK is median *GFW* and the equity premium is 5%.

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 find that the stock market *share* is unaffected, Appendix Figure B.4 shows a noticeable jump in the level of stock market wealth at the boundary, with point estimates similar to those for overall 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,[24] and through it, wealth inequality (Bach et al., 2017). The banking literature has documented considerable dispersion in the (net-of-fee) interest rates on deposits (e.g., Azar et al. 2019). This large dispersion may be supported by switching costs that render the deposit rates less competitive (Sharpe, 1997). I propose the hypothesis that households may choose to suffer these non-pecuniary costs, i.e., supply more effort, in order to offset the adverse effects of more aggressive capital taxation. I test this by considering the average realized returns on bank deposits,

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

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.[25]

[24] Bach et al. (2020) and Fagereng et al. (2020) document return heterogeneity in wealth in Norway and Sweden.

[25] As a benchmark, it is useful to establish what a hypothetical, large effect would be. Appendix 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 $1[wtax > 0]$ of 0.2659). This hypothetical effect is eight larger than the upper bound of the 95% confidence interval in Panel A.

In other words, my findings are inconsistent with a substantial "searching for interest" channel. However, this does not imply that I can rule out scale dependence in returns, which is discussed in the context of optimal capital taxation by Schulz (2021). This is because the behavioral saving response is likely too 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).[26] 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 includes the core elements needed 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 wealth taxation in such a way that the effect on the marginal and average net-of-tax rates-of-return differ. The model accounts for the fact that the average household is close to retirement and thus faces lower incomes in the near future. I abstract from frictions, but discuss how they may play a role in interpreting the mapping between my empirical findings and the EIS.

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 elasticity of labor supply on the intensive and extensive margins.

$$
\begin{aligned}
\max_{\{c_t, s_{t+1}, l_t\}_{t=0}^T} \quad & \textstyle\sum_{t=0}^T \quad \beta^t \left(\frac{1}{1-\gamma} c_t^{1-\gamma} - \psi \frac{l_t^{1+\nu}}{1+\nu} \right), && (11) \\
\text{s.t. } c_t + s_{t+1} \quad & = \quad y_t + l_t w_t && (12) \\
& + \quad s_t R - wtax_t(s_t).
\end{aligned}
$$

ψ is the (dis)utility weight on labor supply, and β is the time discount factor. Households choose

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

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

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

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

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

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 and mapping to empirical setting*

I set $R = 1.03$.[27] 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 ΔMTR, 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 ΔATR equal to 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 $s'_t(\Delta MTR - \Delta ATR)$, where s'_t is the savings of the 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, $\frac{1}{\nu}$, 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%.[28]

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

[27] Fagereng et al. (2020) 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.

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

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.[29]

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 Appendix Figure B.3). We see that the cutoff for when we see a change in the sign of the saving response is around 0.40. This is lower than the canonical cutoff 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.

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.[30] On the higher end, Jakobsen, Jakobsen, Kleven, and Zucman (2020) find that the implied EIS from responses to wealth taxation in Denmark ranges from 2 to 6. Jakobsen et al. (2020) note, however, that their EIS may be inflated by changes in evasion or avoidance behavior.

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 that I average over time.[31] Interestingly, labor earnings responses are almost as sensitive to the EIS as the

[29] Absent any mortality risk, this roughly corresponds to (1) assuming that the bequest elasticity equals the EIS, and (2) that the strength of the (warm-glow) bequest motive ensures that households wish to bequeath an amount large enough to finance their own planned consumption for 15 years. If instead agents ended their life-cycle at age 85 with zero residual assets, income effects would be weaker, and even lower values of the EIS would be needed to obtain simulated treatment effects consistent with the confidence intervals on my empirical findings.

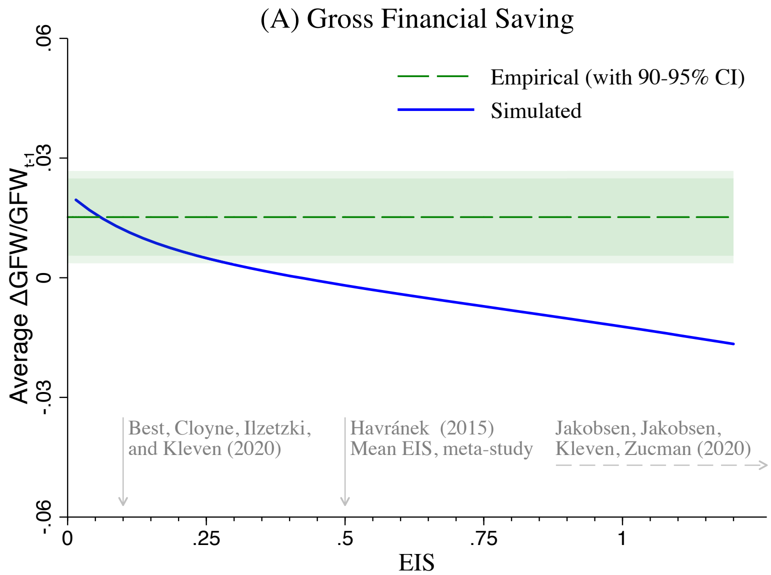
[30] See also, e.g., Attanasio and Weber 1995; Gruber 2013; Vissing-Jørgensen 2002; Bonaparte and Fabozzi 2017; Crump et al. 2015; Cashin and Unayama 2016; Calvet et al. 2021

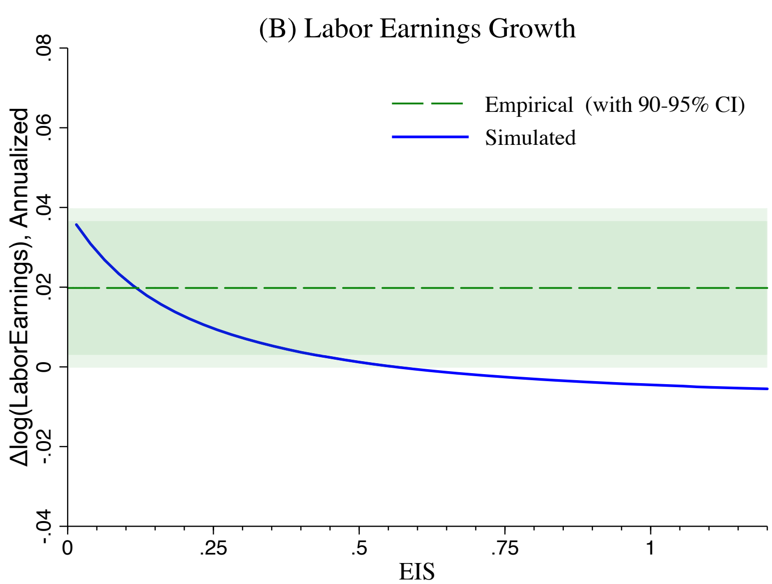
[31] In the simple model used for simulating treatment responses, labor supply responses are immediate. This is because labor supply is determined through the intratemporal first-order conditions, which leaves the level of labor earnings log-proportional to consumption. The adjustment to increased taxation thus comes immediately as the level of consumption is decreased. This differs from my empirical findings, in which household labor earnings growth is affected smoothly across time. This may be caused by households readjusting at different

savings responses. The EIS cutoff 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.

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 Appendix 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.





Appendix Figure B.12 shows that the exact choice of the Frisch elasticity does not have a

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.

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 on financial saving 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 adjust consumption in the short-run would make it more costlier, in a utility sense, to save more. This may explain why labor earnings responses account for a large part of the saving effect but cannot 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 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 $\widetilde{TNW_{i,2009}}$, 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. See Appendix B.10 for the empirical specification and Appendix Table B.1 for the underlying first-stage and reduced-form coefficients. 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 debt.

	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	0.7478	-2.7466
	(4.7364)	(0.5680)	(3.3506)
ATR^{GFW}	5.9364*		
	(3.1937)		
ATR^{W}		4.9303*	21.0731*
		(2.8386)	(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.[32] Households initially below the threshold should largely see MTR effects while households initially above the threshold should 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.[33] 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 $TNW_{i,2009} > -0.25$ MNOK 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 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*

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 treated households sell their home, which would be the first (albeit costly) 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

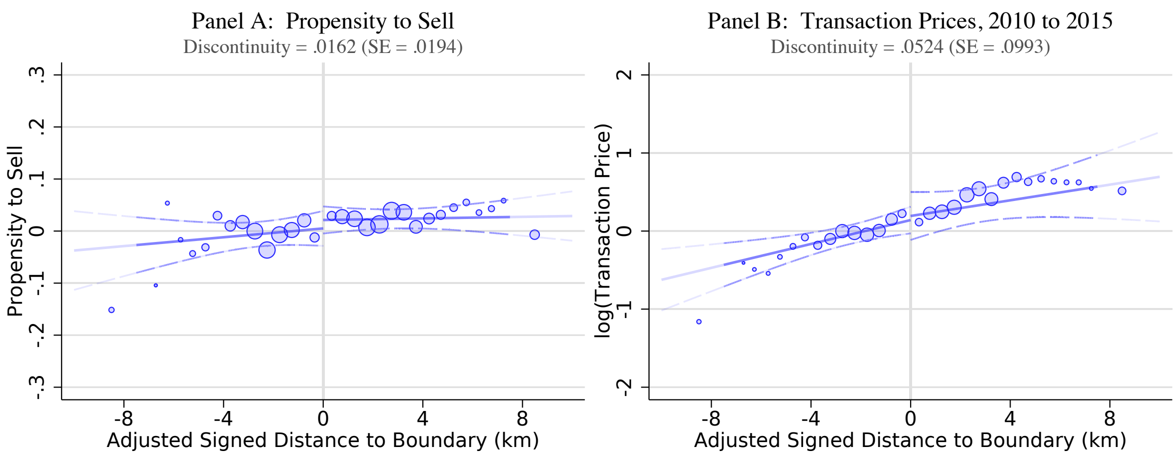
[32] Gruber and Saez (2002) do this in the context of income taxation: A reduction in income-tax thresholds affect the marginal income tax rate primarily for those ex-ante below the threshold, while those above see a reduction in their average tax rates.

[33] That 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.

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.

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.



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. Beyond this, capital taxation may have positive spillover effects on income taxation. Nevertheless, Wealth taxation, and capital taxation in general, may have important general equilibrium effects or effects that operate through the corporate sector that are not considered in this paper.[34] To account for this, 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 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 et al. 2019) and migration (Agrawal et al., 2020).

My results on the savings effects of wealth taxation are qualitatively different from the main findings in the existing empirical literature. The likely explanation is that my empirical setting, with largely third-party reported measures of savings, comes closer to estimating real 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.[35] While Jakobsen et al. (2020), for example, find strong negative effects on taxable wealth, their wealth measure presumably consists largely of self-reported wealth.[36] In addition, their sample consists of very wealthy households (in the top 1% to 2% of the wealth distribution) who likely had 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). Timing likely matters as well. The opportunities to evade wealth taxation has likely declined substantially in the decades following the Danish 1988 reform.

In terms of external validity, finding a positive effect of wealth taxes on saving is unlikely driven by characteristics specific to Norway. If anything, generous pension and social insurance programs should create an environment in which income effects are weaker and more easily dominated by the substitution effects. It is further unclear why ultra-wealthy households would respond differently than the moderatlet wealthy households in my sample. The ultra-wealthy households are not subject to a "human wealth effect" that work against income effects. In a pure-capitalist model, positive responses only require that the EIS is below 1 (Straub and Werning, 2020), as opposed to 0.5 in my calibrated model. Furthermore, as I discuss in Section 4.3, the EIS of 0.1 found by Best, Cloyne, Ilzetzki, and Kleven (2020) in the U.K. would also produce positive saving responses.

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, constrained households, as in Wong (2019), for whom the cash-flow and housing channels are

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

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

[36] In 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.

likely important (Flodén et al. 2019; Hedlund et al. 2017).

Data Availability Statement

The replication package for this article is available on Zenodo at https://doi.org/10.5281/zenodo.13851211.

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