

THE MORTGAGE INTEREST DEDUCTION AND ITS IMPACT ON HOMEOWNERSHIP DECISIONS

Christian A. L. Hilber and Tracy M. Turner*

Abstract—This paper examines the impact of the combined U.S. state and federal mortgage interest deduction (MID) on homeownership attainment, using data from 1984 to 2007 and exploiting variation in the subsidy arising from changes in the MID within and across states over time. We test whether capitalization of the MID into house prices offsets the positive effect on homeownership. We find that the MID boosts homeownership attainment only of higher-income households in less tightly regulated housing markets. In more restrictive places, an adverse effect exists. The MID is an ineffective policy to promote homeownership and improve social welfare.

I. Introduction

ONE of the largest tax expenditures in the United States, under federal law and some state law, is the mortgage interest deduction (MID). It is justified as a means to broaden access to homeownership. There is some evidence, particularly from urban areas, that homeownership has important externalities. Hilber and Mayer (2009) find, however, that the positive externalities of homeownership may be confined to places with an inelastic supply of housing. A large literature suggests that in densely populated areas, homeownership is associated with lower crime rates, higher voting rates, more participation in collective action, and other benefits.¹ Much earlier work has investigated the

impact of the MID on national homeownership rates, but to the best of our knowledge, no study to date has sorted out the extent to which the MID impacts may vary depending on local housing supply conditions. Since the externalities appear to depend on location, it is appropriate to consider how the benefits depend on location. This is the main objective of this paper.

Using a measure of restrictions on new housing developed for 83 metropolitan areas in the United States (Saks, 2008), we investigate how local housing market conditions and income status affect the way the MID influences household homeownership decisions. Our priors are that the impact of the MID may be positive or negative, depending on market conditions. The MID reduces the after-tax cost of homeownership for a given price of home. However, by increasing house prices, the MID increases the costs for downpayment-constrained households, and for all households, this increases the opportunity costs of homeownership and the transaction costs of purchasing a home. Our empirical analysis suggests that the MID has no discernible impact on the level of U.S. homeownership. However, the MID has a perverse effect in highly regulated housing markets. Because the supply of housing in such areas is inelastic, rather than boosting homeownership attainment, much of the MID is capitalized into housing prices. At the higher housing price, certain types of households (e.g., downpayment-constrained households) opt out of the market for owner-occupied housing, yet full capitalization of the subsidy and utilization of the housing stock can occur if the remaining market segment increases housing consumption in response to the subsidy. Only in markets with lax land use regulation does the MID have a positive impact on homeownership attainment, and the positive effect of the MID occurs only for higher-income households. Our cost simulations suggest that the subsidy cost per converted homeowner amounts to a staggering \$28,397 per new homeowner per year.

These findings imply a disconnect between the context in which a market failure exists and the context in which the MID can correct the market failure. The proposition that homeownership creates positive externalities, that the MID creates homeownership, and therefore that the MID is a Pigouvian subsidy may be intuitive at first glance, but it is empirically false, for a subtle reason. Where a positive externality from homeownership is likely to exist, in inelastically supplied housing markets, we find the MID acts as a

Received for publication August 13, 2010. Revision accepted for publication April 10, 2013. Editor: Philippe Aghion.

* Hilber: London School of Economics; Turner: Kansas State University.

We thank Lance Bachmeier, Colin Cameron, Steve Cassou, Paul Coomes, John Crespi, Steve Gibbons, Richard Green, Bob Helsley, Hilary Hoynes, Dwight Jaffe, Doug Krupka, Dong Li, Stan Longhofer, Jim Poterba, Steven Raphael, Hui Shan, Wouter Vermeulen, and Nancy Wallace for valuable comments. We wish to thank Daniel Feenberg for NBER-generated tax data and David Roderer for FHFA data. We thank seminar participants at the Tinbergen Institute, University of Aberdeen, University of California at Berkeley, University of California at Davis, and University of Southern California as well as conference participants at the 2009 North American Meetings of the Regional Science Association International in San Francisco, 2009 Swiss Economists Abroad Meetings in Basel, 2010 American Real Estate and Urban Economics Association meetings in Atlanta, and 2010 FDIC/Federal Reserve Bank Research Symposium “Mortgage Foreclosures and the Future of Housing Finance,” in Arlington for excellent feedback. This paper was also presented at the 2011 National Tax Association Meetings. Finally, we thank the editor, Philippe Aghion, for guidance and three anonymous referees for insightful comments and valuable suggestions. We are grateful to Josh Holmgren and Madina Mukhanova for helpful research assistance. C.H. thanks IPFET for a travel grant to present this research. Senior authorship is shared.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/REST_a_00427.

¹ See, for example, Dietz and Haurin (2003) for an overview. Hoff and Sen (2005) provide a theoretical rationale for why homeowner communities are associated with better civic environments: households with identical preferences and abilities may segregate into communities by tenure and income due to credit market imperfections. Civic effort improves community quality but is noncontractible. Only homeowners gain from the increase in property values; thus civic effort and its positive externalities are confined to owner-occupied neighborhoods in which

civic effort is capitalized into house prices. A corollary, relevant for our analysis, is that in places with elastic supply and no capitalization, homeownership may not be positively associated with civic effort (see Hilber, 2010, for empirical evidence consistent with this corollary).

tax on homeownership. The MID does increase homeownership attainment in elastically supplied housing markets, but in these places, no positive externality from homeownership is likely to exist.

The remainder of this paper is organized as follows. Section II discusses related research. Section III examines the mechanisms by which the MID can affect incentives to own and presents a stylized present value model to illustrate these mechanisms. Section IV describes the measure we use to capture the MID tax subsidy to homeowners. Section V details the data and sample issues, outlines our empirical approach and identification strategy, presents our empirical findings, and discusses the quantitative significance and robustness of the findings. Section VI concludes. In several places, we refer interested readers to an online appendix that reports several additional robustness checks not discussed in detail here.²

II. Related Research

While the exclusion of imputed rental income of owner-occupied housing from taxable compensation is the key tax benefit to homeowners (Poterba & Sinai, 2008), in the presence of asymmetric tax treatment of property owners (i.e., landlords versus owner-occupiers), the MID also represents a sizable tax break to owner-occupied housing. According to the tax law of the United States and most U.S. states, landlords are taxed on their net rental income. The interest on their mortgages is not a personal expense but an expense necessary to earn the rental income. Owner-occupiers do not pay taxes on their imputed rental income, yet they can deduct mortgage interest from their taxable income. A voluminous literature recognizes the importance of taking into account federal tax policy when examining housing market outcomes (Rosen, 1979; Dynarski & Shefrin, 1985; Poterba, 1992; Turner & Smith, 2009). Early efforts to determine the impacts of removing the preferential tax treatment of owner-occupied housing on homeownership attainment include papers by Rosen (1979), Hendershott and Shilling (1982), Rosen, Rosen, and Holtz-Eakin, (1984), and Berkovec and Fullerton (1992), and although the findings are not entirely conclusive, they suggest that the tenure choice impacts of removing the MID in isolation from other tax changes are likely to be small.

Several studies highlight the need to consider housing supply elasticities when examining the housing market impacts of tax reform (Capozza, Green, & Hendershott, 1996; Green & Vandell, 1999). Capozza et al. (1996) maintain that the stock of prime residential land is inelastic, and thus altering the current tax treatment of owner-occupied housing will have price rather than quantity effects. In an examination of rent-price ratios in 63 metropolitan areas, Capozza et al. conclude that eliminating the mortgage

interest and property tax deduction would reduce house prices by 2% to 13% depending on the metropolitan area. Using the Public Use Microdata Sample of the 1990 Census, Green and Vandell (1999) examine the likelihood of homeownership, controlling for state fixed effects in an effort to adjust for differing supply elasticities across states, and find that replacing the MID with a revenue-neutral tax credit would boost the national homeownership rate by about 5 percentage points. More generally, the importance of the supply elasticity in determining equilibrium prices in local housing markets is highlighted in recent papers such as Glaeser, Gottlieb, and Gyourko (2010) and Mayer (2011).

Several papers document that the distribution of (primarily) federal housing tax benefits favors young and higher-income homeowners and homeowners residing in regions with high incomes and high house prices (Glaeser & Shapiro, 2003; Sinai & Gyourko, 2004; Poterba & Sinai, 2008). However, high-income households also tend to be higher-wealth households and therefore they are likely to use equity financing to purchase their homes in the absence of the mortgage interest deduction (Gervais & Pandey, 2008), thus further suggesting that the MID may have little impact on homeownership attainment.

Two papers broach the subject of state mortgage subsidies. Consistent with Capozza et al.'s (1996) finding that the tax subsidies to homeowners primarily generate price effects, Bourassa and Yin (2008) find that the combined state and federal mortgage interest deduction has an adverse effect on homeownership attainment of the young. In contrast, in an examination of state mortgage subsidies, Glaeser and Shapiro (2003) report that state homeownership rates are unrelated to the size of state subsidies. Taken as a whole, the research suggests that the MID may not be a particularly effective policy tool for boosting homeownership attainment. However, to our knowledge, no study to date has sorted out the extent to which the MID impacts may vary depending on local housing supply conditions, a task we turn to next.

III. Capitalization Effects and Homeownership

A. Individual Incentive and Market-Wide Effects

There are two channels by which the MID may affect a home buyer's incentive to own. First, a positive, direct homeownership incentive effect can occur because the MID reduces the after-tax interest costs for buyers who finance and itemize. This effect may increase the home buyer's willingness to pay and, thus, market demand along both the extensive and intensive margins (i.e., the decision to own and the amount of housing consumed). Second, as long as the supply of housing is not perfectly elastic, a home buyer's incentives are also affected by the indirect market-wide effect of rising house prices occurring due to the aggregate behavior of market participants in response to the

² Available at <http://www.mitpressjournals.org>.

subsidy. Through this channel, the subsidy, indirectly via raising house prices, increases costs for down-payment-constrained households, reduces the odds that severely constrained households can obtain a mortgage, and increases the transaction costs of buying a home.

Market adjustment is initiated by the direct incentive effect, by which the subsidy increases individual demand of households for owner-occupied housing but is then driven by the resulting aggregate market demand and supply responses. Due to an increase in the demand, the quantity of owner-occupied housing may adjust along three margins: conversions of nonresidential property, conversion of rental stock to owner-occupied use, and the quantity of land used for housing. The market-wide house price effect can be expected to be stronger in tightly regulated, inelastically supplied housing markets, wherein the MID can be expected to mainly induce a reallocation of housing space across individuals since increasing the supply of owner-occupied housing and conversion from rental to owner-occupied space are difficult. At one extreme, if the long-run supply of owner-occupied housing in an area is perfectly elastic (i.e., land use controls are lax and developable land is available in abundance), the equilibrium purchase price of housing can be expected to return to its presubsidy level. The subsidy in this case results in an expanded housing stock, an increased homeownership rate, and zero house price capitalization at the new equilibrium. At the other extreme, if tight regulatory constraints in a locale make the supply of owner-occupied housing perfectly inelastic, the subsidy will be fully capitalized into the purchase price of owner-occupied housing, the owner-occupied housing stock will not expand, and the subsidy fails to increase the homeownership rate (in fact, it may reduce it, as we point out below). The capitalization of the subsidy into house prices represents a one-time windfall gain for existing homeowners. In elastically supplied markets, no such windfall gain persists.

We speculate that the market-wide impacts of rising house prices may have adverse consequences for at least three types of households. First, households facing down payment constraints have three choices when house prices rise: (a) exit the market, (b) purchase later after they acquire more wealth to meet the higher down payment amount, or (c) own, but, to do so, accept increased leverage as the house price increase is rolled into the loan amount. In the event of rigid loan-to-value requirements, the household may not have the option of accepting higher leverage and may instead be priced out of the market. Second, residents with relatively short expected durations in a given location may opt out of the owner-occupied market when house prices rise due to increasing transactions costs of owner occupation. These transactions costs include realtor fees that are proportional to house values (Hsieh & Moretti, 2003), financing costs, and opportunity costs (Haurin & Gill, 2002). Third, home buyers who are relatively risk averse may opt out of the local market for

owner-occupied housing due to the subsidy-driven increase in house prices. By driving up house prices, an increase in the MID will require an increase in the amount of a household's portfolio allocated to owner-occupied housing, an increase in the LTV, or both. A shifting of more assets into owner-occupied housing to meet a higher down payment decreases portfolio diversification and therefore increases the household's exposure to investment risk. Higher leverage similarly increases this risk. Greater exposure to investment risk, all else equal, is predicted to decrease the likelihood that households own (Turner, 2003; Hilber, 2005).

In a setting with inelastic supply and heterogeneous households, certain types of households such as those detailed above may opt out of the market for owner-occupied housing as the asset price of housing rises, while other households remain in the market and boost housing consumption in response to the tax break induced by the MID. In a sense, those who are down-payment-constrained, have a short expected duration, or are relatively risk averse are bid out of the market. We thus speculate that at the aggregate market level, the total physical quantity of owner-occupied housing may remain unchanged, yet the homeownership rate may fall because the subsidy results in an increase in housing demand on the intensive margin: those who remain in the market take up the slack by increasing housing consumption in response to the subsidy.

B. Present Value Description

In this section, we examine in more detail the two channels by which the MID can affect homeownership incentives. To do so, we use a simple net present value (NPV) model of the purchase price a home buyer is willing to pay for a housing investment to illustrate the direct incentive effect of the subsidy. Second, we refer to a theoretical model by Glaeser et al. (2010) to assess the relevance of market-wide equilibrium effects. Third, we consider the effect of the MID on the NPV of a housing investment by allowing the MID to be reflected both directly and indirectly in the NPV expression. We find that the overall effect of the MID on incentives to own is theoretically ambiguous and depends on the housing supply elasticity as well as other factors. When housing supply is perfectly elastic, the MID will always have a positive effect on incentives to own. However, when supply is not perfectly elastic, our simulations suggest there are realistic scenarios when the MID can have an adverse effect on the incentives to own.

Asset market equilibrium requires that the price of a house equals the present value of its net service flows discounted at the individual's real after-tax interest rate (Poterba, 1984). Let P_0 be the purchase price of a unit house in the initial period. For a holding period of N years, the NPV of the housing investment is

$$NPV = -(1 - \alpha_0 P_0) + \sum_{t=1}^N \left[\frac{R_t - (d_t + (1 - MSR_t)\alpha_t r_t + (1 - \tau_t)\delta_t)P_t}{(1+i)^t} \right] + \frac{(1-\varphi)P_N - L_N}{(1+i)^N}, \quad (1)$$

where α_t represents the loan to value ratio, P_t is the house value, and R_t is the rental value of the housing services generated by the housing stock in period t . d_t equals the depreciation and maintenance rate on the housing stock, r_t equals the nominal mortgage interest rate, τ_t represents the marginal tax rate, and δ_t represents the property tax rate in period t . MSR_t is the mortgage subsidy rate at time t . i is the real after-tax discount rate. φ is the transaction cost of selling the housing stock as a percent of the selling price, and L_N is the remaining loan balance at the end of the holding period. The first term of equation (1) is the down payment amount, a cost incurred at the start of the holding period. The middle term is the net consumption value of the house (the rental value minus outlays), received in each year t , over the holding period. The third term is the net proceeds from the sale of the home at time N .³

Households may have different NPVs depending on their marginal tax rates, down payment constraints, and their expected duration in the property; however, in equilibrium, the marginal buyer's willingness to pay will equal the equilibrium price, which equates to marginal cost. Glaeser et al. (2010) present a model of homeownership on the extensive margin with worker heterogeneity and variable housing supply elasticity. While the objective of Glaeser et al. differs from ours, their model holds implications for the price effects of the mortgage subsidy rate (MSR) according to the supply elasticity, when the extensive margin is taken into account. This is the context of our NPV model (i.e., whether to purchase, whereby a condition for purchase is that the home buyer's NPV of the home investment is greater than or equal to the market price). Utility maximization ensures that buyer valuation on the margin just equals the cost of buying at time t ; by modeling the supply side directly, Glaeser et al. derive an equilibrium price expression in their equation (7) that depends on a number of parameters, including the value of the MSR. To generate a semi-elasticity of house prices with respect to the MSR that is a function of the housing supply elasticity, we differentiate the log of the Glaeser et al. equilibrium price expression with respect to the MSR. In what follows, we combine the market-wide potential price effects implied by Glaeser et al. in their equation (7) with the basic NPV model above to generate some implications regarding the decision to buy that embody both the individual incentive and market-wide effects.

We differentiate equation (1) with respect to the MSR, allowing for the direct effect of the MSR, the home buyer's tax break, and the indirect effect through market prices, $\partial P_0 / \partial MSR$, which yields

$$\frac{\partial NPV}{\partial MSR} = -(1 - \alpha_0) \frac{\partial P_0}{\partial MSR} + \sum_{t=1}^N \left[\frac{\alpha_t r_t P_t}{(1+i)^t} \right] + \sum_{t=1}^N \left[\frac{-(d_t + (1 - MSR_t)\alpha_t r_t + (1 - \tau_t)\delta_t) \times \frac{\partial P_0}{\partial MSR} (1 + \sum_{j=1}^t \pi_j)}{(1+i)^t} \right] + \frac{(1 - \varphi) \frac{\partial P_0}{\partial MSR} (1 + \sum_{j=1}^N \pi_j) - \frac{\partial L_N}{\partial P_0} \frac{\partial P_0}{\partial MSR}}{(1+i)^N}. \quad (2)$$

We assume that a one-time increase in the MSR will affect the purchase price, P_0 , not subsequent rates of house price appreciation, π_t . Now consider the case of a perfectly elastic housing stock, which implies zero long-run house price capitalization. Setting $\partial P_0 / \partial MSR = 0$ eliminates all but the second term of equation (2), resulting in an unambiguously positive effect of the MSR on the NPV. This positive effect is independent of the holding period and intuitively plausible: the MSR increases the home buyer's incentive to own by providing a tax break without inducing higher equilibrium prices. In this context, the comparative statistic is positive, but only for households that itemize deductions and such households tend to have a higher income. Thus equation (2) suggests that an increase in the subsidy will tend to increase the desirability of a house purchase for higher-income households in elastic markets.

In the case of a less than perfectly elastic housing stock, some extent of market-wide house price capitalization is implied and $\partial P_0 / \partial MSR$ is positive. In this instance, equation (2) cannot be readily signed. As noted in section IIIA, we suspect that an adverse effect of a mortgage subsidy may arise for certain households, such as those that are down-payment-constrained. To examine whether this is so, we undertake the following thought experiment: What is the change in the NPV of a house purchase due to implementing a MSR at 26% (the mean MSR in our sample) when partial or full capitalization of the MSR into house prices occurs, households have a fixed amount (20% of the presubsidy house price) available for a down payment, and households vary in their expected duration in the property? To incorporate the supply elasticity and market-wide price effects, taking into account the extensive margin, we use the Glaeser et al. parameter assumptions and their equation (7) to evaluate the semielasticity of prices with respect to the MSR. We then compute the resulting equilibrium price change under two different assumptions about the housing supply elasticity: a supply elasticity set equal to 2 and 0. We also consider the change in the NPV assuming equilibrium house prices rise by the present discounted value of the subsidy

³ For simplicity we ignore capital gains taxes in equation (1). Since the Tax Reform Act of 1997, only capital gains on owner-occupied housing in excess of \$250,000 for single households (\$500,000 for married couples filing jointly) are subject to the tax. See Shan (2011) for a recent discussion.

over a 20-year holding period. Note that even in the case of perfectly inelastic supply in the Glaeser et al. model, given the parameter values, the equilibrium price increase is smaller than the presented discounted value of the subsidy.⁴ An increase in price equal to the present discounted value of the subsidy reflects demand on both the extensive and intensive margins, whereas the model by Glaeser et al. is one of demand on the extensive margin only.

The stylized story we are presenting is that the capitalization of the MSR into higher house prices is rolled into a larger loan amount post subsidy since the household has a fixed amount available for a down payment. While a potential home buyer will ultimately benefit from the MSR-induced higher value of the home at the time of sale, all else equal, the potential buyer will also experience higher annual operating and financing costs as a result of the MSR-induced increase in house prices. The simulation of equation (2) under the various scenarios is presented in web appendix table A1. Our simulations suggest that the MSR decreases the NPV of the housing investment for down-payment-constrained potential home buyers with short holding periods (up to three to four years) and under the assumption of inelastic supply, either assuming the price response implied by equation (7) of Glaeser et al. (2010) or assuming full capitalization. Our simulations suggest that a negative NPV is feasible even in the case of fairly (but not perfectly) elastic supply, but only for an unrealistic holding period of one year. The simulation is suggestive of a potentially noticeable negative impact of the MSR on the probability of homeownership in metropolitan areas with inelastic supply, particularly since most households in those places face down payment constraints and the median holding period in the United States is six years.⁵ While this is a stylized example, we think it is a plausible one for many potential home buyers. It is not intended to prove that an adverse effect of the MSR exists in inelastically supplied markets but that an adverse effect may exist. Next we empirically test for such an effect.

C. Measures of Housing Supply Elasticity

There is ample evidence that indices of the restrictiveness of land use regulation are good proxies for the housing supply inelasticity and thus for the potential for house price adjustment as a consequence of a demand shock or, conversely, expansion of owner-occupied housing through new construction. For example, Quigley and Raphael (2005) use a city-level index of regulatory stringency for California cities and relate this index to local house prices in 1990 and 2000.

⁴ For example, assuming an initial purchase price of \$200,000 and perfectly inelastic housing supply, as detailed in the online appendix table A1, using equation (7) and parameter values of Glaeser et al. (2010), we find the equilibrium price increase resulting from implementing an MSR of 26% equals \$43,628. In contrast, the discounted present value of a subsidy equal to 26% on a \$200,000 house value over a twenty-year holding period is \$51,942.

⁵ The statistic is based on data from the National Association of Realtors (NAR) for the years 2001 to 2006 (statistic provided by Walter Molony, analyst for the NAR, April 2007).

They document that more regulated cities have more expensive housing and a slower growth in housing stock. They confirm that these more regulated places also have a lower price elasticity of housing supply. In a similar vein, Saks (2008) demonstrates that locations with relatively few barriers to construction experience more residential construction and smaller increases in house prices in response to an increase in housing demand. Lutz (2009) examines the effect of a large exogenous shift in property tax burdens induced by a 1999 school finance reform in New Hampshire. His estimates suggest that in most of the state, municipalities with a reduced tax burden experienced a large increase in residential construction. In the area of the state near Boston, the region's primary urban center, however, the shock cleared through price adjustment. Lutz attributes these differing responses to differing housing supply elasticities, likely caused by spatial differences in regulatory restrictiveness. Finally, Saiz (2010) uses a current measure of regulatory restrictiveness—the Wharton regulatory index that captures the restrictiveness of regulation around 2005—and relates this directly to measures of supply elasticity, demonstrating that more regulated metropolitan areas have more inelastic supply.

In the empirical analysis that follows, we employ a measure of regulatory restrictiveness, compiled by Saks (2008), as our proxy for the responsiveness of the owner-occupied housing stock to changes in house prices. Saks (2008) derives a combined measure of regulatory restrictiveness for the late 1970s and the 1980s by using the simple average of six independent surveys conducted during this time period. The index is scaled to have a mean of 0 and a standard deviation of 1. The index ranges from 2.21 for New York (most restrictive) to -2.40 for Bloomington-Normal, Illinois. Generally, desirable coastal metropolitan areas such as New York, San Francisco, San Diego, Los Angeles, and Boston are most tightly regulated, whereas metropolitan areas in the Midwest and the South tend to have lax land use controls. However, there is considerable within-state variation in the regulatory index. For example, while most metropolitan areas in California are tightly regulated, the index ranges from $+2.1$ for San Francisco to -0.32 for Orange County. Similarly, while metropolitan areas in Texas tend to have relatively lax regulation controlling the expansion of the housing stock, the index ranges from -1.18 for Dallas to $+0.98$ for Tyler. The most extreme difference can be found in the state of New York. Whereas New York City tops the index table with $+2.21$, Buffalo-Niagara Falls is the second least tightly regulated place with an index of -1.96 . (See Saks, 2008, and in particular her table A2 for further details.)

Our regression analysis relies on the assumption that the Saks index is exogenous to individual tenure decisions. This assumption seems plausible for two reasons. First, the Saks index has the important advantage, compared to more recent measures of regulatory restrictiveness, that it essentially predates our sample period and hence is exogenous to (not determined by) subsequent changes in tax policies and

subsequent housing tenure decisions (and changes in homeownership rates) during the 1990s and 2000s.⁶ Second, the index captures regulatory restrictiveness at the MSA-level (not at the local level, where planning boards are elected), and we control for MSA-level fixed effects that capture time-invariant unobservable MSA-level specific characteristics (e.g., the city-specific prospects for long-run future business investment) that may jointly determine past regulatory constraints and future individual tenure decisions. It is therefore plausible to assume that our measure of regulatory restrictiveness is uncorrelated with the error term. The Saks index may also be preferable over geographical or physical constraints measures. First, whereas tight regulatory constraints may always be binding and magnify price responses to demand shocks even if ample developable land is available, places with lax regulation and comparably little developable land may still have quite elastic supply. Various studies are indicative that geographical and physical constraints may be affecting price responses to demand shocks only in highly urbanized areas such as Boston or the Greater London area (Lutz, 2009; Hilber & Vermeulen, forthcoming).⁷ Third, in contrast to geographical constraints, regulatory constraints may also complicate or hinder the conversion of existing housing that is suitable for renter occupation to housing that is more amenable for owner occupation, thereby limiting the quantity supplied of owner-occupied housing.

For these reasons, we conduct our empirical analysis with the index measure generated by Saks (2008). In this context, Saks's finding that in more strictly regulated metropolitan areas house prices respond more strongly to changes in housing demand is particularly reassuring, as it supports our implicit assumption that in more tightly regulated places (defined as in our study), the extent of capitalization of demand factors (e.g., the mortgage subsidy), is greater. In a further attempt to confirm our implicit assumption that house price capitalization effects are greater in more tightly regulated places, we conduct a simple test of the proposition that regulatory restrictiveness affects the extent to which the mortgage subsidy rate raises house prices within our sample. Table A2 in the appendix reports the results of regressing the log of the house price index on the MSR (panel A) and the house price appreciation rate on the percentage change in the MSR (panel B), respectively, controlling for year and MSA fixed effects as well as state and MSA time trends. Results are reported separately for highly

regulated places (all metropolitan areas with regulatory stringency of at least 1 standard deviation above the mean) and little regulated places (all metropolitan areas with a regulatory stringency of at least 1 standard deviation below the mean). The results in both panels confirm that more regulated places have a much greater extent of capitalization of the MSR. The effect is between four and twelve times as large in the more regulated places and is statistically significant only in those places. While this is a preliminary and rather coarse look at capitalization, it is suggestive.

It is also worth noting that other studies (e.g., Quigley & Raphael, 2005, for the United States; Hilber & Vermeulen, forthcoming, for the United Kingdom) that use different measures to proxy for regulatory stringency also come to the same conclusion: house prices react more strongly to demand shocks (i.e., the extent of house price capitalization is greater) in more tightly regulated markets and hence, all else equal, housing is more expensive in those markets. Finally, in addition to the regulatory control, the homeownership specifications we estimate control for housing stock composition in the census tracts in which the households reside in order to capture at least in part the other aspect of housing supply elasticity: the extent to which the existing rental stock can be converted to owner-occupied use.

IV. Measuring the Combined State and Federal Mortgage Interest Deduction

Our key variable of interest is the combined federal and state subsidy to home owners through use of the federal and state (where applicable) mortgage interest deductions. While data reported in the PSID allow for the construction of each household's mortgage interest paid, itemization status, and an approximation of the marginal tax savings the household receives from claiming the MID, using the household's actual marginal tax savings from the MID is not appropriate. The household's actual mortgage subsidy rate is a complicated function of the household's characteristics that also determine the likelihood of homeownership and would therefore be endogenous in a tenure choice model. Instead, we use a measure generated in the spirit of Cutler and Gruber (1996) that is correlated with the individual's mortgage subsidy rate, but exogenous and not correlated with the other determinants of homeownership. This measure is the NBER average state and federal combined mortgage interest subsidy rate, which is publicly available and generated by the NBER based on a large, fixed, nationally representative sample of 1995 individual tax returns for each state and year, provided by the Statistics of Income Division of the U.S. Internal Revenue Service.

The NBER measure is generated as follows (Feenberg & Coutts, 1993). State and federal income tax liabilities owed by a large sample of taxpayers in each state in each year are calculated, holding the sample and income distribution fixed. The mortgage interest is then increased by 1% for each taxpayer, the state and federal taxes are recalculated,

⁶ More recent measures of regulatory restrictiveness, such as the Wharton Residential Land Use Regulation Index (WRLURI) from around 2005 (see Gyourko, Saiz, & Summers, 2008) have the drawback that the level of regulatory restrictiveness may be caused by changes in homeownership rates during our sample period, which may in turn be affected by changes in the MSR.

⁷ Saiz (2010) considers the impact of the presence of water bodies and slopes steeper than 15 degrees. While such constraints significantly restrict coastal areas and areas with major mountain chains, many metropolitan areas are neither coastal nor located near major mountain chains, yet they likely vary significantly in their supply elasticity. Saiz also computed a direct measure of supply elasticity, but this is based in part on the—for our purposes—endogenous WRLURI index.

and the mortgage interest subsidy is generated as the ratio of the additional tax (savings) to the additional mortgage interest. The measure captures the tax savings from an additional dollar of mortgage interest, or, equivalently, the marginal subsidy rate on mortgage interest. The average MSR in a given state and year is then computed by averaging over taxpayers by state and year.

The NBER measure has a number of desirable features. First, it varies only due to changes in the federal and state tax laws, not due to changes in income or other household characteristics of the taxpayer sample. Second, using a large microsample to generate a taxpayer-level subsidy measure and then averaging over all taxpayers by state and year captures the nonlinearity and richness in the tax code that would not be captured by the use of a more aggregated approach (such as running state median income through a tax calculator). Moreover, because the NBER MSR measure is the simple average of all taxpayers' MSRs, we can derive the marginal effect of interest, the variation of the impact of the MSR by income status, without having an income-specific measure of the MSR: the marginal effects by income status controlling for the average MSR by income group and the marginal effects by income status controlling for the NBER MSR will be proportional (by a factor equal to the number of income categories).⁸ Third, using the average MSR in the state and year in which a household is observed provides an exogenous measure of the MSR for our household-level analysis.

As presented in section IIIA, there are two effects of the MSR. First is the direct incentive effect of the subsidy for individuals. By use of the noted interaction terms, we can sort out the incentive effects by income group. Second is the indirect effect on house prices through the average market effect—essentially a reduced-form effect. As presented in section V, we empirically distinguish the incentive versus market effects by controlling for supply conditions. The MSR controls are capturing the incentive effect, and the degree of regulatory restrictiveness interacted with the MSR captures the market price effect. In models that fail to control for the supply elasticity, the MSR effect would be a combination of the incentive and market effects.

The variation in the combined state and federal NBER measure across states and within states over time can result from changes in the federal tax code, the state tax code, or both. The federal subsidy rate is affected by changes in the federal tax code that alter income definitions, itemization status, and marginal tax rates in particular. During the time period we examine, there are five major instances of federal

tax law changes. These occur in 1986, 1993, 1997, 2001, and 2003. Reductions in marginal tax rates at the federal level may arise due to tax reform, as in the Tax Reform Act of 1986 (TRA86), or fiscal stimulus (2001 and 2003 Bush-era tax cuts), but in both instances they reduce the value of the federal MID. In contrast, the 1993 and 1997 tax law changes increased marginal tax rates, increasing the value of the federal MSR, but they also put in place phase-outs on some itemized deductions. Although states have a high degree of sovereignty in designing their tax codes, changes in the federal tax code may directly or indirectly trigger changes in state tax laws. For example, a change in federal marginal tax rates changes the value of deducting state taxes paid (income, sales, and property), which can affect the mix of tax instruments used at the state level.⁹ Changes in federal tax structure can also directly affect the value of a state-level MID. For example, eight states have reciprocal deductibility whereby federal taxes are also deductible from state taxable income (Fisher, 2007), and therefore changes in federal taxes paid affect the state marginal tax rate faced by the taxpayer and hence the value of the state MSR.

Some changes in the state MSR come about independent of changes in the federal tax code. States implement tax law changes when state fiscal crises arise or to mimic neighboring states' policies (Howe & Reeb, 1997). A series of papers has examined the impact of tax competition between states on state tax law structure (Besley & Case, 1995). Heterogeneity in state tax structure also arises due to variation in states' efforts to rely on taxes that allow an "exporting" of tax burdens to nonresidents. This includes the example of relying on tax sources that are deductible from federal taxable income. Other examples include the use of sales and business taxes that are expected to be shifted to residents of other states (Fisher, 2007).

In general, state income tax structures are not uniform across states and do not necessarily conform to the federal tax structure (Fisher, 2007). Based on the variation in state tax structures and states' reactions to changes in federal tax structure, we expect that the variation in the MSR across and within states over time may be large. To determine if this is indeed the case, we examine a second NBER series, the average net state mortgage interest subsidy by state and year, which we refer to as the state MSR. A state's MSR gives the mortgage subsidy rate arising from the state income tax structure. This NBER series is constructed in the same manner as the combined mortgage interest subsidy but is derived from the state income tax liabilities only of the fixed 1995 taxpayer sample. Table 1 reports summary statistics for both the state MSR and the combined MSR series in each state for the time period we analyze (1984–2007).

⁸ To see this is so, consider a simplified example with two types of households: high income ($D_1 = 1$) and low income ($D_2 = 1$). Let X_1 equal the MSR received by high-income households and X_2 equal the MSR received by low-income households. Then the NBER MSR measure can be expressed as $X = (X_1 + X_2)/2$. A regression controlling for X_1 and X_2 (interacted with D_1 and D_2 , respectively) will yield coefficient estimates that equal two times the corresponding estimates from controlling for X (interacted with D_1 and D_2).

⁹ A strong consensus exists in the literature that the federal deductibility of state taxes (income, sales, and property) causes states to rely more heavily on these sources of revenue than on non-federally deductible taxes (Feldstein & Metcalf, 1987; Feenberg & Rosen, 1986; Metcalf, 2011; Holtz-Eakin & Rosen, 1988).

TABLE 1.—NBER MORTGAGE SUBSIDY RATE BY U.S. STATE IN %, 1984–2007 (PSID SAMPLE YEARS ONLY)

U.S. State	Average State Net MSR	S.D.	Minimum	Maximum	Average Combined MSR	S.D.	Minimum	Maximum
Alabama	3.56	0.12	3.29	3.72	25.19	2.10	22.8	29.37
Alaska	0	0	0	0	26.92	3.10	23.21	33.3
Arizona	4.21	0.86	3.37	5.61	26.14	2.11	23.19	30.51
Arkansas	5.46	0.83	3.81	6.43	28.26	1.62	25.95	31.22
California	6.01	0.32	5.43	6.54	26.67	1.41	24.94	29.48
Colorado	4.71	0.27	4.44	5.28	27.08	2.07	24.55	31.48
Connecticut	0.06	0.07	0	0.22	25.60	2.45	22.89	30.55
Delaware	6.41	0.87	5.1	8.56	27.37	2.21	24.06	31.95
District of Columbia	8.98	0.56	7.94	10.17	34.68	2.60	32.36	40.48
Florida	0	0	0	0	22.97	2.14	20.15	27.22
Georgia	5.32	0.11	5.21	5.56	27.78	2.26	25.32	32.68
Hawaii	8.86	0.67	7.57	9.46	28.20	1.75	25.31	31.83
Idaho	5.74	0.37	4.96	6.56	25.81	2.22	22.76	29.71
Illinois	0	0	0	0	24.50	2.37	21.73	29.48
Indiana	0	0	0	0	23.62	2.35	20.26	28.11
Iowa	5.59	0.21	5.25	5.81	27.63	2.03	25.1	31.93
Kansas	5.33	0.84	3.07	6.19	28.83	2.33	25.85	33.66
Kentucky	5.26	0.72	3.96	5.83	27.80	1.93	25.63	31.4
Louisiana	2.23	1.37	-1.45	3.08	26.78	2.71	21.74	31.23
Maine	7.28	0.36	6.31	7.78	28.13	1.79	25.98	31.53
Maryland	3.89	1.70	0.06	4.69	26.49	0.97	24.56	28.08
Massachusetts	0	0	0	0	24.18	2.12	21.65	28.74
Michigan	0	0	0	0	25.03	2.42	21.93	29.94
Minnesota	7.05	1.08	5.34	9.59	29.40	3.36	25.05	37.39
Mississippi	4.04	0.31	3.47	4.53	27.80	1.67	25.22	31.08
Missouri	4.19	0.53	3.38	4.93	27.26	1.84	24.95	30.58
Montana	5.25	0.86	3.56	6.19	26.12	1.93	24.13	29.59
Nebraska	5.02	0.52	4.17	6.3	27.05	1.82	25.09	30.79
Nevada	0	0	0	0	24.23	1.90	21.77	28.11
New Hampshire	0	0	0	0	23.00	2.08	20.49	27.46
New Jersey	0	0	0	0	24.70	2.29	22.2	29.68
New Mexico	5.29	0.80	3.69	6.22	26.88	1.30	24.15	28.9
New York	5.73	1.21	4.44	8.49	28.26	2.60	25.88	34.23
North Carolina	6.27	0.53	5.52	7.05	28.49	1.78	26.53	31.81
North Dakota	3.28	0.17	3.08	3.58	27.51	2.61	24.89	33.36
Ohio	0	0	0	0	24.23	2.31	21.35	28.9
Oklahoma	4.56	2.44	0.4	6.41	26.70	2.09	24.72	30.79
Oregon	8.12	0.51	6.7	8.86	28.97	2.11	26.45	33.64
Pennsylvania	0	0	0	0	24.03	2.26	21.25	28.56
Rhode Island	5.22	0.50	4.31	6.07	26.10	2.46	23.37	31.69
South Carolina	5.90	0.44	5.3	6.52	27.29	2.14	24.23	31.84
South Dakota	0	0	0	0	22.86	2.11	20.52	27.59
Tennessee	0	0	0	0	24.50	2.42	20.96	29.25
Texas	0	0	0	0	25.55	2.68	22.26	30.83
Utah	6.07	0.41	5.41	7.34	25.70	1.62	23.73	29.13
Vermont	5.72	0.70	4.4	6.76	27.48	2.67	24.07	33.25
Virginia	5.29	0.12	5.15	5.49	27.99	1.89	25.82	32.04
Washington	0	0	0	0	22.12	1.88	19.37	25.8
West Virginia	0.87	2.06	0	5.6	23.00	2.77	19.66	28.89
Wisconsin	4.84	0.79	3.73	7.15	27.56	2.30	24.98	32.96
Wyoming	0	0	0	0	21.77	3.20	18.71	28.58

Notice in table 1 the significant variation in the state MSR across U.S. states. The average subsidy rate in Oregon, for example, is 8.12 cents for every dollar of mortgage interest, whereas the average subsidy rate in Alabama is only 3.56 cents. Comparing the minimum to the maximum values in table 1, we see that of the 34 states that have a state MSR, over half have a state subsidy rate that changes by at least 2 percentage points over this time period; in some states, the change in the state MSR is sizable: in Arizona, New York, and Wisconsin, the state MSR changes by nearly 100% over the 24-year period under consideration. Finally, note that for the 16 states without a state MSR, the combined MSR nonetheless changes, reflecting the change

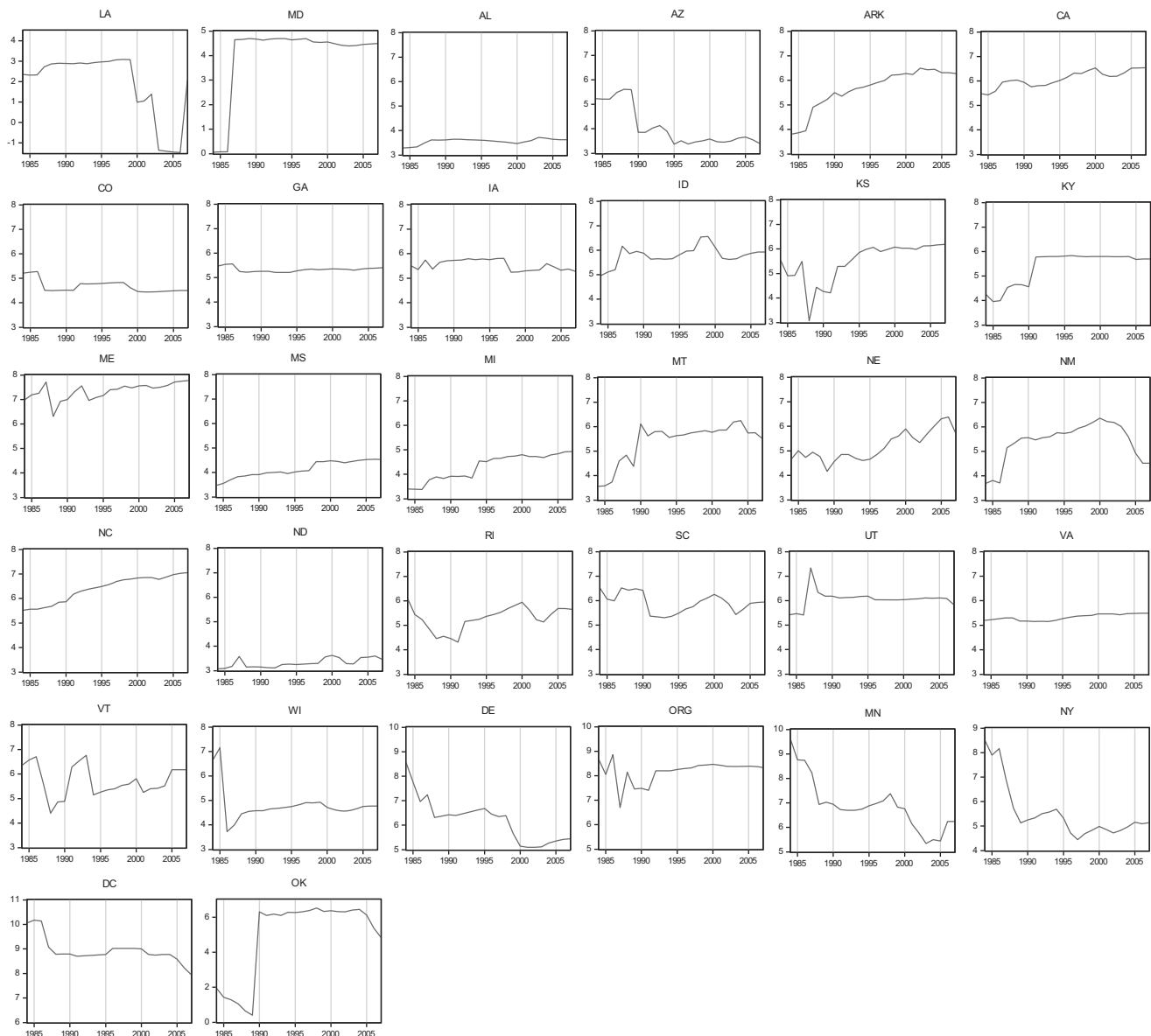
in the federal MSR subsidy received by the taxpayers in these states at different points in time.¹⁰

How much of the variation shown in table 1 is common across states and hence would be swept up by year fixed effects? Figure 1 shows the variation in the state MSR by state over time.¹¹ No typical pattern emerges. The subsidy

¹⁰ The following states are not represented in our PSID sample, but are included in table 1 and figure 1 for illustrative purposes: Delaware, Iowa, Maine, Montana, Nebraska, New Mexico, North Dakota, and Vermont.

¹¹ All graphs are normalized to a bandwidth of 5 percentage points, except Oklahoma, which has a vertical range from 0 to 7 percentage points. States not pictured do not have a state MSR during the time period considered, except Connecticut. It has a state MSR, but it is very small and graphically indistinguishable from 0 if the regular bandwidth is applied.

FIGURE 1.—NET STATE NBER SOI MORTGAGE SUBSIDY RATE BY U.S. STATES, 1984–2007 (%)



The series are the NBER SOI average net state mortgage subsidy rate (MSR) in each state and year and show the state-level mortgage interest subsidy rate. The series are generated based on a large, fixed, representative sample of U.S. taxpayers (the income distribution is held fixed) and vary only due to changes in federal and state tax laws that affect specifically state-level income tax structure. States not pictured do not have a state-level MSR during the time period considered. All graphs are normalized to a range of 5 percentage points, except Oklahoma, which has a range from 0 to 7 percentage points.

rises over time in some states and declines in others. Importantly, there is significant variation across states in the changes in the state MSR following federal tax reform. For example, following TRA86, the state MSR rose in a number of states, including Louisiana, Maryland, and Arkansas, but fell in others, such as Rhode Island and Minnesota.

V. Empirical Analysis

A. Data and Sample Issues

This paper uses data from multiple sources. The primary data source is three decades of data from the confidential

version of the PSID, a longitudinal survey of families—from whom we (confidentially) know their census tract of residence—that has been carried out continuously since 1968 and provides a unique opportunity to follow households over time and across space.¹² We select all PSID households observed from 1984 to 2007. We begin the panel in 1984, the first year in which the PSID collects information on the household wealth holdings. Data are collected annually until 1997 and biennially after 1997, pro-

¹² The PSID tract and MSA location indicators are confidential data from the PSID GEOCODE data files and can be obtained from the PSID under special contract. These data are not available from the authors.

viding up to nineteen observations per household.¹³ The data include (a) the original 1968 PSID core sample of 5,000 households selected as a random cross-section sample of the U.S. population with an additional low-income sample and (b) persons living within a household unit that enter the sample as a separate household when they form their own household. The PSID reconstituted its sample in 1997 by dropping one-third of the core sample, changing to biennial data collection, and reformatting sample weights. Thus, our sample includes only households observed from 1984 through 2007, roughly two-thirds of the original core sample. All of the household data used in this study are collected in each year of observation, except wealth data. Prior to 1997, the wealth data are collected every five years. After 1997, they are collected with each survey. For the pre-1997 wealth data, we apply a linear function to impute annual estimates of total net wealth.

In addition to the confidential version of the PSID, we use four secondary data sources, all publicly available, that report data at the tract, metropolitan area, or state level. The NBER provides the mortgage subsidy rate, our key variable of interest discussed above, as well as a property tax subsidy rate (generated similarly). Our second source is the Federal Housing Finance Agency (FHFA). From the FHFA we derive mortgage interest rate data as well as house price indexes.¹⁴ Specifically, the FHFA provides data on metropolitan and state average effective mortgage interest rates at the time of mortgage origination for conventional, single-family, nonfarm loans. The data are from the FHFA's Monthly Interest Rate Survey and are computed based on fully amortized loans. Refinances, nonamortized loans, and balloon loans are excluded from the FHFA data, as are non-conventional loans (www.fhfa.gov). We use metropolitan-area data whenever available and state-level data for PSID households that are not residing in one of the FHFA reported metropolitan areas. The effective mortgage interest rate is the contracted rate adjusted for fees and charges. We use the mortgage interest rate data as part of the user cost controls in a robustness check of our main specifications. The house price index and appreciation data, used in web appendix table A2 as well as in specifications controlling for the relative cost of homeownership, also come from the FHFA. FHFA produces public use house price indexes at the metropolitan and state levels using a repeat sales methodology and data on single-family properties whose loans

have been purchased or securitized by Freddie Mac or Fannie Mae over the years (see www.fhfa.gov). As with the FHFA interest rate data, we use the metropolitan-level indexes where available and the state-level indexes for households that are not residing in one of the FHFA metropolitan areas. The third source is the 1980 U.S. Census, which provides tract-level data on the composition of the housing stock. The specific variables we examine include the share of housing units in the tract that are single family and the share of units that are in multiplexes (structures with five or more units). We use the 1980 composition of the housing stock as it will be exogenous in an analysis of the probability of homeownership after 1980. Finally, as noted in section IIIC, we use the metropolitan-level regulatory index generated by Saks (2008) as a measure of the housing supply inelasticity. We link all of these data to PSID households using confidential PSID geographic location information.

The final sample contains 4,197 households corresponding to 53,279 household-year observations residing in metropolitan and nonmetropolitan areas for the base empirical specifications and 2,620 households corresponding to 29,621 household-year observations residing in metropolitan areas for which we have Saks (2008) regulatory index data. Roughly 2.5% of households move to a different state and 4% of households move to a different MSA in any given year. All dollar amounts are adjusted to 2007 dollars using the urban Consumer Price Index. All analysis is weighted using the PSID 2005 sample weights.¹⁵

B. Empirical Approach

We estimate the following base specification for household i in location j at time t as a linear probability model:

$$\Pr(\text{own}_{ijt}) = \alpha_0 + \alpha_1 \text{MSR}_{jt} + X'_{it}\beta + L'_{jt}\delta + D'_i\lambda + e_i, \quad (3)$$

where MSR is the mortgage subsidy rate, which is expected to have a positive coefficient to the extent that it facilitates homeownership. The household's MSR varies over time even if the household does not move at all or moves only within the state. This is because the MSR varies within state over time. X is a vector of household characteristics that vary over time, L is a vector of time-invariant and time-varying location controls, and D is a vector of individual fixed effects. The vector of time-varying household characteristics includes controls for total family income, total net wealth, age of head, marital status, children, and unemployment of head and spouse if present. We control for income

¹³ Due to missing data, we allow for an unbalanced panel in our analysis in order to include the greatest number of households. Our full regression sample underlying the specifications reported in table 4 consists of 53,279 observations, which is roughly 67% of the fully balanced sample. Nineteen percent of households are observed every year, roughly 50% are observed in at least fourteen years, and 15% are observed for five years or less. The sample underlying the regulatory interaction specifications reported in table 5 is slightly more unbalanced. Due to missing values, this sample consists of 29,621 observations, which is roughly 60% of the fully balanced sample.

¹⁴ Until 2008, the most recent entity to generate the interest rate series was the Federal Housing Finance Board (FHFB). It was combined with OFHEO in 2008 to form the FHFA.

¹⁵ The PSID sample is not representative of the U.S. population without the application of sample weights. The post-1997 weights are stratified to the U.S. population according to data from the Current Population Survey. See Heeringa and Connor (1999) for more discussion. We use the 2005 combined family weight because the more recent 2007 weight is preliminary and not available for as many households as the 2005 weight.

by the use of three income categories: low, moderate, or high income. A low-income household is one whose annual income is less than or equal to 80% of state median income; moderate-income households include households with incomes between 80% and 120% of state median income, and high-income households are those with incomes above 120% of state median income.¹⁶ The vector of location characteristics includes tract-level housing composition controls (the share of housing units that are single family units and the share of housing units in multiplexes), MSA fixed effects, and state fixed effects. The rationale for including both MSA and state fixed effects is that not all households reside in MSAs. The state fixed effects provide location controls for those places. Also, there could be unobservable time-invariant effects at the MSA and state level. We also estimate equation (3) with MSA and state time trends to control for unobserved factors at the MSA and state level that may affect homeownership attainment and may be changing over time. We estimate equation (3) with a cluster correction to generate standard errors that are robust to heteroskedasticity and clustering on two dimensions: households and State \times Year. We simultaneously cluster on these two dimensions to address the possibility that the errors may be serially correlated or spatially autocorrelated at the state level. While clustering on households deals with the serial correlation issue, clustering on State \times Year addresses the possibility of spatial autocorrelation at the state level.¹⁷ We also run specifications that allow for a differential impact of tax subsidies depending on the household's income by interacting MSR with income status.

One advantage of estimating equation (3) as a fixed-effect model is that household fixed effects capture all unobserved heterogeneity in household characteristics, such as race/ethnicity of the household head, that are time invariant. To the extent that households do not move, the fixed effects also capture time-invariant location characteristics (at neighborhood, municipality, county, state, region, and national levels). However, households do move across space, and we observe such changes in our panel. As a result, we also include the location controls discussed above. Regarding total net wealth, note that changes in net

asset wealth are driven in part by changes in income. Hence, once we control for fixed effects and household income, the impact of household net wealth on homeownership attainment can be expected to be quite limited.

The use of state fixed effects in our empirical setup implies that we identify the effect of the MSR on the propensity to own off of variation in the MSR over time within states as well as across states. As noted above, the household's MSR varies over time even if the household does not move at all or moves only within state. This is because the MSR varies within state over time. The household fixed effects allow us to also identify off of across-state moves. Being able to use across-state moves in addition to within-state moves is arguably an added benefit of our approach, particularly since across-state moves are often associated with substantive changes in the MSR. However, importantly, the household fixed effects do not preclude us from identifying off of within-state moves or nonmoves. In fact, most of the variation in the MSR of households is driven by within-state changes of the MSR over time, which affect both within-state movers and nonmovers. Only roughly 3% of all changes in the MSR are driven by across-state moves. We document the relevance of the two sources of variation (i.e., arising from changes in the MSR within state over time or arising from moves across states at different points in time) in the section VC.

One concern with across-state movers is that they may not be similar over time and across states, and this may lead to a selection bias. In particular, households that move across states may be different from the rest of the population (i.e., nonmovers and within-state movers), and it may be the characteristics of the across-state movers that explain our estimated effects rather than the subsidy rate itself. To address this concern, we check for whether our results are being driven by across-state moves. To do this, we reestimate our core specifications but additionally include Household \times State fixed effects in order to control for all state-specific unobserved characteristics of across-state movers.¹⁸ Put differently, for each household, we exploit only within-state variation in the MSR, ignoring variation that arises from across-state moves.

To explore the impact of regulatory restrictiveness, we estimate the following specification for household i in location j at time t , again, as a linear probability model:

$$\Pr(\text{own}_{ijt}) = \alpha_0 + \alpha_1 \text{MSR}_{jt} + \alpha_2 \text{MSR}_{jt} * \text{reg}_{jt} + \alpha_3 \text{reg}_{jt} + X'_{it} \beta + L'_{jt} \delta + D'_i \lambda + e_i, \quad (4)$$

¹⁶ We use state median income data from the U.S. Census Bureau, table H-7, which provides annual median income estimates by state from 1984 to 2007, based on the Current Population Survey. Regarding the income classifications, note that state homeownership assistance programs, such as Florida's State Housing Initiatives Partnership Program, the largest state housing trust fund, use these income definitions. For an example, see <http://www.floridahousing.org/Home/HousingPartners/LocalGovernments>. The U.S. Department of Housing and Urban Development's HOME program, which supports homeownership, defines low income as 80% of MSA median income (<http://www.hud.gov/offices/cpd/affordablehousing/lawsandregs/index.cfm>).

¹⁷ The reported standard errors are similar to those generated in specifications that use only a Huber-White sandwich estimator to correct for heteroskedasticity and those resulting from specifications that cluster only on households but not on State \times Year groupings. Clustering by state is problematic in our empirical setup because households move across states over time so the panels are not nested within state clusters (but they are nested within a given state and year).

¹⁸ To see how we construct the Household \times State fixed effects, consider an example. Suppose a household resides in two states during our observation period: the household is observed living in California and then moves to Texas. We create two mutually exclusive indicator variables for this household: the first equals 1 in each year the household is in California and 0 otherwise. The other equals 1 in each year the household is in Texas and 0 otherwise. These fixed effects ensure that we only identify off of changes in the MSR that are not due to households moving across states.

where reg equals the value of the regulatory index, scaled to have a mean of 0 and a standard deviation of 1, with higher values of the index indicating greater regulatory restrictiveness and hence more inelastic housing supply. The theoretical considerations presented in section III suggest that $\alpha_2 < 0$: the positive impact of the subsidy on homeownership attainment ought to be weaker (and the negative impact stronger) in more regulated metropolitan areas. Note that reg_{jt} varies in the panel even though our regulatory proxy is time invariant and varies only by location. This is because reg_{jt} varies as households move between metropolitan areas and thereby move from more to less restrictive places and vice versa. We also run specifications where the regulatory index is interacted with the MSR and with income status in order to investigate the extent to which different income groups are differentially affected by the mortgage subsidy rate in different regulatory environments. As with estimating equation (3), we estimate equation (4) simultaneously clustering on households and State \times Year.

Missing from the analysis so far is a control for the relative cost of homeownership: the cost of housing services in the owner mode relative to the cost of housing services in the rental mode. In studies of homeownership, the annual cost of housing services in the owner mode is generally approximated as the user cost of housing, which is a household-specific variable measuring the expected consumption value of the housing services from purchasing a home. The user cost is the sum of depreciation and maintenance costs, the after-tax opportunity cost of the down payment, the after-tax mortgage interest payments, and after-tax property tax payments minus the expected, nominal capital gain on the housing structure (Poterba, 1984). Of these components of user cost, equations (3) and (4) control for the MSR. As a robustness check, we also run the models in equation (4) adding controls for additional determinants of user cost: the FHFA reported effective mortgage interest rate, the NBER property tax subsidy rate, and the FHFA contemporaneous house price appreciation rate, as well as the price of rental housing, which we control for as the average annual rent in the city and year in which the family is observed.¹⁹

C. Results

Table 2 presents population-weighted summary statistics for the full sample and the regulatory restrictiveness subsample. Table 3 summarizes the sources of variation in the MSR. Table 3A reports the distribution of moves by type (within state and across state) for the full regression sample

according to five possibilities: no change in the MSR, a change in the MSR, and then by three different magnitudes of change in the MSR. Categories 1 and 2 in table 3 show that there are 50,216 household-year observations in the panel for which we observe data from one year to the next. Of these, 49,873 household-year observations experience a change in the MSR from the prior year and 343 do not. Among the 49,873 household-year observations that experience a change in the MSR since the previous year, the vast majority, 97%, are not across-state moves. Category 1 shows that some moves occur both within and across states, even though the MSR is unchanged. Category 2 shows that we observe 9,161 household-year moves that are accompanied by a change in the MSR. Of these, 7,653 are within-state moves and 1,508 are across-state moves. Note that among identified moves in category 2, 84% are within-state moves (this percentage may actually be a little higher since we cannot identify within-Census-tract moves).²⁰ When we consider the distribution of moves by type for varying degrees of change in the MSR, we see that only for the most substantial changes in the MSR (5% or higher), as shown in category 5, the across-state moves dominate the sample, but they are not the only source of variation. Of the 372 household-year observations that experience a change in the MSR greater than 5% from one year to the next, 55% are associated with across-state moves, the remaining changes are associated with either within-state moves (6%) or non-moves across tracts (39%). Households that elect not to move when the MSR changes also provide identification of the impact of the MSR on homeownership attainment. In fact, in principle, nonmovers can also change their housing tenure: renters can buy their rental property and homeowners can sell and lease back their homes. Table 3B documents the equivalent statistics for the regression sample with information on regulatory restrictiveness. Overall, table 3 illustrates that the variation in the subsidy arises mainly from within-state changes in the MSR over time (affecting both within-state movers and nonmovers) and, to a lesser extent, time-varying across-state differences (affecting across-state movers).

Table 4 reports the results for the baseline estimations on the full PSID sample. Column 1 provides results for the specification that includes only the MSR, household controls, and household fixed effects. Column 2 then adds location controls (the housing composition variables, MSA

¹⁹ The remaining terms in the user cost formula, depreciation and maintenance, are each typically set to a value of 0.02 (see Poterba, 1992), and thus would be part of the constant in an estimation. For the rent data, we compute the average self-reported rent in the PSID in the city and year in which we observe the household. For households residing in nonmetropolitan areas or metropolitan areas with a relatively small sample size (fewer than 100 PSID respondents), we compute a regional rent based on the metropolitan areas being located in one of the nine census divisions.

²⁰ We use 1980 census tract indicators and boundaries from the confidential PSID to identify whether households moved in any particular year. A household is identified as a mover household if a change in the tract occurs. It is identified as an across-state mover if the state changes as well. We cannot categorize moves that occur within the tract. While the PSID does have variables that indicate moves, these indicators are not consistent over the 1984 to 2007 time period. Since all within-census tract moves are also within-state moves, table 3 may underrepresent the share of within-state moves. It is important to emphasize that while table 3 does not capture within-census tract moves, our empirical analysis does. We pick up every move for which there is a change in tenure status. That is, if a household changes tenure status within tract over time, we capture that move through a change in tenure status.

TABLE 2.—POPULATION-WEIGHTED SUMMARY STATISTICS: PSID HOUSEHOLDS, 1984–2007

Variable	Observations	Mean	S.D.	Minimum	Maximum
Full Regression Sample					
Owner-occupier—yes	53,279	0.716	0.451	0	1
Mortgage subsidy rate (absolute)	53,279	0.260	0.0284	0.187	0.405
Household income in 2007, US-\$10,000	53,279	8.29	10.20	0	583.91
Household has low income (\leq 80% state median)	53,279	0.234	0.423	0	1
Moderate income	53,279	0.190	0.392	0	1
High income ($<$ 120% state median)	53,279	0.576	0.494	0	1
Age of household head	53,279	45.10	13.51	0	97
Married	53,279	0.643	0.479	0	1
One child	53,279	0.176	0.380	0	1
Two children	53,279	0.173	0.379	0	1
Three or more children	53,279	0.0917	0.289	0	1
Head in labor force and unemployed last year	53,279	0.0802	0.272	0	1
Wife in labor force and unemployed last year	53,279	0.0317	0.175	0	1
Share units in tract that are single family	53,279	0.648	0.243	0	1
Share units in tract in apartment b. (5+ units)	53,279	0.155	0.191	0	1
Total net wealth in 2007, US\$1 million	53,279	0.331	1.21	-1.30	50.48
Year of observation	53,279	1994.3	6.88	1984	2007
Sample of Observations with MSA-Level Information on Regulatory Restrictiveness					
Owner-occupier = yes	29,621	0.694	0.461	0	1
Mortgage subsidy rate (absolute)	29,621	0.261	0.0293	0.194	0.405
Household income in 2007 US\$10,000	29,621	9.06	11.26	0	583.91
Household has low income (\leq 80% state median)	29,621	0.218	0.413	0	1
Moderate income	29,621	0.170	0.376	0	1
High income ($>$ 120% state median)	29,621	0.612	0.487	0	1
Age of household head	29,621	45.08	13.46	18	96
Married	29,621	0.621	0.485	0	1
One child	29,621	0.173	0.379	0	1
Two children	29,621	0.175	0.380	0	1
Three or more children	29,621	0.0863	0.281	0	1
Head in labor force and unemployed last year	29,621	0.0764	0.266	0	1
Wife in labor force and unemployed last year	29,621	0.0276	0.164	0	1
Share units in tract that are single family	29,621	0.617	0.279	0	1
Share units in tract in apartment b. (5+ units)	29,621	0.194	0.225	0	1
Total net wealth in 2007 US\$1 million	29,621	0.353	1.27	-1.30	50.48
Year of observation	29,621	1994.2	6.94	1984	2007
Regulatory index compiled by Saks (2008)	29,621	0.191	0.985	-2.40	2.21
Property tax subsidy rate	29,621	0.254	0.0419	0.161	0.501
Effective mortgage interest rate	29,621	0.0836	0.0187	0.0543	0.132
House price appreciation rate (only years w/o move)	29,621	0.0363	0.0474	-0.174	0.276
Av. annual rent in MSA/region in 2007 US\$10,000	29,621	0.698	0.161	0.351	1.34
Additional income categories for robustness check					
Household has income					
Between 1.2–1.6 \times state median	29,621	0.159	0.366	0	1
Between 1.6–2.0 \times state median	29,621	0.124	0.330	0	1
Between 1.2–2.0 \times state median	29,621	0.284	0.451	0	1
Above 2.0 \times state median	29,621	0.270	0.444	0	1

fixed effects, and state fixed effects). Column 3 adds year fixed effects, column 4 adds state time trends, and column 5 adds MSA time trends. Column 6 includes all of these controls and allows for separate effects of the MSR by income group. Across all six specifications, the key variable of interest, the MSR, has no statistically significant impact on the likelihood of homeownership, not even for the highest-income households, in column 6, who tend to receive the greatest tax breaks from this feature of the tax code. This result is consistent with Glaeser and Shapiro (2003) and suggests that, on aggregate, this very costly tax subsidy to U.S. homeowners has no discernible impact on the likelihood of homeownership attainment.

The control variables all generate results that are sensible, intuitive, and robust across all models. Income, wealth, age,

being married, and having children all positively affect the likelihood of homeownership, with income and being married having particularly large impacts: based on the coefficients reported in column 5, high-income households are 13.8 percentage points more likely to own than low-income households; being married increases the likelihood of homeownership by 17.1 percentage points. An episode of head or spouse unemployment lowers the likelihood of homeownership by 4 and 3.2 percentage points, respectively. The location controls indicate that the composition of the housing stock matters for homeownership attainment: a greater fraction of single-family units boosts homeownership attainment, whereas a greater fraction of multiplexes lowers it.

Table 5 addresses the central question of this paper: To what extent does the impact of the MID on the likelihood of

TABLE 3.—SOURCES OF VARIATION IN MORTGAGE SUBSIDY RATE

	(1)		(2)		(3)		(4)		(5)	
	No Change in MSR		Any Change in MSR		Change in MSR >1%		Change in MSR >3%		Change in MSR >5%	
	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%	<i>N</i>	%
A. Full Sample (Regression Sample for Table 4)										
No move across tract	305	88.9	40,712	81.6	11,336	76.7	3,317	72.7	145	39.0
Moves across tract within state	36	10.5	7,653	15.3	2,289	15.5	670	14.7	21	5.7
Across-state moves	2	0.6	1,508	3.0	1,157	7.8	576	12.6	206	55.4
Total number of observations	343	100	49,873	100	14,782	100	4,563	100	372	100
B. Sample with Information on Regulatory Restrictiveness (Table 5)										
No move across tract	124	88.6	22,051	80.2	6,046	74.5	1,765	70.3	105	41.8
Moves across tract within state	14	10.0	4,597	16.7	1,401	17.3	398	15.9	17	6.8
Across-state moves	2	1.4	859	3.1	664	8.2	348	13.9	129	51.4
Total number of observations	140	100	27,507	100	8,111	100	2,511	100	251	100

We use 1980 census tract indicators to identify whether households moved in any particular year. A household is identified as a mover household if a change in the census tract occurs. It is identified as an across-state mover if the state identifier changes as well. We cannot categorize moves that occur within tract, and hence the displayed statistics slightly underrepresent the fraction of within-state moves. The probability that a household moves tract from one PSID period to the next is 18.3% in the full regression sample and 19.8% in the sample with information on regulatory restrictiveness. The total number of observations reported in this table differs from the regression samples because this table considers changes in the MSR from one year to the next for all observations in the regression sample with available information. The full regression sample consists of 53,279 observations. We do not compute changes in the MSR for the 2,342 observations in 1984, as 1983 is not in our regression sample. For a further 721 observations, no census tract information is available for the previous year, resulting in a total of 50,216 (343 + 49,873) observations in table 3A. The regression sample used in table 5 consists of 29,621 observations; of these 1,505 are for 1984. A further 469 observations do not have census tract information for the previous year, resulting in a total of 27,647 (140 + 27,507) observations in table 3B.

homeownership vary by location? Specifically, we report results for specifications where the MSR is interacted with regulatory tightness and with income status. Our proposition, theoretically motivated in section III, is that in more regulated places (with inelastic supply), the tax subsidies get capitalized into house values rather than expand the (owner-occupied) housing stock and thereby have little impact on homeownership attainment or may in fact have a negative impact, for example, because homeownership becomes comparably less attractive for down-payment-constrained households with short expected durations in their homes. Columns 1 to 3 allow for the impact of the MSR to vary by regulatory restrictiveness on the full sample for which we have regulatory data, with column 2 adding state time trends and column 3 also adding MSA time trends. Columns 4 and 5 further decompose the impact of the MSR on homeownership attainment by interacting the subsidy with regulatory restrictiveness and with income status. Column 4 adds state time trends to the standard controls; column 5 adds MSA time trends. Columns 6 and 7 replicate the specifications reported in columns 3 and 5 except that columns 6 and 7 additionally control for Household \times State Fixed Effects. The last two specifications allow us to test to what extent our results may be driven by across-state movers who may be quite different from the rest of the population. The inclusion of Household \times State Fixed Effects controls for all state-specific unobserved characteristics of across-state movers. Put differently, for each household, we exploit only within-state variation in the MSR, ignoring any variation that arises from across-state moves.

Columns 1 to 3 indicate that the MSR has no statistically significant impact on the likelihood of owning if a household lives in a metropolitan area with an average degree of regulatory restrictiveness. If a household lives in a place with relaxed land use controls (with a regulatory index below 0), the MSR will have a positive impact on homeownership attainment, whereas the effect is negative in more

tightly constrained locations (with a regulatory index above 0), in line with our theoretical conjectures. According to column 3, evaluating the regulatory index at its sample mean of 0.191 suggests that the marginal effect of a 1 standard deviation increase in the MSR is negligible, increasing the homeownership rate by 0.03 percentage points. Evaluating the regulatory index at its extreme values of -2.4 (Bloomington-Normal, Illinois) and 2.21 (New York, New York) generates the following range: a 1 standard deviation increase in the MSR increases the likelihood of homeownership by 3.5 percentage points in the least regulated place and reduces the same by 2.7 percentage points in the most tightly regulated place.

Referring to columns 4 and 5, we see that a further decomposition is insightful. It reveals that the impact of the subsidy on homeownership attainment by regulatory status varies considerably by income status. Our findings indicate that the subsidy has no effect on the likelihood that low-income households will attain homeownership, regardless of the regulatory status of the city in which they reside. We conjecture that this result is a combination of two stylized facts: housing markets are segmented and very few low-income households itemize.²¹ Previous research indicates that housing markets tend to be segmented at the metropolitan level by house value (e.g., Case & Mayer, 1996). Low-income households, which typically are nonitemizers, tend to own lower-valued houses and live in housing tracts with other lower-income households (Belsky & Duda, 2002), suggesting that for middle- or high-income households, the low-income housing tracts may not be a substitute for the higher-end housing in tightly regulated markets.

²¹ Even among low-income homeowners, itemization rates are low. For example, using 2004 data from the Survey of Finances combined with NBER TAXSIM data, Poterba and Sinai (2008) report in their table 2 that only 23% of low-income homeowners (those earning less than \$40,000 in 2003) itemize, whereas over 98% of high-income homeowners do (those earning \$125,000 or more).

TABLE 4.—BASELINE SPECIFICATIONS: DO TAX SUBSIDIES INCREASE HOMEOWNERSHIP ATTAINMENT?

	Dependent Variable: Household is Owner-Occupier					
	(1)	(2)	(3)	(4)	(5)	(6)
	Household Controls Only	Add Location Controls	Add Year FE	Add State × Time Trends	Add MSA × Time Trends	MSR Varies by Income Group
MSR	-0.128 (0.130)	-0.0453 (0.112)	-0.223 (0.390)	-0.0882 (0.368)	-0.0455 (0.361)	
Low income × MSR						-0.245 (0.382)
Moderate income × MSR						-0.172 (0.384)
High income × MSR						0.0420 (0.380)
Moderate income	0.0781*** (0.00942)	0.0780*** (0.00908)	0.0784*** (0.00906)	0.0785*** (0.00894)	0.0772*** (0.00871)	0.0585 (0.0649)
High income	0.142*** (0.0109)	0.137*** (0.0102)	0.138*** (0.0102)	0.138*** (0.0101)	0.138*** (0.00989)	0.0631 (0.0642)
Total net wealth	0.00542** (0.00228)	0.00446** (0.00188)	0.00453** (0.00189)	0.00486** (0.00201)	0.00435** (0.00179)	0.00443** (0.00179)
Age of head	0.0347*** (0.00184)	0.0313*** (0.00174)	0.0310*** (0.00175)	0.0313*** (0.00175)	0.0305*** (0.00178)	0.0305*** (0.00178)
Age of head ²	-0.000254*** (1.89e-05)	-0.000227*** (1.77e-05)	-0.000226*** (1.81e-05)	-0.000228*** (1.82e-05)	-0.000219*** (1.86e-05)	-0.000220*** (1.86e-05)
Married	0.196*** (0.0128)	0.174*** (0.0118)	0.174*** (0.0119)	0.173*** (0.0118)	0.171*** (0.0115)	0.171*** (0.0115)
One child	0.0572*** (0.00786)	0.0513*** (0.00736)	0.0518*** (0.00731)	0.0535*** (0.00727)	0.0534*** (0.00711)	0.0529*** (0.00714)
Two children	0.0973*** (0.00903)	0.0865*** (0.00857)	0.0867*** (0.00857)	0.0888*** (0.00855)	0.0901*** (0.00830)	0.0895*** (0.00833)
Three or more children	0.125*** (0.0134)	0.107*** (0.0119)	0.108*** (0.0119)	0.109*** (0.0119)	0.113*** (0.0115)	0.112*** (0.0116)
Head unemployed	-0.0427*** (0.00757)	-0.0401*** (0.00721)	-0.0396*** (0.00716)	-0.0400*** (0.00707)	-0.0401*** (0.00703)	-0.0397*** (0.00701)
Wife unemployed	-0.0359*** (0.0106)	-0.0349*** (0.0100)	-0.0344*** (0.0100)	-0.0339*** (0.0101)	-0.0319*** (0.00996)	-0.0318*** (0.00997)
Share of units that are single family		0.0894** (0.0419)	0.0891** (0.0419)	0.0977** (0.0417)	0.0984** (0.0413)	0.0984** (0.0413)
Share of units that are in 5+ unit buildings		-0.312*** (0.0512)	-0.311*** (0.0513)	-0.304*** (0.0507)	-0.308*** (0.0506)	-0.308*** (0.0506)
Household FEs and constant	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	No	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	No	No	Yes	Yes	Yes	Yes
State × Time Trends	No	No	No	Yes	Yes	Yes
MSA × Time Trends	No	No	No	No	Yes	Yes
Observations	53,279	53,279	53,279	53,279	53,279	53,279
Number of households	4,197	4,197	4,197	4,197	4,197	4,197
Centered R ²	0.221	0.288	0.288	0.294	0.315	0.315
Uncentered R ²	0.221	0.288	0.288	0.294	0.315	0.315

Cluster-robust standard errors in parentheses (statistics are robust to heteroskedasticity and clustering on households and State × Year). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

To the extent that there is indeed no (or very little) substitutability between low-income and higher-income housing and the MSR generates little benefit for low-income homeowners, economic theory predicts that the MSR may not affect the demand for lower-end housing and thus will have no effect on the price of lower-end housing, independent of the supply price inelasticity proxied by our regulatory constraint measure. Taking these considerations into account, our finding that the MSR has no effect on homeownership attainment of low-income households appears to be quite plausible.

The coefficients on the three-way interaction terms (Income status × MSR × Regulatory index) for moderate- and high-income households in columns 4 and 5 are statistically

significant and meaningful. Consider column 5, which includes MSA time trends in addition to state time trends. Evaluating the regulatory index at its extreme values generates the following range for moderate-income households: a 1 standard deviation increase in the MSR increases the likelihood of homeownership attainment by 3.3 percentage points in the least regulated location and reduces it by 3.3 percentage points in the most tightly regulated place. For high-income households, the impact of a 1 standard deviation increase in the MSR on the likelihood of homeownership ranges from a 4.7 percentage point increase (least restrictive) to a reduction of 3.0 percentage points (most restrictive).

Columns 6 and 7 report the findings of our robustness check whereby we include Household × State fixed effects

TABLE 5.—RESULTS FOR SPECIFICATIONS WITH INTERACTION TAX SUBSIDY × REGULATORY RESTRICTIVENESS
Dependent Variable: Household Is Owner-Occupier

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	No State × Time Trends	With State × Time Trends	And With MSA × Time Trends	Specification 2 but × Income Group	Specification 3 but × Income Group	Specification 3 but with State × HH FEs	Specification 5 but with State × HH FEs
MSR	0.101 (0.515)	0.0531 (0.452)	0.100 (0.452)			-0.00603 (0.457)	
Mortgage subsidy rate × Regulatory index	-0.329*** (0.127)	-0.485*** (0.143)	-0.457*** (0.156)			-0.472*** (0.157)	
Regulatory index	-0.00572 (0.0711)	0.0384 (0.0736)	0.0379 (0.0874)			0.216 (0.147)	
Low income × MSR				-0.106 (0.486)	-0.0281 (0.485)		-0.282 (0.489)
Low income × MSR × Regulatory index				0.149 (0.290)	0.177 (0.288)		0.136 (0.294)
Low income × Regulatory index				-0.114 (0.0942)	-0.118 (0.103)		0.0584 (0.164)
Moderate income × MSR				-0.0720 (0.503)	-0.0424 (0.501)		-0.244 (0.510)
Moderate income × MSR × Regulatory index				-0.544* (0.300)	-0.507* (0.297)		-0.527* (0.303)
Moderate income × Regulatory index				0.0564 (0.0995)	0.0503 (0.106)		0.223 (0.163)
High income × MSR				0.195 (0.468)	0.237 (0.467)		0.192 (0.474)
High income × MSR × Regulatory index				-0.619*** (0.164)	-0.589*** (0.180)		-0.601*** (0.180)
High income × Regulatory index				0.0744 (0.0789)	0.0712 (0.0936)		0.238 (0.153)
Moderate income	0.0577*** (0.0131)	0.0563*** (0.0130)	0.0583*** (0.0128)	0.0515 (0.0868)	0.0659 (0.0874)	0.0569*** (0.0130)	0.0508 (0.0905)
High income	0.139*** (0.0151)	0.138*** (0.0149)	0.139*** (0.0148)	0.0631 (0.0852)	0.0738 (0.0861)	0.136*** (0.0152)	0.0171 (0.0902)
Total net wealth	0.00352* (0.00197)	0.00385* (0.00202)	0.00371* (0.00194)	0.00393* (0.00205)	0.00379* (0.00197)	0.00324 (0.00220)	0.00333 (0.00222)
Demographics/employment	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing composition controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FEs and constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Time Trends	No	Yes	Yes	Yes	Yes	Yes	Yes
MSA × Time Trends	No	No	Yes	No	Yes	Yes	Yes
State × Household FEs	No	No	No	No	No	Yes	Yes
Observations	29,621	29,621	29,621	29,621	29,621	29,621	29,621
Number of households	2,620	2,620	2,620	2,620	2,620	2,620	2,620
Centered R ²	0.248	0.248	0.245	0.249	0.246	0.228	0.229
Uncentered R ²	0.248	0.248	0.245	0.249	0.246	0.228	0.229

Cluster-robust standard errors in parentheses (statistics are robust to heteroskedasticity and clustering on households and state×year). ***p < 0.01, **p < 0.05, *p < 0.1.

to gauge to what extent our results may be driven by across-state movers. The coefficients of the two specifications with Household × State fixed effects are qualitatively unchanged and quantitatively very similar to the corresponding specifications without the Household × State fixed effects, reported in the corresponding columns 3 and 5. These findings imply that our key findings are not driven by across-state movers who may not be similar over time and across states. Regarding all the other results from table 5, the household and location controls continue to be intuitive, plausible, and robust across samples and specifications (the coefficient estimates are available from the authors on request). Finally, as a robustness check, we reestimate the specifications in table 5 controlling for additional compo-

nents of user cost: the NBER combined state and federal property tax subsidy rate, the FHFA effective mortgage rate, and the FHFA metropolitan house price appreciation rate as well as the price of rental housing. These estimations are reported and discussed in the online appendix and indicate that the additional controls have a negligible impact on our key findings.

D. Quantitative Effects

One way to gauge the cost of the MID is to compute the cost per net new homeowner created by the MID. To do so, we first determine the net number of households that are hypothetically moved into homeownership as a result of the

TABLE 6.—QUANTITATIVE EFFECTS

Specification	Rent → Own	No Change	Own → Rent	Net Impact
A. Implied Overall Impact of MID on Homeownership Attainment (in Percentage Points) Using 0.5 Threshold				
Table 4 (1)	0.0	97.3	2.7	-2.7
Table 4 (2)	0.0	98.9	1.1	-1.1
Table 4 (3)	0.0	94.9	5.1	-5.1
Table 4 (4)	0.0	97.9	2.1	-2.1
Table 4 (5)	0.0	98.9	1.1	-1.1
Table 4 (6)	0.3	96.2	3.5	-3.2
Table 5 (1)	5.0	92.2	2.8	+2.2
Table 5 (2)	6.0	89.2	4.8	+1.2
Table 5 (3)	6.4	89.6	4.0	+2.4
Table 5 (4)	5.9	89.9	4.2	+1.7
Table 5 (5)	6.6	90.0	3.4	+3.2
Table 5 (6)	2.6	94.6	2.8	-0.2
Table 5 (7)	3.4	93.9	2.7	+0.7
B. Implied Average Change in Propensity to Own Due to Introduction of Mortgage Interest Deduction of 26% (= Sample Average)				
Specification	Income Level	Highly regulated (<i>average regulatory index of MSAs with index at least 1 SD above mean</i>) (average index: +1.59)	Little regulated (<i>average regulatory index of MSAs with index at least 1 SD below mean</i>) (average index: -1.40)	
Table 5 (4)	Low	<i>Not Statistically Significant</i>	<i>Not Statistically Significant</i>	
	Moderate	-24.3%	+17.9%	
	High	-20.5%	+27.6%	
Table 5 (5)	Low	<i>Not Statistically Significant</i>	<i>Not Statistically Significant</i>	
	Moderate	-22.0%	+17.4%	
	High	-18.1%	+27.6%	
Table 5 (7)	Low	<i>Not Statistically Significant</i>	<i>Not Statistically Significant</i>	
	Moderate	-28.1%	+12.9%	
	High	-19.8%	+26.9%	

Quantitative effects in italics reported in panel A are based on statistically insignificant coefficients.

mortgage interest subsidy. Using the specifications in tables 4 and 5, we compute the probability of homeownership for each household with and without the mortgage subsidy. If in a given year the subsidy moves a household from a less than 50% likelihood of homeownership to a likelihood that exceeds 50%, the household is counted as moving from renting to owning. If the household's likelihood of homeownership decreases from above 50% to less than 50% as a result of the subsidy, this household is counted as moving from owning to renting. If the household does not experience a change in the likelihood of homeownership that crosses the 50% threshold, the household is counted as not having experienced a change in its tenure status.

We then compute the fraction of the sample that falls into each category: moving from renting to owning, moving from owning to renting, or having no change in tenure status. The net impact is computed as the percent of the sample moved into homeownership minus the percent of the sample moved out of homeownership, as defined above, as a result of the MID. Table 6A reports these results by specification. Notice that for the United States on average, based on the econometric results in table 4, this exercise suggests a net negative impact of the MID on the likelihood of homeownership (although the effects are all not statistically significant), whereas all but one specification reported in table 5 imply a relatively small positive (and statistically significant) impact. (The specification in column 6 of table 5 implies a very small but statistically significant negative

net effect.) Our core specification reported in column 5 of table 5, which allows the impact of the MID to vary by regulatory restrictiveness and by income status, results in a net positive gain in the number of homeowners by 3.2%, and this is the estimate we proceed with to compute the subsidy cost per net additional homeowner.

There are an estimated 115 million households in the United States in 2010 (the most recent Census Bureau estimate available).²² Hence, specification 5 in table 5 implies that the subsidy in any given year generates 3.68 million new homeowners in the United States (3.2% times 115 million). At an estimated total cost of \$104.5 billion in 2011 (Office of Management and Budget, 2010), the subsidy per converted homeowner thus amounts to a staggering \$28,397 per year.²³ The (nonsignificant) coefficients on the MID variable reported in the various specifications in table 4, if taken at face value, all imply that the taxpayer may spend \$104.5 billion in 2011, with the overall net effect being that fewer households own, as a consequence of the MID.

²² See www.census.gov/population/projections/nation/hh-fam/table1n.txt.

²³ The costs are substantially higher according to the results reported in column 7 of table 5, which allows the impact of the MID to vary by regulatory restrictiveness and income status, net of the influence of across-state movers. This specification implies a net positive gain in the number of homeowners of 0.7%, suggesting that to move one renter household into homeownership through the MID costs U.S. taxpayers \$129,814 in forgone tax revenue annually.

Table 6B documents the implied average change in the propensity to own for low-, moderate-, and high-income households as a consequence of the implementation of an MID of 26%, the sample average. Results are reported separately for tightly and loosely regulated places (corresponding to the categorization in table A2). Whereas in these polar cases, the effects of the MID on the propensity to own are never statistically significant for low-income households, the effects for moderate- and high-income households are not only statistically significant but also quantitatively meaningful. In the most tightly regulated places, the introduction of the MID reduces the propensity to own, depending on the specification and income category (moderate or high), by between 18% and 28%. In the least regulated places the propensity to own increases by between 13% and 28%.

E. Additional Robustness Checks

In this section, we briefly describe additional robustness checks undertaken and refer interested readers to the online appendix for more detail. First, we consider whether the results of our key specification are sensitive to the inclusion of 1980 tract-level housing composition controls. We reestimate our core specification, model 5 of table 5, without these controls. Next, we examine our three-way interaction effects by income groups more closely by reestimating our core specification using four or even five income categories instead of only three, both with and without housing composition controls. Summary statistics for the additional income categories are reported at the bottom of table 2. These robustness checks yield results that are consistent with our key findings. Interestingly, the negative three-way interaction effect between the MSR and regulatory restrictiveness, as reported in the online appendix, is consistently strongest for the second-highest income group.

Finally, we compare the findings of our core analysis with the results from an aggregate MSA-level analysis. Our household level (core) analysis allows us to carefully identify the effect of the MSR on individual tenure decisions, depending on income status and supply conditions and controlling for numerous time-invariant and time-varying characteristics. We two-way-cluster on households and State \times Year in the household-level analysis, accounting for household correlations. An MSA-level analysis allows for an alternative manner to account for household (within MSA) correlations and provides a useful robustness check. Moreover, our key variable of interest, the MID, varies at the MSA level; thus, it is sensible to consider a specification that aggregates the other variables to the MSA level. We therefore use our household data to aggregate up, for each PSID year, to the MSA level, dropping MSAs that cross state borders. Summary statistics of the resulting MSA-level panel and model estimates are reported in the online appendix. Overall, the results are comparable to those of our core analysis: the MID has a positive effect on the pro-

portion of homeowners in elastically supplied markets and a negative effect in inelastically supplied markets.

VI. Conclusion

This paper provides a first look at the impact of the combined state and federal mortgage interest tax subsidy on homeownership attainment, taking into account housing supply conditions via a measure of regulatory restrictiveness in local housing markets. We find that the MID has no statistically significant impact on homeownership attainment in aggregate. However, the MID does have an impact on individual homeownership decisions—both positive and negative—depending on the restrictiveness of land use regulations at the place of residence and the income status of the household. In places with more elastic housing supply, the MID has a positive effect on homeownership attainment, but only for higher-income groups. In more restrictive places, the mortgage tax subsidy has a significant adverse impact, again only for higher-income groups. The MID has no impact on the homeownership attainment of low-income households regardless of regulatory status. We speculate that this is because the housing market within a city tends to be segmented by income and the MID provides a tax subsidy only to the relatively higher-income households that itemize. Consequently, we expect that lower-income housing will generally not experience house price changes due to changes in the subsidy.

One argument in favor of the MID is that it increases homeownership attainment and as a result creates positive externalities. Recent research has highlighted that the positive externalities associated with homeownership may help improve local communities confronted with underperforming public schools, lack of social capital, and poor governance (Hoff & Sen, 2005; DiPasquale & Glaeser, 1999; Hilber & Mayer, 2009; Fischel, 2001). However, these positive externalities are likely confined to—typically highly urbanized—places with inelastic housing supply, wherein civic engagement and investments into local public goods are capitalized into local house prices. Our research suggests that the MID decreases rather than increases homeownership attainment in these places. In places with lax land use controls, the MID has a positive impact on homeownership attainment, yet in these elastically supplied—typically less urbanized—places, homeownership may generate few or no positive externalities (Hilber & Mayer, 2009; Hilber, 2010). Thus, a central implication of our paper is that there is a disconnect between the places in which positive externalities of homeownership exist and the places in which the MID is able to generate increases in homeownership. We conclude that the MID is a costly and ineffective policy for boosting homeownership and social welfare.

To fully understand the efficiency (and distributional) impacts of the MID, future work might examine its impact on the “overconsumption” of owner-occupied housing by income and regulatory status. This paper examines only a

portion of the total subsidy to homeowners. We did not examine the effect of other subsidies to homeowners: imputed rent is untaxed, capital gains are untaxed for most households, and property taxes are tax deductible. Another area for future research is to explore the extent to which these other tax subsidies to homeowners also generate unintended consequences, particularly in more inelastically supplied housing markets.

REFERENCES

- Belsky, Eric S., and Mark Duda, "Asset Appreciation, Timing of Purchases and Sales, and Returns to Low-Income Homeownership" (pp. 208–238), in Nicolas P. Retsinas and Eric S. Belsky, eds., *Low-Income Homeownership: Examining the Unexamined Goal* (Cambridge, MA: Joint Center for Housing Studies, 2002).
- Berkovec, James, and Don Fullerton, "A General Equilibrium Model of Housing, Taxes, and Portfolio Choice," *Journal of Political Economy* 100:2 (1992), 390–429.
- Besley, Timothy, and Anne Case, "Incumbent Behavior: Vote-Seeking, Tax-Setting, and Yardstick Competition," *American Economic Review* 85:1 (1995), 25–45.
- Bourassa, Steven C., and Ming Yin, "Tax Deductions, Tax Credits and the Homeownership Rate of Young Urban Adults in the United States," *Urban Studies* 45:5–6 (2008), 1141–1161.
- Capozza, Dennis R., Richard K. Green, and Patric H. Hendershott, "Taxes, Mortgage Borrowing, and Residential Land Prices" (pp. 171–198), in Henry J. Aaron and William G. Gale, eds., *Economic Effects of Fundamental Tax Reform* (Washington, DC: Brookings Institution, 1996).
- Case, Karl E., and Christopher Mayer, "Housing Price Dynamics within a Metropolitan Area," *Regional Science and Urban Economics* 26:3 (1996), 387–407.
- Cutler, David, and Jonathan Gruber, "Does Public Health Insurance Crowd Out Private Insurance?" *Quarterly Journal of Economics* 111:2 (1996), 391–430.
- Dietz, Robert D., and Donald R. Haurin, "The Social and Private Micro-Level Consequences of Homeownership," *Journal of Urban Economics* 54:3 (2003), 401–450.
- DiPasquale, Denise, and Edward L. Glaeser, "Incentives and Social Capital: Are Homeowners Better Citizens?" *Journal of Urban Economics* 45:2 (1999), 354–384.
- Dynarski, Mark, and Steven M. Sheffrin, "Housing Purchases and Transitory Income: A Study with Panel Data," this REVIEW 67:2 (1985), 195–204.
- Feenberg, Daniel, and Elisabeth Coutts, "An Introduction to the TAXSIM Model," *Journal of Policy Analysis and Management* 12:1 (1993), 189–194.
- Feenberg, Daniel, and Harvey S. Rosen, "The Interaction of State and Federal Tax Systems: The Impact of State and Local Tax Deductibility," *American Economic Review* 76:2 (1986), 126–131.
- Feldstein, Martin, and Gilbert E. Metcalf, "The Effect of Federal Tax Deductibility on State and Local Taxes and Spending," *Journal of Political Economy* 95:4 (1987), 710–736.
- Fischel, William A., "Homevoters, Municipal Corporate Governance, and the Benefit View of the Property Tax," *National Tax Journal* 54:1 (2001), 157–173.
- Fisher, Ronald C., *State and Local Public Finance* (Mason, OH: Thompson-Southwestern, 2007).
- Gervais, Martin, and Manish Pandey, "Who Cares about Mortgage Interest Deductibility?" *Canadian Public Policy* 34:1 (2008), 1–23.
- Glaeser, Edward, Joshua Gottlieb, and Joseph Gyourko, "Can Cheap Credit Explain the Housing Boom?" NBER working paper 16230 (2010).
- Glaeser, Edward L., and Jesse M. Shapiro, "The Benefits of the Home Mortgage Interest Deduction" (pp. 37–84), in James M. Poterba, ed., *Tax Policy and the Economy*, Vol. 17 (Cambridge, MA: MIT Press, 2003).
- Green, Richard K., and Kerry D. Vandell, "Giving Households Credit: How Changes in the U.S. Tax Code Could Promote Homeownership," *Regional Science and Urban Economics* 29:4 (1999), 419–444.
- Gyourko, Joseph, Albert Saiz, and Anita Summers, "A New Measure of the Local Regulatory Environment for Housing Markets: The Wharton Residential Land Use Regulatory Index," *Urban Studies* 45:3 (2008), 693–729.
- Haurin, Donald R., and H. Leroy Gill, "The Impact of Transaction Costs and the Expected Length of Stay on Homeownership," *Journal of Urban Economics* 51:3 (2002), 563–584.
- Heeringa, Steven G., and Judith H. Connor, *1997 Panel Study of Income Dynamics Analysis Weights for Sample Families and Individuals: Panel Study of Income Dynamics Documentation* (1999), <http://psidonline.isr.umich.edu/data/Documentation/wts97.pdf>.
- Hendershott, Patric H., and James D. Shilling, "The Economics of Tenure Choice: 1955–79" (pp. 105–133), in C. F. Sirmans, ed., *Research in Real Estate*, Vol. 1 (Greenwich, CT: Jai Press, 1982).
- Hilber, Christian A. L., "Neighborhood Externality Risk and the Homeownership Status of Properties," *Journal of Urban Economics* 57:2 (2005), 213–241.
- "New Housing Supply and the Dilution of Social Capital," *Journal of Urban Economics* 67:3 (2010), 419–437.
- Hilber, Christian A. L., and Christopher Mayer, "Why Do Households without Children Support Local Public Schools? Linking House Price Capitalization to School Spending," *Journal of Urban Economics* 65:1 (2009), 74–90.
- Hilber, Christian A. L., and Wouter Vermeulen, "The Impact of Supply Constraints on House Prices in England," *Economic Journal* (forthcoming).
- Hoff, Karla, and Arijit Sen, "Homeownership, Community Interactions, and Segregation," *American Economic Review* 95:4 (2005), 1167–1189.
- Holtz-Eakin, Douglas, and Harvey S. Rosen, "Tax Deductibility and Municipal Budget Structure" (pp. 107–126), in Harvey S. Rosen, ed., *Fiscal Federalism: Quantitative Studies* (Chicago: University of Chicago Press, 1988).
- Howe, Edward T., and Donald J. Reeb, "The Historical Evolution of State and Local Tax Systems," *Social Science Quarterly* 78:1 (1997), 109–121.
- Hsieh, Chang-Tai, and Enrico Moretti, "Can Free Entry Be Inefficient? Fixed Commissions and Social Waste in the Real Estate Industry," *Journal of Political Economy* 111:5 (2003), 1076–1122.
- Lutz, Byron F., "Fiscal Amenities, School Finance Reform and the Supply Side of the Tiebout Market," Board of Governors of the Federal Reserve mimeograph (2009).
- Mayer, Christopher, "Housing Bubbles: A Survey," *Annual Review of Economics* 3:1 (2011), 559–577.
- Metcalf, Gilbert, "Assessing the Federal Deduction for State and Local Tax Payments," *National Tax Journal* 64:2–2 (2011), 565–590.
- Office of Management and Budget, *FY 2011 President's Budget—Analytical Perspectives* (2010), http://www.whitehouse.gov/omb/budget/Analytical_Perspectives/.
- Poterba, James M., "Tax Subsidies to Owner-Occupied Housing: An Asset Market Approach," *Quarterly Journal of Economics* 99:4 (1984), 729–752.
- "Taxation and Housing: Old Questions, New Answers," *American Economic Review: Papers and Proceedings* 82:2 (1992), 237–242.
- Poterba, James M., and Todd Sinai, "Tax Expenditures for Owner-Occupied Housing: Deductions for Property Taxes and Mortgage Interest and the Exclusion of Imputed Rental Income," *American Economic Review: Papers and Proceedings* 98:2 (2008), 84–89.
- Quigley, John M., and Steven Raphael, "Regulation and the High Cost of Housing in California," *American Economic Review: Papers and Proceedings* 95:2 (2005), 323–328.
- Rosen, Harvey S., "Housing Decisions and the U.S. Income Tax," *Journal of Public Economics* 11:1 (1979), 1–23.
- Rosen, Harvey S., Kenneth T. Rosen, and Douglas Holtz-Eakin, "Housing Tenure, Uncertainty and Taxation," this REVIEW 66:3 (1984), 405–416.

- Saiz, Albert, "The Geographic Determinants of Housing Supply," *Quarterly Journal of Economics* 125:3 (2010), 1253–1296.
- Saks, Raven E., "Job Creation and Housing Construction: Constraints on Metropolitan Area Employment Growth," *Journal of Urban Economics* 64:1 (2008), 178–195.
- Shan, Hui, "The Effect of Capital Gains Taxation on Home Sales: Evidence from the Taxpayer Relief Act of 1997," *Journal of Public Economics* 95:1–2 (2011), 177–188.
- Sinai, Todd, and Joseph Gyourko, "The (Un)changing Geographical Distribution of Housing Tax Benefits: 1980 to 2000" (pp. 175–208), in James M. Poterba, ed., *Tax Policy and the Economy*, Vol. 18 (Cambridge, MA: MIT Press, 2004).
- Turner, Tracy M., "Does Investment Risk Affect the Housing Decisions of Families?" *Economic Inquiry* 41:4 (2003), 675–691.
- Turner, Tracy M., and Marc T. Smith, "Exits from Homeownership: The Effects of Race, Ethnicity and Income," *Journal of Regional Science* 49:1 (2009), 1–32.