Are tax benefits priced into home values? Evidence from the Tax Cuts and Jobs Act

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October 2023

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This paper measures the effect of homeownership tax deductions on home values using the Tax Cuts and Jobs Act, which substantially reduced the tax benefits to homeownership starting in 2018. Homes vary in exposure to the 2018 changes due to differences in the incomes, mortgage amounts, state income taxes, and property taxes of potential buyers. Event studies comparing the price growth of more and less exposed homes within the same city offer no evidence that home prices fell due to the tax change. Estimated standard errors rule out that even a quarter of the tax increase passed through to lower home prices within two years of the policy change. The tax increase reduced housing supply, as evidenced by a slowdown in new building in relatively exposed areas in cities with relatively elastic housing supply, which dampened the price decrease. Because prices did not fall when taxes increased, the after-tax cost of homeownership increased by roughly the same amount that taxes increased.

Email: hortonem@umich.edu. I thank my advisors Jim Hines, Joel Slemrod, Ashley Craig, and Nirupama Rao for their guidance and support. I also thank Lea Bart, Aaron Kaye, Dena Lomonosov, Mike Mei, Emir Murathanoglu, Nikhil Rao, Katherine Richard, and Brock Rowberry for providing helpful comments and suggestions, as well as participants in the University of Michigan Public Finance workshop and seminar and the National Tax Association conference. The CoreLogic data used in this paper is licensed material obtained with the assistance of the University of Michigan Libraries.

1. Introduction

Historically, homeownership has been heavily subsidized in the United States, in large part through income tax deductions for mortgage interest and property tax expenses. These deductions cost roughly $100 billion in revenue in 2017 – almost a quarter of the cost of the entire Medicaid program or more than a sixth of the total defense budget.[1] The Tax Cuts and Jobs Act (TCJA) significantly reduced the value of the mortgage interest and property tax deductions, starting in 2018. Economists and policymakers generally expected this would deflate housing demand, causing a decrease in home values, homebuilding, and/or homeownership rates. However, empirical evidence on the impact of these deductions on housing markets is limited, in large part due to limited policy variation before the TCJA.

This paper estimates the impact of the TCJA on home prices using variation in buyers' exposure to the tax change between different types of homes within the same city. More expensive homes and homes in higher-income and higher-tax areas were more exposed to the tax change, on average. If demand shifted away from the most exposed homes toward rentals or less exposed homes, we would expect to see the prices of the most exposed homes fall relative to their less exposed neighbors. However, price growth of the most exposed homes within a city tracked the price growth of the least exposed homes within 1 percentage point in the years immediately before and after the policy change. Event study estimates rule out with high confidence that even a quarter of the tax increase passed through to lower home prices in the two years after the TCJA was enacted. This is robust to controlling for exposure to the other income and business tax changes in the TCJA.

National microdata on home sale transactions, property tax bills, and mortgages are used to construct measures of home price growth and exposure to the tax change disaggregated by detailed submarket within cities. The exposure measure captures how much the TCJA reduced the present discounted value of the mortgage interest and property tax deductions for the average buyer in each submarket. It directly corresponds to the predicted reduction in home prices if the reduction in the tax benefits were fully capitalized into prices. Buyers' incomes, marital status, mortgages, and property tax bills in the years before the policy change are used to predict exposure in each submarket in the years after the policy change. Submarkets are defined by Census tract and home size because these groups of homes have similar prices and buyer demographics.

[1] JCX-3-17 and CBO January 2017 Budget and Economic Outlook.

The identifying assumption is that absent a policy change, price growth rates in more and less exposed submarkets within the same city would follow parallel trends. This allows for the possibility that high exposure cities experience different market shocks than low exposure cities. It also allows for the possibility that high exposure submarkets have different baseline price growth rates than low exposure submarkets – a pattern observed in the data. Price growth rates in high and low exposure submarkets within the same city tracked each other closely in the two years before the policy change, suggesting this is a reasonable assumption, but diverged slightly in earlier years. Allowing for differential national trends in price growth by buyer income explains this divergence and confirms the finding that prices did not respond to the policy change.

The small price response reflects in part that the quantity of housing supply available to owner-occupiers can adjust to absorb changes in housing demand, reducing the pressure for prices to adjust. It also likely reflects that homebuyers' demand fell less than expected - for example, because the change in tax incentives was not salient or because buyers did not expect the tax change to remain permanent. This paper tests for housing supply adjustments along two margins: a reduction in the amount of new housing built, and a conversion of existing homes to rentals. The tax increase caused new building to slow down in more exposed counties relative to their less exposed neighbors. This reduction in new building was concentrated in cities where housing supply is relatively unconstrained by geography and regulation (Saiz 2010). The tax increase did not cause an increase in investor purchasing activity in relatively exposed submarkets, suggesting the conversion of existing homes to rentals was not an important margin of adjustment.

The small price response indicates that existing homeowners did not lose significant wealth as result of the policy change. The after-tax cost of purchasing a home has increased significantly, because the reduction in tax benefits to homeownership was not offset by a decrease in home prices. For the median homebuyer in the sample, the long-term value of lost tax benefits is equivalent to roughly a 3.4 percent increase in the price of purchasing a home. High-income buyers and buyers in high-tax, high-priced areas were generally impacted most. For example, the median Washington DC area buyer with income between $150-$200k lost tax benefits equivalent to more than a 9 percent increase in the price of purchasing a home.

This paper contributes to the literature on the effect of tax subsidies on housing markets, reviewed in section 2.3, by precisely measuring how a change in tax subsidies affects home prices. This is necessary to understand the effect of tax subsidies on home buying decisions, because buyers are affected by both changes in prices and changes in tax costs. It uses detailed national data to measure home price growth and exposure to the tax change at the necessary level of disaggregation to identify changes in the distribution of home prices within a city. It constructs an exposure measure that captures the effect of all major policy changes in the TCJA on the cost of purchasing a home relative to renting, which directly corresponds to the predicted change in price if the tax change was fully capitalized into home values. Finally, it uses an event study framework to measure the impact of exposure on home prices, relying on transparent identifying assumptions that can be evaluated from how prices evolved before the policy change. To my knowledge, it also presents the first evidence on the effects of the TCJA on housing supply.

2. Background

2.1. The TCJA reduced income tax subsidies for homeownership

The costs of homeownership are directly subsidized by the tax code through itemized deductions for mortgage interest and property tax expenses. If a home was taxed like a business asset, the income it generates (the value of housing services plus capital gains) less the costs (mortgage interest, property taxes, and maintenance) would be taxable income. The mortgage interest and property tax deductions function as a subsidy to homeownership because the income from owning a home is generally not taxable, but the associated expenses are still partially deductible.[2] These deductions are most valuable to high-income households because high-income households are more likely to own their homes, have higher itemized deductions relative to the standard deduction, and are taxed at higher marginal rates.[3] Conditional on income, homeowners in areas with higher housing costs and higher taxes benefit most, because their mortgage interest and property tax expenses are a larger share of their total income.

The TCJA made several changes that reduced the value of itemized deductions. First,

[2] The value of housing services is not imputed in taxable income, and capital gains on the sale of a primary residence are explicitly excluded from taxable income. The exclusion applies up to a cap of $500,000 for married filers. JCT estimated this cost roughly $32 billion in foregone revenue in 2017 (JCX-3-17). This policy was not changed by the TCJA.

[3] Prior to the TCJA, the Congressional Budget Office estimated that tax benefits from the mortgage interest and state and local tax deductions averaged about 2.5% of income for households in the top quintile of the income distribution, more than twice the tax benefits for those in the middle of the income distribution. Congressional Budget Office, The Distribution of Major Tax Expenditures in the Individual Income Tax System, May 2013.

it doubled the standard deduction.[4] Itemized deductions only reduce tax liability to the extent that they exceed the value of the standard deduction, so this change reduced the value of itemized deductions for all itemizers. Second, it limited the itemized deduction for state and local taxes (SALT), including income and property taxes, to $10,000. This reduced or eliminated the value of the property tax deduction for many homeowners. It also introduced some relatively minor limitations on the mortgage interest deduction.[5]

The national share of tax filers that itemize fell from 31% in 2017 to 11% in 2018, indicating these changes combined eliminated homeownership deductions for a large portion of households.[6] Even among homeowners that do still itemize, the value of itemized deductions has fallen significantly. Due to the higher standard deduction and SALT limit, a smaller share of homeownership expenses are deductible. Further, many filers are now taxed at lower marginal rates, so all deductions are less valuable. Most filers that still itemize are constrained by the $10,000 SALT limit - on the margin, they receive a tax subsidy for an additional dollar of mortgage interest, but not an additional dollar of property tax expenses.[7]

The effect of these tax deductions on the economic costs of owning a home can be shown most precisely through the user costs of homeownership, which accounts for all after-tax costs of owning a home, including the opportunity cost of investing capital in housing rather than an alternative asset (Hendershott and Slemrod 1982; Poterba 1984; Gyourko and Sinai 2003). Table 1 shows an expression for each component of the user cost. Here P represents the price of the home and λ represents the loan-to-value ratio. The relevant tax parameters are the property tax rate τ_{prop}, the marginal income tax rate τ_{inc}, the tax rate on investment income τ_{inv}, and the standard deduction STD. D represents the household's other potential itemized deductions, which primarily consist of state income taxes. The returns from house price appreciation are represented by $r_{housing}$ and the returns to an alternative investment (e.g. in the stock market) are represented by r_{alt}. δ is the depreciation rate on housing.

The TCJA directly changed the user costs of homeownership by changing the value of the income tax deductions for mortgage interest and property taxes, which is the

[4]For married couples, the standard deduction increased from roughly $12,000 to $24,000.

[5]Specifically, it restricted the mortgage interest deduction to apply only to the first $750,000 of principal (reduced from $1 million before), and restricted the deduction for interest on home equity lines of credit to funds used for home improvements

[6]IRS Statistics of Income, Individual Complete Report, Table A.

[7]The households with the very largest mortgages receive no tax subsidy for an additional dollar of mortgage interest, because the TCJA limited mortgage interest deductions to the first $750,000 of mortgage principal. Prior to the TCJA, the limit was $1 million of mortgage principal.

TABLE 1. Components of the annual user cost of homeownership

	Total cost of ownership	Marginal cost for an additional dollar of home purchased
Mortgage interest	$r_{mort}\lambda P$	$r_{mort}\lambda$
Property tax	$\tau_{prop}P$	τ_{prop}
Income tax deductions for income and property taxes*	$-\tau_{inc} \times \max\{\left(r_{mort}\lambda + \tau_{prop}\right)P - \max\{STD - D, 0\}, 0\}$	$-\tau_{inc}\left(r_{mort}\lambda + \tau_{prop}\right)P$ if $\left(r_{mort}\lambda + \tau_{prop}\right)P > STD - D$, 0 otherwise
Maintenance	δP	δ
Capital gains (untaxed)	$-r_{housing}P$	$-r_{housing}$
Opportunity cost of investing down payment in housing	$r_{alt}(1-\tau_{inv})(1-\lambda)P$	$r_{alt}(1-\tau_{inv})(1-\lambda)$

*This expression abstracts away from nonlinearities in the income tax schedule beyond the standard deduction. It reflects the tax system before the TCJA, but does not reflect the additional limitation on state income taxes and property taxes implemented in the TCJA. Assuming D is entirely comprised of state income taxes, the value of itemized deductions after the TCJA can be written as $-\tau_{inc} \times \max\{(r_{mort}\lambda P + \min\{\tau_{prop}P, \max\{10000 - D, 0\}\}) - (STD - \min\{D, 10000\}), 0\}$

focus of this paper.[8] Given that expenditures on owner-occupied housing are now subsidized less, homebuyers may respond on the the extensive margin by choosing to delay homeownership or not to buy a house at all, opting to rent instead. They may also respond on intensive margin by choosing a smaller house or a less expensive neighborhood than they would have chosen otherwise. Finally, because mortgage interest is no longer subsidized as heavily, households may choose to reduce their loan-to-value ratios.

These policy changes occurred within a broader landscape of individual and business tax cuts. Many filers received net tax cuts, which could have affected housing markets via increased disposable income in some areas. Businesses received large rate cuts and retained the ability to deduct interest and property tax expenses from their income, potentially increasing the incentives for business investors to move into residential markets.

2.2. Theoretical impact of tax subsidies on home values

Considering households choosing whether to own a home relative to renting generates sharp predictions for the impact of the change in tax benefits on home prices. In the

[8]It is also possible that general equilibrium effects of the TCJA indirectly changed the user cost of homeownership by changing the capital gains on housing $r_{housing}$ relative to the alternative r_{alt}

simplest model of housing markets, where the quantity of housing supply available for owner-occupiers is fixed, tax changes are fully capitalized into home prices. For the marginal buyer to remain indifferent between owning and renting, the after-tax price of buying a home must remain constant. That is, home prices would fall by the same amount that tax benefits decreased.

If demand did not fully respond to the change in tax incentives - either because owner-occupiers do not understand the policy or because they did not expect the policy to remain permanent - the impact of the tax change on housing markets would be dampened. If the quantity of housing supply available for owner-occupiers is not fixed and can adjust to changes in demand, then the impact of reduced demand would be absorbed through a combination of reduced house prices and quantities. This would further dampen the effect of the tax change on home prices.[9]

This section presents a model of the housing market that accounts for tax salience to buyers and two margins of supply adjustment. The first is a change in the total quantity of owner-occupied housing available, which would occur through a decrease in new building. The second is a conversion of some of the existing stock of single family housing to rentals, which would occur through investors in the market that purchase homes and rent them out (Greenwald and Guren 2021).[10]

Market demand for single family homes X is a combination of demand from owner-occupiers X_O and demand from investment buyers X_I. Total supply of single family homes is represented by X_S.

$$X_D = X_O(p, (1-\tau)) + X_I(p) \qquad\qquad X_S = X_S(p)$$

The home price is represented by p and the tax subsidy rate for owner-occupiers is represented by τ. Since itemized deductions reduce tax liability every year, τ represents the present discounted value of these annual tax benefits over the life of the home, as a share of the home price.

Log linearizing with a respect to a change in the net-of-subsidy rate $(1-\tau)$ gives:

$$d\ln p = -\theta \frac{\gamma \epsilon_D}{\epsilon_S + \gamma \epsilon_D + (1-\gamma)\epsilon_I} d\ln(1-\tau) \tag{1}$$

[9] Housing supply is more elastic over the long term than the short term. In order for housing supply adjustments to dampen price decreases in the short term, buyers would need to anticipate future supply reductions and factor them into their expectations of future price growth.

[10] Single family rentals represent a minority of the total rental market, as many rentals are in multifamily buildings. In the 2017 American Community Survey, roughly a third of renter households lived in single family homes.

Where

$$\frac{\partial \ln X_o}{\partial \ln p} = -\epsilon_o \qquad \frac{\partial \ln X_o}{\partial \ln(1-\tau)} = -\theta\epsilon_o \qquad \frac{\partial \ln X_I}{\partial \ln p} = -\epsilon_I \qquad \frac{\partial \ln Y}{\partial \ln p} = \epsilon_s \qquad \gamma = \frac{X_O}{X_O + X_I}$$

The level of tax salience is represented by θ, which governs the extent that owner-occupiers respond to a change in the tax subsidy relative to a change in the before-tax price (Chetty, Looney, and Kroft 2009). Adjustments in the quantity of owner-occupied housing are governed by the price elasticity of total housing supply ϵ_S, the price elasticity of investor demand ϵ_I, and the share of total buyers represented by owner-occupiers γ.[11] The term $\theta\frac{\gamma\epsilon_D}{\epsilon_S+\gamma\epsilon_D+(1-\gamma)\epsilon_I}$ represents the capitalization rate of the tax change into home prices. In the full capitalization scenario, the capitalization rate is 1 and $d\ln p = -d\ln(1-\tau)$.[12]

This model predicts price changes within a relatively homogenous submarket of homes. In the empirical analysis, this is operationalized by grouping together similarly sized homes within the same Census tract.[13] The tax change $d\ln(1-\tau)$ is estimated for the typical buyer in each submarket, and price growth in more exposed submarkets is compared to price growth in less exposed submarkets.

This model assumes the price of the alternative option - renting - is exogenous and remains constant when taxes change. This is a reasonable assumption if the overall supply of rental housing is very elastic, i.e. because the marginal cost of building additional apartments is relatively constant. If not, a decrease in demand for owner-occupied homes relative to rentals can be absorbed by a combination of increased rent prices and reduced home prices within that market segment. In this case, the ideal measure of home prices would be the ratio of home price to the cost of renting a unit of the same quality. Rent data is less available than home price data, but rent prices indices based on online listing and/or survey data are available for broader markets. Appendix E confirms that rent price indices did not change in the most exposed counties relative to their less exposed neighbors after the tax change.

In addition to reducing the incentive to buy relative to renting, the reduction in tax subsidies also reduced the incentive to purchase a higher-quality, more expensive home relative to a lower-quality, less expensive home. Because home quality is positively

[11]Greenwald and Guren (2021) motivate the price elasticity of investor demand with the assumption that there is some variation in the cost of converting homes to rentals within the housing stock, so investor demand is driven by the marginal cost of converting the additional home to a rental.

[12]Full capitalization corresponds to $\theta = 1, \epsilon_S = 0$, and $\epsilon_I = 0$

[13]This is similar to the segment definitions used by Piazzesi, Schneider, and Stroebel (2020), which group homes by zipcode and size and/or price.

correlated with exposure to the tax change, this would drive substitution from more exposed homes toward less exposed homes. These intensive margin responses could drive even larger differentials in price growth between more and less exposed homes than extensive margin responses alone. Appendix F discusses changes in the incentives to purchase a higher vs lower quality home in more detail.

2.3. Evidence on tax subsidies for homeownership

The TCJA presents a much larger and newer source of variation than previously available to study the effects of the mortgage interest and property tax deductions on housing markets. This paper contributes to the literature on the effect of tax subsidies on housing markets by precisely measuring how a change in tax subsidies affects home prices. This is necessary to understand the effect of tax subsidies on home buyers, because home buyers are affected by both changes in prices and changes in tax costs. To my knowledge, it also presents the first evidence on the effects of the TCJA on housing supply.

The evidence of the effect of income tax subsidies on housing demand suggests that buyers do respond to reductions in tax subsidies by purchasing smaller and less expensive homes. Gruber, Jensen, and Kleven (2021) study a 1987 tax change in Denmark that reduced the value of the mortgage interest deduction disproportionately for high-income households. They find that higher-income households who moved after the tax change bought smaller, less expensive homes, implying an elasticity of the value of the home purchased with respect to the net of tax rate of roughly -0.2. Lomonosov (2022) compares Middlesex County, New Jersey buyers likely to be more affected by the TCJA to those likely to be less affected by the TCJA. More affected buyers tend to buy smaller and less expensive homes after the TCJA, implying an elasticity of the value of the home purchased with respect to the net of tax rate of -1.[14]

The evidence on the impact of tax subsidies on homeownership rates is mixed. Gruber, Jensen, and Kleven (2021) find that Denmark's tax reform had no impact on homeowership rates of higher-income households relative to lower-income households, even over the long term. Hilber and Turner (2014) estimate how households in the Panel Study of Income Dynamics were affected by changes in the marginal subsidy rate on mortgage interest caused by moves between states and changes in state and

[14] Both studies also find that affected buyers reduce their loan-to-value ratios in response to a reduction in the value of the mortgage interest deduction. Dunsky and Follian (2000) find homeowners reduced mortgage borrowing after the Tax Reform Act of 1986 reduced the marginal value of the mortgage interest deduction.

federal tax policy over time.[15] Their analysis suggests that higher subsidies increase homeownership rates for high-income households, but the effect on homeownership rates varies both by income and housing supply elasticity in the area. They argue this suggests capitalization of subsidies into home prices is an important mediator of the relationship between subsidy rates and homeownership rates.[16]

While the interpretation of the evidence on the relationship between tax subsidies and home purchases depends on how tax subsidies are capitalized into home prices, the evidence on this is more limited. Poterba (1984) argued that differences in home price appreciation by market tier in the years after the 1986 tax reform were consistent with differential changes in the tax subsidy to homeownership for higher-income relative to lower-income households. Davis (2019) compares prices of observationally similar homes across state borders and estimates that about 80% of the change in subsidy rates from crossing a state border is capitalized into home prices. Lomonosov (2022) estimates the relationship between property tax bills and price growth among New Jersey homes, and finds that homes with higher property tax bills above some threshold had lower price growth in 2018 and 2019. If this is attributed to the SALT cap, it would be consistent with roughly a 60 percent capitalization rate into home prices.[17]

The primary contribution of this paper to the literature on the mortgage interest and property tax deductions is measuring how they impact home prices. It uses national data on home sales and property tax bills to precisely estimate how the subsidy changes in the TCJA were capitalized into prices nationally. Price growth and exposure to the tax change are measured by detailed submarket within city, which captures more variation in exposure to the tax change than broader measures and makes it possible to control for differential underlying trends in price growth between cities. The exposure measure is constructed from the change in tax subsidies for the typical buyer in each submarket, which generates a sharp prediction for the change in price if the tax subsidy was fully capitalized into prices. Finally, the effect of exposure to the tax change on price growth

[15]Hembre and Dantas (2022) compare households across states more vs less affected by the TCJA and find that homeownership rates fell among households in more affected states. This requires the assumption the homeownership rates would evolve similarly between more and less exposed states, conditional on the characteristics of the household, absent a policy change.

[16]Sommer and Sullivan (2018) develop a macroeconomic model to estimate the impact of the elimination of the mortgage interest deduction on homeownership rates and find that it depends on how the mortgage interest deduction is capitalized into prices.

[17]Li and Yu (2022) compare price growth in counties that claimed more vs fewer SALT deductions before the TCJA and find a small reduction in price growth in more exposed counties. McClelland, Mucciolo, and Sayed (2022) compare PUMAs with higher vs lower exposure to the TCJA and find that after controlling for income, more exposed PUMAs had slightly lower mortgage amounts and home prices.

is estimated in an event study framework using transparent identifying assumptions that can be evaluated from how prices evolved in the years before the policy change.

3. Methodology

3.1. Data

The primary data used in the analysis is CoreLogic database, which compiles public records on house sales and property tax assessments, links them together at the property level, and supplements them with proprietary data. This covers nearly the universe of house sales in the United States. The base sample is arm's length sales of residential properties between 2013-2019 that are linked to a property tax record. New builds, condos, and foreclosures are excluded, as these are generally priced differently than sales of existing homes and would introduce noise into measures of home prices. Cities are defined by Core Based Statistical Area (CBSA), and rural areas not located in a CBSA are excluded from the sample.

To measure buyers' tax parameters, sales are linked to mortgage records in the CoreLogic data to obtain the mortgage amount, then matched to mortgage applications in the HMDA data to obtain the buyer's income and marital status. 73 percent of sales in the sample are linked to a mortgage origination record.[18] These loans are then matched to mortgage applications in the HMDA data on Census tract, year, loan type (conventional, FHA, or VA) and loan amount.[19] 75 percent of pre-period mortgages match to the HMDA data. Only loans in the period before the policy change are matched to the HMDA data, because HMDA reporting rules changed starting in 2018, and matching on loan amount is substantially less exact in recent data.

Census tracts and CBSAs without sufficient data coverage are excluded from the analysis. Similarly sized homes are grouped together within each Census tract - these tract by size bins are referred to as submarkets. Submarkets with less than 50 total homes (as measured from 2016 property tax data on the full housing stock) are excluded from the sample due to insufficient sample size. Census tracts are excluded from the analysis if less than 60% of the housing stock is included in the sample, if more than

[18] Sales are linked to mortgages by CoreLogic's proprietary property identifier and recording date (the date the record was submitted to the local government).

[19] This is a simplified version of the matching strategy proposed by Li and Goodman (2014). Loan amount is rounded to the nearest 1000. The match is unique for 75% of mortgages. For the remaining mortgages that match to more than one observation in the HMDA data, the buyer's income is imputed from the average buyer income among matching HMDA loans.

40% of sales are missing data on price, or more than 60% of pre-period sales cannot be matched to HMDA data.[20] A CBSA is excluded from the final analysis if the covered Census tracts represent less than 60% of the total population in the CBSA. This leaves 14 million sales in 43,864 Census tracts in 450 CBSAs in the final sample. Figure 1 shows coverage of the final sample by CBSA.

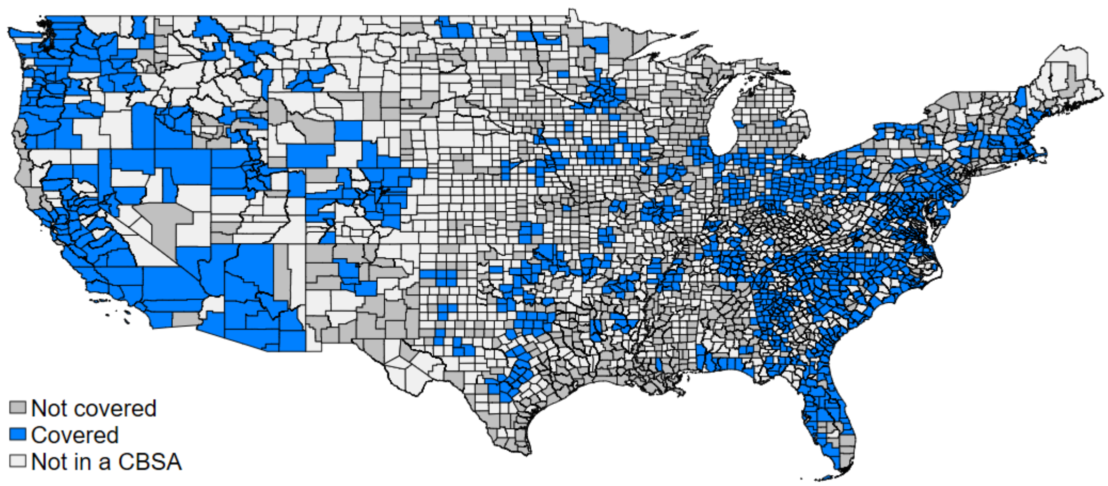

FIGURE 1. Sample coverage by CBSA

The boundaries on the map represent counties. Many CBSAs contain multiple counties.

3.2. Empirical Strategy

3.2.1. Regression specification

The effect of the TCJA on prices is estimated by comparing price growth in relatively more exposed submarkets to relatively less exposed submarkets in an event study framework. The identifying assumption is that, absent a policy change, the price growth rates of more and less exposed homes would follow parallel trends, conditional on the controls included.

$$\Delta \ln(price)_{zst} = \sum_{y \neq 2017} \beta_y * \Delta_{zs} * 1\{y = t\} + \alpha_{zs} + \lambda_{cbsa \times t} + \epsilon_{zst} \tag{2}$$

[20] Tracts in states that do not mandate price disclosure are included in the sample if they meet the criteria that no more than 40% of sales are missing data on price. Results are robust to excluding non-disclosure states.

The index z represents a Census tract, s represents a house size group within the tract (binned in 500 square foot intervals), and t represents the sale year. β_t is the event study coefficient on each year. Δ_{zs} is the exposure measure, which corresponds to an estimate of $-\Delta ln(1-\tau)$ from equation 1. The year before the policy change (2017) is omitted and serves as the reference year. α_{zs} is a unit fixed effect and $\lambda_{cbsa \times t}$ represents CBSA by year fixed effects.

This identification strategy compares more exposed submarkets to less exposed submarkets within the same city. Using within-city variation is a contribution relative to identification strategies that only compare between cities, because houses in the same city should generally experience the same shocks and follow similar price trends absent a policy change. Further, there is more variation in exposure to the tax change within cities than between them, so this approach allows for more precise estimates of the effect of the tax change on price growth than between-city comparisons.

Another advantage of this strategy is that it controls for compositional change in the types of homes being sold over time, including compositional change in response to the policy change. This is important given that sales prices and quantities generally change together, so both would be expected to respond to the policy change. In addition to including unit fixed effects, each observation in the regression is weighted by the stock of homes in that submarket rather than the number of sales. (The stock of homes in a submarket is defined by the total number of homes in that Census tract and size bin appearing in 2017 property tax records.) This ensures that areas where sale prices and volumes fall together will not be downweighted in the regressions.

The event study coefficient β represents the capitalization rate of the tax increase into home prices. Interpreting β as the capitalization rate requires the assumption that the marginal buyer is primarily substituting between buying and renting. If the marginal homebuyer substitutes toward lower quality homes rather than toward renting, the price of high exposure homes will still fall relative to the price of low exposure homes, because home quality and exposure are positively correlated. Appendix F tests this mechanism more directly by comparing price growth of higher quality homes to lower quality homes, where quality is proxied by predicted home price.

The event study specification relies on a continuous exposure measure, which compares more exposed submarkets to less exposed submarkets. This improves power by leveraging detailed variation in exposure, but requires stronger assumptions than comparing exposed submarkets to unexposed submarkets (Callaway, Goodman-Bacon, and Sant'Anna 2021). Interpreting the difference in price growth between a lightly exposed submarket and a heavily exposed submarket as the causal effect of increasing exposure requires assuming that the capitalization rate of taxes into home prices does not systematically differ between markets for high- and low-exposure homes.

The primary specification is estimated using a two-way fixed effects regression with controls. Implementing controls in this way could bias estimates of average treatment effects in settings where there is treatment effect heterogeneity that is correlated with the control variables (Abadie 2005; Sant'Anna and Zhao 2020). Appendix D shows robustness to implementing controls using inverse probability weights, which are more robust to heterogeneous treatment effects. To avoid extrapolating to exposure levels not observed within a CBSA, all event studies are restricted to CBSAs with a broad distribution of exposure to the tax change - at least one submarket in each quartile of the national exposure distribution.[21]

3.2.2. Price growth measure

Home price growth is measured by comparing the prices of houses that sold in one year to prices of observably similar houses that sold in the year before. The relevant observable characteristics are location (Census tract) and home size (square footage, binned in 500 square foot intervals), as these combined explain 82 percent of cross-sectional variation in log prices.[22] Price growth is estimated from the change in average yearly sale price within each tract z by size group s. Price growth is winsorized at the 1st and 99th percentiles within each year in all regressions.

$$\Delta \ln(price)_{zst} = \ln(\overline{sale\ \ price})_{zst} - \ln(\overline{sale\ \ price})_{zs,t-1}$$

In order for the change in average sale prices from one year to the next to be a valid measure of price growth, conditional on location and square footage, the unobserved quality of houses sold must not systematically change from one year to the next. Appendix A evaluates this assumption by plotting changes in the observable characteristics of homes sold over time after conditioning on location and size. The average lot size

[21] This excludes 100 CBSAs. These are primarily small CBSAs without any highly exposed submarkets - while they represent almost a quarter of CBSAs in the sample, they only represent 5 percent of the housing stock in the sample.

[22] Prices growth is not conditioned on additional observables (like acreage, year built, number of bedrooms, and number of bathrooms) because adding this information to square footage does very little to explain cross-sectional variation in sale prices within a tract, and using them would require making parametric assumptions about the relationship between these observables and prices.

and number of bedrooms remain fairly stable over the sample period, while average age increases steadily. This does not vary significantly by exposure to the tax change, so even if age does impact home quality, it is unlikely to bias regressions comparing price growth of more exposed homes to price growth of less exposed homes.[23]

This approach estimates price growth at a finer level of detail than home price indices based on repeat sales, which is necessary to distinguish differentially affected homes within the same tract. Repeat sales models measure the change in the price of the same house from one sale to the next. The advantage of the repeat sales approach is that it eliminates noise and/or bias from differences in the unobserved quality of homes sold from one year to the next, to the extent that the quality of individual homes and the price premium on quality do not vary over time. The disadvantage is that it can only make use of the subset of the sales data for which data on a prior sale is available, and therefore requires aggregating more broadly by geography and/or time.[24] Appendix C shows robustness to measuring price growth at the Census tract level using the FHFA repeat sales index.

3.2.3. Exposure measure

Exposure to the TCJA is constructed by estimating the change in tax benefits to homeownership for the average buyer within each submarket (Census tract by home size). Exposure is calculated for each individual pre-period buyer using the NBER TAXSIM program, which calculates federal tax liability given data on income, filing status, and deductions. The exposure of pre-period buyers is then averaged within each submarket to predict the exposure of the typical buyer in that submarket after the policy change. Exposure is winsorized at the 5th and 95th percentiles before averaging to avoid introducing unnecessary noise from buyers in unusual tax situations.[25]

The annual tax benefit to homeownership is defined as the difference in federal tax liability with mortgage interest and property tax deductions and federal tax liability without these deductions. The annual tax benefit to homeownership before the TCJA is calculated based on 2017 law and the annual tax benefit to homeownership after the

[23] Repeat sales models often assume that home quality does not decline with age, as changes in prices due to aging and changes in prices due to market growth from one year to the next cannot be separately identified without strong assumptions (Cannaday, Munneke, and Yang 2005).

[24] See Bogin, Doerner, and Larson (2019) for a discussion of the relative advantages and disadvantages of repeat sales indices.

[25] To the extent that this understates exposure in the highest-exposure submarkets, it would bias event study results toward finding larger price effects relative to exposure.

TCJA is calculated based on 2018 law. The exposure measure relies on estimates of the subsidy rate τ, which represents the present discounted value of tax benefits relative to the home's price. To rescale an annual tax benefit to a present discounted value of total tax benefits, the annual tax benefit is discounted by 5% annually over 30 years.[26]

Equation 3 summarizes how the exposure measure is constructed. The exposure measure corresponds to an estimate of how much the sale price would change if the tax benefits were fully capitalized into the price of the home.

(3)

$$\Delta_{zs} = \mathbb{E}[\Delta \ln(1-\tau)]_{zs} = -\mathbb{E}\left[\frac{\Delta\tau}{1-\tau}\right]_{zs} = -\mathbb{E}\left[\frac{PDV[\text{2018 tax benefit} - \text{2017 tax benefit}]}{price - PDV[\text{2017 tax benefit}]}\right]_{zs}$$

Estimating exposure requires data on the buyer's income, filing status, and deductions. Each buyer's income and marital status are observed directly from HMDA mortgage application data.[27] Mortgage and property tax deduction amounts are imputed as a function of the home's predicted price in 2017. Home prices are predicted pre-period sales using the following regression, where i indexes an individual sale, t represents the sale year and z represents Census tract:[28]

$$l\,price_i = \beta_1 t_i + \beta_2 l\,sqft_i + \lambda_z$$

This model captures almost all the cross-sectional variation in prices that drives differences in exposure to the TCJA, while smoothing out noise from unusual sale prices in any one year or size group within a tract.[29] Predicted prices capture 77% of the variation in actual log sale prices in 2017.

Effective property tax rates are allowed to vary by Census tract. The effective property tax rate in each Census tract is estimated from the median ratio of property tax bill to purchase price among pre-period sales in the Census tract.[30] The effective property tax

[26]This makes the simplifying assumption that the annual tax benefit to homeownership remains constant over the life of a 30 year mortgage. It does not account for tax benefits beyond the first 30 years, although they would theoretically affect the home's value. It does not account for inflation adjustments in the tax code or potential growth in property tax bills over time. It also assumes the average annual mortgage interest expense is deducted in every year, rather than accounting for the front loading of mortgage interest expenses early in the mortgage. Accounting for these factors would generally increase estimated exposure.

[27]Only purchases with complete data are used to predict exposure to the tax change.

[28]Log sale prices are winsorized at the 1 and 99 percentiles within each year.

[29]Home prices exhibit mean reversion - a market that experiences unusually high price growth in one year will, on average, exhibit unusually low price growth the next year. Smoothing out prices between years prevents this mean reversion from biasing results.

[30]Property tax bills are measured in the year after the sale.

rate is applied to the predicted home price to impute the buyer's annual property tax expense.

Imputed mortgage interest expenses abstract away from any variation in mortgage parameters by income or CBSA. All buyers are assumed to borrow 92% of predicted price at a 4% rate over 30 years, and deduct their average annual interest expense over the life of the mortgage.[31] TAXSIM accounts for state income taxes in itemized deductions, but do not account for charitable contributions or other itemized deductions unrelated to homeownership.

Any noise in the predicted exposure measure relative to the actual exposure of buyers after the policy change will attenuate the estimated effect of the tax change on prices. Noise in predicted exposure could occur because the assumptions used to impute mortgage interest and property tax expenses smooth over some variation in actual exposure. It could also occur because the demographics of past buyers may not perfectly predict the demographics of future buyers.[32] In order to quantify how much attenuation could be introduced by predicting exposure in this way, predicted exposure from 2013-2016 sales is compared to actual exposure from 2017 sales.[33]

Figure 2 is a binned scatter plot showing the correlation between predicted exposure and actual exposure within each submarket (Census tract by size group). The two measures are closely correlated within CBSA, and a regression of predicted exposure on actual exposure has a coefficient of 0.69. This suggests that noise in the exposure measure attenuates the estimated effect of exposure on prices by no more than one third. Put differently, if the event study results were interpreted in an instrumental variable framework where the predicted exposure measure is an instrument for actual exposure, the magnitude of the estimated relationship between exposure and price growth would be scaled up by about 50% (1/0.69).

Appendix B shows that the exposure measures aligns well with estimates of exposure to the TCJA using alternate data sources. Average exposure among homebuyers in a county correlates positively with the share of households that itemized deductions in that county in 2017, which is a proxy for the share of all households in the county that

[31]92% is the median loan to value ratio in the sample. 4% is roughly the average interest rate on a 30 year mortgage during the sample period, as reported by Freddie Mac from the Primary Mortgage Market Survey.

[32]Noise could also come from the assumption that the average buyer is a decent proxy of the marginal buyer, but this cannot be directly tested.

[33]Actual exposure is calculated using the home price and mortgage amount observed for an individual buyer in the data, rather than the smoothed versions of those variables imputed when constructing the exposure measure. This is validated against 2017 sales rather than 2018 sales because the HMDA data is not matched for 2018 and 2019 sales.

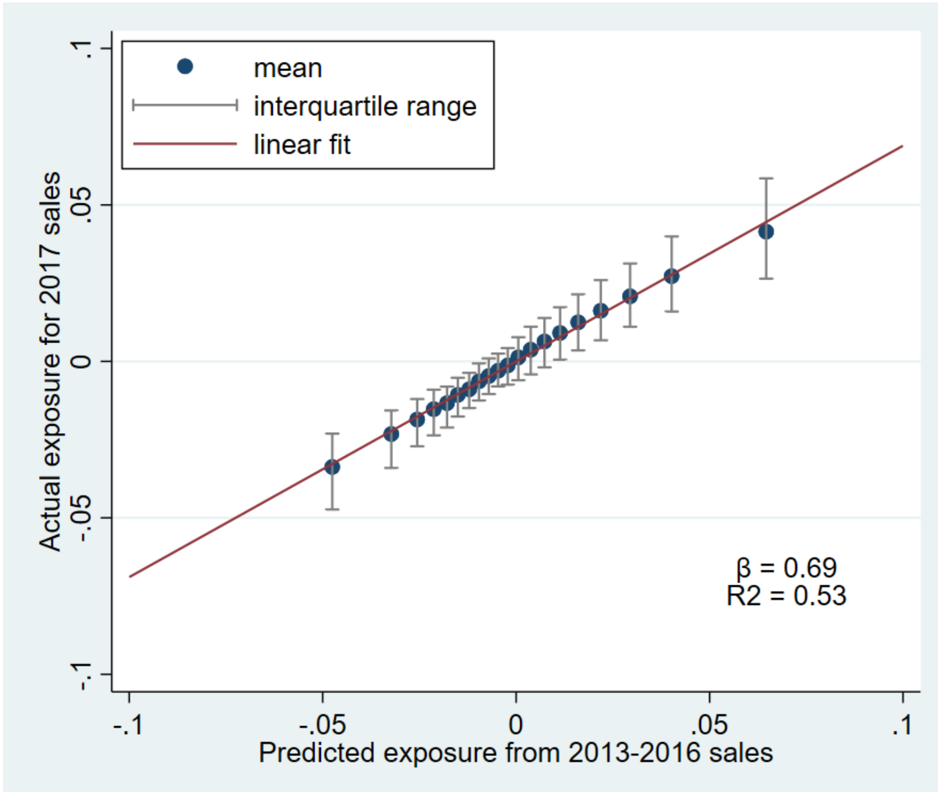

FIGURE 2. Validating predicted exposure against future exposure

Variables are residualized by CBSA to measure correlation within CBSA. Observations are weighted by the stock of homes in each submarket (Census tract by home size).

would be affected by the tax change.

Appendix figure A3 shows the distribution of predicted exposure to the tax change. The mass at zero indicates that 12 percent of houses are in submarkets completely unaffected by the policy change - no buyers in that submarket would have itemized deductions even before the policy change.[34] In the most exposed submarkets, the net-of-subsidy rate increased by more than 10 percent, which means that holding prices constant, the after-tax cost of purchasing a home has increased more than 10 percent. Table 2 shows how the exposure measure correlates with characteristics of the home and its location – as expected, exposed homes are on average more expensive, larger, and located in higher-income areas with higher tax rates.

[34]More than 12 percent of buyers were unaffected. Some submarkets have a mix of buyers that itemized and did not itemize before the policy change, so the average buyer exposure for that home type will be small but positive.

TABLE 2. Home characteristics by exposure to tax change

2017 means	Bottom third	Exposure group Middle third	Top third
Exposure to tax change	0.7%	3.6%	8.2%
Sales price	$159k	$265k	$473k
Square footage	1.5k	1.8k	2.2k
Buyer income	$63k	$88k	$137k
Mean AGI in county	$62k	$71k	$83k
Effective property tax rate in Census tract	1.22%	1.36%	1.63%
Top income tax rate in state	3.6%	5.2%	7.6%
Number of sales (thousands)	734	829	736

Summary statistics are calculated for homes that sold in 2017. Each exposure group has an equal number of homes (rather than an equal number of sales).

4. Results

4.1. Tax change had minimal impact on home prices

This section presents event study regressions that measure the capitalization of the tax change into home prices by comparing price growth in more and less exposed submarkets within the same city. It discusses how the other income and business tax cuts in the TCJA may have affected home prices, and shows the price results are robust to controlling for proxies of exposure to these other tax changes.

Figure 3 shows national trends in home price growth by exposure to the tax change. Note this includes variation between high and low exposure cities, as well as between high and low exposure submarkets within the same city. Price growth was high and steady in the years leading up to the policy change, which was announced at the end of 2017 and took effect in 2018. Price growth remained high in 2018, then cooled off somewhat in 2019. This happened in both high and low exposure submarkets, so it likely represents a combination of market adjustments beyond just the tax change.

On average, prices of less exposed homes grew faster than prices of more exposed homes throughout the sample period. This holds even after conditioning on CBSA - within the same city, price growth in more expensive submarkets was sluggish relative to more affordable submarkets throughout the sample period. This motivates the identifying assumption that, absent a policy change, price growth rates in more and less exposed submarkets within the same city would follow parallel trends, but price growth

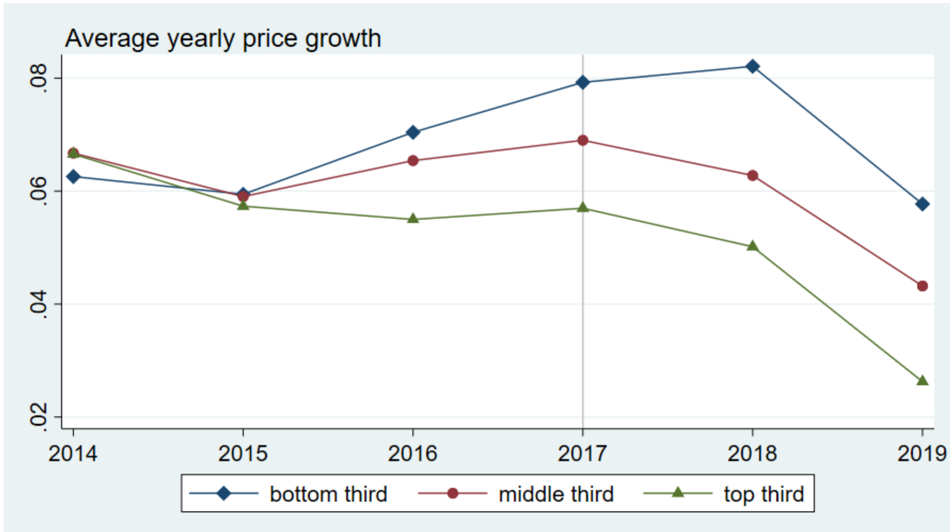

FIGURE 3. National trends in home price growth by exposure to tax change

Price growth and exposure are measured at the submarket (Census tract by home size) level. Observations are weighted by the stock of homes in each submarket.

rates would not necessarily be equal.[35] Appendix F documents within-city variation in price growth in additional detail.

Figure 4 shows coefficients from the event study regression restricting to comparisons between more and less exposed submarkets within the same city:

$$\Delta log(price)_{zst} = \sum_{y \neq 2017} \beta_y * \Delta_{zs} * 1\{y = t\} + \alpha_{zs} + \lambda_{cbsa \times t} + \epsilon_{zst}$$

This controls for market shocks at the city level and controls for the possibility that high exposure cities (which are largely high-income and coastal) experience different price trends, even without a policy change. The event study coefficient β can be interpreted as the capitalization rate of the tax change into prices. If the tax change was fully capitalized into prices immediately in 2018, the event study coefficient in 2018 would be equal to -1. Other estimates of capitalization rates of taxes into home prices are in the -0.6 to -0.8 range (Davis 2019; Lomonosov 2022). The 2018 coefficient in this event study is -0.04, which corresponds to just a 4% capitalization rate. As shown in section 3.2.3, noise in these exposure measure could attenuate the estimated capitalization rate

[35]Assuming price growth rates of high exposure homes would equal price growth rates of low exposure homes absent a policy change is equivalent to assuming parallel trends in log price levels. Attributing all differences in price growth between high and low exposure homes to the TCJA would lead to the incorrect conclusion that the TCJA substantially reduced prices of more exposed homes.

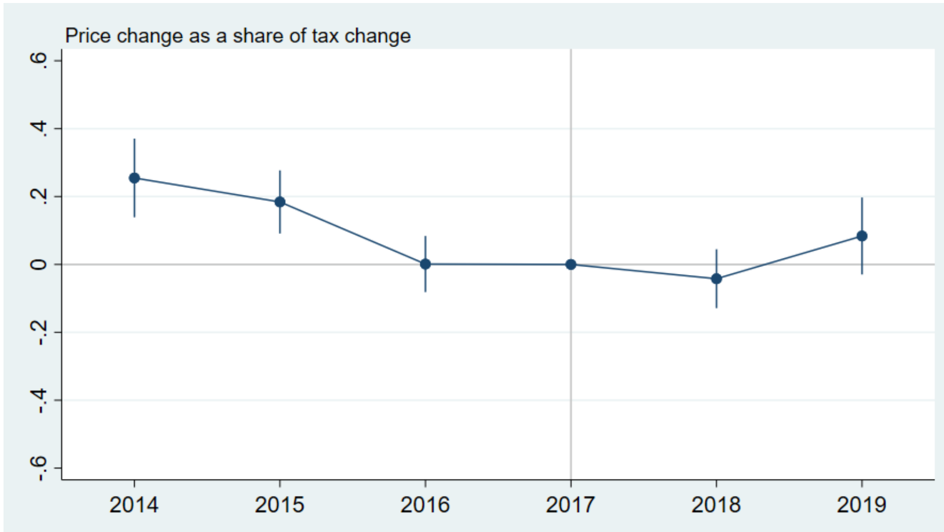

FIGURE 4. Within city event study coefficients

Observations weighted by the stock of homes in each submarket (Census tract by home size). Standard errors clustered by CSA. Price growth winsorized at the 1st and 99th percentiles within each year.

down slightly, but not to this order of magnitude.

The capitalization rate β is identified from variation in exposure within a city. More expensive homes are more exposed because they mechanically come with higher mortgage interest and property taxes. Homes with higher-income buyers are also more exposed, because their buyers have higher state income taxes and higher marginal tax rates. Finally, homes in higher property tax areas within a city are more exposed. All of these factors are positively correlated with each other - more expensive homes also tend to have higher income buyers and be located in higher tax areas.[36]

Note that trends in the price growth of more and less exposed homes diverged slightly in 2014 and 2015. This suggests that some market shocks may affect submarkets with high-income homebuyers differently than submarkets with moderate-income homebuyers. Figure 5 presents a second specification that adds fixed effects for deciles of average buyer income by year. This specification is identified from variation in the relationship between incomes and exposure by CBSA (i.e. due to differences in tax rates or price levels), as well as from variation in exposure within an income group within a CBSA (i.e. because property taxes are higher in one part of the city than another).

Estimates controlling for national trends by income are qualitatively similar to

[36]Restricting to comparisons between more and less expensive homes by removing variation between submarkets with similar price levels but different buyer incomes or property tax rates produces similar results.

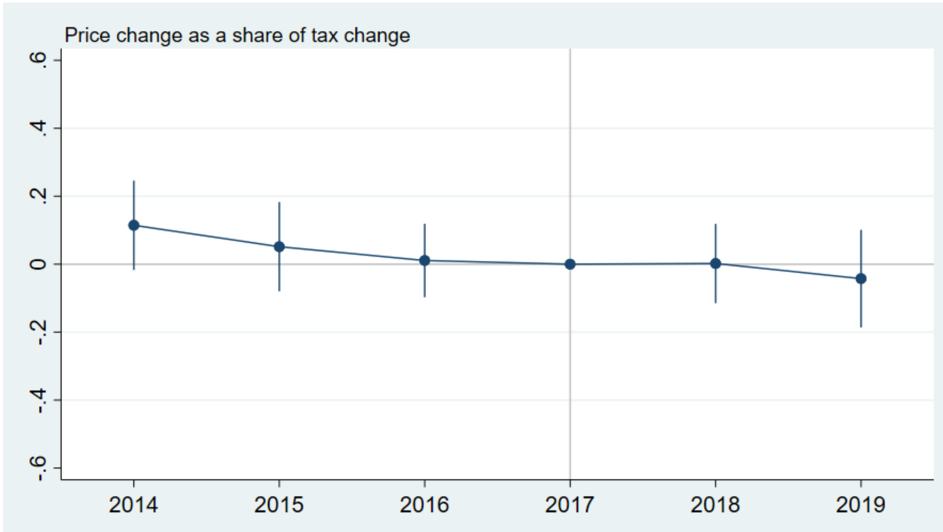

FIGURE 5. Within city event study coefficients, controlling for national trends by income group

Observations weighted by the stock of homes in each submarket (Census tract by home size). Standard errors clustered by CSA. Price growth winsorized at the 1st and 99th percentiles within each year.

the first specification that only controls for trends by CBSA. Both the 2018 and 2019 coefficients are roughly zero. The sum of the two coefficients, which represents the total rate of capitalization of the tax change into home prices over 2018 and 2019, is -0.04 (95% confidence interval -0.23 to 0.15). This rules out that even a quarter of the tax change was priced into home values in the two years after the policy change. Note that the standard deviation of exposure is 3.4 percentage points, which corresponds to just a 0.1 percentage point decrease in home value at a 4% capitalization rate.

One might be concerned that these results are biased by other tax changes in the TCJA that may have also impacted housing markets. The TCJA cut income tax rates, especially for high-income taxpayers. Therefore, demand may have increased in submarkets with high-income buyers, regardless of whether they have high homeownership expenses. Controlling for national trends by income decile controls for changes in housing demand that are correlated with income but not specifically with itemized deductions. The TCJA also cut business tax rates, which benefited many landlords and may have increased housing demand in submarkets with a lot of single family rentals. Rental market activity is measured by the share of the housing stock in each submarket owned by investors in 2016.

Figure 6 shows the event study regression with fixed effects for income decile by year (which control for the income tax cuts) and rental share decile by year (which control for the business tax cuts). Results are qualitatively similar, suggesting these other tax cuts had minimal impact on home prices, or that their impact on home prices was minimally correlated with exposure to the changes in homeownership subsidies.

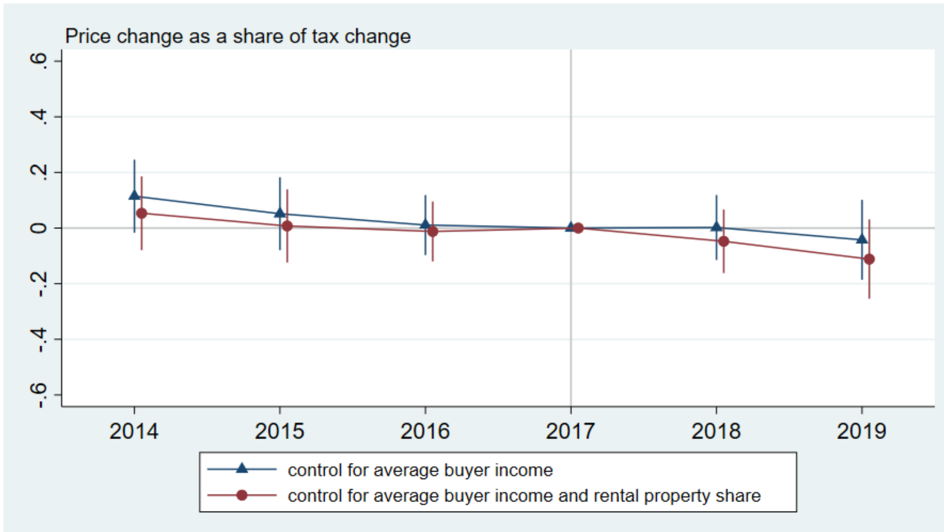

FIGURE 6. Within city event study coefficients, controlling for national trends by income group and rental share

Observations weighted by the stock of homes in each submarket (Census tract by home size). Standard errors clustered by CSA. Price growth winsorized at the 1st and 99th percentiles within each year.

Substitution from more expensive homes to less expensive homes (rather than toward renting) cannot explain why the prices of the most exposed homes did not fall relative to their less exposed counterparts. Rather, we would expect these types of intensive margin responses to amplify the price differential between more and less exposed homes. Because price levels are positively correlated with the exposure measure within a city, substitution from more expensive homes to less expensive homes would decrease demand for more exposed homes and increase demand for their less exposed counterparts.[37] Appendix F directly tests whether the prices in expensive submarkets fell relative to prices in more affordable submarkets within a city. It demonstrates that there was no measurable divergence in price growth in any part of the price distribution after the policy change.

The small price response is likely explained by some combination of elastic housing

[37]This is amplified by the fact that in most cities, buyers in the most expensive submarkets also have the largest incentives to substitute toward a less expensive home. These incentives are governed by changes in the marginal subsidy rate, while the exposure measure used in the event study focuses on changes in the average subsidy rate. Appendix F describes the relationship between marginal subsidy rates, average subsidy rates, and home quality (as proxied by pre-period price levels) in more detail.

supply and a limited demand response to the tax change. If housing supply is relatively elastic, housing supply will adjust to a demand change more than home prices. The following two sections present additional analyses testing whether the quantity of housing supply adjusted to the tax change. Section 6 discusses reasons why homebuyers may have responded to the tax change less than expected, including limited tax salience and expectations regarding future policy.

4.2. Housing supply adjusted through a slowdown in new building

Over the long term, the total stock of housing adjusts to reductions in demand through a slow down in the rate that new housing is built. The analysis in this section shows that the rate of new building slowed down in more exposed counties relative to their less exposed neighbors, and this was concentrated in cities with relatively unconstrained housing supply. This suggests that developers anticipated a drop in demand in more exposed areas and responded by reducing new building.

New building is measured using Census Building Permits Survey data, which reports the number of permits issues to build new homes by county.[38] Exposure to the tax change at the county level (Δ_c) is measured from average exposure in all submarkets in the county, weighted by the housing stock in each submarket. This can be interpreted as the average percent increase in the cost of purchasing a home in the county, holding the price constant.

Figure 7 shows national trends in the total number of new build permits issued, by county exposure to the tax change. Counties are weighted by the total housing stock in each county, so each exposure group has an equal number of homes. New building grew by an average of about 6 percent per year throughout the sample period. The rate of new building was lower and growing slower in the most exposed counties throughout the sample period, while low and moderate exposure counties followed similar trends. An event study comparing more and less exposed counties within the same city is necessary to differentiate the impact of the TCJA from broader differences in growth rates between cities (due to differences in population growth and availability of land, for example). The following event study specification is used to measure the rate of new

[38]This data is collected from surveying permitting offices. It distinguishes between permits for new builds and permits for additions or renovations.

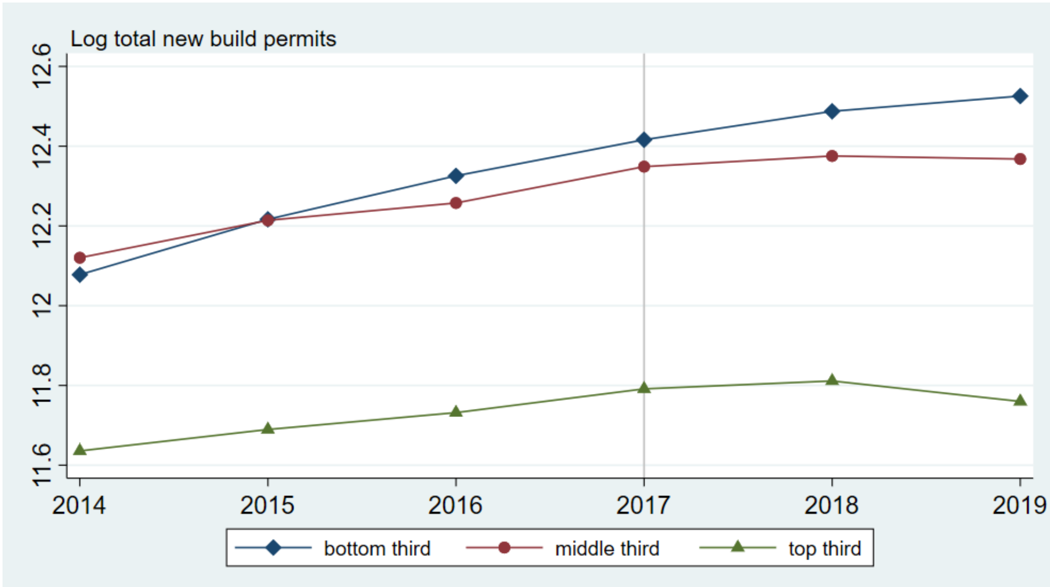

FIGURE 7. Trends in new building, by county exposure to tax change

building in more exposed counties relative to their less exposed neighbors.

$$\ln(\text{newbuilds})_{ct} = \sum_{y \neq 2017} \beta_y * \Delta_c * 1_{y=t} + \alpha_c + \lambda_{cbsa \times t} + \epsilon_{ct}$$

The sample is stratified into cities with relatively unconstrained and constrained housing supply by whether the CBSA's housing supply elasticity is above or below the median, using estimates from Saiz (2010). New building should be able to adjust in cities with relatively elastic housing supply, which generally have more land availability and less restrictive regulation. It may not be able to adjust in cities with less elastic housing supply, where new building is already very constrained. The effect of exposure on new builds is identified from 685 counties 130 CBSAs that contain more than one county and have data coverage for exposure, new builds, and housing supply elasticity.

Figure 8 shows event study coefficients from the regression above, stratified by housing supply elasticity. The event study coefficients in more elastic CBSAs in 2018 and 2019 are roughly -5, which corresponds to a 5% decrease in new build permits per 1 percentage point tax increase (precisely, per 1% increase in the after-tax cost of purchasing the average home, holding the price constant, due to the reduction in tax subsidies). This is economically significant in magnitude, as the standard deviation of county exposure is 2.5 percentage points, which corresponds to a 15 percent decrease

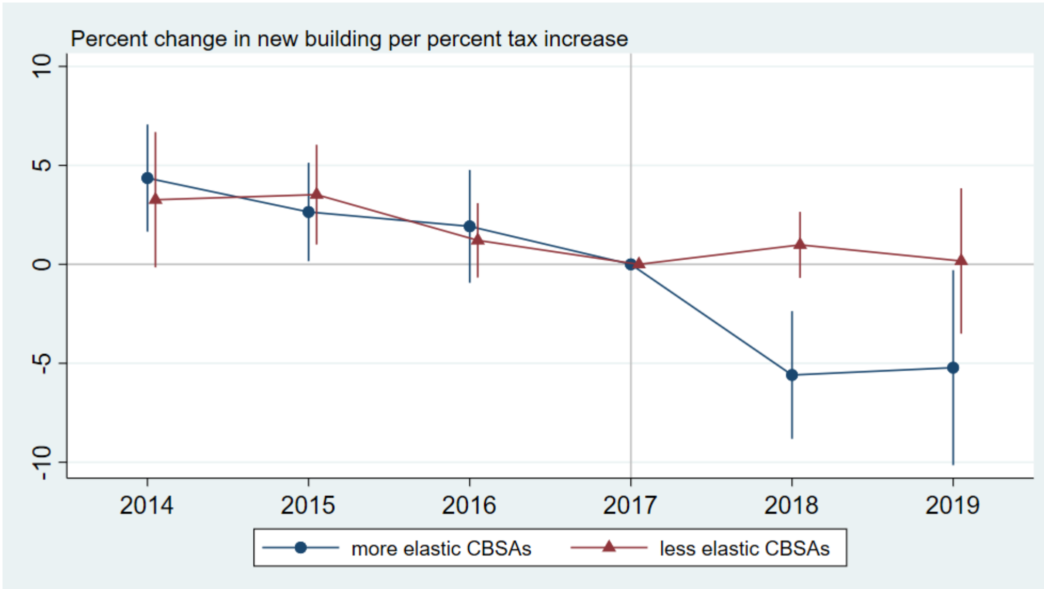

FIGURE 8. New building event study coefficients

Observations weighted by the stock of homes in each county. Standard errors clustered by CSA.

in new building.[39] This same reduction did not occur in the CBSAs with below median housing supply elasticity, where new building is more constrained and less able to respond to demand. This suggests that housing supply can adjust to changes in housing demand, and did respond to the tax increases in the TCJA, at least in cities with relatively elastic housing supply.

4.3. No evidence housing supply adjusted through increased investor purchases

Housing supply could also adjust to a reduction in demand via a conversion of some of the existing house stock to rentals. This section tests whether investor purchases of single family homes increased in the most exposed submarkets, and does not find evidence that this was an important margin of adjustment to the tax change.

Investor purchases are measured from whether the buyer appears to occupy the

[39] Note these event study estimates do not directly correspond to a housing supply elasticity. First, they measure a change in the rate new housing is built, not a change in the total stock of housing available. Second, the mechanism for the quantity supplied to adjust to a demand shift is generally a reduction in prices, which generates a movement along the supply curve. However, stratifying price event studies by housing supply elasticity does not reveal differential price responses between cities with relatively elastic and inelastic housing supply. In this case, builders may observe a change in prices or other market conditions that is not detected in the event study, or may anticipate a future reduction in prices by the time the home is sold.

house after the sale, following the literature on speculative investors (Chinco and Mayer 2016; DeFusco, Nathanson, and Zwick 2022). If the property tax bill in the years after the sale is sent to the same address as the home, it is assumed the home is owner-occupied. If the property tax bill is sent to a different address, it is assumed the home is owned by an investor. This improves on the measure used in prior literature, which measures the owner address from the deed, by linking sales records to property tax records after the sale. This avoids misclassifying owners who list a prior address on the deed but send subsequent tax bills to the property as non-occupants. This is only possible for sales that occurred in recent years where good property tax data coverage is available, so the sample for this analysis is restricted to 2015-2019.

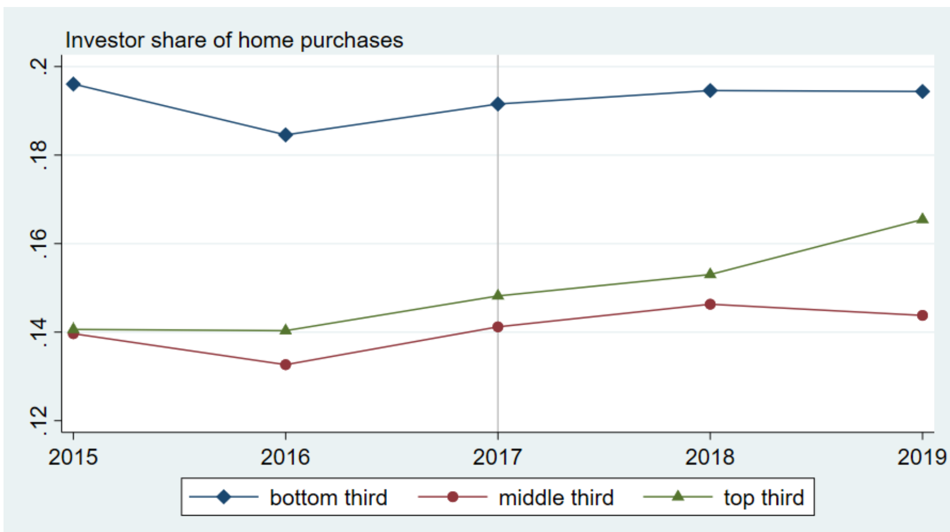

FIGURE 9. Trends in investor purchases by exposure to tax change

Investor purchase share and exposure are measured at the submarket (Census tract by home size) level. Observations are weighted by the stock of homes in each submarket.

Figure 9 shows trends in the share of homes purchased by investors by exposure to the tax change, at the submarket level. This includes variation in exposure both within and between cities. Investors represent an important segment of buyers across all exposure groups in all years. The overall share of homes purchased by owner-occupiers fell slightly over the sample period, reflecting an increase in investment activity. Low exposure homes are most likely to be purchased by investors, likely because there are more active rental markets for these types of homes.

The effect of the TCJA on investor is purchases in measured in an event study regression comparing investor purchasing activity in high and low exposure submarkets before and after the policy change:

$$\frac{\text{investor purchases}}{\text{total purchases}}_{zst} = \sum_{y \neq 2017} \beta_y \Delta_{zs}^{std} 1\{y = t\} + \alpha_{zs} + \lambda_{cbsa \times t} + \theta_{rental\ \ share\ decil\,e \times t} + \epsilon_{zst}$$

The exposure measure is standardized for interpretability of the event study coefficients. Fixed effects for deciles of rental market share by year control for the possibility that submarkets with more rentals at baseline experienced more growth in investor activity, separate from the policy change. Rental market share is measured by the share of the housing stock in each submarket owned by investors in 2016 property tax records. In practice, results are robust to including fewer controls (just CBSA by year) or more controls (CBSA by year, rental share decile by year, and buyer income decile by year).

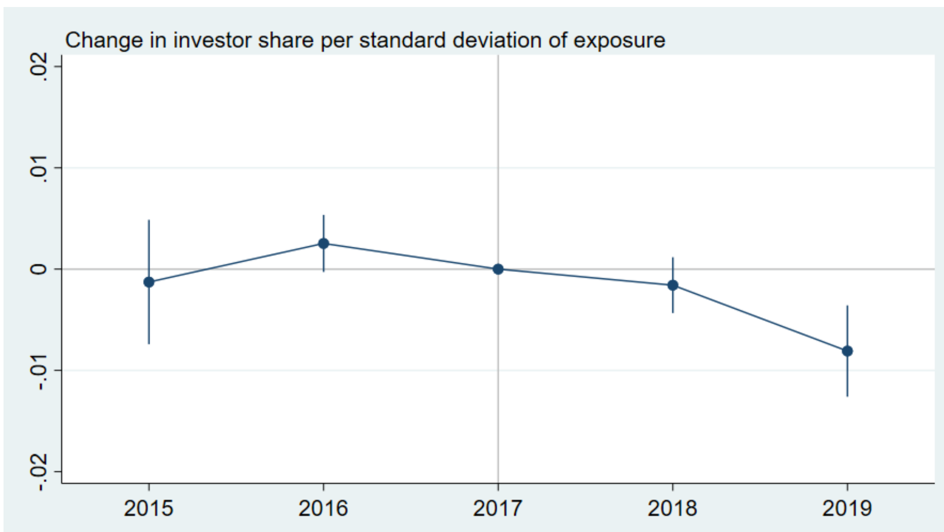

FIGURE 10. Investor purchase activity event study coefficients

Observations are weighted by the stock of homes in each submarket (Census tract by home size). Standard errors clustered by CSA.

Figure 10 shows results from the event study regression above. Investor purchases of more exposed homes did not pick up after the policy change, suggesting a replacement of owner-occupier demand with investor demand did not occur.

5. Discussion

5.1. Limited demand response may have blunted impact of tax change

The full capitalization benchmark assumes that households fully account for the present value of lost tax benefits when making homebuying decisions, and that willingness to pay for any home decreases by the value of the lost tax benefits. If the value of the income tax deductions for homeownership is not salient to homebuyers, they may not fully account for these tax benefits in their demand decisions. Figure 11 shows Google search trends for the mortgage interest and property tax deductions in the years around the TCJA. Searches spiked when the TCJA was enacted and again in early 2019, when homeowners would have filed taxes for the first time under the new law. Even if the new law did not fully become salient until early 2019, demand should have responded by the end of the sample period. It is also possible homeowners were generally aware of the changes in the tax law, but did not precisely understand how they changed the tax benefits to purchasing a home.

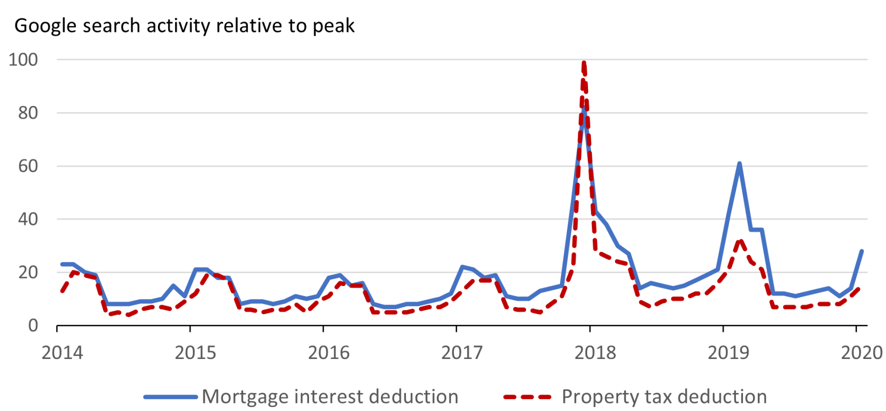

FIGURE 11. Google search trends related to homeownership deductions

Relatedly, if homebuyers expected the tax change would not be permanent, they would expect to lose fewer tax benefits over the life of the home, so willingness to pay for housing would decrease less. If homebuyers expect the tax changes will expire as scheduled in 2025 instead of being extended, the present value of the lost tax benefits would be reduced by almost 60% compared to the baseline assumption that homebuyers expect to lose tax benefits for the full life of a 30-year mortgage. Full capitalization of the tax change through 2025 would correspond to an event study coefficient of roughly -0.4. Note this is an unlikely outcome, as the SALT cap is more politically controversial than the increased standard deduction, but is a useful upper bound for the potential effect of expectations on demand.

The distinction between salience and expectations is primarily relevant for the external validity of these findings. If the tax incentives created by itemized deductions are not well understood, homebuyers may not be very responsive to policy changes in general. If homebuyers did not think the tax changes in the TCJA in particular were credibly permanent, they may have responded less to the TCJA than they would to a different policy change.

5.2. Distributional implications of price results

The change in the after-tax cost of purchasing a home is determined by both the change in the home prices and the change in the tax subsidies. The net-of-subsidy cost of purchasing a home $p_{net} = (1 - \tau)\, p$. The change in p_{net} from a reduction in tax subsidies is a combination of the decrease in home prices and the increase in taxes (relative to renting):

$$d \ln p_{net} = d \ln p + d \ln(1 - \tau)$$

In the full capitalization scenario, $d \ln p = d \ln(1 - \tau)$ and $d \ln p_{net} = 0$. In this scenario, home values fall by the same amount that taxes increase, causing a significant reduction in wealth for current homeowners. Future homebuyers enjoy lower prices but pay higher taxes, so the net impact on the cost of purchasing a home is zero. On the opposite end of the spectrum, in a scenario with zero capitalization, $d \ln p = 0$ and $d \ln p_{net} = d \ln(1 - \tau)$. Home values do not fall as a result of the tax increase, so the wealth of current homeowners is unaffected. Future homebuyers face the same home prices but pay higher taxes, so the cost of purchasing a home increases by the same amount that taxes increase.

Event study results suggest the reality of the TCJA was close to the zero capitalization scenario. Current homeowners did not lose significant wealth from decreased home values, and future homebuyers face similar prices to before the policy change. Both groups but will receive fewer tax benefits to homeownership going forward.

Table 3 approximates the median buyer's percent increase in the net cost of purchasing a home, by income group, within major CBSAs in the sample. This assumes no change in home prices such that $d \ln p_{net} = d \ln(1 - \tau)$ and that 2017 buyers are representative of future buyers. Note p_{net} differs conceptually from the user cost of homeownership, which reflects all the economic costs of owning a home rather than just the price of purchasing the home.[40]

TABLE 3. Distribution of percent increase in after-tax cost of home purchase

		Income group	
	\$50-\$100k	\$100-\$150k	\$150-\$200k
Atlanta-Sandy Springs-Alpharetta, GA	6.1	7.5	8.9
Dallas-Fort Worth-Arlington, TX	5.2	6.1	8.0
Denver-Aurora-Lakewood, CO	4.5	4.6	6.2
Los Angeles-Long Beach-Anaheim, CA	5.0	7.4	9.9
Minneapolis-St. Paul-Bloomington, MN-WI	4.1	7.3	11.4
New York-Newark-Jersey City, NY-NJ-PA	6.8	10.1	13.3
Philadelphia-Camden-Wilmington, PA-NJ-DE	6.1	6.9	9.1
Phoenix-Mesa-Chandler, AZ	0.0	1.5	4.6
Riverside-San Bernardino-Ontario, CA	4.0	7.2	9.8
Seattle-Tacoma-Bellevue, WA	3.4	4.4	5.1
Washington-Arlington-Alexandria, DC-VA-MD	6.4	7.4	9.5

Percent increase in after-tax cost of home purchase is calculated from the median exposure of 2017 buyers in each CBSA by income group. Chicago is excluded because a major Illinois state tax change took effect concurrently with the TCJA. New York is excluded from the event study sample because a large portion of Census tracts do not have single family homes, but included here.

The effect of the tax change on the cost of purchasing a home varies significantly by both income and geography. Exposure in a city is determined by the interaction of home prices, property taxes, and state income taxes. Phoenix was least affected of the cities shown here, as it has relatively low prices, low property taxes, and low state income taxes. The New York metro area was most affected due to its combination of high prices, high property taxes, and high state income taxes. Relatively high-income buyers in Phoenix (those with incomes between \$150,000 and \$200,000) experienced less than a 5 percent increase in the net cost of purchasing a home. By contrast, in many other areas, even moderate-income buyers were notably affected by the tax change. Moderate income buyers in New York (those with incomes between \$50-\$100k) experienced more than a 6 percent increase in the net cost of purchasing a home - more than the high-income group in Phoenix. High-income buyers in the most affected cities saw more than a 10 percent increase in the net cost of purchasing a home.

[40]This paper's results suggest that the effect of the TCJA on the user cost of homeownership could be estimated fairly accurately based on past home prices, without needing to account for policy-induced changes in home prices.

6. Conclusion

This paper studies how the mortgage interest and property tax deductions are capitalized into home prices using the Tax Cuts and Jobs Act, which significantly reduced the value of these deductions. It uses national data on home sales, property tax records, and mortgages to construct estimates of exposure to the tax change and price growth by detailed submarket within a city. The exposure measure captures the effect of all major income tax changes on the tax benefit to homeownership relative to renting, and directly corresponds to the predicted change in home price if the tax increase was fully capitalized into home prices.

Event study estimates comparing price growth in more exposed submarkets to less exposed submarkets within a city find little to no capitalization of the tax change into home prices, and can rule out that even a quarter of the tax increase was capitalized into home prices in the two years after the policy change. These findings are robust to controlling for the other individual and business tax changes in the TCJA.

Supplemental analyses show that the lack of price response could partly be explained by elastic housing supply - the tax increase was absorbed in part by a reduction in the quantity of housing available to owner-occupiers, rather than a reduction in home prices. In cities with relatively elastic housing supply, new building slowed down in more exposed counties after the TCJA. I also test for evidence that housing supply adjusted through the conversion of owner-occupied properties to rentals, but do not find that this was an important margin of adjustment.

Tax salience and expectations likely also played a role, as these housing supply adjustments cannot fully explain the lack of price response to such a large tax change. If buyers do not fully understand the tax incentives generated by the mortgage interest and property tax deductions, or if they did not believe this particular policy change would remain permanent, demand would not fully respond to a change in tax incentives.

Measuring the impact of homeownership tax subsidies on home prices is important because buyers are affected by both changes in prices and changes in tax costs. It also determines the effect of the policy change on the wealth of existing homeownership. Because the policy change had little impact on home prices, existing homeowners did not lose significant wealth. The cost of homeownership has increased for future buyers, who face similar before-tax prices but higher tax costs.

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Appendix A. Trends in observable home characteristics

The price growth measure used in this paper requires the assumption that after conditioning on location and home size, the quality of homes sold does not change from one year to the next. Figure A1 shows trends in the observable characteristics of homes sold, by exposure to the tax change, after conditioning on location and home size. Average lot size and number of bedrooms are is relatively constant over time, but the average age of homes sold increases somewhat over the sample period. If homes are maintained such that quality does not decline with age, this may not affect measures of home price growth. However, if home quality declines with age, this could bias measures of price growth downward. (Measures of price growth based on repeat sales indices would also generally be biased downward, since age is a time-varying dimension of home quality.)

Compositional change in the quality of homes sold over time would only bias event study estimates if it differs between high- and low- exposure homes. All exposure groups follow similar trends in observable characteristics over the sample period. This suggests that conditioning home prices on size and location are sufficient for this analysis, and that regressions are unlikely to be biased by differential change in home quality between treatment groups.

Appendix B. Validation of exposure measure

Figure A2 shows that the exposure measure correlates with tax data at the county level, to validate the exposure measure against a separate data source. Exposure in the tax data is proxied the share of filers that itemized deductions before the policy change, as reported in the Statistics of Income. This roughly corresponds to the share of all households in the county that would be exposed to the reduction in homeownership tax incentives. This should be correlated with the exposure measure constructed in this analysis but not perfectly, because the itemization rate measures binary exposure among all households, while the average tax change as a share of home price measures continuous exposure only among homebuyers.

Figure A3 shows the distribution of predicted exposure to the tax change at the submarket level. The mass at zero represents the submarkets in which no buyer would be expected to itemize deductions even before the TCJA, so no buyer would be expected to be affected by the changes to itemized deductions in the TCJA.

Appendix C. Robustness to alternate price growth measure

Figure A4 shows robustness to measuring price growth using a repeat sales index, rather than the growth measure constructed in this paper (percent change in average sale price of observationally similar homes). It uses the FHFA repeat sales index at the Census tract level, since this is the finest level of geography available. It shows event study coefficients from the following regression, with exposure and price growth aggregated to the Census tract level. Results are qualitatively similar using either measure of price growth.

$$\Delta log(price)_{zt} = \sum_{y \neq 2017} \beta_y * \Delta_z * 1\{y = t\} + \alpha_z + \lambda_{cbsa \times t} + \epsilon_{zt}$$

Appendix D. Robustness to alternate method of controlling for CBSA

Event study estimates using a fixed effects framework with control variables can be biased when treatment effects are heterogeneous and correlated with the control variables. For example, if CBSAs with higher overall exposure to the tax change have larger capitalization rates of the tax change into prices on average, part of the price response would be absorbed in the CBSA fixed effects rather than attributed to the effect of exposure to the tax change, biasing the estimated effect of the tax change on prices toward zero.

Abadie (2005) shows that reweighting the sample such that the treated and untreated groups are balanced on the relevant observable characteristics achieves the same goal as controlling for those observables using fixed effects (eliminating differential trends between observable groups) without introducing bias from heterogeneous treatment effects. In the spirit of Murto (2022), I adapt this approach for the case of a continuous treatment variable. The sample is split into treatment groups based on quartiles of the continuous treatment variable, and reweighted so that each treatment group is balanced on CBSA. This is implemented using the following weights, where exp represents quartiles of the continuous treatment exposure variable:

$$weight_{zs} = \frac{P[exp = exp_{zs}]}{P[exp = exp_{zs} | CBSA = CBSA_{zs}]}$$

The relevant probabilities can be estimated nonparametrically. $P[exp = exp_{zs}]$

corresponds to the overall share of the sample in each treatment quantile, and $P[exp = exp_{zs}|CBSA = CBSA_{zs}]$ corresponds to the share of the sample in each treatment quantile within each CBSA. The overlapping support assumption requires that $P[exp = i|CBSA = j] > 0$ for all possible exposure levels i in each possible CBSA j. If a CBSA does not have at least one observation in each treatment quantile, it is excluded from the analysis. This stricter requirement of overlapping support is also applied to the main specification. Note that these weights are applied on top of the base weights used in all analyses, which weight each cell by the stock of homes in 2016 property tax data. This shuts down any compositional in the sample over time.

Figure A6 shows event study coefficients from a regression without CBSA by year fixed effects, but reweighted using the inverse probability weights shown above such that the distribution of exposure within each CBSA matches the overall distribution of exposure. Results are qualitatively similar to the main specification, which controls for differential trends by CBSA using CBSA by year fixed effects.

Note this analysis does not control for differential trends by predicted buyer income decile. Rebalancing the sample on both CBSA and income group is possible, but would require using a parametric estimator for $P[exp = exp_{zs}|CBSA = CBSA_{zs}, incbin = incbin_{zs}]$.

Appendix E. No evidence that tax change was absorbed by rent prices

This section tests for evidence that rent prices fell in more exposed areas relative to less exposed areas. If the tax change was absorbed by a combination of increased rents and decreased home prices, the event studies measuring the change in home prices would understate the effect of the tax change. This is likely not the case, as rent growth remained stable between more and less exposed areas after the tax change.

Rent growth is measured from the percent increase in the Zillow rent price index by county, averaged at the annual level, winsorized at the 1st and 99th percentiles. Rent growth in more exposed counties is compared to rent growth in less exposed neighboring counties within CBSA using in the following event study regression.

$$\Delta log(rentindex)_{ct} = \sum_{y \neq 2017} \beta_y * \Delta_c * 1\{y = t\} + \alpha_c + \lambda_{cbsa \times t} + \epsilon_{ct}$$

This is a coarser test than the test for home price changes, because rent data is not collected as widely or in as much detail as home price data. However, there is no evidence that rents increased in more exposed areas relative to less exposed areas.

Appendix F. No evidence that intensive margin responses drove price changes

Interpreting the price event study coefficients as a precise capitalization rate requires the assumption that the marginal homebuyer responds on the extensive margin (renting rather than buying). However, intensive margin demand responses cannot explain the small price effects found in this paper. Even if the marginal homebuyer responds on the intensive margin (purchasing a lower quality, less expensive home) rather than the extensive margin, the prices of more exposed homes would fall relative to the prices of less exposed homes, because exposure and home quality are highly correlated. Further, this section shows that prices of higher quality homes did not fall relative to prices of lower quality homes within a city.

This paper parameterizes exposure to the TCJA using changes in the average tax subsidy rate, which measures the change in the cost of buying relative to renting. Incentives to respond on the intensive margin are governed by changes in the marginal subsidy rate, which measures the share of the cost of buying a slightly more expensive home that is offset by tax benefits.[41] Figure A7 shows how changes in the marginal and average subsidy rate correlate with price tiers within a city.[42] In the average city, buyers of the most expensive homes experience the largest changes in both marginal and average subsidy rates, meaning they face the largest incentives to substitute to renting and the largest incentives to substitute to a lower-quality, less expensive home.[43] Therefore, three factors could have caused demand to shift away from higher quality, more expensive homes:

- Overall, buyers have an incentive to substitute to more affordable homes

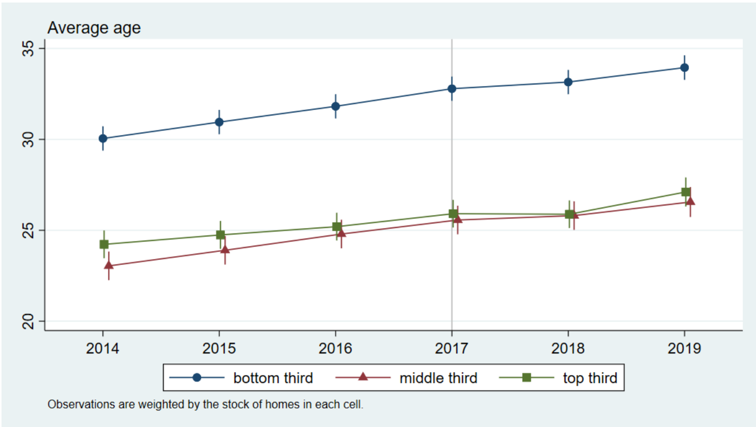
[41] See Landvoight, Piazzesi, and Schneider (2015) for a precise model demonstrating how intensive margin demand changes affect the relationship between prices and home quality within a city, assuming markets are stratified (the marginal buyer differs at different points in the quality distribution) and quantities are fixed.

[42] The marginal subsidy to purchasing a more expensive home is calculated from the present discounted value of the increase in annual tax benefits to homeownership from purchasing a home that is 10% more expensive. (It is assumed mortgage interest and property taxes expenses scale proportionally to home price, so mortgage interest and property tax deductions also increase by 10%).

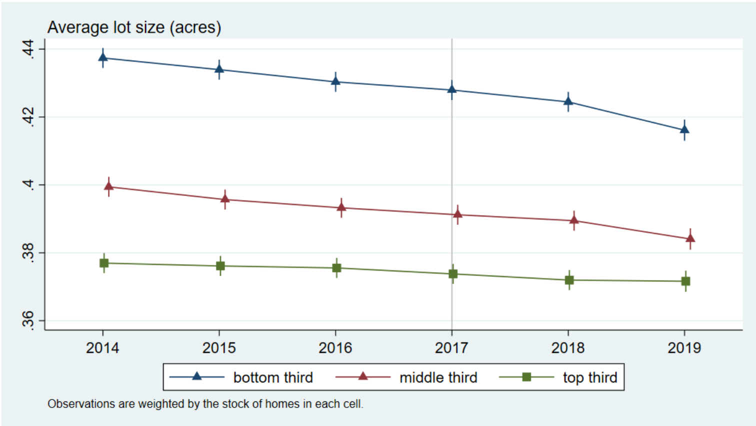
[43] This is true on average, but not in every city. In some of the highest-income, highest-cost cities, buyers in the upper middle part of the distribution experience larger changes in tax incentives than buyers at the very top of the distribution, because buyers at the top of the distribution are less affected by the increase in the standard deduction.

- The incentive to substitute to more affordable homes is largest for buyers in the most expensive submarkets
- The incentive to substitute to renting is largest for buyers in the most expensive submarkets

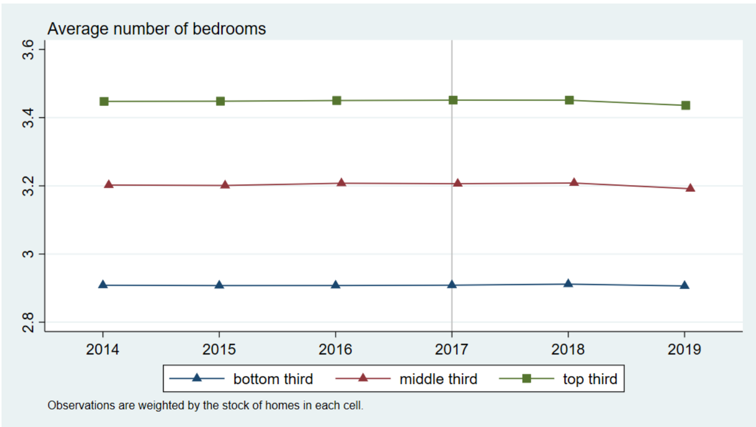
Figure A8 shows average price growth by price decile within city in the years around the tax change. Price growth was higher in more affordable submarkets in every year, but this pattern remained very stable from 2016-2018. Price growth slowed down across the board in 2019, but this was not more pronounced in the most expensive submarkets. This is not consistent with the prediction that demand shifted away from the most expensive homes toward either renting or more affordable homes.



A. Trends in home age



B. Trends in lot size



C. Trends in number of bedrooms

FIGURE A1. Observable characteristics of homes sold, by exposure to tax change

Observations are weighted by the stock of homes in each submarket (Census tract by home size), which eliminates compositional change from year to year by location and home size. Age is measured by the number of years since the last renovation. Age and lot size are winsorized at the 5th and 95th percentiles. Number of bedrooms is capped at 6.

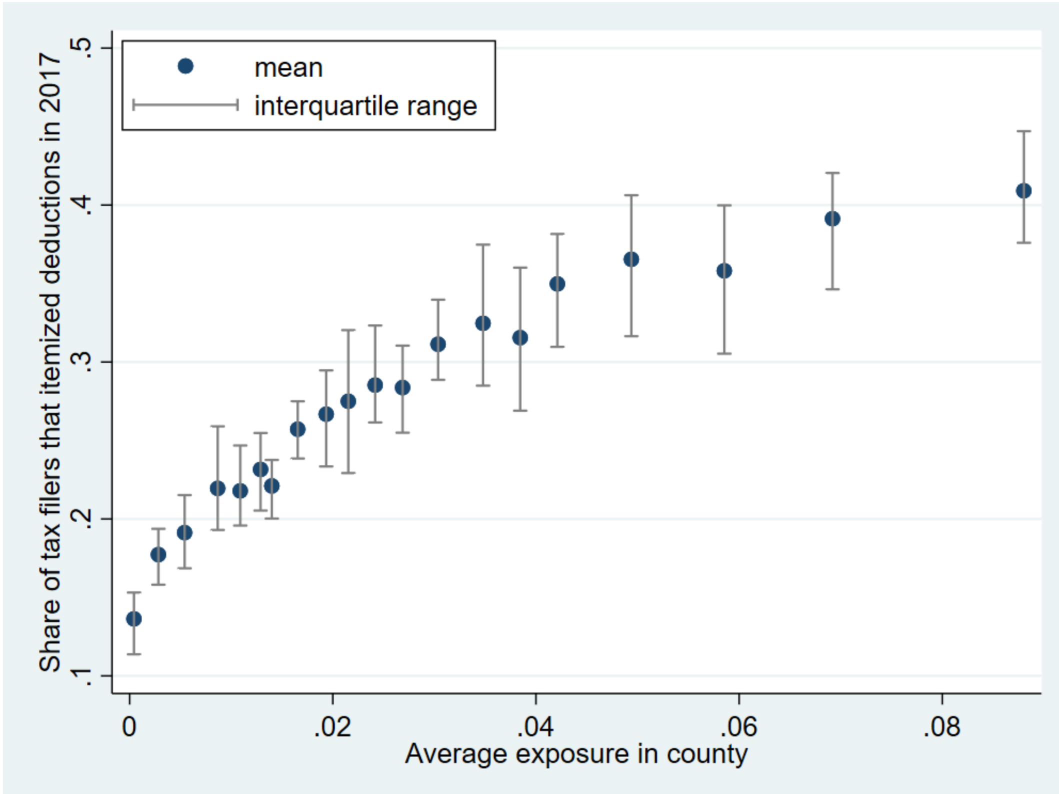


FIGURE A2. Correlation between exposure measure and itemization rates, by county

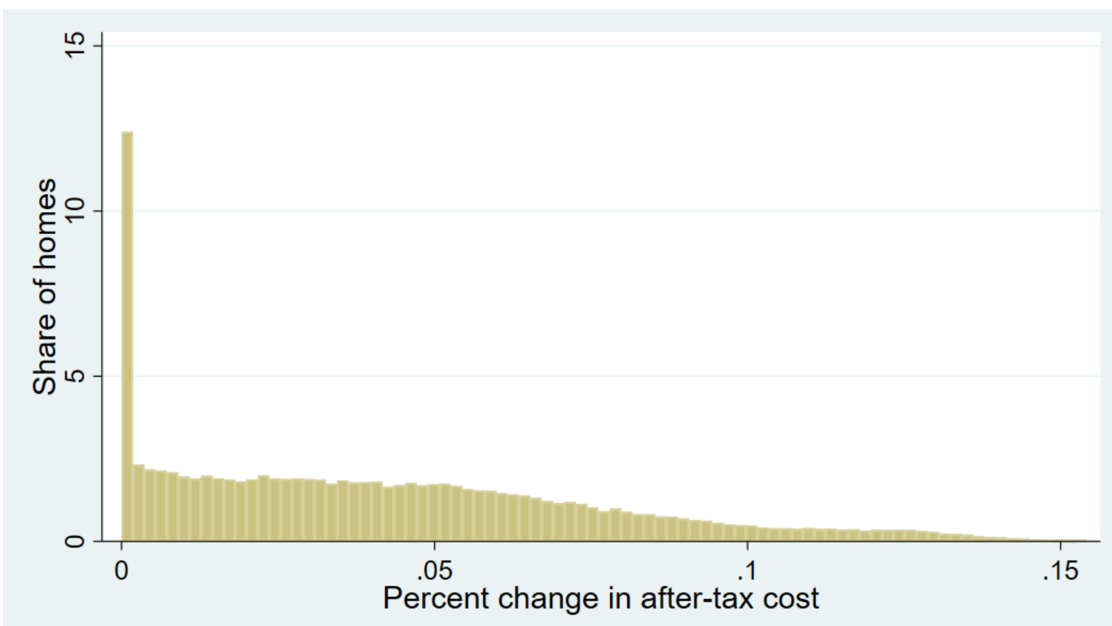


FIGURE A3. Distribution of exposure measure

Observations are weighted by the stock of homes in each submarket (Census tract by home size).

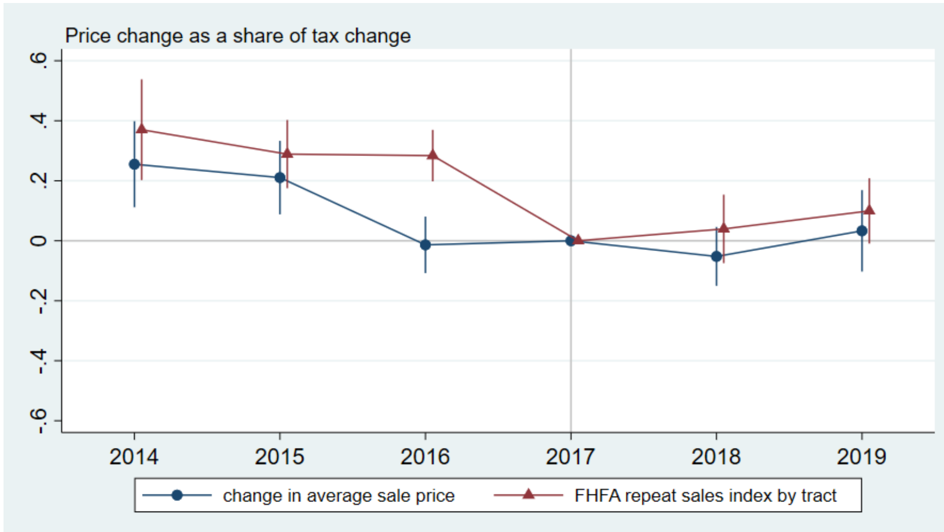


FIGURE A4. Robustness to alternate measure of price growth

Observations weighted by the stock of homes in each tract. Standard errors are clustered by CSA. Price growth is winsorized at the 1st and 99th percentiles in each year

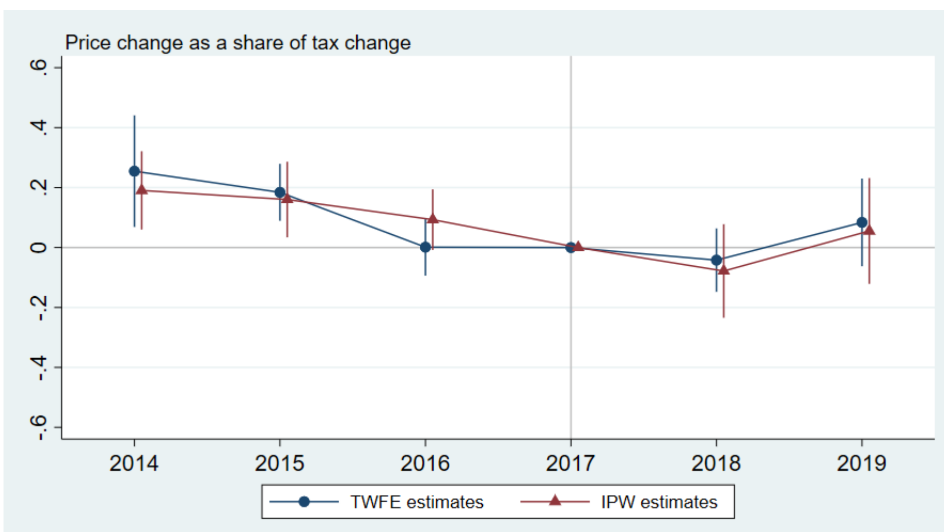


FIGURE A5. Robustness to alternate method for controlling for trends by CBSA

Observations are weighted by the stock of homes in each submarket (Census tract by home size). Standard errors are clustered by CSA. Price growth is winsorized at the 1st and 99th percentiles in each year.

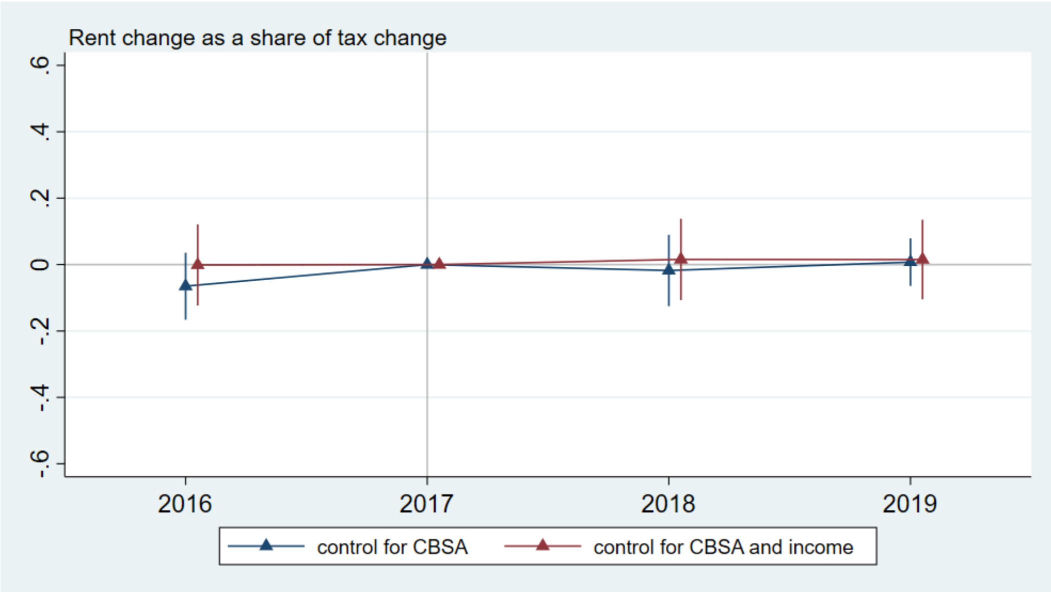


FIGURE A6. Effect of tax change on rent prices

Weighted by the stock of homes in each county. Standard errors clustered by CSA.

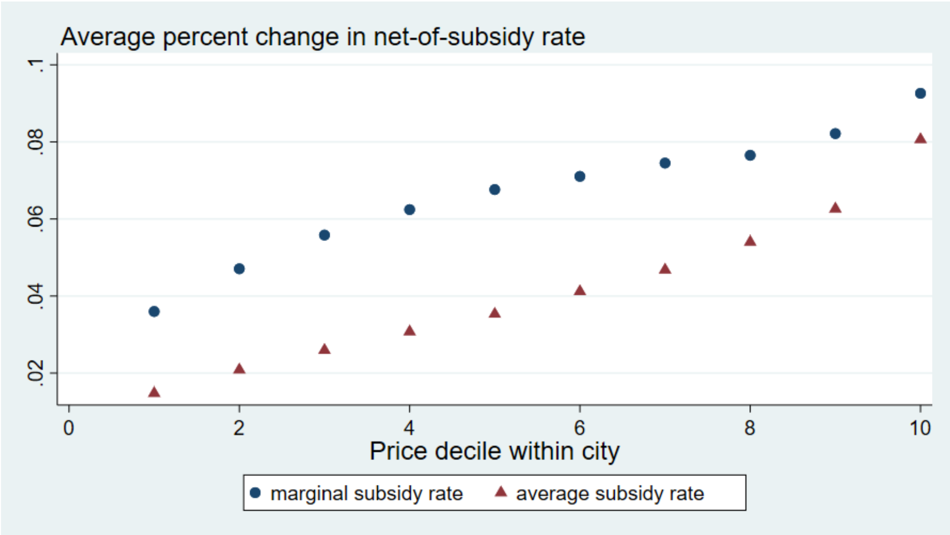


FIGURE A7. Changes in marginal vs average net-of-subsidy rates

Weighted by the stock of homes in each submarket (Census tract by home size).

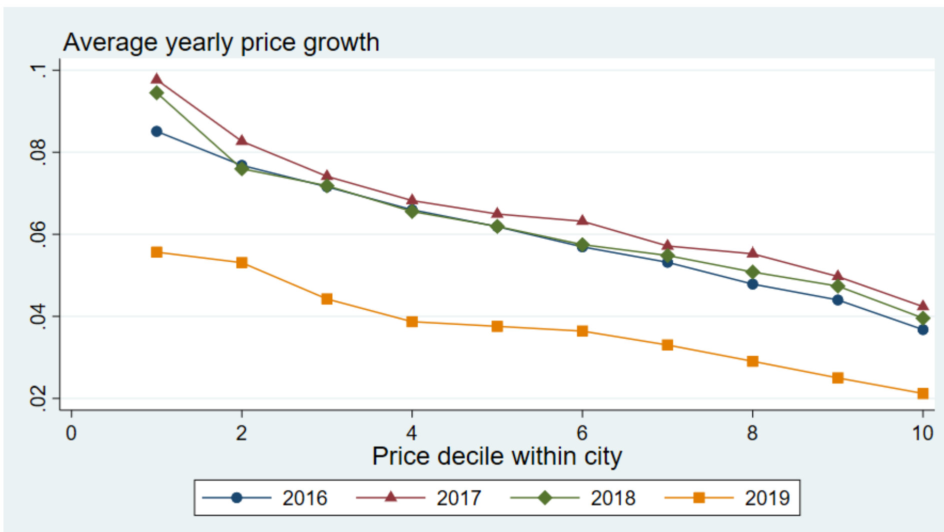


FIGURE A8. Price growth by price tier

Weighted by the stock of homes in each submarket (Census tract by home size).