The Incidence and Efficiency of Land Value Taxation*

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PRELIMINARY. COMMENTS GREATLY APPRECIATED.

Abstract

Land value taxes are often seen as particularly desirable because the fixed supply of land implies no efficiency loss from taxation, with the entire tax burden falling on current landowners. We study the incidence and efficiency of land taxes using a unique quasi-experiment that generated persistent variation in land tax rates across Danish municipalities. In contrast to the predictions of standard, neoclassical models, we estimate a precise zero effect of land taxes on residential home prices. The precision of our estimates allows us to confidently rule out full capitalization of taxes into home prices using leading estimates of housing discount rates. Our results imply that the burden of land taxes is shared with tenants and future purchasers. We also estimate null effects of land taxes on measures of housing development, mobility, and homeownership, though we do find that older homeowners sort away from high tax areas. Our results are consistent with limited efficiency costs of land value taxation but imply that land taxes are more regressive in our setting than predicted by standard models.

JEL Classification: H21, H22, R31

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We do need to provide for certain essential government functions... So the question is, which are the least bad taxes? In my opinion the least bad tax is the property tax on the unimproved value of land, the Henry George argument of many, many years ago.

Friedman (1978)

1 Introduction

Governments need tax revenue to provide public services, but commodity taxes induce costly economic distortions. It is well understood that taxes on inelastic commodities reduce the deadweight loss from taxation because distortions in *relative prices* cause smaller changes in *relative quantities*. As a result, economists and other social scientists have long recognized that taxes on the value of land can be uniquely efficient. Because the supply of land is thought to be fixed, taxes on land should not reduce the quantity of land; thus, there should be zero efficiency cost of taxation.[1]

Taxes on land also have a unique place in intellectual history, having been a subject of active debate by philosophers and social scientists for centuries. The Physiocrats discussed taxes on land rents before modern economics emerged as a discipline. The efficiency arguments for land taxes were well understood by Smith (1776), and the desirability of land taxes has been the subject of discussion by Marx, Hayek, Friedman, Vickrey, and many others. Land taxes today are most prominently associated with George (1880), who argued for a single land tax in lieu of other commodity taxes, and whose work inspired political movements in both the United States and around the world.[2] Though they are less common in practice than traditional property taxes, land taxes are also the subject of an active policy debate. Detroit is considering a proposal to transition the property tax to a land tax, and California and Colorado are considering or studying similar proposals.

Despite this historical interest and contemporary policy debate, we have relatively little modern empirical evidence about the effects of land taxes. In this paper, we use a natural experiment in Denmark to study individual and aggregate responses to land taxes. We exploit a municipal reform that forced nearby municipalities with distinct land tax rates to set new, unified rates. These new rates varied across municipalities but were constrained by a formula created by the central government. As a result, bordering municipalities

[1] Though recent efforts to reclaim land in places like the Netherlands and Denmark suggest the supply curve is not perfectly inelastic over the long term.

[2] Denmark, which is the setting for this paper, has a political party centered on the work of Henry George, which held seats in Parliament as recently as the 1950s.

experienced very different changes in their land tax rate. Based on this formula, we construct an instrument for the change in the tax rate experienced by these neighboring municipalities using historical data from a decade before the reform. Importantly, because these bordering municipalities were constrained to set unified rates going forward, the differential changes experienced by these bordering municipalities as a result of the reform were permanent. Because this reform happened almost two decades ago, we can investigate both the short- and long-term responses to tax changes. We compare market and aggregate outcomes across these bordering municipalities to identify the effects of land taxes using an instrumental variables (IV) difference-in-differences strategy.

We first build a neoclassical model with land taxes to guide our empirical analysis. Our model highlights a critical assumption which drives how individuals respond to land taxes: how the government spends tax revenue. When tax revenue is fully refunded to incumbent homeowners, a land tax does not affect any outcome except the asset price of land. A decrease in the purchase price for land offsets land tax payments. The full response to the tax comes through price changes, and the incidence falls entirely on the incumbent landowner who is then fully compensated by the government. In contrast, if households are not refunded their tax payments, behavioral responses emerge due to income effects. Because the household is poorer due to unrebated tax payments, they demand less of all normal goods, including leisure, buildings, and non-durable consumption.

Guided by the theoretical model, we focus on two main sets of empirical results. First, we study the effects of land taxes on the prices of residential property. The model states that taxes must be capitalized into property prices. Because quantities cannot adjust to clear the market, prices must change instead. We use two sources of variation to study price capitalization. First, we compare homes in nearby bordering municipalities that experienced distinct changes in land value tax rates due to municipal reform (which we refer to as the "policy shock"). Second, we compare homes that are differentially exposed to changes in land tax rates because they have different levels of land intensity (which we refer to as "exposure"). While land taxes are levied on land value alone, we only observe prices for the entire housing unit, including land and structures. However, because different housing units have different land intensities, they should experience different price changes in response to the same policy shock. For example, for an empty plot of land, 100% of the value of the property comes from land, and thus, the land tax will be equivalent to a tax on the value of the entire property. In contrast, in the case where the land and the structure are owned separately, such as a ground lease, the value of the structure will be completely distinct from the value of the land it sits on. Thus, the land tax should have no effect on the structure price. While there are relatively few of these extreme cases in the data, there is significant variation in the land intensity of different housing units. Because land intensity depends on endogenous development decisions, we instrument for exposure using historical land intensity at the plot level.

In stark contrast to the predictions of the theoretical model, we estimate precise null effects of land taxes on housing prices. We use a repeat-sales index, where we compare the transacted prices of the same home over time, before and after the tax change. In our baseline, IV specification, our point estimate is very close to zero and statistically insignificant. The degree to which taxes should affect housing prices depends on the interest rate households use to discount housing cash flows (or, alternatively, the rent-price ratio). In public finance, it is standard to assume a discount rate between 3 - 5%. Recent work has also used institutional features of housing markets to estimate discount rates: Giglio, Maggiori and Stroebel (2014) and Bäcker-Peral, Hazell and Mian (2024) estimate discount rates of 1.9% and 2.8 - 5.3% respectively in the U.K. housing market. He et al. (2024) estimate housing discount rates of 2.4%, while Koster and Pinchbeck (2022) use variation in property taxes to estimate discount rates between 3-4%. Finally, in Denmark, the setting of our study, Jordà et al. (2019), estimates that rental yields in the most recent period are approximately 3.4%. Our baseline estimates rule out all of these estimates with a high degree of confidence. The lower bound of our 95% confidence interval rules out discount rates below 8%.

Our findings have significant implications for the relative desirability of taxing land as opposed to other commodities. A key feature underlying the efficiency properties of land taxes is that the land tax burden is unaffected by behavioral responses. If taxes are fully capitalized into land prices, landowners cannot avoid land tax increases by selling their land: future purchasers of the land are fully compensated for their higher tax burden with a lower purchase price today. In contrast, when land taxes are not capitalized into prices, as in our setting, incumbent homeowners can avoid taxes by selling to new purchasers, which affects the allocation and efficiency of housing. Because we find land taxes are not capitalized into home prices, our results suggest that taxes on land may be less efficient than in theoretical models. Similarly, when taxes are not capitalized into land prices, the tax burden does not entirely fall on incumbent landowners. Instead, a lack of capitalization implies that the economic incidence of the tax is equivalent to the statutory incidence. This means that the tax burden is shared between current landowners and future purchasers.

Beyond the focus on land taxes directly, our setting is ideal for studying the capitalization of taxes into housing more broadly. First, price capitalization depends not just on contemporaneous prices but rather on expectations of future tax changes. Thus, naive regressions of prices on taxes will be biased downwards if tax changes are *expected* because prices will have already adjusted to the tax change. Similarly, if the market expects the tax change to be transitory, it will have smaller effects on prices, which can be interpreted as a lack of capitalization. We study a single unexpected reform that generated heterogeneous tax changes across space. Thus, we can carefully follow the narrative record and test whether prices adjusted during periods before the statutory tax rate changed when the reform was announced and debated. We find no response in either the period where the reform was being discussed or after the tax rate changed. In addition, because of the nature of the municipal reform we study, the quasi-experimental changes in tax rates we used were permanent. The effect of the policy shock instrument on effective tax rates is identical for each year in our sample period from 2007, when tax rates changed, through 2019.

Second, if households value the municipal services purchased with higher tax revenue, the effect of taxes on prices will be confounded by the effect of changes in municipal service levels on prices. Two aspects of our setting allay this concern. First, in our setting, land taxes are a small share of the municipal budget, and the reform we study was explicitly formulated not to change service levels. Second, in a robustness check, we use only within-area variation from differential exposure to land tax changes. Thus, we compare price changes among homes that are entitled to the exact same level of municipal services but differentially affected by land taxes because they have different land shares. Though less precisely estimated, we once again estimate zero effects that imply less capitalization than the baseline estimates, suggesting municipal service changes do not confound our estimates. Finally, the degree of price capitalization depends on the slope of the housing supply curve. As a result, disagreements about the extent of price capitalization may arise if there are imprecise estimates of the housing supply elasticity. Because land is more inelastically supplied than housing, uncertainty about the housing supply elasticity is less relevant for interpreting price capitalization.

Theoretically, land taxes should generate few behavioral responses. In the model, land taxes generate no substitution effects but can generate income effects if households are not compensated for the taxes that they pay. In our setting, however, behavioral responses may be more relevant because the lack of price capitalization implies households can avoid taxes by moving or selling their property. In addition, the behavioral responses to land taxes have been a key part of the ongoing policy debate about the desirability of land taxes. In particular, land taxes are viewed as a potential substitute for traditional property taxes because land taxes, unlike property taxes, do not disincentivize increased development. Some public commentators in the Georgist tradition even argue that higher land taxes alone can increase development if property owners need to generate additional cash flow to cover higher tax payments or by reducing the purchase price of land, which might improve land allocation in the presence of financial frictions.

We use the variation caused by the policy shock to study whether land taxes have long-run, aggregate effects on property development, mobility, and housing allocation. Because these variables are slow-moving, the long-term, persistent nature of our shock is particularly valuable. We estimate precise null effects on aggregate residential development. Our estimates imply, consistent with the theoretical benchmark, that land taxes do not disincentivize property development. This means that substituting land taxes for property taxes might be an effective way to increase aggregate density, though the quantitative magnitude depends on the slope of the housing supply curve. We also focus on the development of empty plots, as much of the public discussion of land taxes has focused on whether the shift to land taxes might increase the development of empty plots. Again, we find null effects on the rate of development of undeveloped land plots. Overall, we find that higher land taxes do not affect aggregate development. This suggests that the move to land taxes and away from more distortionary taxes may encourage property development. However, we also provide evidence against the optimistic viewpoints expressed by George (1880) and public commentators that higher land taxes alone might incentivize development.

Finally, we study the effect of higher taxes on housing allocation. Theoretically, land taxes should not affect homeownership unless incumbent landowners respond to income losses from higher taxes by selling their homes. When taxes are not capitalized into land prices, however, homeowners can move to avoid higher tax payments. In addition, a perennial concern about property taxes is that liquidity-constrained homeowners may have to move if they cannot meet their annual tax obligations. Publicly, much of this fear is especially focused on elderly retirees that are not in the labor force. We estimate precise zero effects of land taxes on the probability that homeowners sell their existing property. However, we find that land taxes change the composition of homeowners in an area. In particular, we find that areas that experienced tax increases had fewer older homeowners in the years after the tax change. Taken together, we interpret this evidence as suggesting that higher land taxes do not push incumbent homeowners out of their homes but that older individuals are less likely to move to high tax areas.

In the final part of the paper, we discuss potential drivers of the deviations we document from the neoclassical benchmark. One potential explanation for our result is that landlords pass through increases in land taxes to tenants. While we do not have direct evidence on rent increases during this period, pass-through of *property taxes* to rental prices has been documented in a variety of different settings (Löffler and Siegloch, 2021; Baker, 2024;

Watson and Ziv, 2024). In addition, rents in Denmark are regulated and the most common types of rent control specifically allow tax increases to be passed through to rents. We show in an extension of our model that when rents are capped but taxes can be passed through to tenants, the price of land no longer depends on the tax rate. However, this implies that the incidence of the tax falls on tenants, rather than landowers. Because landowners earn higher incomes and have more wealth than tenants and future home purchasers, the tax is significantly more regressive than implied by standard models. Overall, our results suggest that land taxes are less desirable on both efficiency and equity grounds than the neoclassical benchmark. In addition, our work provides evidence in the context of property and capital taxes that demand and supply elasticities are not sufficient statistics for tax incidence (Auerbach, 2006; Benzarti, 2024). Instead, our work highlights how institutional factors and interactions with other regulations may shape tax incidence.

Altogether, our work sheds light on the real effects of land taxes. We find evidence that contrasts with the stark predictions of neoclassical models. In particular, we find strong evidence that tax increases are not capitalized into housing prices. However, we also see that higher land taxes do not disincentivize development but do affect the allocation of housing within a local area. Overall, we interpret our evidence as suggesting that land taxes have limited efficiency consequences but that the tax incidence does not fall on incumbent landowners. Instead, tax payments fall on tenants and are shared between incumbent and future homeowners.

1.1 Related Literature

Our project is related to two main literatures. First, our project contributes to the literature on property taxation (Oates, 1969; Rosen, 1982; Löffler and Siegloch, 2021; Koster and Pinchbeck, 2022; Brockmeyer et al., 2024; Wong, 2023; Baker, 2024; Horton, 2024; Kopplin, 2024; Wong, 2024) and land taxation(Bonnet et al., 2021; Schwerhoff, Edenhofer and Fleurbaey, 2022). We add to this literature by studying a unique natural experiment that led to persistent changes in land tax rates across space and time to study the effects of tax rates over the long run on prices and aggregate outcomes. In contemporaneous work, Murphy and Seegert (2024) create a measure of "implicit" land taxes based on differences between assessed values and market valuations to show that higher implicit taxes are associated with a variety of economic outcomes, including density. Coven et al. (2024) use a theoretical model to argue that property taxes can change housing allocation by increasing homeownership, especially for financially constrained households. In contrast, we use a quasi-experimental variation in statutory tax rates to estimate the causal effect of land taxes, providing new evidence on these important questions of homeownership, density, and housing allocation.

Two papers study the short-term effects of the same reform on home prices (Nielsen and Rzeźnik, 2014; Høj, Jørgensen and Schou, 2018). Relative to both papers, we use a different empirical strategy over a longer time horizon, which results in substantial differences between our estimates. We discuss differences between our estimates and previous work in Section 7. We also expand on this previous work by looking at longer-term effects, heterogeneity in the price results, and real effects on aggregate outcomes.

Second, we relate to a long literature on tax incidence. Benzarti (2024) reviews the literature on the incidence of consumption and labor taxes and argues that, in contrast to theoretical models, the demand and supply elasticities are not sufficient statistics for tax incidence. We show that similar results hold in the case of taxes on real property, in this case for land. Benzarti (2024) argues that non-standard factors, like fairness norms, shape the incidence of labor taxes in particular. In the final part of the paper, we argue that institutional regulations related to Danish rental markets may provide an explanation for the limited price capitalization we estimate. As a result, we also show how regulations and institutions can be important drivers of tax incidence.

The rest of the paper is organized as follows. Sections 2 and 3 lay out our theoretical model and present a series of empirical predictions. Section 4 presents institutional context about the reform we study and Danish land taxes. Section 5 outlines the data we use, while Section 6 describes our instrument and identification strategy. Sections 7 present the results of taxes on home prices. Section 8 shows the effect on aggregate development and housing allocation. Section 9 discusses potential drivers of the main results and concludes.

2 Model

We build a neoclassical model of land-value taxation to guide the empirical analysis and highlight the unique theoretical properties of land taxes.

2.1 Households

Households maximize utility derived from nondurable consumption (C_t), housing services (H_t), and leisure over an infinite horizon. Housing services are an aggregate of land and buildings owned by the household ($H_t = H(L_t, B_t)$). Individuals also invest in a real financial asset (A_t) and receive lump-sum transfers from the government (T_t). The government levies a tax (τ^L) that is paid each period based on the price of land. The stock of buildings depreciates at a rate δ. Formally, the household problem can be written as:

$$U_0 = \sum_{t=0}^{\infty} \beta^t u(C_t, N_t, H_t)$$

The household optimizes while respecting a series of flow budget constraints:

$$C_t + P_t^L(L_t - L_{t-1}) + P_t^B(B_t - (1-\delta)B_{t-1}) + (A_t - A_{t-1}) = W_t N_t + r_{t-1}A_{t-1} - \tau^L P_t^L L_{t-1} + T_t \tag{1}$$

for $t = 0, 1, \ldots$, and given initial values L_{-1}, B_{-1}, A_{-1}. Consumption is the numeraire. P_t^L and P_t^B are the asset prices of land and buildings, respectively. The first order conditions for labor and consumption are standard. The first order conditions for the land and building holdings of the household are given by the following expression:

$$\frac{u_{L,t}}{u_{C,t}} = P_t^L - \frac{(1-\tau^L)P_{t+1}^L}{1+r_t} \tag{2}$$

$$\frac{u_{B,t}}{u_{C,t}} = P_t^B - \frac{(1-\delta)P_{t+1}^B}{1+r_t} \tag{3}$$

Equation 2 shows that the household will purchase land until the marginal utility of land consumption (left-hand side) equals the **user cost of land** (right-hand side). Because land is a durable good, the allocative price of land is not the asset price (P_L), but rather the cost of holding a unit of land for one period and then selling it. This cost is increasing in the land tax rate and in the interest rate (which represents the opportunity cost of investing in financial assets relative to land) and is decreasing in any capital gains.

Suppose that $r_t = r$ is constant (for expositional purposes). Then, based on the first order condition of the household, we can solve for the asset price of land. Denote the marginal rate of substitution for land: $MRS_t^L = \frac{u_{L,t}}{u_{C,t}}$. Then, we have that the asset price of land P_t^L is given by $P_t^L = MRS_t^L + \frac{(1-\tau^L)}{1+r} P_{t+1}^L$. Solving the equation forward results in:

$$P_t^L = \sum_{s=0}^{\infty} \left(\frac{1-\tau^L}{1+r}\right)^s MRS_{t+s}^L \tag{4}$$

Thus, the asset price of land is the sum of future expected marginal rates of substitution, discounted by the interest rate and the land value tax rate. Note that this is a standard asset pricing condition, where the price is the discounted sum of future cash flows. In a model with renters, this would be equivalent to the price of land equalling the discounted sum of future rents generated by the land.

2.2 Production

Consumption goods are produced by a firm that hires labor and produces according to the following production technology:

$$Y_t = F(N_{Y,t}) \tag{5}$$

Similarly, there is a construction firm that produces new buildings I_t by hiring labor and producing using the following production function:

$$I_t = G(N_{I,t}) \tag{6}$$

Both firms are price takers in both input and output markets and maximize profits with the following equations for the consumption ($\Pi_t^Y = Y_t - W_t N_{Y,t}$) and construction firms, ($\Pi_t^I = P_t^B I_t - W_t N_{I,t}$). Their first order conditions for production are given by:

$$F_N(N_{Y,t}) = W_t \tag{7}$$

and

$$P_t^B G_N(N_{I,t}) = W_t \tag{8}$$

2.3 Government

The government collects tax revenue $\tau^L P_t^L L_{t-1}$ at the beginning of each period and pays lump-sum transfers T_t to households. Many of the model's key results are driven by how the government spends its collected tax revenue. As a result, we consider two cases. In the first case, the government fully rebates all the tax revenue back to landowners as a lump-sum transfer: $T_t = \tau^L P_t^L L_{t-1}$. We refer to this as the "full compensation" case. In the second case, the government does not return tax revenue to the landowners. To nest both of these cases, we write the lump-sum transfer as:

$$T_t = \tau^L P_t^L L_{t-1} \times \mathbb{I}(\text{Comp.}) \tag{9}$$

where $\mathbb{I}(\text{Comp.}) = 1$ if the government compensates landowners and zero otherwise.

2.4 Market Clearing

We impose four market-clearing conditions:

$$N_t = N_{Y,t} + N_{I,t} \tag{10}$$
$$A_t = 0 \tag{11}$$
$$L_t = \bar{L} \tag{12}$$
$$B_t = I_t + (1-\delta)B_{t-1} \tag{13}$$

Equation 10 is the labor-market clearing condition. Labor supply equals the combined demand for labor from the consumption and construction firms. Equation 11 is an asset-market clearing condition, which states that the financial asset is in zero net supply. Equation 12 is the land-market clearing condition. Our key assumption is that the supply of land is inelastic and endowed by nature. Thus, demand for land from the household has to equal $\bar{L}$, the fixed supply of land. Equation 13 states that building demand equals the supply of new construction produced by the construction firm, plus the non-depreciated stock of buildings from the previous period. The market for non-durable goods clears by Walras' law.

2.5 Equilibrium

Given an initial allocation for (L_{-1}, B_{-1}, A_{-1}), an equilibrium is prices (P_t^L, P_t^B, W_t, r_t) and quantities $(C_t, Y_t, N_t, N_{Y,t}, N_{I,t}, L_t, B_t, I_t, A_t, T_t)$ such that the household optimizes, the buildings and consumption firms optimize, the government collects taxes and rebates transfers, and markets clear.

2.6 Special Case

To highlight the analytical predictions of the model, we make a series of functional-form restrictions. Assume utility is given by:

$$u(C_t, N_t, L_t, B_t) = \log C_t - \vartheta \frac{N_t^{1+\frac{1}{\chi}}}{1+\frac{1}{\chi}} + \theta \log L_t + \varphi \log B_t.$$

In addition, assume the production functions for non-durable consumption and buildings are given by $F(N_{Y,t}) = Z_Y N_{Y,t}^{\gamma}$ and $G(N_{I,t}) = Z_I N_{I,t}^{\eta}$, respectively.

3 Theoretical Predictions

We use the model outlined in Section 2 to highlight how land taxes affect equilibrium prices and quantities.

3.1 The Effect of Land Taxes on Land Prices

Higher land taxes make holding land less desireable. However, because land is in fixed supply, the equilibrium quantity of land cannot fall as a result of higher taxes. As a result, all of the adjustment to higher land taxes **must come through changes in the price of land**. In steady state, the asset price of land is given by the following expression:

$$P^L = \frac{1+r}{r+\tau^L}\theta \bar{L}^{-1}C \tag{14}$$

The asset price of land is decreasing in the tax rate τ_L. This effect is highlighted in Figure 1, which shows supply and demand in the market for land after a land tax increase. The line labeled D shows the demand for land prior to a tax increase, while D' shows the demand for land after a tax increase. As tax rates go up, higher taxes are *capitalized* in the price of land. That is, higher taxes reduce the price of land such that future landowners are fully compensated for future higher tax payments with a lower purchase price today.

Since the supply curve for land is vertical, the change in the *asset price* of land does not change the quantity of land in equilibrium. As a result, there is no deadweight loss of higher taxes, implying no efficiency cost of land taxes. In fact, when the tax is fully compensated, the tax rate has no affect on any aggregate quantity or price except for the asset price of land.

3.2 Income Effects

When taxes are not fully rebated to households, income effects cause behavioral responses. We focus on the effects of land taxes on labor supply. The following expression gives the steady-state level of labor:

$$N = \vartheta^{-\frac{1}{1+\frac{1}{\chi}}}\left(1 + \frac{1+r}{r+\delta}\varphi\delta + \frac{1+r}{r+\tau^L}\theta\tau^L(1-\mathbb{I}(\text{Comp.}))\right)^{\frac{1}{1+\frac{1}{\chi}}} \tag{15}$$

Equation 15 highlights how the government's redistributive preferences drives the effects of land taxes on equilibrium prices and quantities. Consider the case where the government fully compensates landowners by returning the land tax payments as a lump sum. In that case, the steady-state labor supply is given by:

$$N = \vartheta^{-\frac{1}{1+\frac{1}{\chi}}} \left(1 + \frac{1+r}{r+\delta}\varphi\delta\right)^{\frac{1}{1+\frac{1}{\chi}}}$$

When land taxes are fully rebated, the tax rate has **no effect on equilibrium labor supply**. Labor supply depends only on the parameters that determine household disutility of labor and household demand for buildings. The expression highlights the tradeoff faced by households in the model: the optimal quantity of labor balances household demand for consumption goods and buildings with leisure. Household preferences over buildings matter for period-by-period labor choices because buildings depreciate over time, and households must cover the cost of maintaining the housing stock. In contrast, consider the case where the government spends the land tax revenue on services that do not enter the household utility function. Then, the labor allocation is given by:

$$N = \vartheta^{-\frac{1}{1+\frac{1}{\chi}}} \left(1 + \frac{1+r}{r+\delta}\varphi\delta + \frac{1+r}{r+\tau^L}\theta\tau^L\right)^{\frac{1}{1+\frac{1}{\chi}}}$$

When taxes are not remitted back to the household, labor supply is increasing in the tax rate. This is a wealth effect on labor supply. Households' work decisions now **also** reflect the tradeoff between leisure and covering land tax payments. Similar tradeoffs affect the allocations of other normal goods in the model: higher land taxes reduce the demand for consumption of non-durable goods, the demand for housing, and the demand for land. Because the supply of buildings and consumption goods is upward-sloping, this also reduces equilibrium quantities of both goods.

In contrast, because land supply is fixed, the quantity of land is invariant to the tax rate. Because land is a normal good, like leisure or consumption, household demand for land decreases due to wealth effects. However, when taxes are not returned to owners, income effects can reduce land demand, further depressing land prices. This effect is highlighted by Equation 14: when there is no income effect, the **user cost of land** does not change in response to a change in taxes $UC^L = \theta\bar{L}^{-1}C$ because C is invariant to the tax rate. In contrast, when taxes have income effects, households reduce C in response to higher taxes, which results in the user cost falling in response to higher tax rates. In practice, in the presence of income effects, prices must fall more to make the household indifferent between land consumption and non-durable consumption, which now has a higher marginal utility. This puts further downward pressure on P_t^L in response to a land tax increase.

Taken together, our model highlights two main effects of land taxes. First, higher tax rates should reduce the price of land (and thus housing) as the inelastic land supply implies that taxes should be fully capitalized into land prices. Second, if taxes are not rebated to landowners, or if the government spends tax revenue on services that do not enter the household utility function, there will be behavioral responses due to income effects. In particular, households should reduce their consumption of normal goods: buildings, non-durable goods, and leisure. In addition, income effects can also reduce demand for land, which will put further downward pressure on land prices.

4 Institutional Details

We study a municipal reform in Denmark that generated permanent, quasi-experimental variation across towns in land tax rates.

4.1 Municipal Reform

Before 2007, Denmark had three primary levels of government: the central government, the counties, and municipalities. Municipalities are the smallest level of government and are responsible for a variety of public services. Most prominently, the municipalities provide primary schooling and elder care, though they also have some responsibility for infrastructure and other social and child care services. At the time of the municipal reform, the thirteen counties were primarily responsible for healthcare services.

In 2002, discussion of a potential reform to consolidate the subnational governments into larger entities began. Both Christiansen and Klitgaard (2008) and Christiansen (2012) date the beginning of the discussion of the reform to the Summer of 2002, when newspaper articles indicated support for reform among both Denmark's largest business organization and younger members of a governing party at the time. The primary motivations for the reform were to improve public sector service provision through economies of scale, especially in the hospital sector, where it was thought that the counties were too small to be effective. Crucially, the goal of the reform was not to change **the level** of municipal services. Before the increased discussion of the reform, a municipal reform of this type was considered extremely unlikely. Christiansen and Klitgaard (2008) write that:

> "There is no shortage of arguments that, before it is actually implemented, the structural reform must indeed be considered almost unthinkable...The unthinkable, as is well known, happened."

Appendix Figure A1 displays the time series of references to structural reform in major Danish newspapers. Before 2002, there were very few mentions of structural reforms. This changed in the summer of 2002 when the number spiked. Structural reform was also not part of the debate during the general election in 2001 (Bundgaard and Vrangbæk, 2007).

In August of 2002, the Prime Minister announced that the government intended to appoint a commission to study the administrative structure of the Danish state (Bundgaard and Vrangbæk, 2007). In January 2004, the commission presented its report. The report included proposals similar to the municipal reform that was eventually adopted, including proposals eliminating the counties and requiring larger municipalities. Between April and June 2004, the government presented a proposed reform, which was debated and negotiated between parties. In June 2004, a law implementing the structural reform was passed.

The structural reform required the creation of larger municipalities, which were constructed through municipal mergers. The counties were eliminated and replaced by five regions, primarily responsible for the healthcare sector but also for some tasks that would benefit from less decentralization (such as regional development and critical infrastructure). The larger municipalities retained most of their responsibilities but took over some that were previously the purview of the counties.

Once the reform passed, individual municipalities had leeway in determining which other municipalities to merge with. The government required a baseline population level for new municipalities and that merging municipalities bordered each other. Even before the agreement by the central government, several municipalities had announced their merger plans, and most of the remaining municipalities were very close to merger agreements (Lassen and Serritzlew, 2011). By 2005, the mergers were finalized, and local residents had voted for representatives for the new merging municipalities. The reform took effect in January of 2007.

4.2 The Reform Changes Land Tax Rates

Denmark has a land tax set at the municipal level.[3] The land tax is one relatively small source of financing for municipal spending. In practice, most local tax revenue comes from income taxes. Municipalities also receive significant financing from the central government through grants and an equalization scheme. Before the reform, the overall land taxes paid by households ranged from around 1.6% to 3.4% of the assessed value of land annually. Of this, 1% was used to fund services provided by the county, while the rest was used to

[3]There is also a national-level property tax, which was not affected by the reform.

fund municipal services. After the reform, the municipalities kept all of the tax revenue generated by the land tax.

The municipal reform generated substantial variation in land taxes because merging municipalities that previously set distinct land tax rates were now required to set a unified one. The central government also constrained how municipalities could set this new unified tax rate. The government set an upper bound for the new tax rate that was a weighted average of the existing land tax rates in the merging areas (Høj, Jørgensen and Schou, 2018). In practice, most of the new merging municipalities set their new tax rate very close to the ceiling provided by the government (Høj, Jørgensen and Schou, 2018).

Land assessments are the responsibility of the central government and are based on sales of unimproved land and hedonic regressions. The amount that land tax assessments can increase each year is capped. From 2011 until the 2020s, land assessments remained fixed as the government developed a new system for land valuations. However, **taxable** land values continued to increase because the cap had resulted in taxable valuations being well below market values after the increase in home prices in the early 2000s (Klein et al., 2016). There are some exemptions from land taxes, such as for private schools and non-profits. In addition, agriculture is entitled to lower land tax rates and benefits from low property valuations.

There are three primary benefits to using the variation in tax rates induced by the municipal reform. First, by restricting our comparison to nearby municipalities that merged, we can isolate a permanent change in tax rates. Consider two hypothetical towns that merged into a single municipality, in which one town had previously set a 1% tax rate, and the other had previously set a 2% tax rate. The government constrained the newly merged municipality to set their tax rate at 1.5%. Households in one town experienced a 50 basis point (b.p.) tax increase, while the other experienced a 50 b.p. decrease. The 100 b.p. difference between the two towns is held fixed **even if the newly merged municipality chooses to change its tax rate later** because the towns are constrained by the reform to set a unitary tax rate going forward. Suppose that later on, the new municipality chooses to increase its tax rate to 2%. Now, one town is at its old tax rate, while the other has experienced a 100 b.p. increase, but the difference remains at 100 b.p. Because price and behavioral responses to capital tax changes depend not just on contemporaneous policy but also on expectations of future policy, our permanent variation ensures our results are not driven by expectations of future policy changes, that may have occurred subsequent to the reform, and allows us to interpret our effects as the effect of a permanent tax change.

Second, the ceiling set by the central government generated formula-based changes in tax rates. Naive regressions of the effect of property tax changes may be confounded if local economic conditions or financing needs drive both the stance of policy and behavioral responses. We create an instrument for the tax change experienced by an individual municipality based on historical data and the central government formula. This allows us to isolate variation driven by the municipal reform and ensures our results are not confounded by municipal expenditure needs or other local changes in economic conditions or demographics. Third, because asset prices are forward-looking, behavioral responses to tax changes should reflect expectations of future policy. Because we focus on a single reform with a detailed narrative record that generated substantial variation in tax rates, we can carefully test whether expectations of future policy affect asset prices and individual behavior. Finally, municipal reform generated large tax changes in towns that bordered each other. Our empirical strategy studies the effect of land taxes by comparing nearby towns that experienced these differing changes in land tax rates. These nearby towns serve as a natural control group that allows us to carefully control for other cyclical and regional factors that might influence aggregate and individual outcomes.

5 Data

Our analysis combines a variety of Danish administrative registers covering land use and the housing market, as well as administrative data from tax records on individual households.

5.1 Housing Data

We combine a series of data on residential housing in Denmark. We start with data on land and property taxation, which provides information for each property on the assessed value of land and property taxes owed in each year. We calculate effective tax rates for each property by dividing taxes owed in each year by the property's assessed value. We combine this with property-level data from the Danish Housing Census, which contains comprehensive property-level characteristics. We merge this with data on property sales, which provide us with information on the sale date and price of sold properties. To create a consistent panel of properties before and after the reform, we rely on a key that links properties over time. We sum individual property level outcomes, including property development and residential square footage, to the treatment area level to study aggregate effects of the policy. When studying the effect of taxes on prices, we focus on residential properties and drop vacation homes.

5.2 Individual Data

We create an annual panel of individual-level income and wealth using administrative tax data. The data contain detailed information about the amount and composition of income and wealth holdings. Nearly all components of income and wealth are third-party reported (Jakobsen et al., 2024). We use a property ownership registry to link individuals to their properties, which we use to assign treatment. We use the individual data to study whether land taxes change the number and composition of homeowners along observable dimensions like wealth, income, and age.

5.3 Municipal Data

We collect annual, municipality-level data from Statistics Denmark. The data include demographic and economic variables and information about local government services and spending. We use the statutory land tax rates to construct our instrument and define treatment. We also include pre-treatment data on demographic characteristics as controls. Note that because our treatment areas are smaller than municipalities after the reform, we cannot use *municipal-level* variables after 2007.

6 Instrument and First Stage

Our empirical strategy regresses various property-level and aggregate outcomes on tax rates. However, because the change in the tax rate was formula-based, transitory changes in tax policy or population before the reform may have mechanically affected the area's post-reform tax rate. Thus, a potential concern with a simple OLS regression of outcomes on tax rate changes is biased by the effects of other changes immediately before the reform. To account for this concern, we construct an instrument for the change in the tax rate based on historical data more than a decade before the reform. As a result, our empirical results are only based on variation from historical tax rates and populations, and not from changes that occur close to the reform.

Our instrument is based on the formula the central government used to constrain the new land value tax rates set by the merged municipalities. The central government set a ceiling for the new tax rate for each new municipality that was a weighted average of the land tax rates of the existing, merging municipalities (Høj, Jørgensen and Schou, 2018). We construct a predicted value for the ceiling set by the central government for each new municipality (M), using the following formula:

$$\tilde{T}_M = \sum_{i \subset M}^{M} T_{i,1995} \frac{p_{i,1995}}{p_{M,1995}}$$

Here, $\tilde{T}_M$ represents the predicted tax rate ceiling for new municipality M that is imposed by the central government. It is a weighted average of the tax rates in the existing municipalities i that would merge into municipality M, where the weights are municipality i's share of the total population of M in 1995. We then define the instrument at the treatment area (a) level, which is a cross of each old municipality i with a new municipality M.[4] For each treatment area, we define the instrument as the difference between the area's 1995 tax rate and the predicted tax rate ($\tilde{T}_M$), according to the following formula:

$$\Delta\tilde{\tau}_a = T_{i,1995} - \tilde{T}_M$$

Thus, the instrument represents the predicted change in land tax rate experienced by treatment area a, based on historical data from 1995. Figure 2 tests whether the first-stage relationship is strong. Figure 2 plots the following regression specification:

$$\Delta\tau_a = \beta\Delta\tilde{\tau}_a + \eta_{m(a)} + \delta' X_a + e_a$$

In this specification, $\Delta\tau_a$ is the actual change in the statutory land tax rate experienced by treatment area a between 2007 and 2005. The primary coefficient of interest is the tax change instrument ($\Delta\tilde{\tau}_a$). The regression also includes controls at the treatment area level, as well as new municipality fixed effects. Thus, the regression compares bordering towns that merged together, and as a result of the merger experienced heterogeneous changes in land tax rates, mimicking the variation used in the paper.

Figure 2 shows that the first stage is strong: the points are closely bunched around the line of best fit, and the regression coefficient is highly statistically significant. Overall, we find evidence that our instrument is highly predictive of changes in statutory land tax rates.

Figure 3 shows that this change in statutory tax rates led to changes in effective tax rates at the property level. We use the following event study specification:

$$y_{j,a,t} = \sum_{h \neq -1} \beta_h \Delta\tilde{\tau}_a \mathbf{1}_{\{t-h=2005\}} + \gamma_j + \eta_{m(a)\times t} + e_{j,i,t} \tag{16}$$

[4]Note that the treatment areas are not exactly equal to old municipalities i because some old municipalities split apart during the reform.

In this specification, j represents a property, which is assigned to a treatment area a. The outcome variable is the property-level effective tax rate, defined as the taxes owed in year t divided by the property's assessed land value. We plot β_h coefficients, representing the interaction between the treatment instrument and year fixed effects. The regression includes a property fixed effect (γ_j) and new municipality by year fixed effects.[5]

Figure 3 plots the β_h coefficients. For the decade before the reform, trends in land tax rates are relatively flat. In 2007, the year the reform took effect, there is a strong increase in effective tax rates for properties in treatment areas where statutory tax rates increased. The magnitude of the effect is similar to the effect on statutory tax rates, with a 1 p.p. predicted increase in the tax rate corresponding to an actual increase in effective tax rates of around 80 basis points.

Crucially, this effect is persistent throughout the entire sample period. The point estimates and standard errors in the years after 2007 are nearly identical to those in 2007. This highlights that our empirical strategy successfully isolates changes in land taxes that have a strong first stage and are very persistent. This provides a clean interpretation of our price and behavioral results as reflecting the effect of persistent tax changes.

To study the effect on price capitalization, we combine this policy-induced variation with variation in exposure to the land tax change. While all residential properties in a treatment area face the same effective tax rate, the extent to which this passes through to prices depends on the share of the property's value derived from land. Consider a simplified Gordon growth model, with a land tax, that is only levied on the share of the value of the property that is assessed to derive from the land. Then, the effect of land taxes on the housing prices is equal to:

$$\frac{\frac{\partial V}{V}}{\partial \tau} = -\frac{\theta}{r}$$

where τ is the land tax rate and θ is the land share of the property. Thus, property's with higher land shares, will have larger price changes in response to the same change in land tax rates. This is the second source of variation we use to study tax capitalization.

A property's land share is an endogenous variable. Thus, we instrument for the land share with the property's land share in 1996, a decade before the reform. We run the following regression:

$$y_{j,a} = \beta \Delta \tilde{\tau}_a \theta_j + \zeta_a + \psi_j + e_a \tag{17}$$

[5] For computational tractability, we omit the controls interacted with year fixed effects.

In this specification, $y_{j,a}$ represents total land taxes owed, normalized by the total assessed value of the property, at the property level. In this specification the main coefficient of interest is the policy shock $\Delta\tilde{\tau}_a$ interacted with the properties pre-existing land share ω_j. ζ_a is a treatment area fixed effect. Thus, all of the variation in land tax exposure comes from properties that are in the same treatment area, exposed to the same tax rate and municipal services, but with differential exposure to land taxes. ψ_j is a land-share decile fixed effect.

Figure 4 shows the binscatter plot at the property level of total land taxes paid normalized by the total assessed value of the property. Figure 4 shows that there is substantial variation, even within the same treatment area, in exposure to land tax changes. The coefficient is close to 0.5 and highly statistically significant.

Overall, we find substantial variation, both from the policy shock and exposure to the land tax changes, with which to study the effect of land taxes on residential property prices. Figure 5 shows the instrument combining both the policy shock variation and the exposure variation, mimicking the combined instrument from our baseline price capitalization specification. In this case, the regression takes the following form:

$$y_{j,a} = \beta\Delta\tilde{\tau}_a\theta_j + \eta_{m(a)} + \delta' X_a + \psi_j + e_a$$

This regression has the same dependent variable and instrument as Equation 17. However, here, we replace the treatment area fixed effect with a new municipality fixed effect. As a result, we use both variation from the exposure shares, as well as variation in tax rates from the policy shock. Figure 5 shows that, as with the instruments used separately, the first stage is strong, with the instrument being highly predictive of changes in taxes owed normalized by the total valuation. Overall, both the policy shock and exposure shares induced strong changes in land taxes paid as a result of the reform.

7 Capitalization in Housing Prices

The efficiency of land value taxes rests on the assumption the supply curve for land is vertical. While a vertical supply curve implies that the economy does not adjust to the tax with changes in total quantities of land, it also implies that all adjustment to the tax comes through changes in prices. Without price adjustments, incumbent landowners can shift the tax burden to future purchasers of the land. Similarly, limited price effects may reflect the ability of landowners to avoid the tax by shifting tax payments onto tenants.

We use our empirical setting to investigate the effect of land taxes on prices in the residential housing market. We use a repeat-sales methodology to study the effect of land taxes on the prices of homes. Our outcome variable is the log sale price of transacted homes. We run the following regression for our baseline specification:

$$y_{j,a,t} = \sum_{h \neq -1} \beta_h \Delta \tilde{\tau}_a \theta_j \mathbf{1}_{\{t-h=2001\}} + \gamma_j + \eta_{m(a)\times t} + \delta' X_a \mathbf{1}_{\{t-h=2005\}} + \psi_{j\times t} + e_{j,i,t} \qquad (18)$$

Our baseline specifications include a property fixed effect and new municipality by year fixed effects. Thus, our estimator identifies the impact of land taxes by comparing houses in neighboring towns with different tax rate changes pre- and post-reform, and by comparing homes with differing exposure to the land tax through different land shares. By including property fixed effects, we hold fixed time-invariant property characteristics and ensure that the results are not driven by the land tax changing the composition of transacted homes across time. We also include $\psi_{j\times t}$ land share decile by year fixed effects, to ensure our results are not affected by time-varying demand for more vs. less land intensive properties. We normalize the year coefficients relative to 2001, the year before discussion of the reform began.

Figure 6 shows that higher land taxes do not affect residential home prices. The coefficients show the impact of a one percentage point increase in the land tax (normalized by the assessed home price) on sale prices. Between 1995 and 2001, prices of homes in areas that faced increases in their land tax rate experienced very similar trends to areas where land tax rates would eventually fall. All of the coefficients are close to zero and statistically insignificant. In 2002, discussion of the reform began. However, between 2002 and 2006, when details of the reform are finalized, there is no stark change in home prices. All coefficients are close to zero, and only one is (marginally) significant. After 2007, when the tax rates actually changed, there is still no change in home prices. Across all post-period years, the coefficients are close to zero and insignificant.

The line in red plots the post-period coefficient, pooling all post-period years together and comparing it to 2001, the last year before the discussion of the reform began. The post-period coefficient lies directly on top of the zero line. Overall, we estimate precise null effects of higher land tax rates on residential property prices. This suggests that, in contrast to the predictions of the theoretical model, land taxes are not capitalized into home prices. This is consistent with the slope of the demand and supply curves not acting as sufficient statistics for tax incidence, as discussed in the context of labor and consumption taxes by Benzarti (2024).

Figure 7 shows that the results are consistent using either source of identification separately. Panel (A) plots the beta coefficients, using only the policy shock to instrument for the change in the tax rate. The results look nearly identical to the baseline. All but one of the post-period coefficients are statistically insignificant and close to zero. As a result, the overall post-period coefficient is very close to zero. Panel (B) shows the effect after including treatment area by year fixed effects. Thus, this estimate compares homes that are in the same area and have the same municipal services but have different land intensities. Though much less precisely estimated, the effect is again close to zero, and all post-period coefficients are statistically insignificant. If anything, the point estimate is larger, implying less pass-through than in the baseline specification. This provides suggestive evidence, consistent with our empirical setting, that the estimates are not affected by changes in municipal services.

Finally, Figure 8 shows the effect in a sample of single-family homes. Single-family homes are a particularly important sub-sample because they are more land-intensive than most apartments or condominiums and because the vast majority of single-family homes are owner-occupied as opposed to rented. If landlords can pass on cost increases to tenants, this will mute the price pass through. However, because for owner-occupied homes the owner is both the landlord and the tenant, reactions to tax rates may be higher. However, Figure 8 shows that the results are remarkably consistent in the single-family sample. Once again, all post-period coefficients are statistically insignificant and close to zero. Across all samples and identification strategies, we find consistent evidence of zero pass through of taxes to home prices.

Because the right-hand side variable in our event study is the instrument for the change in the tax rate, Figures 6, 7, and 8 show the intent-to-treat effect of land taxes. To quantify the magnitude of the tax effect, we instrument directly for the property-level change in taxes owed divided by assessed land value. The results are shown in Table 1. In the IV specification, we pool together years into an “Anticipation" period (2002 - 2006), which corresponds to the period when discussion of the reform was moving expectations around prices, and a “Post" period (2007 - 2019). Table 1, Column (1) shows the OLS coefficients. The results indicate that a 1 p.p. increase in land taxes (relative to total assessed values) lowers home prices by 3% during the anticipation period and 7% during the post period, though neither coefficient is statistically significant.

Table 1, Column (2) shows the baseline IV specification. The bottom row of the table reports that the IV has an F-Statistic of over 200, implying that the first stage is strong and the instrument meets the relevance requirement for a valid IV. Home prices are unchanged in the anticipation period. In the post period, the coefficient is positive, implying higher taxes increase home prices, though it is very close to zero and statistically insignificant.

Table 1 reports the discount rate consistent with full capitalization using the lower bound of the 95% confidence intervals we estimate. This represents the lowest possible discount rate that lies within our 95% confidence interval. In our baseline specification, we can rule out full capitalization at discount rates that are lower than 8%. This is substantially higher than most recent estimates of the discount rate for housing cash flows. Recent work has carefully estimated this parameter in a variety of settings. Giglio, Maggiori and Stroebel (2014) estimate net discount rates of 1.9%, He et al. (2024) estimate discount rates of 2.4%, while Bäcker-Peral, Hazell and Mian (2024) provide estimates between 5.3% and 2.8%. Most relevant to our work, Jordà et al. (2019) estimate housing returns of 3.4% in Denmark during this period. All of these estimates, except for the earliest estimates from Bäcker-Peral, Hazell and Mian (2024), are outside of our 99.9% confidence intervals. Thus, our estimates rule out price movements consistent with all of these estimates with a high degree of confidence.

Table 1, Columns (3) - (7) show robustness checks for our baseline estimate. Columns (2) and (3) trim outlier observations in the data, with minimal effects on the coefficients or standard errors. Column (4) restricts attention to the single-family home sample. The coefficients are even closer to zero, and all of the previous estimates lie outside our 95% confidence interval. Columns (6) + (7) show the results when using the policy shock and exposure instruments separately. Though somewhat less precisely estimated (especially for the exposure instrument alone), when using the policy instrument alone, we can rule out the estimates from Jordà et al. (2019), Giglio, Maggiori and Stroebel (2014), He et al. (2024), as well as the more recent estimates from Bäcker-Peral, Hazell and Mian (2024). Using the exposure instrument alone leads to larger coefficients, implying less pass-through and providing suggestive evidence that service changes are unlikely to affect our estimates.

However, the literature on property tax capitalization provides less of a consensus on whether changes in taxes are passed through to home prices. In canonical work, Rosen (1982) estimates price effects that are consistent with full capitalization of taxes into prices with discount rates of approximately 14%. Recent work finds varying results: Horton (2024) finds zero effects on home prices, while Koster and Pinchbeck (2022) finds nominal discount rates between 3 and 4%. The persistence of our policy shock, variation from the exposure instrument within municipal boundaries, and the fact that our setting studies land, as opposed to property taxes, makes our setting uniquely suited to study this question.

More relevant to our work is previous work studying the same reform. Høj, Jørgensen and Schou (2018) estimate significant effects of land taxes on home prices. In Appendix C, we provide a detailed comparison of our results. While we use distinct empirical strategies and different sources of variation, we show that we can replicate the significant effects of land taxes on prices. However, we show that this effect is confounded by a correlation between the change in the tax rate and the number of vacation homes in a treatment area, as well as diverging price trends in those areas that coincided with the reform. Once we drop vacation homes from the sample and include a control for the share of vacation homes in a treatment area, the price effect is eliminated.

The reform we study provides a unique window to study tax capitalization. We document precise null effects of land taxes on residential home prices. Our estimates allow us to easily rule out full capitalization at standard discount rates. Overall, we provide crucial new evidence on how land taxes affect residential property markets. Our results provide new evidence that the slope of the supply and demand curves is insufficient for understanding and predicting tax incidence for taxes on residential property. Our results suggest that households can avoid paying land taxes by selling their property or moving. This contradicts the basic efficiency argument for land taxes, which is that an individual's tax burden is unaffected by their behavioral responses. In the next section, we empirically test for behavioral responses by studying the effect of land taxes on development, mobility, and housing allocation.

8 Aggregate Effects of Land Taxes

Much of the policy debate about land taxes has focused on their efficiency. Unlike the conventional property tax, land taxes do not contain any **explicit** disincentive to develop property. However, we find no effect of land taxes on prices, suggesting that land taxes operate differently in practice than in theoretical models. In addition, policymakers and the public often express concern about whether or not property taxes may push older retirees out of their homes because they do not have the liquidity to pay taxes on (potentially rising) home values. We use our setting to provide new evidence on these policy debates by investigating the long-term effect of land taxes on development, mobility, and housing allocation.

8.1 The Effect of Land Taxes on Property Development

The effects of land taxes on development is uncertain. In our model, when landowners are fully compensated, taxes do not affect the demand or supply of buildings. However, without full compensation, income effects reduce demand for buildings and, thus, the total amount of developed property. In addition, if taxes are capitalized into land values, and land is used as collateral for development or if developers have short-term liquidity constraints, higher land taxes might serve as a negative net wealth or liquidity shock that can reduce the ability of developers to increase the housing supply. In contrast, a Georgist viewpoint suggests that **higher taxes** can actually **increase development**. George (1880) argued that higher taxes would improve land usage. This could come about either because higher taxes would reduce the asset price of land, which would improve land allocation and reduce speculation in the land market, or because the need to pay higher land taxes would lead to higher investment in land.

We provide new empirical estimates on the effects of land taxes on property development. Because property development is a slow-moving stock variable, we estimate the long-term effects of changes in land taxes using a long differences specification. We estimate the following regression at the treatment area level:

$$y_{a,2015} - y_{a,2006} = \beta \Delta \tilde{\tau}_a + \eta_{m(a)} + \delta X_a + e_a \tag{19}$$

In this specification, the dependent variable is the change in our outcome variables from the year before the tax change occurred until the last year of our data.[6] Our primary coefficient of interest is β, which represents the ITT effect of the policy shock instrument. We only use variation from the policy shock to study the effects of land taxes on aggregate outcomes, because only the policy shock varies at the treatment area level. We include treatment area controls, including detailed fixed effects for the share of vacation homes given the correlation documented in Appendix C, and a new municipality fixed effect.

We first investigate the effect of land taxes on total residential square footage. We sum up the total area dedicated to residential housing in each treatment area, investigating whether the total amount of housing increased or decreased more in areas with higher land tax increases after the reform.[7] Panel (A) of Figure 9 shows that we estimate a precise null effect of land taxes had on aggregate housing development. The β coefficient is 0.001 and statistically insignificant. We can rule out decreases or increases of more than 1 p.p. in total residential development in response to a 1 p.p. increase in (predicted) land value tax

[6] We end most of the property development specifications in 2015 because of changes in the Housing Census.

[7] We once again drop vacation homes.

rates over 9 years. Figure 9, Panel (B) shows no pre-trends in residential development in nearby areas that were eventually exposed to tax increases vs. decreases before the reform. Our results suggest that higher land taxes do not affect aggregate residential housing. This is consistent with our baseline neoclassical model, where land taxes do not affect the incentive to develop housing.

Much of the public debate on land taxes has focused on the effects on undeveloped plots. A revenue-neutral reform that moves from a property tax to a land tax (as proposed in Detroit) shifts the tax burden from highly developed properties (low θ) to land-intensive, empty plots (high θ). While the tax should not change the **marginal** incentive to develop undeveloped plots, many commentators have suggested this could improve the usage of undeveloped plots. To investigate the effects of land taxes on undeveloped plots, we focus on a set of undeveloped plots in 1995 and test whether the propensity to develop those plots depends upon the land tax rate.[8] We say a property is developed if it is recorded as having a business use by the property tax authorities or if the housing census records residential development on that plot. In this case, our outcome variable is the log change in the number of undeveloped properties.

Figure 10, Panel (A) shows the effect of land taxes on the change in the number of undeveloped properties in each treatment area that **remain undeveloped** in 2015 relative to 2006. Our results show that higher taxes do not have an effect on the development of empty plots. The point estimate is positive, suggesting that higher land taxes lead to less development of empty plots. However, the point estimate is smaller than the standard error, indicating the effect is highly statistically insignficant. The results suggest that a 1 p.p. change in the tax change instrument increases the number of properties that remain undeveloped by around 1.3 p.p. Panel (B) of Figure 10 shows that there is no evidence of pre-trends. Before the reform, undeveloped land was developed at a similar rate in areas that experienced tax decreases and increases. Overall, our results suggest that land taxes have limited effects on the development of empty plots. As a result, our results suggest that moving from a property tax to a land tax might increase housing development if property taxes are a significant impediment to residential growth. However, our results also allow us to rule out that land taxes alone might increase development. The 95% confidence interval for our ITT estimates can rule out that a 1 p.p. increase in land taxes increases the number of developed plots by more than 1.5 p.p. over nine years.

Figure 11 investigates heterogeneity in the effects of land taxes on the usage of undeveloped properties. Panels (A) and (B) show the effect split by whether the plot is on

[8]We focus on a set of properties for which we have a balanced panel and can observe the use of the property in each year.

agricultural or non-agricultural land. Public discussion of land taxes has focused chiefly on empty plots in urban land. In addition, no response on agricultural land might mask significant results in urban areas because agricultural land is subject to lower tax rates and advantageous land assessments, and the propensity to develop on agricultural land is much lower. Figure 11, however, shows that we estimate null effects on both agricultural and non-agricultural land. The estimate for building on non-agricultural plots is exactly zero, while the estimate for non-agricultural plots is close to zero and statistically insignificant.

Overall, our results suggest that higher land taxes have precisely estimated zero effects on property development. Our findings hold when looking specifically at the development of empty plots of land, or when looking at aggregate residential development. Our results imply that higher land taxes do not reduce the total housing stock or residential building in an area. As a result, a move from property taxes to land taxes might lead to higher development. However, our results suggest that land taxes alone do not increase growth over the medium- to long term.

8.2 The Effect of Land Taxes on Mobility

An especially strong concern in policy circles is that higher property taxes may push individuals out of their homes. This concern is especially prevalent for retirees on fixed incomes, who may be unable to keep up with tax payments if property values increase. This concern is somewhat mitigated in our setting because retirees in Denmark are eligible for a subsidized loan to cover their land tax payments. However, in our setting, these mobility concerns may be amplified because, in contrast to the predictions of theoretical models, we find null effects of taxes on home prices. As a result, the statutory incidence is identical to the economic incidence, and households can avoid the tax by moving to low-tax areas. This has important implications for the efficiency of land value taxes. If households resort where they live in response to higher tax rates, this reduces the efficiency case for land taxes. We study this empirically by investigating whether property owners exposed to higher vs. lower land taxes become more likely to sell their home in subsequent years. We focus on owners of primary residences and run the following regression:

$$y_{a,t} = \beta \Delta \tilde{\tau}_a + \eta_{m(a)} + \delta X_a + e_a \tag{20}$$

This specification mirrors the long difference specification, but our outcome variable is the (log) share of incumbent homeowners (from 1995) who retain ownership of their home in year t. Figure 12, Panel (A) shows the effect on ownership in 2019. We estimate a precise zero effect of land taxes on the probability an individual still owns their home in 2019. This indicates that the tax shock does not affect individual decisions to sell or remain in their house. Panel (B) shows the effect in 2001, before the shock, and serves as a placebo test. As with the long-term effect, we estimate a small, statistically insignificant effect of (eventual) tax changes on the propensity of incumbents to move. Our results show that higher taxes do not push incumbent homeowners out of their homes. This suggests that, although homeowners could theoretically avoid the land tax by moving, the tax shock does not prompt households to sell their house.

8.3 The Effect of Land Taxes on the Allocation of Housing

Though our model abstracts from the homeownership decision, in a standard neoclassical model, land taxes should not affect the decision about whether or not to purchase a home. Theoretically, higher land taxes should be offset by lower purchase prices, and households should be indifferent between purchasing a home and paying the rental price (the user cost of housing) each period. However, in models with financial frictions, individuals who are down-payment constrained may be induced to purchase homes if higher taxes reduce the asset price of housing, thus allowing some households that were previously down-payment constrained to purchase homes. Coven et al. (2024) argue that higher property taxes would increase homeownership, especially among financially constrained households. However, since we estimate null effects on purchase prices, this channel is unlikely to be operative in our setting.

We use our experiment to study whether higher land taxes increased homeownership rates. We re-estimate our long-difference specification but replace the outcome variable with the (log) change in the number of residential homeowners. Since we find null effects on development, this specification will capture whether higher land taxes affect the decision of whether to own or rent a home. Figure 13, Panel (A) plots the long difference between 2019 and 2006. While the estimate is positive, suggesting that homeownership numbers rose, the estimate is close to zero and statistically insignificant. Our results suggest that higher land taxes did not induce higher homeownership rates in the medium-to-long run. Panel (B) serves as a placebo test and shows that in the pre-period, the slope is nearly identical to the post-period slope and statistically insignificant (though only marginally). Overall, we find little evidence that higher taxes increase homeownership by relaxing financial constraints.

We also investigate whether higher land taxes changed the composition of homeowners. We repeat the long-difference specification but replace the dependent variable with the log share of homeowners in a treatment area with particular characteristics. Figure 14, Panel (A) shows the change in the share of young homeowners, which we define as under 35. We find no change in the share of young homeowners in areas that experienced tax increases relative to areas that experienced tax decreases. This provides additional evidence against the arguments made in Coven et al. (2024) that higher property taxes can increase home purchases by young, financially constrained households. If the price of housing does not fall in response to higher taxes, then there is no change households ability to pay for a home.

In contrast, Panel (B) finds a significant decrease in the share of homeowners over 60 years old. The effect is highly statistically significant ($p < 0.001$). Appendix Figure A2 also shows no significant differences between areas that eventually experienced tax increases versus decreases in the pre-period. As a result, we find that while higher land taxes did not lead to increased home sales in high-tax areas among incumbent homeowners, older individuals sorted away from high-tax areas to low-tax regions.

In contrast, Panels (C) and (D) show that higher taxes have no effect on the share of lower-income or very high-income homeowners. Panel (C) shows the effect of taxes on the share of working households with below-median income.[9] We find a precise zero effect of higher taxes on the share of lower-income homeowners. Panel (D) shows the effects of higher taxes on the share of rich households, which we define as households in the top 10% of the income distribution. The coefficient is negative but has a relatively small magnitude and is statistically insignificant.[10]

Taken together, we estimate that higher land taxes did not affect the number of homeowners in a treatment area. This contradicts the argument that higher property taxes reduce financial constraints by lowering sales prices, leading to increased homeownership (Coven et al., 2024). However, we find that higher taxes reallocated the existing housing stock: higher taxes led to a decrease in the share of older homeowners. This is consistent with older homeowners sorting away from areas with higher land taxes during retirement years. Because we find no effect of higher taxes on the probability of remaining in a home, the decrease in the share of older homeowners is unlikely to be driven by older homeowners selling their homes in response to higher tax payments. This suggests that land taxes affect the composition of homeownership within an area, and represents an efficiency cost of land taxation not present in standard models. This also shows that differentiated local taxes affect the allocation of homeowners across space.

[9]We focus on working households to avoid the results being driven by retirees, who have low reported incomes.

[10]Appendix Figure A2 shows that none of these outcomes have significant pre-trends.

9 Discussion

9.1 Discussion

What can explain our null effect on home prices? A natural explanation in the Danish context comes from rent control for housing. In Denmark, approximately 50% of households are renters, and nearly all private rental housing is subject to rent controls. In addition, for the major types of rent control regulations, rent can explicitly be raised when taxes are increased (Whitehead, 2012). In Appendix B, we present a version of our model where land is owned by landlords who rent to the household. In this model, the highest rent a landlord can charge for a unit of land is:

$$R = \bar{R} + \tau P^L$$

When the rent cap is binding, the price of land is given by the following expression:

$$P^L = \frac{1+r}{r}\bar{R}$$

Thus, when landlords can pass through the tax increases, the price no longer depends on taxes: $V = \frac{\bar{R}}{r}$. In contrast, in this model higher taxes lead to higher rent payments:

$$R = \bar{R} + \tau P^L = \bar{R} + \tau \frac{1+r}{r}\bar{R}$$

As a result, a key implication of this setting and explanation is that because the price does not adjust, but the rents do, renters pay the tax rather than landlords. After the land tax increase, the landlord's net profits are the same (as higher rents exactly offset higher tax payments), but the rent owed by the tenant has increased.

Given this potential explanation, it is particularly interesting that we also estimated precise null effects of land taxes in the single-family home market. In this market, more than 80% of households are both landowners and tenants (to themselves). As a result, even in markets dominated by owner-occupied housing, higher taxes do not lead to higher prices. It may be the case that prices are set by the outside option of renting, in which case rental restrictions will still affect market prices, even if they bind no tenants in equilibrium. However, given the substantial evidence of non-standard pricing behavior in the housing market (Genesove and Mayer, 2001; Andersen et al., 2022), this channel is speculative.

Similarly, given the recent evidence or pass-through of property taxes to rents documented across a variety of institutional contexts (Löffler and Siegloch, 2021; Baker, 2024; Watson and Ziv, 2024), it is entirely plausible that pass-through of land taxes to renters could hold in markets less bound by rent control. This study also provides new evidence of how non-neoclassical forces may affect tax incidence. Benzarti (2024) emphasizes how fairness norms play a role in determining incidence of the payroll tax. Our work suggests that other institutional regulations may be important for understanding the incidence of property taxation. More work on land and property taxes across a variety of institutional settings is vital for understanding how regulations may interact with tax policy and affect incidence.

9.2 Conclusion

Despite a long and rich intellectual history, we have surprisingly little evidence on the effects of land-value taxation. In this paper, we exploit a unique institutional reform that generated large, persistent, quasi-experimental variation in land-value tax rates across towns to study how individuals and markets respond to land taxes. We have two main sets of results. First, we show that land taxes are not capitalized into home prices. We estimate precise null effects of taxes on residential property prices using a repeat-sales index. This result contradicts the standard view the incumbent owners pay for higher taxes. This result is also inconsistent with the efficiency argument for land taxes: incumbents can sell their homes to avoid paying taxes, implying taxes may generate an inefficient allocation of housing. Finally, it suggests that taxes are paid by renters and future purchasers of land, as well as incumbent homeowners.

Figure 15 shows the CDF of land ownership in Denmark in 2001, prior to the reform. Unsurprisingly, land holdings are concentrated at the top of the distribution. The top 3 deciles own nearly 50% of the value of the land. In a neoclassical world, it is these landowners who would pay a land tax. In contrast, our results show that the tax burden is shared with renters and future purchasers. The bars in pink show the share of individuals in each income decile who own no real property. In contrast, non-property owners are concentrated at the bottom of the income distribution: in each decile in the bottom half of the income distribution, more than 60% of individuals are renters. In the bottom decile, the number is more than 80%. Our results show that land tax payments are shifted to these individuals, implying a much more regressive tax than in standard models.

Second, we study how higher taxes affect aggregate development, mobility, and housing allocation. Consistent with the neoclassical benchmark, but inconsistent with the most optimistic Georgist predictions, we find null effects of higher land taxes on aggregate development. This suggests that moving from traditional property taxes to land taxes may spur new development, though higher land taxes alone do not lead to more intensive land use. We find this effect when looking at aggregate residential development or specifically at the development of empty plots, which has been the subject of an active policy debate. We also find that higher taxes do not affect a region's total quantity of homeownership. Taken together, these results speak against recent work that has suggested higher land taxes may spur increased density or homeownership (Murphy and Seegert, 2024; Coven et al., 2024).

However, we find evidence that higher land taxes may have reallocated housing away from older homeowners. Because we find null effects on the probability that incumbent homeowners sell their homes, we interpret these results as suggesting that older households sort away from areas with high taxes. This work also provides evidence against the view that land taxes are perfectly efficient because taxes affect where households choose to locate.

Our findings have important implications for policymakers choosing from a range of possible tax bases. Land taxes have traditionally been viewed favorably because they do not induce substitution effects. In contrast, our results suggest that land taxes have consequences for efficiency. However, these effects are somewhat limited as land taxes do not disincentivize housing production or land usage. However, land taxes are paid broadly across the income and wealth distribution. Our findings suggest that the land tax is paid by renters, future landowners, and current owner-occupied homeowners, who are both landlord and tenant. The only people who do not pay any tax are absentee landlords, who can pass off the tax to tenants. This imperils the political viability of land and property taxes and provides a potential explanation for the limited popular support for property taxation (Elmendorf, Nall and Oklobdzija, 2024). In ongoing work, we study how individual renters and residents of owner-occupied housing responded to this persistent change in their land tax burden.

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10 Figures

Figure 1: A Model of the Market for Land

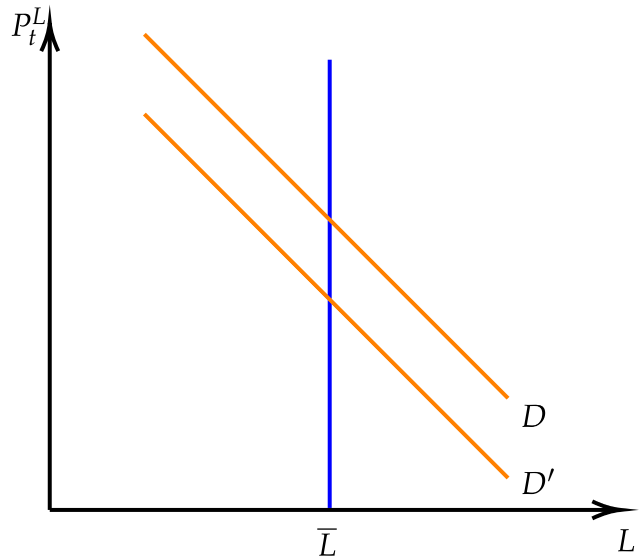
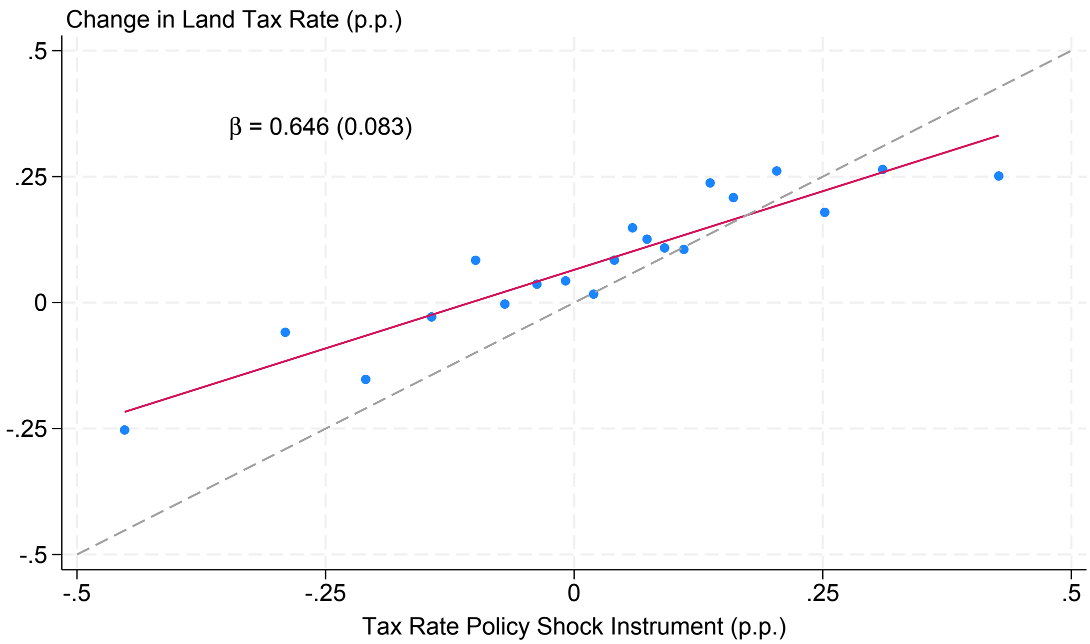
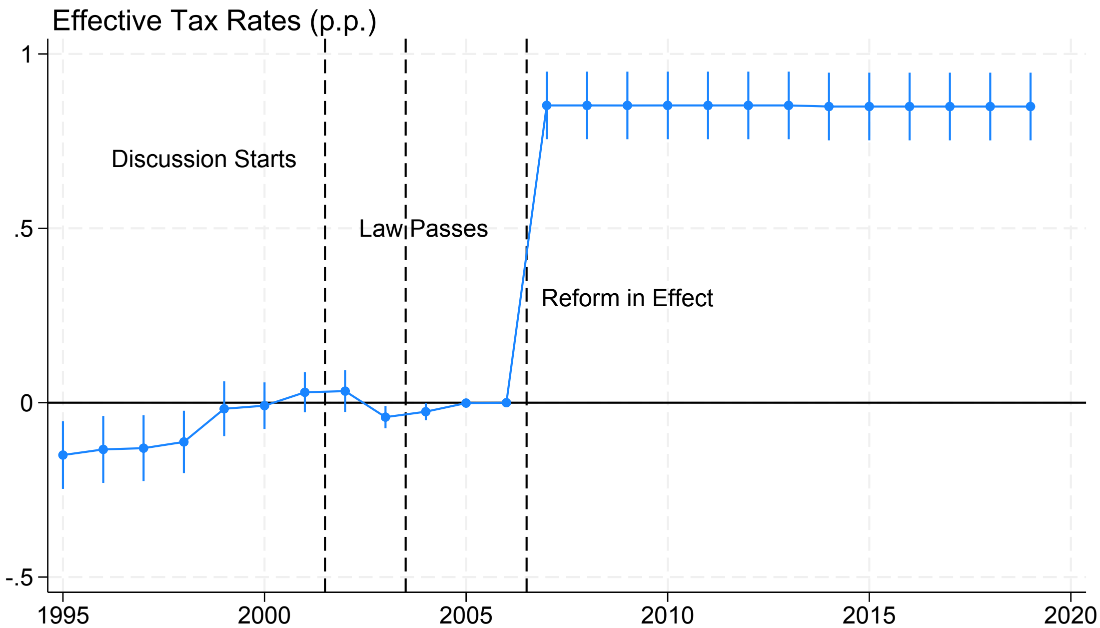


Figure 2: First Stage: The Effect of Tax Change Instrument on Statutory Tax Rate



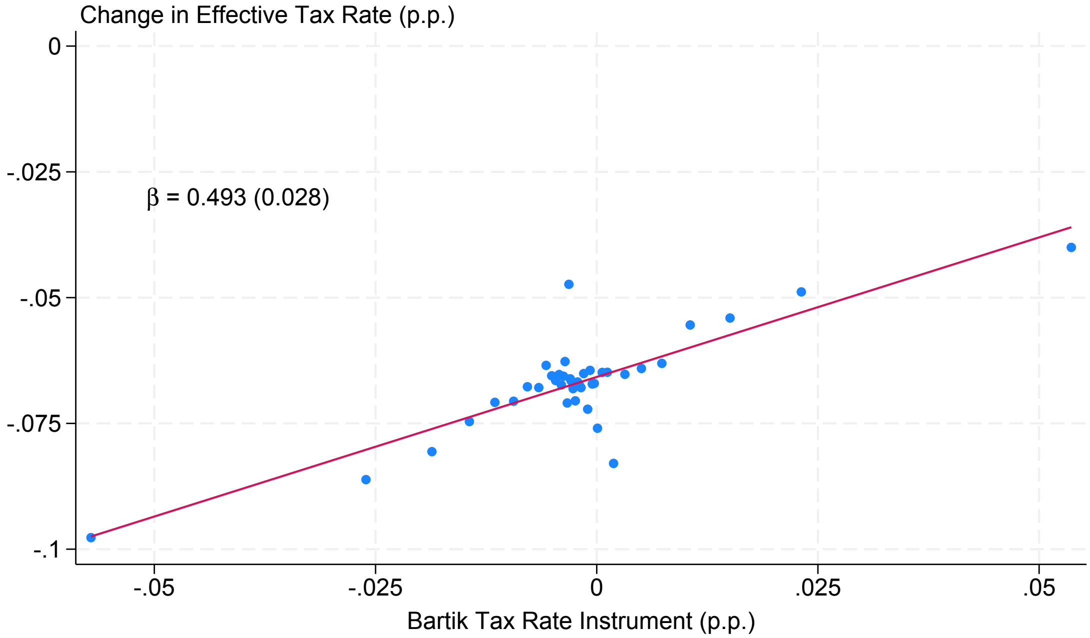
Notes: This figure plots the first stage regression of the land tax instrument on the actual change in statutory tax rates from 2005 to 2007. For both the x- and y-axes, the units are in percentage points: thus, a value of 1 represents a 1 p.p. change in the land value tax rate. The blue line represents the 45-degree line. Beta coefficients represent and 95% confidence intervals, with robust standard errors are plotted.

Figure 3: The Effect of Statutory Tax Changes on Property-Level Effective Tax Rates



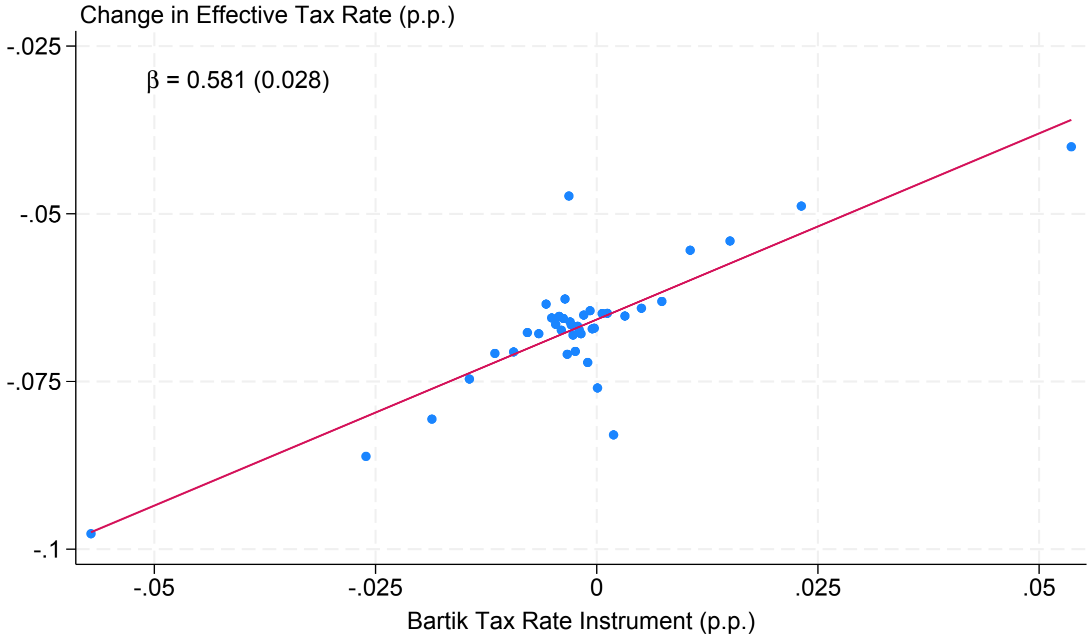
Notes: This figure plots the first stage regression of the land tax instrument on effective tax rates at the property level. 95% confidence intervals are shown, with standard errors clustered at the treatment area level. We estimate the regression without controls for computational tractability.

Figure 4: First Stage: The Effect of Land-Tax Exposure Instrument Within A Treatment Area



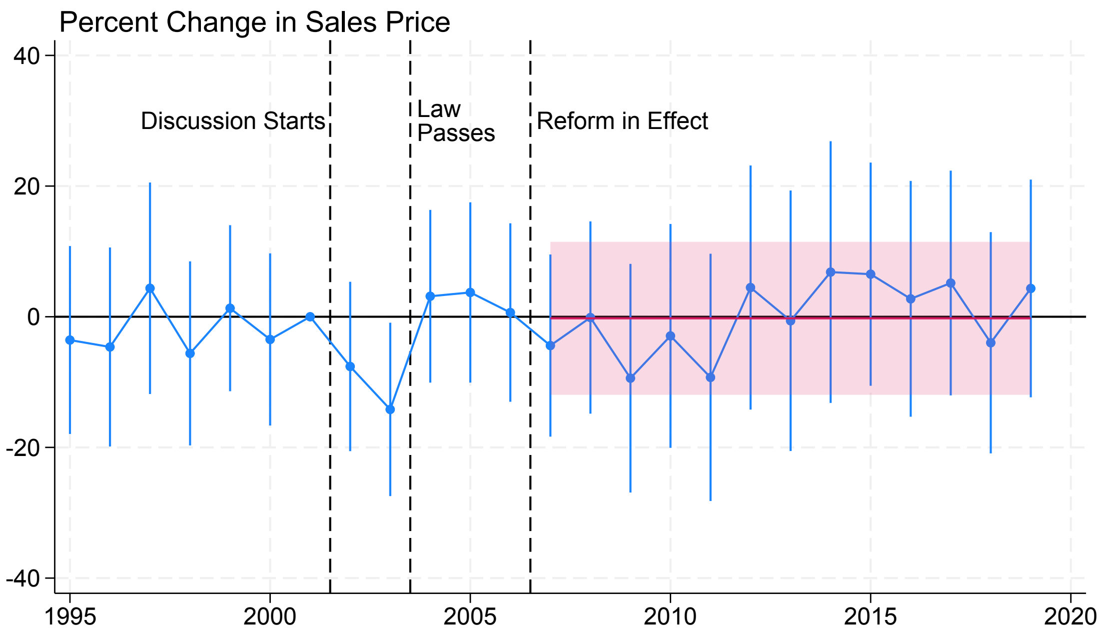
Notes: This figure plots the first stage regression of the land tax instrument on the actual change in effective taxes paid, normalized by the assessed value of the property, from 2005 to 2007. For both the x- and y-axes, the units are in percentage points: thus, a value of 1 represents a 1 p.p. change in the tax rate, normalized by the assessed value. The blue line represents the 45-degree line. Beta coefficients and 95% confidence intervals, with standard errors clustered at the treatment area by land share decile level, are plotted.

Figure 5: First Stage: The Effect of Land-Tax Exposure Instrument Combining Policy Shock and Exposure Measures



Notes: This figure plots the first stage regression of the land tax instrument on the actual change in effective taxes paid, normalized by the assessed value of the property, from 2005 to 2007. For both the x- and y-axes, the units are in percentage points: thus, a value of 1 represents a 1 p.p. change in the tax rate, normalized by the assessed value. The blue line represents the 45-degree line. Beta coefficients and 95% confidence intervals, with standard errors clustered at the treatment area by land share decile level, are plotted.

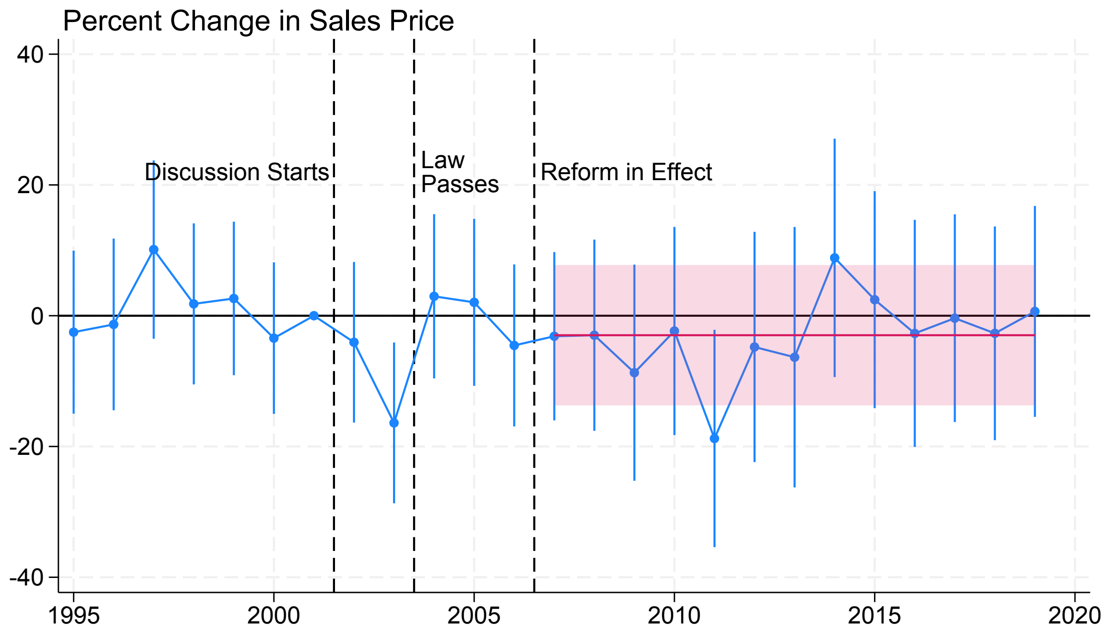
Figure 6: Higher Taxes Have No Effect on Sales Prices



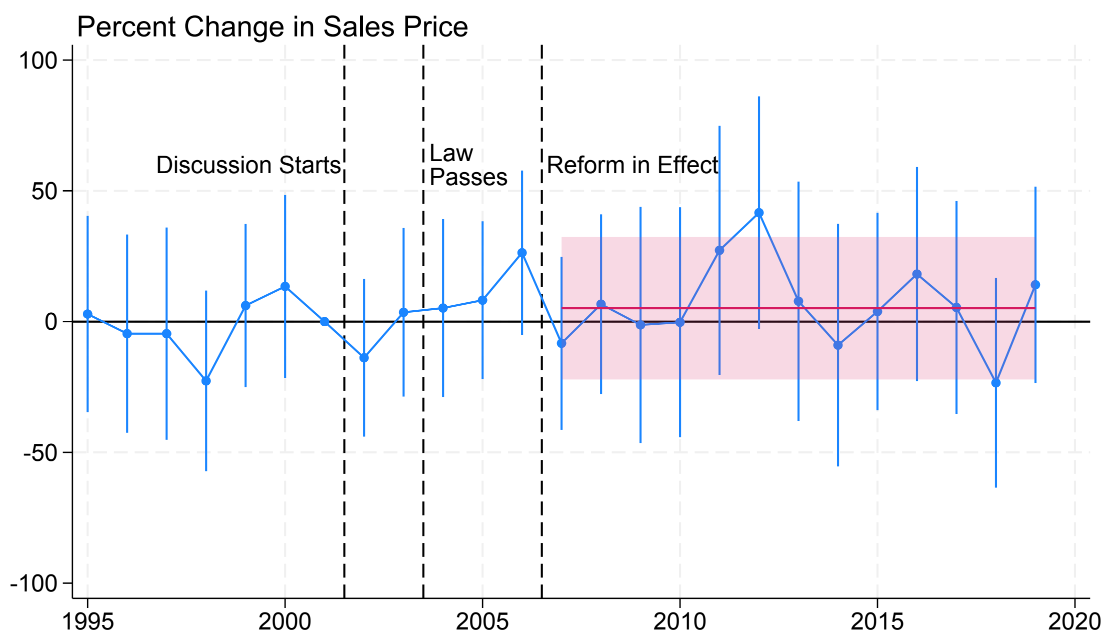
Notes: This figure plots the effect of land taxes on sales prices. 95% confidence intervals are shown for the event-study coefficients, with standard errors clustered at the treatment area by land share decile level. The red line shows the difference in difference estimate that pools all post-2007 years compared to the price in 2001, and the red shading shows the 95% confidence intervals from that regression.

Figure 7: The Effect of Land Taxes Using Separate Identification Strategies

A. “Policy Shock" Only

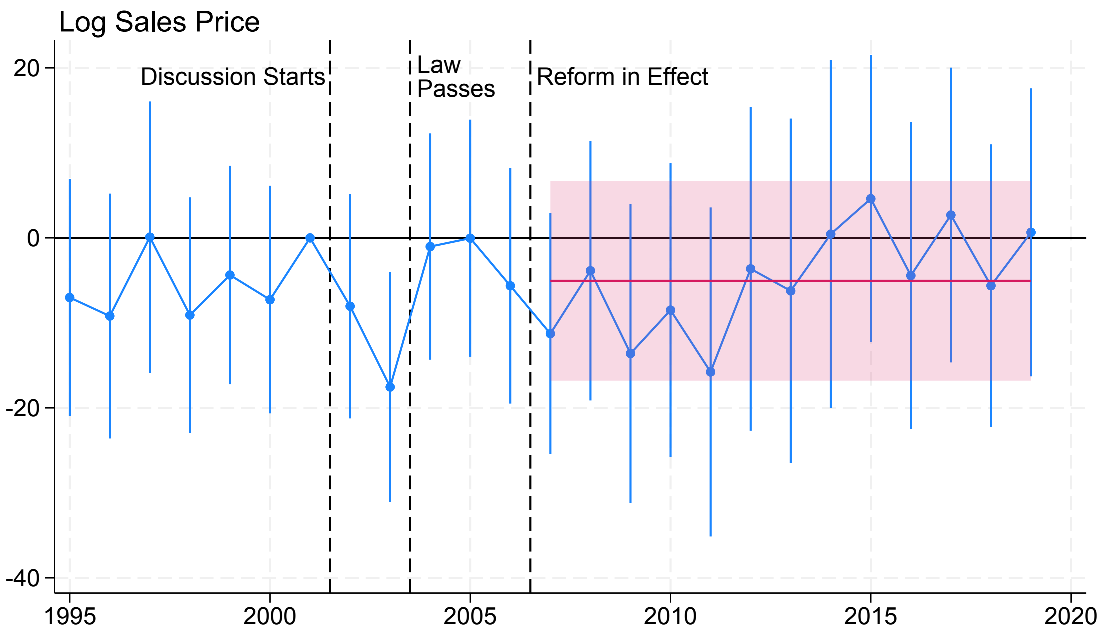


B. Land Share “Exposure" Only



Notes: This figure plots the effect of land taxes on sales prices. 95% confidence intervals are shown for the event-study coefficients, with standard errors clustered at the treatment area by land share decile level. The red line shows the difference in difference estimate that pools all post-2007 years compared to the price in 2001, and the red shading shows the 95% confidence intervals from that regression. Panel (A) shows the effect using the policy shock instrument only, while Panel (B) shows the effect after residualizing on treatment area fixed effects. Thus, Panel (B) just uses variation from within the same treatment area, coming from heterogeneous exposure to land tax rates.

Figure 8: Higher Taxes Have No Effect on Sales Prices of Single-Family Homes

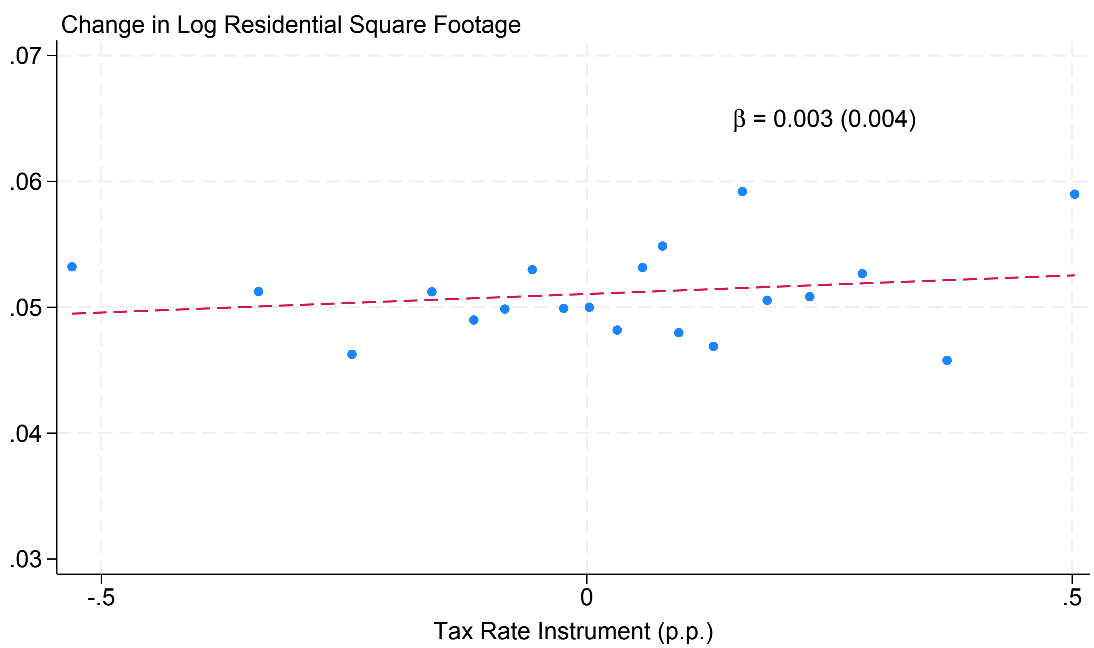


Notes: This figure plots the effect of land taxes on sales prices in a subsample of single-family homes. 95% confidence intervals are shown for the event-study coefficients, with standard errors clustered at the treatment area by land share decile level. The red line shows the difference in difference estimate that pools all post-2007 years compared to the price in 2001, and the red shading shows the 95% confidence intervals from that regression.

Figure 9: The Effect of Land Taxes on Aggregate Residential Development

A. Long Difference: Effect of Land Taxes on Residential Development

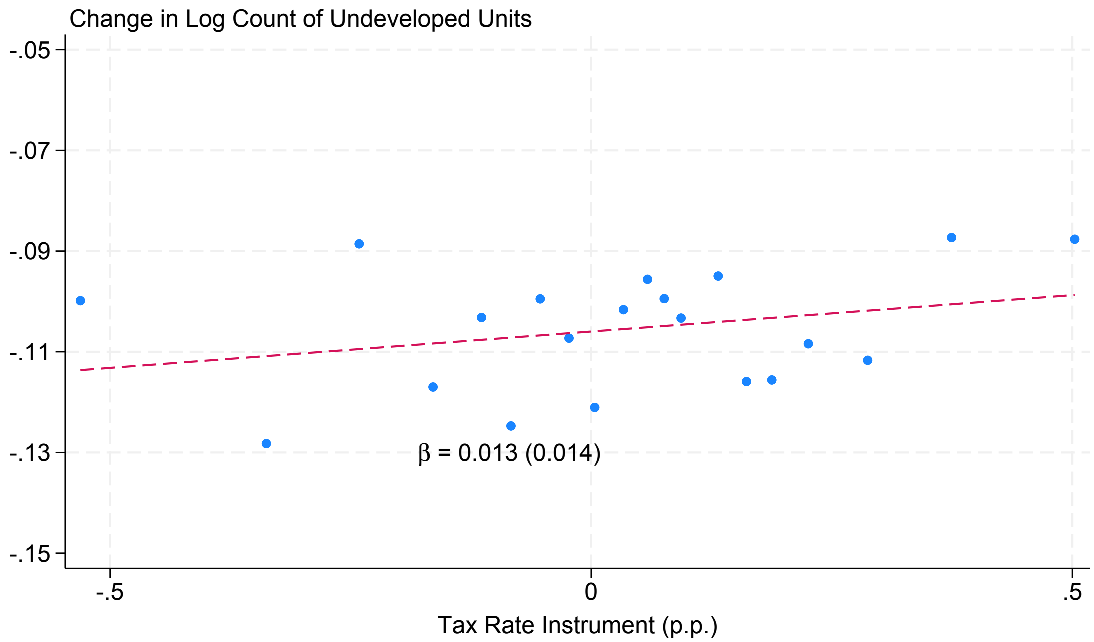
B. Placebo Test: Effect of Land Taxes on Residential Development in Pre-Period



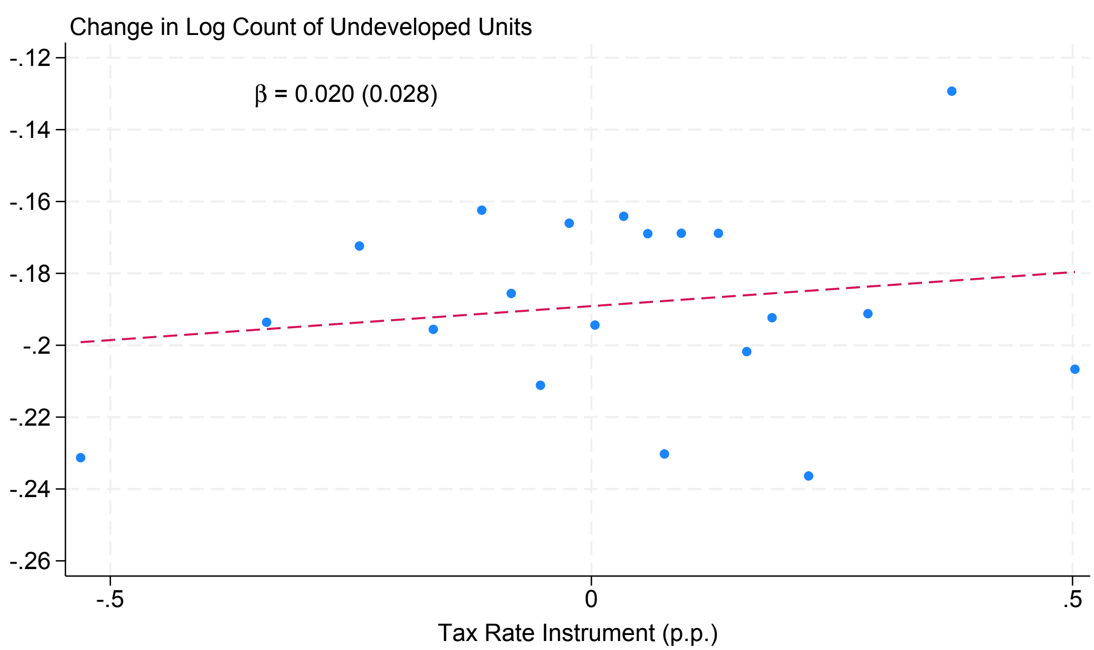
Notes: Panel (A) plots the long difference between 2015 and 2006 in total residential square footage on the land tax change instrument. Panel (B) plots the same effect, but from 1995 to 2001, before the tax change, as a test for pre-trends. Point estimates and standard errors are shown, with standard errors clustered at the new municipality level.

Figure 10: The Effect of Land Taxes on Development of Empty Lots

A. Long Difference: Effect of Land Taxes on Development of Empty Plots



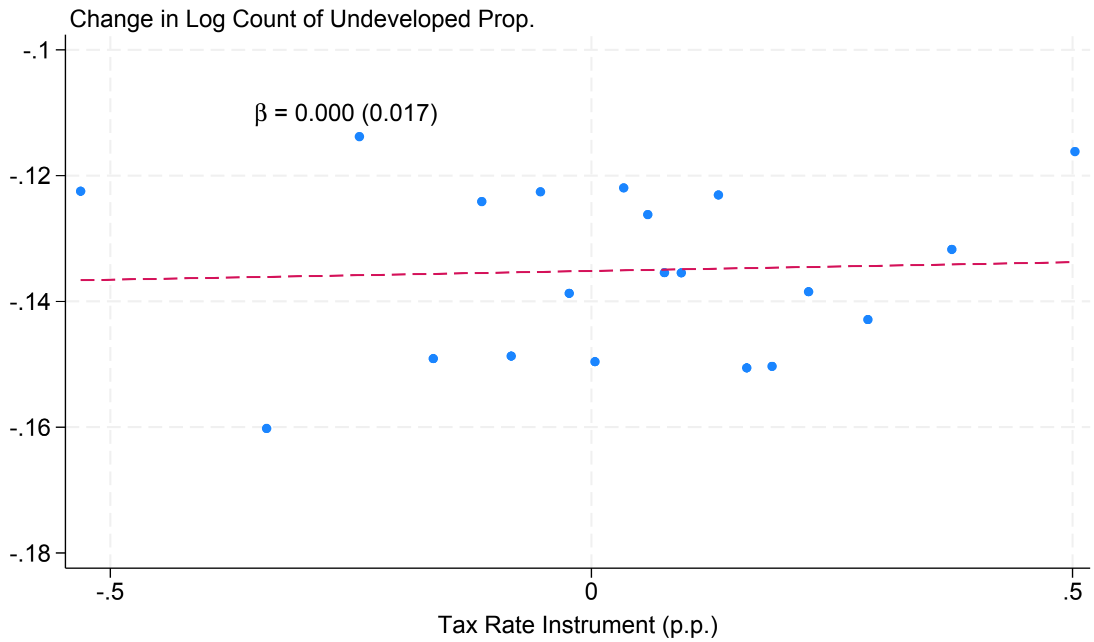
B. Placebo Test: Effect of Land Taxes on Development of Empty Plots in Pre-Period



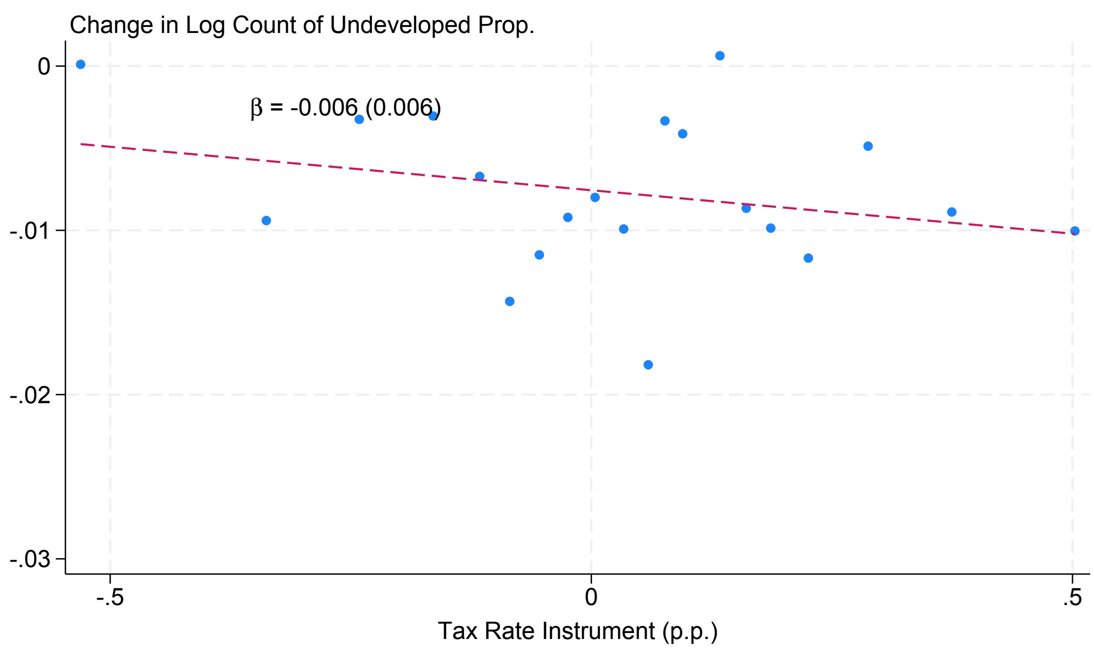
Notes: Panel (A) plots the long difference between 2015 and 2006 in log count of undeveloped plots from 1995 that remain undeveloped on the land tax change instrument. Panel (B) plots the same effect, but from 1995 to 2001, before the tax change, as a test for pre-trends. Point estimates and standard errors are shown, with standard errors clustered at the new municipality level.

Figure 11: The Effect of Land Taxes on Development of Empty Lots

A. Long Difference: Effects on Non-Agricultural Land



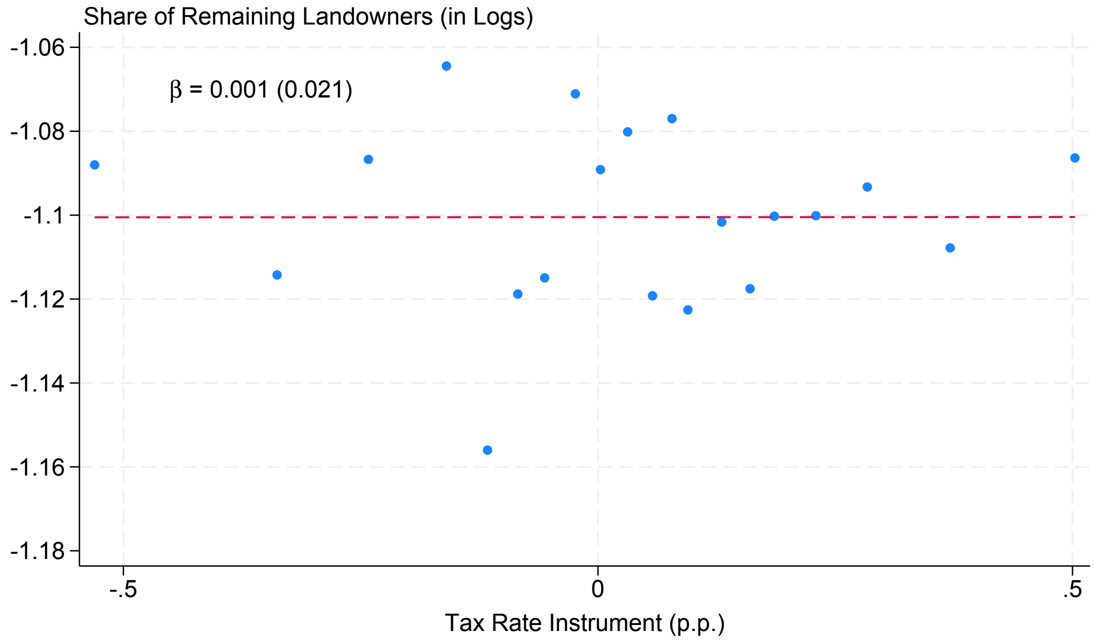
B. Placebo Test: Effects on Agricultural Land



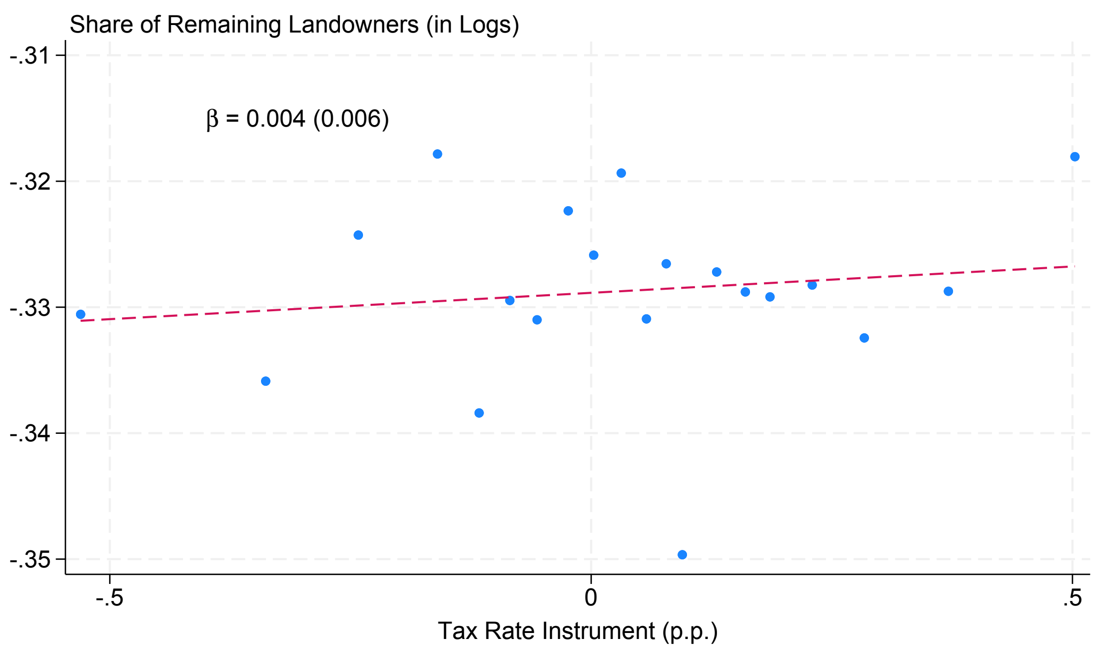
Notes: Panel (A) plots the long difference between 2015 and 2006 in log count of undeveloped non-agricultural plots from 1995 that remain undeveloped on the land tax change instrument. Panel (B) plots the same effect, but for agricultural plots. Point estimates and standard errors are shown, with standard errors clustered at the new municipality level.

Figure 12: Land Taxes Do Not Cause Homeowners to Sell Their Home

A. Share of Remaining Homeowners in 2019



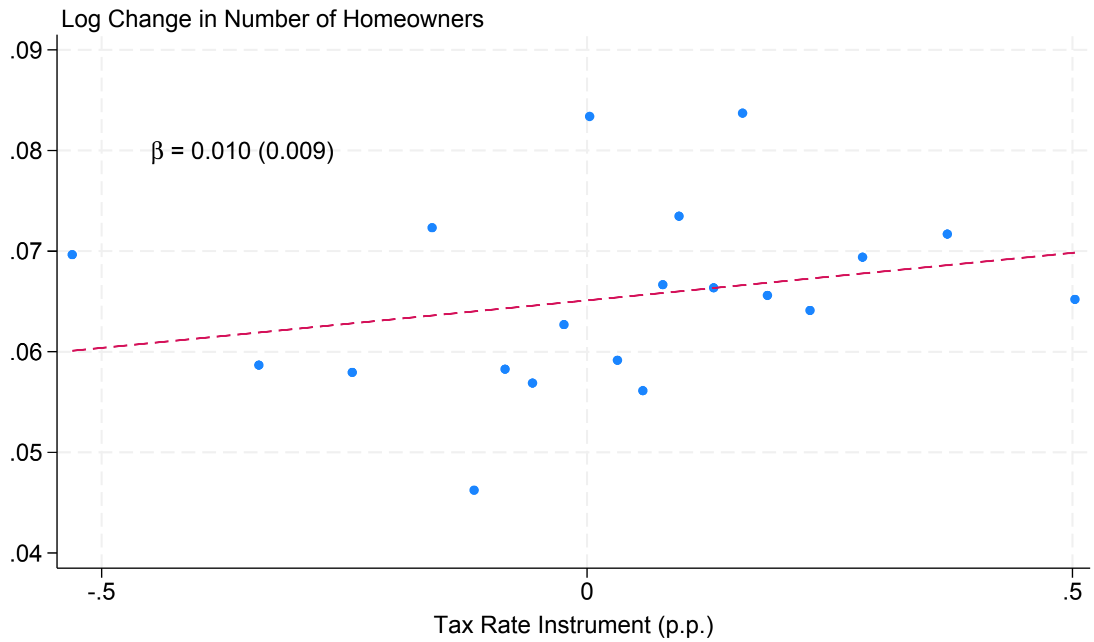
B. Share of Remaining Homeowners in 2001



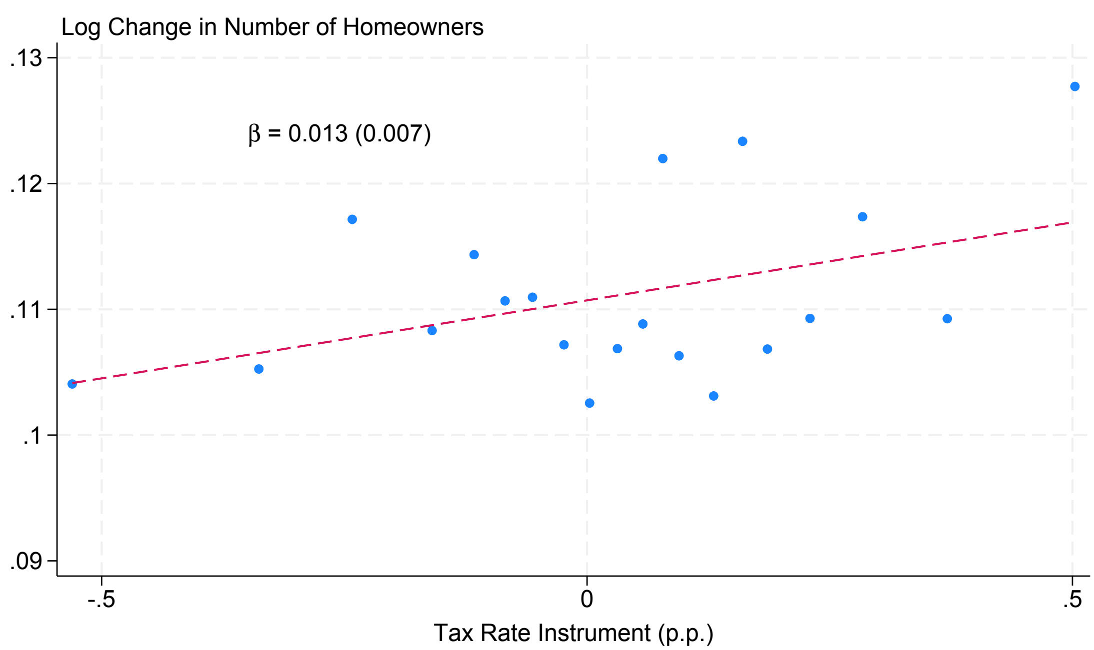
Notes: Panel (A) plots the share of primary, residential homeowners from 1995 who still own their home in 2019 on the land tax change instrument. Panel (B) plots the same effect, but in 2001, to test for pre-trends. Point estimates and standard errors are shown, with standard errors clustered at the new

Figure 13: Land Taxes Do Not Increase in Homeownership

A. Long Difference: Change in the Number of Homeowners

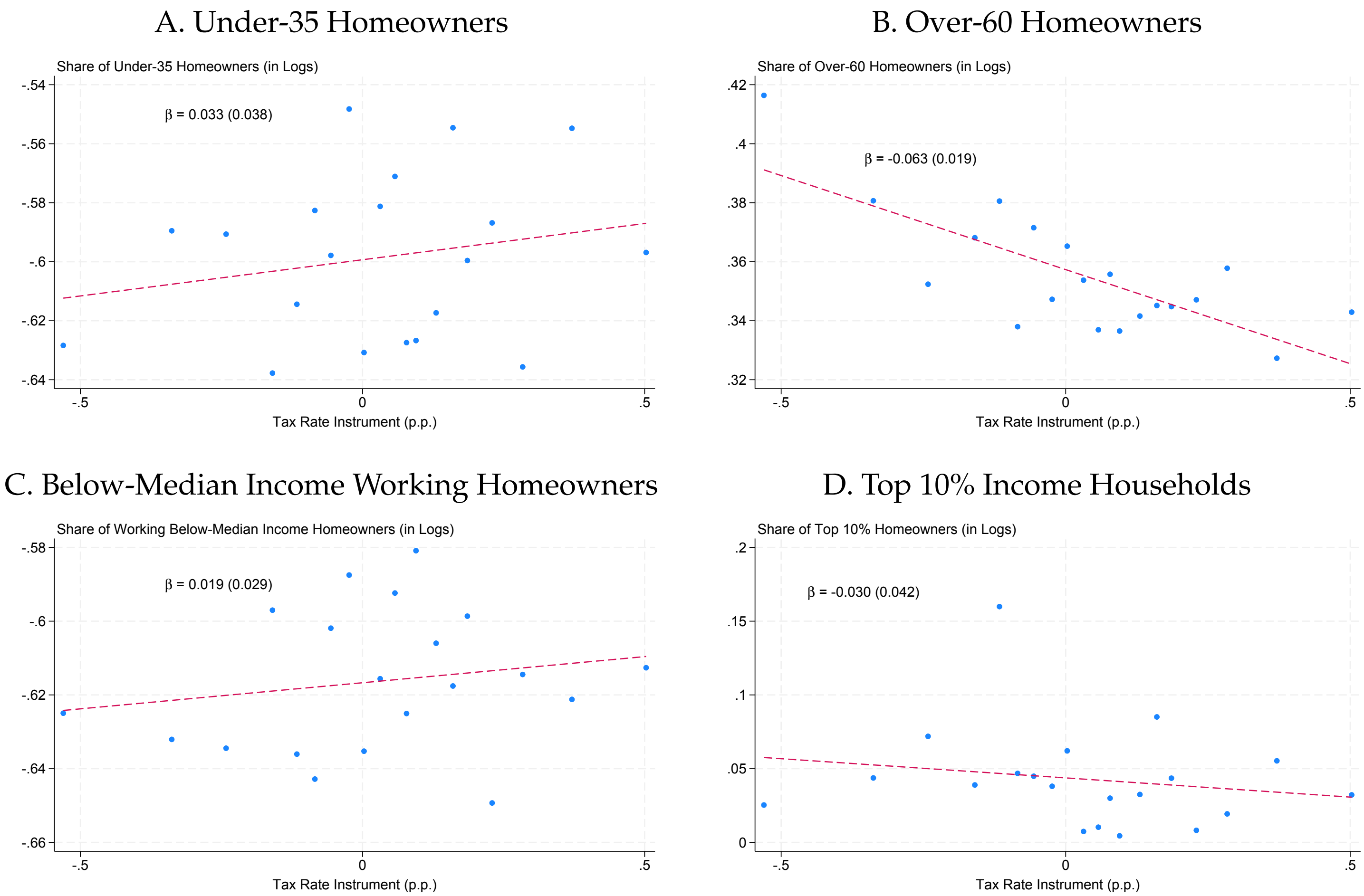


B. Placebo Test: Change in the Number of Homeowners in the Pre-Period



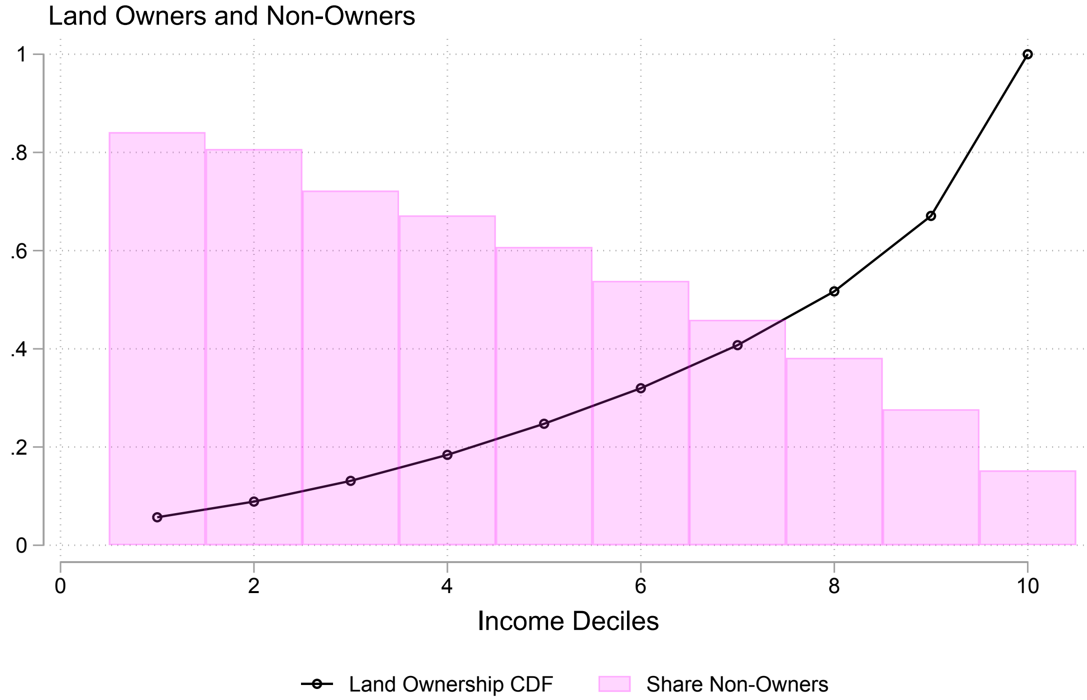
Notes: This figure plots binned scatterplots of the change in the number of primary homeowners in a given treatment area. Panel (A) shows the long difference between 2006 and 2019, while Panel (B) shows the long difference between 1995 and 2001, as a placebo test. Point estimates and standard errors are shown, with standard errors clustered at the new

Figure 14: Land Taxes and the Allocation of Housing



Notes: The binscatters in this figure plots the long difference between 2019 and 2006 in log share of homeowners in a given treatment area on the land tax change instrument. Panel (A) plots the effect for homeowners who are under 35 years old. Panel (B) plots the same effect, but for homeowners who are over 60 years old. Panel (C) plots the effect for below-median income working homeowners, while Panel (D) plots the effect individuals in the top 10% of the income distribution. Point estimates and standard errors are shown, with standard errors clustered at the new municipality level.

Figure 15: Land Ownings Across the Income Distribution



Notes: This figure plots CDFs of holdings of land across the income distribution. The bars in pink show the share of individuals in each income decile who own no real property.

11 Tables

Table 1: Effect on Home Prices and Implied Discount Rates

	OLS	IV: Baseline	IV: Trim 1%	IV: Trim 5%	IV: SF	IV: Policy Shock	IV: Exposure
Ant. x Treat	-3.722	-0.904	-1.240	-4.062	0.754	-8.562	15.95
	(2.802)	(6.665)	(6.637)	(6.371)	(6.145)	(6.167)	(20.02)
Post x Treat	-7.174	2.655	3.845	0.606	0.325	-7.391	13.20
	(4.220)	(7.762)	(7.673)	(6.944)	(7.857)	(7.901)	(23.02)
Obs.	1015414	1015414	1007798	942671	314045	616419	1015385
Rate L.B.	6.500	8	8.900	7.700	6.600	4.400	3.100
F-Stat		209.2	208.2	213.6	215.3	197.2	161.1

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

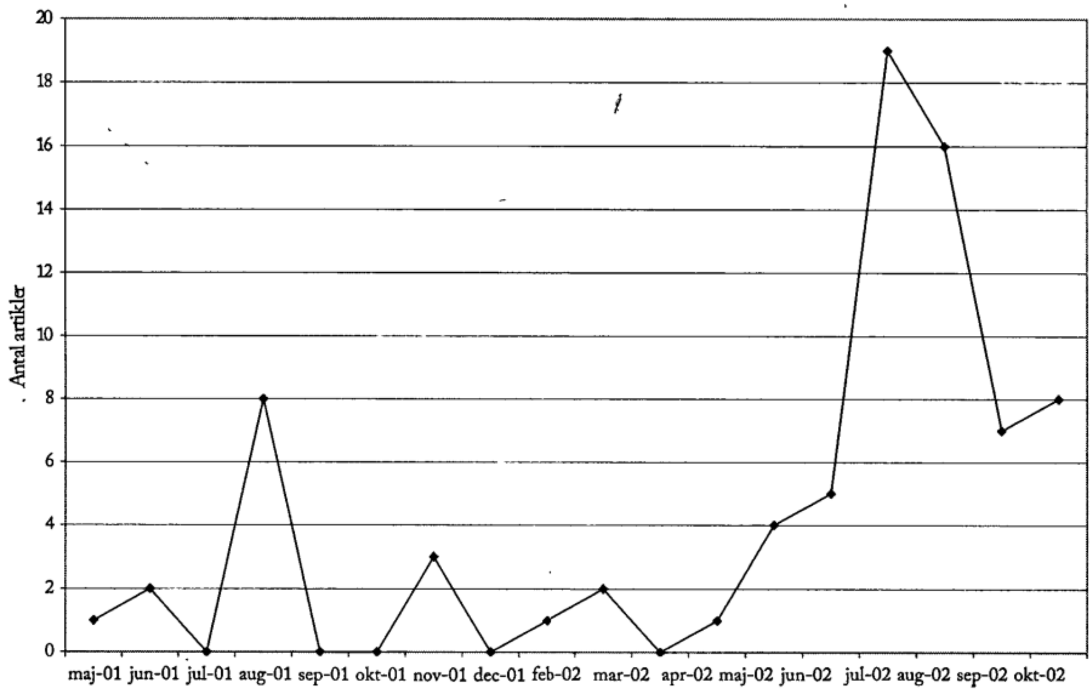
Notes: Rate L.B. row denotes the discount rate implied by our results assuming full capitalization of taxes into home prices. The discount rate is based on the lower bound of the calculated 95% confidence interval.

Appendices

Appendices Contents

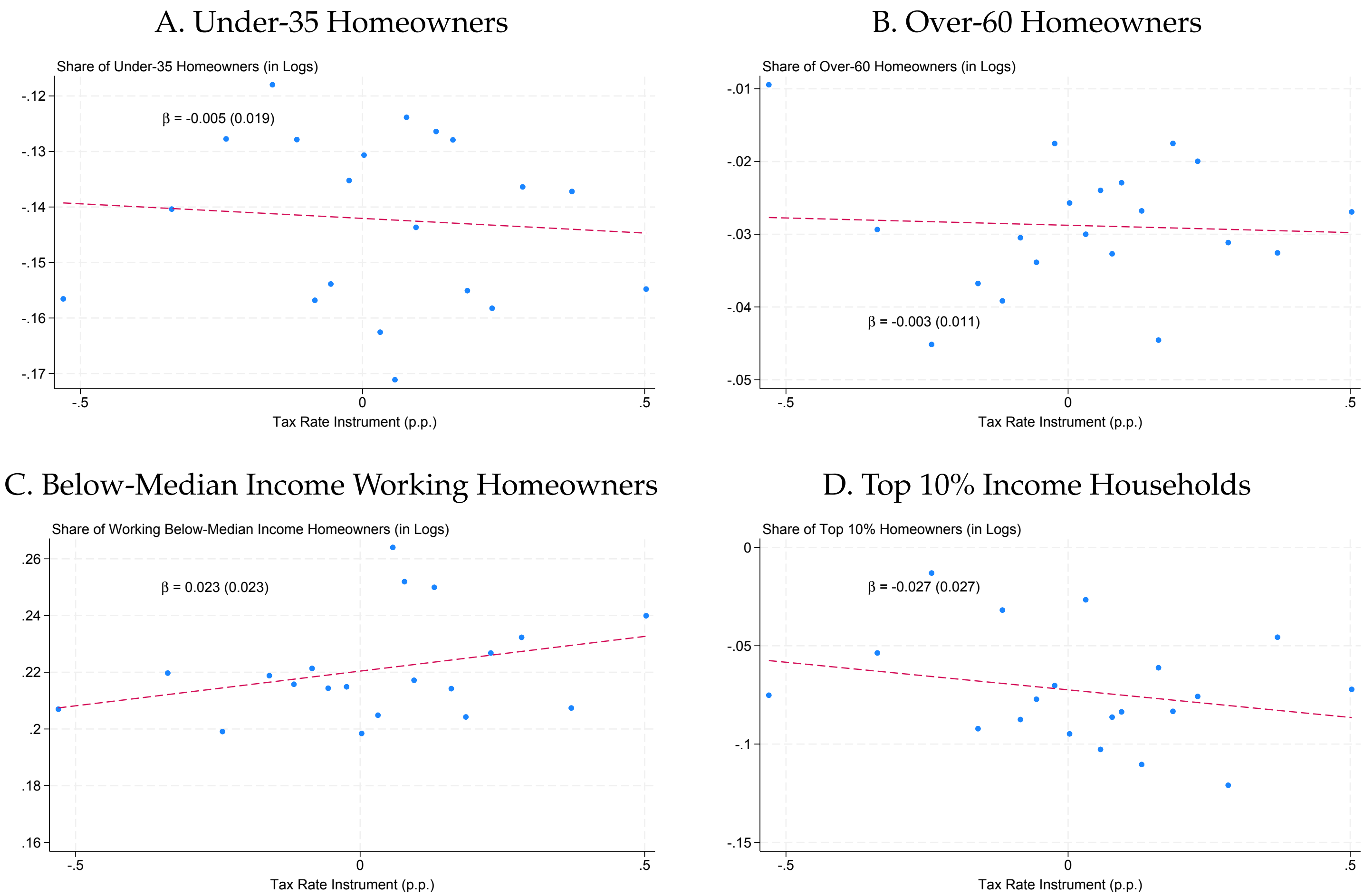
A Appendix Figures

Figure A1: Mentions of Structural Reform in Major Danish Newspapers



Notes: This figure plots mentions of structural reforms in major Danish newspapers across time. Figure is taken from Christiansen and Klitgaard (2008)

Figure A2: Land Taxes and the Allocation of Housing: Placebo Tests



Notes: The binscatters in this figure plots the long difference between 2001 and 1995 in log share of homeowners in a given treatment area on the land tax change instrument. Panel (A) plots the effect for homeowners who are under 35 years old. Panel (B) plots the same effect, but for homeowners who are over 60 years old. Panel (C) plots the effect for below-median income working homeowners, while Panel (D) plots the effect individuals in the top 10% of the income distribution. Point estimates and standard errors are shown, with standard errors clustered at the new municipality level.

B Model with Rent Ceiling

We build a neoclassical model of land-value taxation with a price ceiling on rents to provide a possible interpretation of our empirical results. The economy consists of households, landlords, producers, and government.

Households

Households consume non-durables C_t and supply labor N_t. They rent land L_t from landlords.[11] They have access to a real financial asset A_t. They receive lump sum transfers T_t and profits Π_t from the goods producer (both of which they take as given). They maximize utility

$$U_0 = \sum_{t=0}^{\infty} \beta^t u(C_t, N_t, L_t)$$

subject to

$$C_t + P_t^L L_t + A_t = W_t N_t + (1 + r_{t-1}) A_{t-1} + T_t + \Pi_t.$$

The first-order conditions are

$$u_{C,t} = \beta(1 + r_t) u_{C,t+1} \tag{21}$$

$$-\frac{u_{N,t}}{u_{C,t}} = W_t \tag{22}$$

$$\frac{u_{L,t}}{u_{C,t}} = R_t \tag{23}$$

where $u_{X,t} \equiv \frac{\partial u}{\partial X}(C_t, N_t, L_t)$.

We note that equation (23) determines the demand for land services (i.e. renting land). With a binding rent ceiling land will be in excess demand. The equilibrium quantity of land will be determined by the intersection of the supply curve and the rent ceiling, so the household will be off their demand curve. However, we can still use (23) to learn the amount demanded at the prevailing price (the rent ceiling).

Landlords

Landlords own land L_t and rent it to households at rental price R_t. Landlords can trade land at price P_t^L and they pay land value tax at rate τ^L. They take prices as given and

[11] For simplicity, this model considers only land (not buildings).

choose L_t to maximize

$$\sum_{t=0}^{\infty} q_t(R_t L_t - P_t^L(L_t - L_{t-1}) - \tau^L P_t^L L_t)$$

where $q_0 \equiv 1$ and $q_t = \prod_{s=1}^{t} \left(\frac{1}{1+r_s}\right)$ for $t \geq 1$. The first order condition is

$$R_t = (1+\tau^L)P_t^L - \frac{P_{t+1}^L}{1+r_t}. \tag{24}$$

This equation tell us that landlords will supply land services to households until the rental price R_t equals the user cost of land.

Producers

Producers hire labor N_t and produce a non-durable output good Y_t with the technology

$$Y_t = F(N_t). \tag{25}$$

They are price-takers in input and output markets, and they maximize profits $\Pi_t = Y_t - W_t N_t$. The first-order condition is

$$F_N(N_t) = W_t. \tag{26}$$

Government

The government collects land taxes $\tau^L P_t^L L_{t-1}$ from landlords at the beginning of each period and pay lump sum taxes T_t to households. We assume

$$T_t = 0 \tag{27}$$

i.e. the government “throws the revenue in the ocean”.

Market Clearing and Rent Ceiling

The goods and asset market must clear:

$$C_t + R_t \bar{L} = Y_t \tag{28}$$

$$A_t = 0 \tag{29}$$

The aggregate amount of land in the economy is fixed:

$$L_t = \bar{L}$$

There is a price ceiling on the rental price of land:

$$R_t \leq \bar{R} + \tau^L P_t^L. \tag{30}$$

The rent ceiling increases with land taxes: When the landlords' tax bill increases, the ceiling increases by the same amount. We will assume the rent ceiling binding in steady state. (We can verify this by checking that there excess demand for land services.)

B.1 Functional Forms

We assume

$$\begin{aligned} u(C,N,L) &= \log C_t + \vartheta \frac{N^{1+\frac{1}{\chi}}}{1+\frac{1}{\chi}} + \varphi \log L_t, \\ F(N) &= Z_Y N^\gamma. \end{aligned}$$

B.2 Model Equations

The model features 11 variables $(C_t, N_t, L_t, A_t, Y_t, R_t, P_t^L, W_t, r_t, T_t, L_t^D)$ and 11 equations:

$$\frac{1}{C_t} = \beta(1+r_t)\frac{1}{C_{t+1}} \tag{31}$$

$$\vartheta N_t^{\frac{1}{\chi}} C_t = W_t \tag{32}$$

$$\varphi \frac{C_t}{L_t^D} = R_t \tag{33}$$

$$R_t = (1+\tau^L)P_t^L - \frac{P_{t+1}^L}{1+r_t} \tag{34}$$

$$Y_t = Z_Y N_t^\gamma \tag{35}$$

$$\gamma \frac{Y_t}{N_t} = W_t \tag{36}$$

$$T_t = 0 \tag{37}$$

$$L_t = \bar{L} \tag{38}$$

$$C_t + R_t L_t = Y_t \tag{39}$$

$$A_t = 0 \tag{40}$$

$$R_t = \bar{R} + \tau^L P_t^L \tag{41}$$

B.3 Steady State

Since we are studying a permanent change in land taxes, we are primarily interested in the steady state. We denote steady values of the variable simply by removing the t-subscript. We are particularly interested in how land rents R and the price of land P^L changes after an increase in the land value tax rate τ^L. We solve for the steady state of the model as follows:

1. From the Euler equation, we have $r = \frac{1}{\beta} - 1$. We also immediately have $L = \bar{L}$, $A = 0, T = 0$.

2. Combine the landlord's FOC with the rent ceiling:

$$\bar{R} + \tau^L P^L = (1+\tau^L)P^L - \frac{P^L}{1+r} \quad \Rightarrow \quad P^L = \frac{1+r}{r}\bar{R}.$$

3. The rental price of land is then

$$R = \bar{R} + \tau P^L = \bar{R} + \tau \frac{1+r}{r}\bar{R} = \frac{(1+\tau)(1+r)-1}{r}\bar{R} \approx \frac{r+\tau}{r}\bar{R}.$$

4. Labor market clearing implies $\vartheta N^{\frac{1}{\chi}} C = \gamma Z_Y N^{\gamma-1}$. Goods market clearing implies $C = Z_Y N^{\gamma} - R\bar{L}$. Combining these to eliminate C yields

$$\vartheta N^{\frac{1}{\chi}}(Z_Y N^{\gamma} - R\bar{L}) = \gamma Z_Y N^{\gamma-1}.$$

   We solve this equation numerically.

5. Once we know N, it is straightforward to back out C, Y, W.

6. Finally, we can check the demand for land services and verify that there is excess demand $L^D > \bar{L}$ (so our initial assumption that the rent ceiling is binding is valid).

B.4 The Effect of Land Tax Increase on Land Prices and Rents

How does the steady state change when the land value tax rate τ^L increases? The price of land P^L does not change: It depends only on the interest rate r and the fixed part of the rent ceiling $\bar{R}$. When the tax rate increases, landlords pass through the tax increase to tenants (the households) through higher rents. This happens because the rent ceiling increases and there is excess demand for land.

C Comparison with Høj, Jørgensen and Schou (2018)

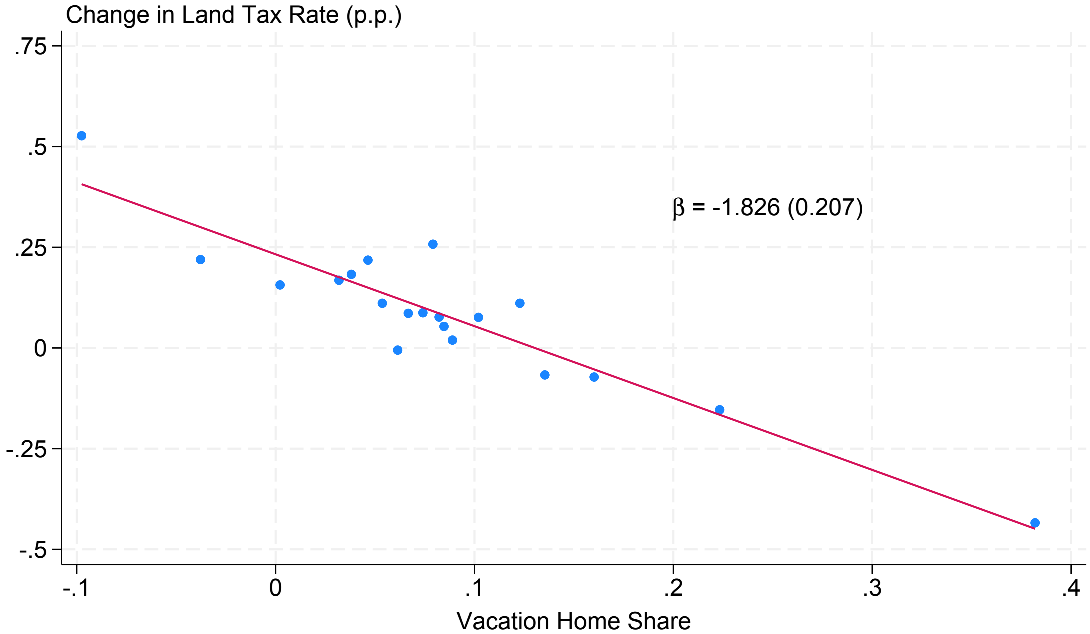
In this appendix, we explain potential differences between our results and those in Høj, Jørgensen and Schou (2018). Høj, Jørgensen and Schou (2018) study the effect of the same reform on prices of single-family homes and find large price effects. They use a different regression than the one we do; for example, they do not instrument for the change in the land tax rate, do not use a repeat-sales index, do not restrict the treatment group to border towns, and only examine the years immediately surrounding the reform, from 2001 - 2008. However, we argue that these differences in the empirical strategy do not drive the differences between our results and those found by Høj, Jørgensen and Schou (2018). Instead, we argue that a confound likely drives the differences between their results and ours.

Appendix Figure A3 shows a binscatter plot of a regression of the actual change in the land tax rate and the number of vacation homes in a treatment area, after residualizing on new municipality fixed effects. The share of vacation homes in an area is strongly predictive of the land tax change. Areas that experienced land tax decreases had significantly more vacation homes than areas that experienced tax increases. Due to this correlation, in our main specification, we control for the share of vacation homes in an area and drop vacation homes from our baseline sample.

Appendix Figure A4 shows an event study that uses variation in land tax changes alone (the policy shock) on the prices of single-family homes. In this specification, we keep all of our baseline controls and primary sample, except we drop our control for the share of vacation homes in the treatment area and add vacation homes to the sample. In this specification, we find large, persistent effects of land taxes on home prices. Our baseline results are actually quite similar to those in Høj, Jørgensen and Schou (2018), multiplying our ITT effect by our first stage results in a discount rate of $\approx 2.4\%$. This is nearly identical to what Høj, Jørgensen and Schou (2018) estimate for 2007. However, their treatment effect falls by nearly half in 2008, relative to 2007. In contrast, our estimates are quite persistent through 2019.

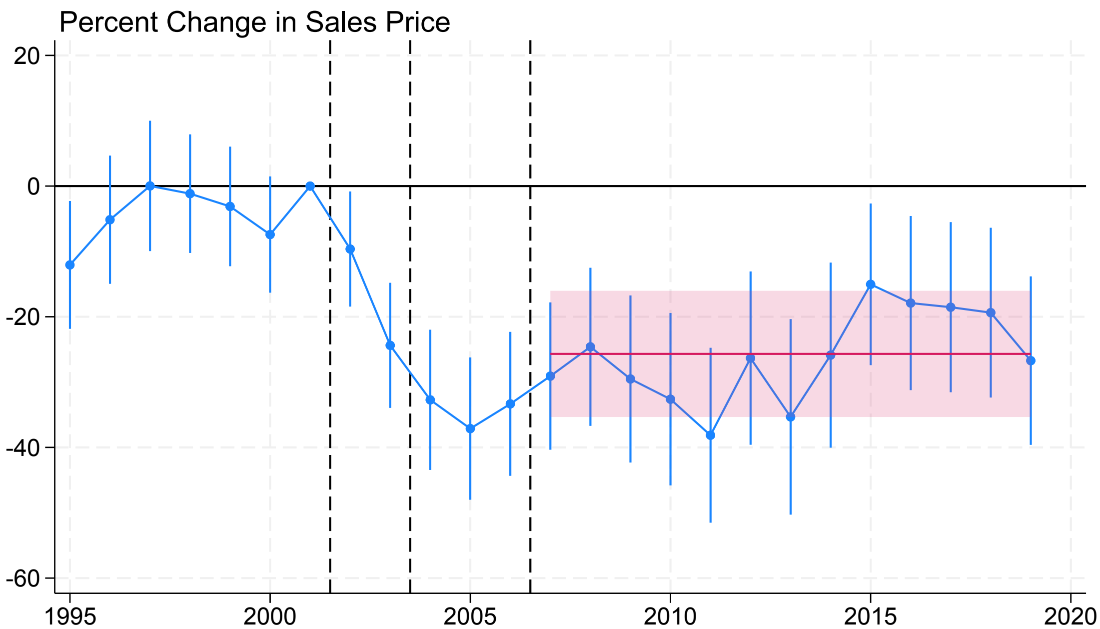
Appendix Figure A5 shows the results of the exact same specification as Appendix Figure A4 after dropping vacation homes and adding in our control for the pre-existing vacation home share in a treatment area. These two choices are enough to give us the baseline result that land taxes have no effect on sales prices. We once again get a precise zero effect, and can rule out with a high degree of confidence the point estimate from Høj, Jørgensen and Schou (2018).

Figure A3: Land Tax Changes Strongly Correlated With Share of Vacation Homes in Treatment Area



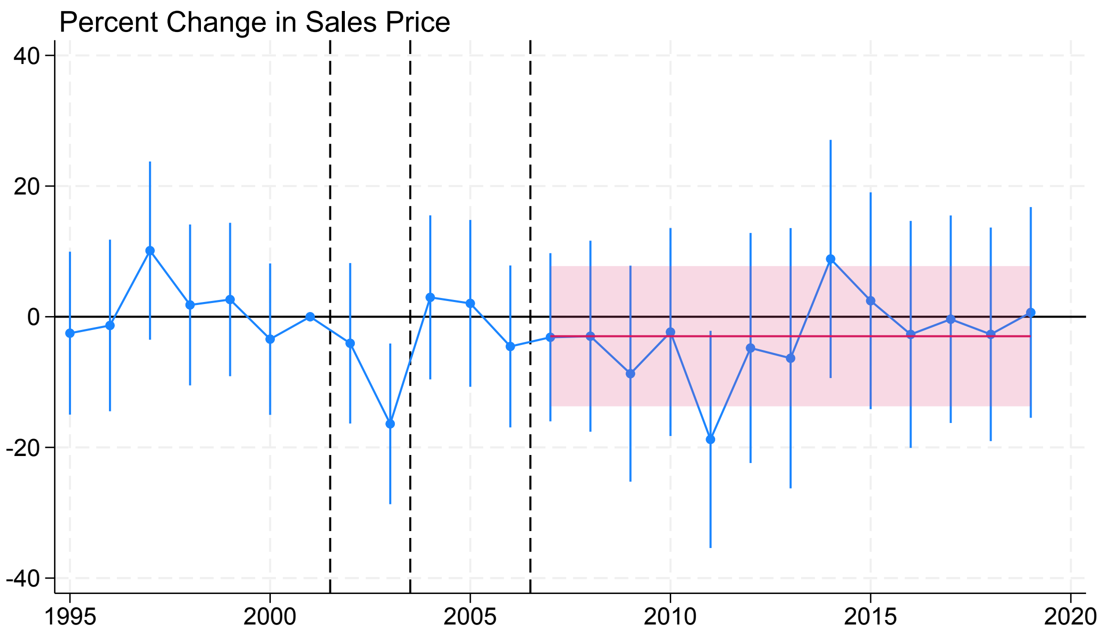
Notes: This figure plots the first stage regression of the land tax instrument on the actual change in effective taxes paid, normalized by the assessed value of the property, from 2005 to 2007. For both the x- and y-axes, the units are in percentage points: thus, a value of 1 represents a 1 p.p. change in the tax rate, normalized by the assessed value. The blue line represents the 45-degree line. Beta coefficients and 95% confidence intervals, with standard errors clustered at the treatment area by land share decile level, are plotted.

Figure A4: ITT Effect of Land Taxes Including Vacation Homes and Without Control



Notes: This figure plots the effect of land taxes on sales prices, including vacation homes and without the treatment area vacation share control. 95% confidence intervals are shown for the event-study coefficients, with standard errors clustered at the treatment area by land share decile level. The red line shows the difference in difference estimate that pools all post-2007 years compared to the price in 2001, and the red shading shows the 95% confidence intervals from that regression.

Figure A5: ITT Effect of Land Taxes Without Vacation Homes and Including Control



Notes: This figure plots the effect of land taxes on sales prices. 95% confidence intervals are shown for the event-study coefficients, with standard errors clustered at the treatment area by land share decile level. The red line shows the difference in difference estimate that pools all post-2007 years compared to the price in 2001, and the red shading shows the 95% confidence intervals from that regression.