The Effect of Tax Reform on the Owner-Occupied Housing Market

by

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Submitted to the Department of Economics
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Abstract:

The U.S. tax code subsidizes investment in owner-occupied housing since the implicit return on the house is untaxed and, for homeowners who itemize, mortgage interest and property tax payments are deductible from taxable income. This thesis uses the tax rate changes in the 1970s and 1980s to identify the effect that changes to the tax subsidy would have on housing demand and house prices. The first chapter estimates demand elasticities for owner-occupied housing, accounting for the fact that due to moving costs and housing capital gains, families do not necessarily adjust their housing consumption quickly in response to changes in the tax subsidy. Using the Panel Study of Income Dynamics, I examine the dynamic impact of changes in the tax treatment of owner-occupied housing. Estimates from a duration model with gamma heterogeneity and a nonparametric baseline hazard indicate that changes in the tax code significantly affect the likelihood that a family chooses to move from renting to owning or to a larger house. Eliminating the capital gains tax on housing will generate small increases in the likelihood that families move to smaller houses or permanently from owning to renting. Correcting for endogenous mobility in a housing demand equation with a semiparametric dynamic sample selection method similar to Newey (1988) suggests that standard demand estimates may be biased upward. Chapter two identifies how a change in the tax code is capitalized into the price of owner-occupied housing by exploiting the fact that marginal tax rates have changed differentially by income over the 1970 to 1991 time period. If a family with a higher income tends to purchase a more expensive house, on average, than a lower-income family, then a house belonging to a family that undergoes a larger change in tax rates should exhibit a greater change in price. By examining changes in self-reported house value conditional on a family's not moving, the real quantity of housing services consumed is held constant and the price response can be separately identified. Using the same PSID data, I find that house prices nearly perfectly capitalize changes in the tax code in the short run.

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Chapter 2: Are Tax Reforms Capitalized into House Prices?

Acknowledgments

The common misperception about Ph.D. theses is that they are products of single individuals, toiling away for years in dark corners of university libraries and in tiny, poorly-ventilated offices. While the latter half of that statement may be true for some, the former rarely is. This thesis was the culmination of several years of effort by many others besides myself, a product of countless influences and inspirations, and only possible with the support of a number of people. I acknowledge many of the contributions and contributors here and apologize in advance for any errors of omission.

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Introduction

In the United States, the tax code subsidizes investments in owner-occupied housing. Unlike other assets, the implicit return on the house — the amount the homeowner would have to pay to rent the exact same house — is untaxed. Also, homeowners who itemize can deduct mortgage interest and property taxes paid from their federal taxable income. Additionally, housing capital gains are virtually untaxed due to generous roll-over and exclusion provisions.

These tax provisions provide a considerable subsidy to owner-occupied housing. For the average homeowning family, assuming tax return itemization, the combined subsidy reduces the annual flow cost of owning a home by 24.5 percent. Eliminating the mortgage interest and property tax deductions alone would raise the annual cost of homeownership by 10 percent and save the government $56.2 billion per year if families did not change their homeowning behavior in response.

During the 1970s and 1980s, several macroeconomic and legislative changes affected the value of the subsidy to owner-occupied housing. High inflation during the 1970s combined with marginal tax rate brackets that were not indexed for inflation led to marginal tax rates that ratcheted upwards for most families as nominal incomes rose. Legislative changes during the 1980s reduced marginal tax rates, especially for high income taxpayers, and raised standard deductions, thereby reducing the number of families that itemized on their tax returns.

This thesis uses these exogenous changes in the tax treatment of owner-occupied housing to identify the effect that changing this tax subsidy would have on two aspects of the market for owner-occupied houses: the consumption of real housing services and the asset price for houses. For example, a reduction in the tax subsidy to owner-occupied housing would increase the annual cost of maintaining a home. Families would be expected to reduce their demand for housing services as a result, with most of the effect seen as a decline in the consumption of real housing services or a fall in the price of houses. These two channels are intertwined, with a high elasticity of consumption corresponding to a low rate of capitalization into house prices and vice versa.

Although previous research has devoted considerable attention to the issue of the steady-state demand elasticity, it is lacking in two areas. The first is its focus on the equilibrium demand for housing given a particular tax system, as opposed to estimating how housing demand responds over time to changes in the tax code. In many areas where taxes are thought to have a large impact, such as gifts to charities and realizations of capital gains, the steady-state elasticity may be achieved fairly quickly and the time profile of demand may be steady. However, in the housing market, substantial adjustments in housing consumption can occur only through moving. Since changing houses is costly and families move infrequently, the response of actual housing consumption to a tax change may lag behind the response of desired consumption.

This discrepancy has several implications for tax reform and the housing market. First, because many tax policies may not be long-lived enough for a steady-state to be reached in the housing market, the transition to the steady state may be more relevant for evaluating policy than the equilibrium outcome. Second, the revenue impact of a policy change may be miscalculated if steady-state parameters are used. Moreover, infrequent changes in housing consumption can affect how the housing market responds to a change in tax policy. In a q-theory asset model with perfect foresight, elastic demand would generate a large initial house price response. However, this response may be mitigated by slow consumption adjustment.

Finally, typical steady-state estimates of housing demand are confounded by lumpy consumption behavior. While many housing demand analyses recognize that estimation based only on the observed housing consumption of homeowners may lead to sample selection bias, simply accounting for whether a family is in the ownership state assumes that families independently decide at every moment whether to be owners or renters and how much housing to consume. However, these decisions may depend on the length of time the family has lived in their house and might occur dynamically. Therefore, sample selection arises from families having decided in the past to move into a house and currently choosing not to move out. Accounting for this dynamic sample selection would correct for bias induced by endogenous mobility.

The second area where previous housing research has been unable to generate convincing results is in determining how a change in the tax-treatment of owner-occupied housing is capitalized into house prices. Systemic movements in the price of owner-occupied housing can have substantial balance sheet and economic efficiency effects. The median family of retirement age has 40 percent of its net worth tied up in its home and younger families hold an even larger proportion of their wealth in their houses. An unexpected house price decline could significantly reduce a highly-leveraged young homeowner's wealth and also decrease her mobility if her house price falls enough that she cannot afford to pay off her mortgage in order to move. For older families, the decrease in wealth accompanying a house price decline comes at a time when they cannot increase labor earnings or savings to offset it. Changes in house prices also affect whether families become first-time homebuyers and may influence the decision to save for a down payment.

The distortions created by the tax code with respect to real quantity consumed and house price each contribute to efficiency loss. The user cost elasticity of housing demand is the key parameter for estimating the aggregate dead-weight loss arising from distorted behavior induced by the tax subsidy to owner-occupied housing. The rate of capitalization of user costs into house prices also determines the magnitude of an efficiency loss. Since changes in house prices are transfers of wealth from current homeowners to or from future ones, an aggregate price shift may have a potentially significant effect on dead-weight loss because the government uses distortionary taxes to raise revenue and the return on housing is untaxed. An increase in tax rates that raised house prices would increase dead-weight loss since current homeowners would receive a windfall capital gain that is virtually untaxed in the current system while future homeowners would have to save more in houses since they are more expensive. Since the return on houses is untaxed, the government's tax base dwindles in the future, lowering efficiency.

Chapter one of this thesis explores the effect of tax reforms on housing demand, mobility, and the dynamics of housing consumption, explicitly taking moving costs into account. Home ownership is modeled as an investment in a durable consumption good with transactions costs to moving and uncertainty about the time path of the factors that underlie housing demand. The model implies that changes in taxes, income, and other characteristics since the last move can be used to proxy for the difference between the family's latent desired housing consumption and its actual consumption. The model also predicts that the tax treatment of housing capital gains can be thought of as a transaction cost to moving for families with appreciated house values, thereby influencing the willingness of a family to move. Using a 22-year panel from the Panel Study of Income Dynamics, I begin by estimating how changes in the tax code affect the likelihood that families move. A duration model with a nonparametric baseline hazard is used to estimate the impact of income, taxes, and other demographic factors on homeowner and renter mobility. Each possible transition, such as renters becoming owners or owners purchasing larger houses, is estimated separately. I then use the predicted hazards of moving from the duration model to implement a dynamic sample selection correction in a housing demand model. The resulting estimates of price and income elasticities are used to simulate the effect of various tax reforms on aggregate housing consumption over time.

I find that changes in after-tax income and the user cost of housing significantly affect moving behavior, especially for renters transiting to homeownership. Higher capital gains taxes reduce a family's likelihood of purchasing a smaller house or moving permanently from ownership to renting. The decision to move is shown to be duration dependent and controlling for parametric individual heterogeneity increases the magnitude of the estimated coefficients. Results from the demand estimation suggest that the user cost elasticity of housing demand is −0.43 while the after-tax income elasticity of demand is 0.66. The dynamic sample selection corrections are statistically significant but do not make a substantive difference in the estimated elasticities. Also, families with higher capital gains tax liabilities are shown to purchase larger quantities of housing. This finding is consistent with the idea that if families with capital gains trade down, they do not trade down as much as they would have absent the gains. Simulating the elimination of the mortgage interest and property tax deductions reveals that, absent a price or interest rate response, average housing consumption would fall by 3.5 percent over twenty years, with half the decline coming in the first five years.

Chapter two determines the effect of tax reform on house prices by exploiting the fact that marginal tax rates vary by *family* at any point in time due to differences in income and demographic characteristics and, over the 1970 to 1991 time period, have changed differentially by income. If a high-income family tends to purchase in a different housing market on average than a lower-income family, then houses in markets where families undergoes larger changes in tax rates should exhibit greater changes in price. By examining changes in self-reported house value for families conditional on them not moving, the quantity of house consumed is held constant and the price response can be separately identified.

Again using family-level longitudinal data drawn from the Panel Study of Income Dynamics covering the period from 1970 to 1991, I find short-run capitalization rates of user cost into self-reported house value of as much as -0.198 (0.028). This estimate controls for the differential effect of the widening income distribution on house prices and accounts for the fact that endogenous moves out of houses creates sample-selection bias in family-level data. Scaled so the average changes in self-reported house value match changes in the state house price index, the estimated capitalization rate rises to -1.383 (0.196).

The two chapters in this thesis provide another piece of evidence about the effect of tax reforms on the owner-occupied housing markets and establish a framework for thinking about behavioral responses to tax changes when it is costly to adjust. Hopefully, the estimates of moving behavior and the dynamics of the response of housing demand to changes in the tax code, the careful accounting for possible sample selection bias, and the exogenous identification of house price capitalization rates will aid in understanding this complicated market. In addition, the two chapters in this thesis can be used together to help predict the behavior of the owner-occupied housing market in the aftermath of a tax reform. The effect of a tax change on the asset price of houses, families' decisions to move, and real housing consumption can be distinguished. With the estimates of the dynamic demand response, the short-term effects of policies can then be properly evaluated.

Chapter 1

Taxation, Mobility, and the
Demand for Owner-Occupied Housing

The lack of taxation of imputed rental income and the deductibility of mortgage interest and property taxes for homeowners that itemize generate a subsidy to investment in owner-occupied housing. The current revenue cost of the tax expenditure on the deductions alone is estimated to be $56.2 billion, using the NBER TaxSim model. Moreover, for homeowners that claim them, the deductions represent a significant subsidy to housing consumption. On average, the tax code generates nearly a 10 percent reduction in the annual cost of owner-occupied housing.[1]

An accurate measure of the elasticity of housing demand with respect to the user cost of housing is crucial for determining the impact of the current tax subsidy on the housing market and house prices. The price elasticity of housing demand is also a key parameter for evaluating potential tax reforms and estimating the aggregate efficiency loss from the tax subsidy to owner-occupied housing. Using standard elasticity estimates, Poterba (1992) concludes that the annual dead weight loss from the tax treatment of home ownership in 1990 ranged from $50 to $1,600 per household. However, assuming a user cost elasticity of demand of −0.5 rather than −1.0 would lower the estimated dead weight loss by two-thirds.

Although determining the effect of the tax code on housing demand is important, one can observe only housing *consumption*. Actual consumption of housing services does not necessarily equal desired consumption, although during the more than two decades of empirical research on this topic, most authors have assumed the two quantities to be the same. Although families may wish to consume a different quantity of housing services than they are currently, substantial

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[1]Revenue figures were calculated by the author using the National Bureau of Economic Research's TaxSim tax calculator for 1994 and the 1991 IRS Statistics of Income Public Use Microsample, extrapolated to 1996. The percent of user cost figure is computed using the 1990 wave of the Panel Survey of Income Dynamics.

adjustments in housing consumption can occur only through moving.[2] Since changing houses is costly and families move infrequently, the response of actual housing consumption to a tax change may lag behind the response of desired consumption.

This discrepancy has several implications for tax reform and the housing market. First, because many tax policies may not be long-lived enough for a steady-state to be reached in the housing market, the transition to the steady state may be more relevant for evaluating policy than the equilibrium outcome. Second, the revenue impact of a policy change may be miscalculated if steady-state parameters are used. Moreover, infrequent changes in housing consumption can affect how the housing market responds to a change in tax policy. In a q-theory asset model with perfect foresight such as the one in Poterba (1984), elastic demand would generate a large initial house price response. However, this response may be mitigated by slow consumption adjustment. Finally, only a few authors have estimated dynamic demand models and none compute the transition paths of housing consumption in response to a tax change.[3]

Finally, typical steady-state estimates of housing demand are confounded by lumpy consumption behavior. While many housing demand analyses recognize that observing housing consumption only for homeowners may lead to sample selection bias, simply accounting for whether a family is in the ownership state assumes that families independently decide at every moment whether to be owners or renters and how much housing to consume. However, these

[2]Renovations and additions are other potentially tax-advantaged ways to adjust housing consumption upwards.

[3] Rosenthal (1988) uses a cross-section of households and a semi-Markov model of tenure choice to find the steady-state ownership rate, and Rosen and Rosen (1980) incorporate a three-year distributed lag in the relative cost of homeowning to renting in their time-series analysis of the homeownership rate. Henderson and Ioannides (1989) estimate a lognormal duration model to compute expected length of stay and use the predictions to generate steady-state estimates in a housing demand model.

decisions may depend on the length of time the family has lived in their house and might occur dynamically. Therefore, sample selection arises from families having decided in the past to move into a house and currently choosing not to move out. Accounting for this dynamic sample selection would correct for bias induced by endogenous mobility.

This paper explores the effect of tax reforms on housing demand, mobility, and the dynamics of housing consumption, explicitly taking moving costs into account. Home ownership is modeled as an investment in a durable consumption good with transactions costs to moving and uncertainty about the time path of the factors that underlie housing demand, as in Caballero and Engel's (1993) generalized (S-s) model. The model implies that changes in taxes, income, and other characteristics since the last move can be used to proxy for the difference between the family's latent desired housing consumption and its actual consumption. Using a 22-year panel from the Panel Study of Income Dynamics, I begin by estimating how changes in the tax code affect the likelihood that families move. The tax treatment of capital gains on housing may also affect the cost of moving for families with appreciated house values, influencing the willingness of a family to move. A duration model with a nonparametric baseline hazard is used to estimate the impact of income, taxes, and other demographic factors on homeowner and renter mobility. Each possible transition, such as renters becoming owners or owners purchasing larger houses, is estimated separately. I then use the predicted hazards of moving from the duration model to implement a dynamic sample selection correction in a housing demand model. The resulting estimates of price and income elasticities are used to simulate the effect of various tax reforms on aggregate housing consumption over time.

This paper is divided into seven sections. The theory and the econometric procedure for the mobility model are detailed in section 1. In section 2, I describe the data and the identifying variation. Since differences in tax rates are more likely to be an exogenous source of variation than the geographic house price variation used in many microdata studies, I identify the estimates of price and income elasticities of mobility and housing demand using "clean" variation created by the bracket creep of the 1970s and the major tax code revisions in the 1970s and 1980s. The mobility results are reported in section 3. I find that changes in after-tax income and the user cost of housing significantly affect moving behavior, especially for renters transiting to homeownership. Higher capital gains taxes statistically significantly reduce a family's likelihood of purchasing a smaller house or moving permanently from ownership to renting. The decision to move is shown to be duration dependent and controlling for parametric individual heterogeneity increases the magnitude of the estimated coefficients.

In section four, I estimate user cost and after-tax income elasticities of housing demand. Since families endogenously choose to move, I correct for possible sample selection bias using a semiparametric method based on Newey (1991). I incorporate dynamic behavior and possible duration dependence into the sample selection correction, using predicted hazard rates from the mobility estimation.

Results from the demand estimation are presented in section 5. The user cost elasticity of housing demand is estimated to be −0.43 while the after-tax income elasticity of demand is estimated to be 0.66. The dynamic sample selection corrections are statistically significant but do not make a substantive difference in the estimated elasticities. Also, families with higher capital gains tax liabilities are shown to purchase larger quantities of housing. This finding is consistent with the idea that if families with capital gains trade down, they do not trade down as much as they would have absent the gains.

Section 6 uses the results from the mobility and demand equations to simulate the effect of various tax reforms on housing consumption. Since the tax code can affect both housing demand and the likelihood of moving, both sets of estimates are combined to evaluate the total elasticity of housing consumption. These simulations detail the dynamic transition path in the homeownership rate, quantity of housing consumed, and mobility rates. Eliminating the mortgage interest and property tax deductions, absent a price or interest rate response, lowers average housing consumption by 3.5 percent over twenty years, with half the decline coming in the first five years. The homeownership rate declines by almost one percentage point. Overall mobility declines -- the rate of owners trading up falls by six percent alone -- reducing the volume in the housing market. Section 7 briefly concludes.

1. Modeling Mobility Decisions

Since there is significant cost to switching houses, families move infrequently. In fact, homeowners wait 14 years between moves on average while renters spend four years in a residence [Henderson and Ioannides (1989), Gronberg and Reed (1992)]. With moving costs, a family will not adjust until its current housing consumption is different enough from its desired consumption to make the cost of switching houses worthwhile. While changes in factors such as age, number of children, and marital status may generate a series of moves over a family's life-cycle, shocks to the annual cost of owner-occupied housing or to income may speed or slow a family's moving pattern. Capital gains taxes may raise the cost and thus lower the probability of certain types of moves.

I model optimal moving as a standard durables problem: a house is a durable good and housing consumption is adjusted by changing residences. Under uncertainty about the evolution of desired housing consumption, this class of model generates the result that each family will set upper and lower bounds for the gap between desired and actual housing consumption.[4] A family will purchase a larger house or switch from renting to owning if it crosses its upper bound and vice versa if it crosses its lower bound. Marginal tax rate shocks may change the size of a particular family's consumption gap and may generate a house price response as well.

1.1 Housing demand with moving costs and uncertainty

A simple dynamic model will help motivate the empirical work that follows. A utility-maximizing household has latent desired housing demand Q^* and actual housing consumption Q. If families derive utility from consuming Q, Caballero and Engel (1993) show that optimizing households can be thought of as minimizing the difference between the derived demand Q^* and Q.[5] With moving costs, M, a household will adjust its housing consumption if the value to doing so, taking into account the impact of moving now on expected future moves, is at least as large as the cost.[6] In Bellman equation form, a family chooses housing consumption for the current

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[4]Henderson and Ioannides (1989) heuristically describe this type of model under certainty to justify accounting for a mover/stayer sampling problem and for including estimated length of stay in a demand equation. Edin and Englund (1991) show that a (S-s) model of housing demand is consistent with the fact that the sum of squared residuals from estimating a cross section demand equation is declining with duration.

[5]Hanushek and Quigley (1979) apply an aggregate stock adjustment process to the individual level and also generate that whether a household moves should be a function of the difference between the latent demand for housing and actual housing consumption. Artle and Varaiya (1978) and Engelhardt (1994) formulate life-cycle models of the transition from renting to owning that focus on the accumulation of wealth for a down payment.

[6]Moving costs include realtors fees, typically about 5 percent of the value of the house, paying the movers, the cost of searching for a new residence, the opportunity cost of temporarily owning two houses if the old house is not sold before the new one is purchased, the opportunity cost of packing, and psychic costs. If the family has

residence spell, Q, and length of stay, T, to minimize the expected present discounted "loss" of utility, $v(Q,Q^*_t)$, from Q not being equal to Q^* in each period and from the cost of moving at duration T. The family knows that once it moves the minimization problem starts again given the updated housing consumption, Q_+, so it incorporates the expected present discounted effect of moving at duration T on future "loss" with a $v(Q_+,Q^*_T)$ term, where Q_+ is the quantity of housing consumption the family would choose in its next residence spell. The result, for an infinitely-lived family, is a recursive sequence of loss minimizations over residence spells with each one taking into account the impact on future minimizations:

$$v(Q,Q_t^*) = \min_{Q,T} E_0\left[\int_0^T aQ\, g\left(\ln(Q_t^*/Q)\right)e^{-\rho t}dt + e^{-\rho T}M + e^{-\rho T}v(Q_+,Q_t^*)\right] \tag{1}$$

where g(•) is a convex loss function and ρ is the discount rate. Grossman and Laroque (1990) prove that the solution to this type of problem is an (S-s) rule for each household, with upper, lower, and target levels of $\ln(Q^*_t/Q)$.[7] At the time of a move, the family sets consumption equal to the target level -- a fixed proportion, C, of its latent housing demand that incorporates potential moving costs and expected trends in housing demand. If latent demand grows enough that it is above the upper level, U percent greater than Q, the family will trade up. If latent demand

appreciated house value, they may have to pay a capital gains tax if they do not move to a larger house within two years. Venti and Wise (1984) estimate that an average renter household would have to receive a 14 percent increase in utility to induce it to move.

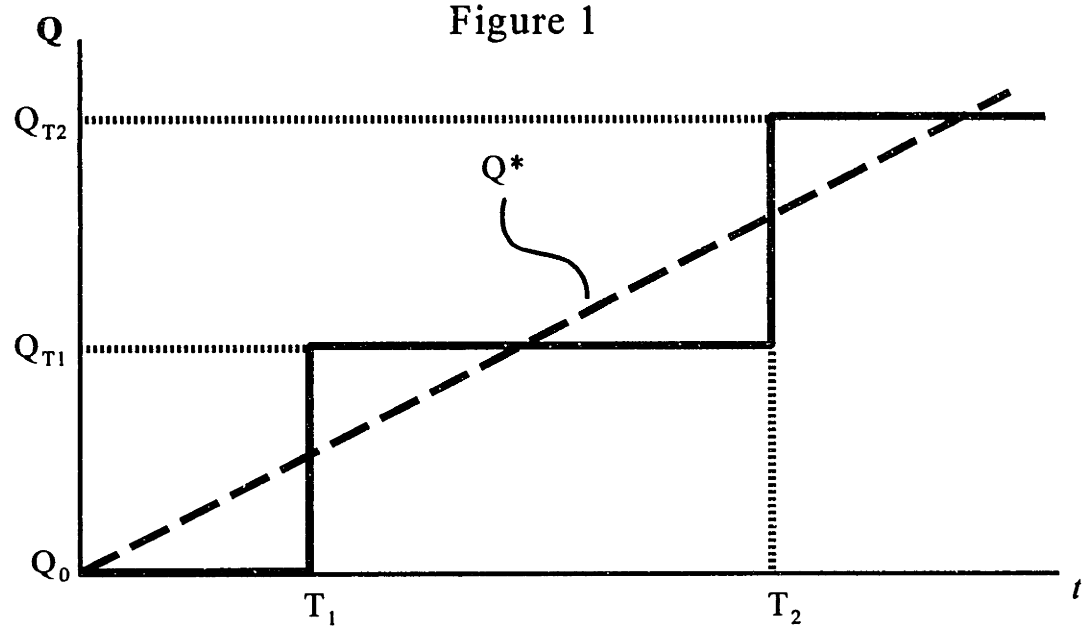
[7]This result requires some structure on the dynamic behavior of Q^*_t. Noting that the Bellman equation can be rewritten solely in terms of one state variable, $z=\ln(Q^*/Q)$, if dz follows a Brownian motion with drift ρ and variance σ, Grossman and Laroque (1990) prove that the family will follow an L-C-U adjustment policy.

contracts until it is below the lower level, where the ratio of it and housing consumption is less than L, the family will trade down. Greater uncertainty about the time path of desired housing demand will make a family less willing to move since the cost of forgoing the option to move is higher.[8]

Figure 1 illustrates a family's housing consumption over time in a world with no down payment constraints, no borrowing or saving, and a zero discount rate. The family starts with a latent housing demand, Q*, of zero and no house. Over time, this family's latent housing demand grows linearly for autonomous reasons, but actual housing consumption remains at zero until time T_1. At that point, it is worthwhile for the family to pay M and move to a house of size Q_{T1}. The new house is larger than the latent demand would require, but the family conserves on the number of moves by overconsuming early in the residence spell and underconsuming later in the spell.

[8]I will not address the issue of uncertainty in my empirical work. Haurin (1991) finds that income variability reduces the quantity of housing demanded.

Figure 1



This example demonstrates an implication of this model for mobility: an exogenous change in the cost of housing services many not have any immediate effect on a family's consumption behavior. In this example, if the Q^* line shifts slightly upwards just after T_1, the family is not likely to immediately change its housing consumption, although it will move sooner than it otherwise would have. However, if the shift occurs near T_2, the family may move immediately in response. For an infinitely-lived family, a shift in Q^* will lead to a transitory change in mobility rates. Changes in the tax treatment of capital gains, which affects the cost of moving, can have a permanent effect on mobility.

Aggregate vs. individual shocks to the annual cost of housing

One important issue is the response of house prices to an aggregate shift in the cost of housing services. If the annual cost of housing changes for only one family, the asset price of housing will not change and the family will evaluate whether to move as described earlier. However, if the cost of holding housing falls proportionally for everyone, house prices may rise in response to families' increased demand for housing. If house prices perfectly capitalize the change in cost, families may not move in response.

This scenario is unlikely for a number of reasons. First, a single family facing an idiosyncratic shock to user cost would not affect the asset price of housing. Second, any non-proportional change in user cost would induce moving since families with the biggest tax increases would want larger houses and vice versa, so families must move to switch. User costs do not usually change proportionally for all families — in fact, every major tax reform in the 1980s has modified tax rates differentially by income level.

Finally, even a proportional change in user cost would probably not lead to exact price capitalization. One potential sufficient condition for full price capitalization is a perfectly inelastic supply of land and structures. Capozza, Green, and Hendershott (1996) point out that in urban areas the supply of land is quite inelastic and provide some evidence that, in the cross-section, low user-cost cities have higher land prices consistent with full capitalization. This paper implicitly considers the supply of structures, which could be very elastic.[9] Also, the supply of land on the urban fringe may be elastic as well. However, even with perfectly inelastic supply, moving involves waste due to transactions costs, which affects the price. Finally, the price response depends on how families form their expectations of capital gains; if they do not have perfect foresight, the price response may be indeterminate.

[9]Poterba (1984) considers construction to be inelastic in the very short run and shows that the immediate asset price response "overshoots" the steady-state asset price level. As long as construction is not inelastic forever, there is room for a dynamic consumption response.

Lastly, the addition of moving costs may complicate the standard view of price dynamics in the housing market. Even if builders could construct enough new houses overnight to meet increased latent demand after an aggregate shock to the cost of housing, many families would not be ready to move into them immediately. If house price (or market rent) is the signal to builders to increase or decrease the rate of housing growth, the perfect-foresight price transition path must take immobility into account and the initial price response would be less than in a model with perfect mobility, such as Poterba (1984). In a simple aggregate stock-flow model of housing [DiPasquale and Wheaton (1996)], the change in housing supply, H, is a function of house price (which is itself a function of market rent), $\dot{H} = \psi(P)$, and the expression for the aggregate change in consumption over time can be written as $\dot{Q} = \kappa(Q^* - Q)$. The function $\kappa(\bullet)$ is a "technology" that slows consumption adjustment.

The mobility results from this paper provide insight into the microfoundations of the consumption adjustment function $\kappa(\bullet)$. The function can be highly nonlinear: since adjustment depends on families' crossing their upper or lower bounds, the cross sectional distribution of families in $\ln(Q^*/Q)$ and any expected trends determine the response to an aggregate shock.

1.2 The tax code and the relative cost of owner-occupied housing

Changes in the tax code can generate potentially significant shocks to the demand for owner-occupied housing. The impact of the marginal tax rate faced by the family on the carrying cost of owner-occupied housing is incorporated in a user cost concept, which can be thought of as the annual flow cost of owner occupation per housing service unit. In a perfectly competitive housing market, the user cost would be the rent a landlord would charge per unit of house. Since the derivation of user cost is presented by Hendershott and Slemrod (1983), Poterba (1984) and others, only the result is stated here:

$$UC=[(1-\tau_{HMI})(\alpha i+\tau_p)+(1-\tau_{int})(1-\alpha)r+\delta+m+\beta-\pi^e]P_H \qquad \textbf{(2)}$$

Dividing both sides by P_H gives the user cost per unit of house. The mortgage interest rate is denoted by i, τ_p is the property tax rate, r is the interest rate equity would earn if it weren't invested in the house, δ is the depreciation rate, m is the maintenance cost per unit of housing services, β is the risk premium required for investing in a house, and π^e is the expected rate of inflation, or expected capital gain. The proportion of the unit of housing services financed by debt is denoted by α. The remaining tax parameters, τ_{HMI} and τ_{int}, correspond to the marginal subsidy rate on home mortgage interest and property tax deductions and the marginal tax rate on interest income, respectively.[10]

The user cost breaks down into four components. The mortgage interest cost, the $(1-\tau_{HMI})\alpha i$ term, reflects the fact that the homeowner pays interest only on the part of the housing unit financed with debt and that, if the homeowner itemizes, the interest is tax-deductible. The property tax cost, $(1-\tau_{HMI})\tau_p$, recognizes that property tax payments are deductible if the homeowner itemizes. If the homeowner does not itemize, τ_{HMI} is zero. The foregone equity cost, $(1-\tau_{int})(1-\alpha)r$, accounts for the opportunity cost of using equity to finance the purchase of the unit of housing rather than debt. The portion of the unit financed with equity could have been invested elsewhere to earn interest at the rate r. That interest would be taxed at the rate τ_{int},

[10]Although the literature has assumed these two rates to be the same, they can be different. Beginning in 1991, deductions were phased out for high-income taxpayers, driving a small wedge between the two rates.

however. The remaining components account for costs and rates of capital gain that I assume do not vary between families. The user cost formula indicates that increases in marginal tax rates lower the marginal user cost of owner-occupied housing. Increases in interest rates raise the user cost while higher inflation lowers it.

The cost of an excess standard deduction

One of the benefits to homeownership is the mortgage interest and property tax deductions. However, only deductions above the amount of the standard deduction are worth anything to the homeowner. Even if the marginal dollar of housing expenditure generates useable deductions, some of the total housing expenditure may generate deductions that must be used just to reach the level of the standard deduction. Families that have larger gaps between their exogenous non-housing deductions and their standard deductions face a higher average cost of homeowning since they get no marginal tax benefit from a greater proportion of their housing deductions. As demonstrated by Hendershott and Slemrod (1983), while the marginal cost may help families determine how much housing to consume conditional on consuming housing, families with a higher average cost of homeownership may be more likely to want to rent than to own. I follow Hendershott and Slemrod (1983) in labeling the gap the "excess standard deduction."[11]

For example, if the standard deduction were $5,000 and a homeowner had $2,000 in non-housing deductions, no housing deductions, and a tax rate of 0.5, he would take the standard

[11]Follain, Hendershott, and Ling (1992) and Follain and Ling (1991) call this concept the "wasted deduction."

deduction for a decrease in his tax burden of $2,500. If he had an additional $10,000 in housing deductions, he would itemize a total of $12,000, for a decrease in his tax burden of $6,000. Although the $10,000 of housing deductions should have provided the homeowner with a $5,000 decrease in his tax liability, he only received a $3,500 decrease since the tax value of the excess standard deduction was $1,500. If the homeowner had more than $5,000 in non-housing deductions in the first place, all of the $10,000 of housing deductions would be used to reduce the homeowner's tax liability.

Capital Gains

The tax treatment of capital gains on a primary residence might also affect a family's incentives to move. A family must pay tax on any capital gain on their house within two years of selling it unless they purchase another house during that period. If the new house is more expensive than the previous one, the family can roll over their capital gains. If the family has purchased a less expensive house, tax is owed at the time of the purchase on the difference between the two house prices, or the capital gain, whichever is less. Once a homeowner reaches age 55, he or she is allocated a $125,000 capital gains exclusion which can be used to offset taxable capital gains on the house.[12] This exclusion can only be applied once in a person's lifetime. Any accrued capital gain on a house is set to zero if the homeowner dies. A larger capital gains tax liability raises the cost of moving permanently to a smaller house or to renting.

[12]The age cutoff and exclusion amount cited are the current values. The exclusion rules have varied over time and are described in section 3.

1.3 An econometric approach to modeling mobility decisions

A number of authors have tested and estimated adjustment cost models for durables using micro data. Eberly (1994) derives an optimal (S-s) rule for automobile purchases and examines whether the model's prediction of the cross-sectional ergodic distribution of automobile value relative to family wealth matches that found in the Survey of Consumer Finances. Lam (1991) estimates the parameters of a stock-adjustment model for automobiles using panel data. Attanasio (1995) estimates an (S-s) model for automobile consumption relative to non-durable consumption using a relatively flexible parameterization with unobservable heterogeneity in the target level and the band width.

While these approaches have the obvious benefit of estimating the parameters of the model directly, a number of compromises must be made. Eberly (1994) and Attanasio (1995) require that changes in automobile consumption is the only factor inducing families to adjust and that other family characteristics only shift the upper and lower bounds and the target level. In addition, the authors require that automobile consumption is measured relative to wealth or non-durable consumption. Lam (1991) makes fairly strict functional form and stochastic assumptions on the structure of the problem and Attanasio (1995) loosens the assumptions only by substantially increasing computational complexity.

Applying a more reduced-form estimation procedure can potentially minimize the number of restrictive assumptions while still capturing several key features of the model. As in Caballero and Engel's (1993) generalized (S-s) model, the probability of a move should be increasing in the percent difference between actual housing consumption and housing demand. Also, a family can undertake one of several discrete types of moves, either to a larger house or from renting to owning, or to a smaller house or from owning to renting.[13] Since the moving cost may differ for each type of move, it is important to distinguish between them to avoid confounding the effects of a tax change.

Moving behavior may also exhibit duration dependence. In the (S-s) model, an unobservable trend in a characteristic that underlies housing demand or the accumulation of unobservable shocks to demand will generate a higher likelihood of moving over time. On the other hand, moving costs may grow the longer the family lives in one house -- they accumulate more belongings or develop ties in their town -- in ways that are unmeasurable. In that case, the probability of moving decreases with the length of time the family stays in the house. Since duration is likely to be correlated with other important covariates such as age or income, it is important to include it in a regression model to avoid a potential source of bias.[14]

One key empirical issue is how to construct $\ln(Q^*/Q)$ since Q^* is not observed. I proxy for the percent difference between latent housing demand and actual housing consumption with the percent changes since the last move in the variables that underlie housing demand.[15] This approach requires two implicit assumptions: that the percent difference between desired and actual housing consumption is approximately linear in the percent changes and is affected by the

[13]Attanasio (1995) describes similar transitions with respect to automobile purchases.

[14]Other researchers have recognized the importance of duration. Henderson and Ioannides (1989) estimate a parametric duration model on a cross section of the PSID. Harmon and Potepan (1988) find that a linear duration term enters a probit moving equation (significantly) with a negative sign.

[15]Hanushek and Quigley (1979) propose using the difference between actual housing consumption and housing demand predicted from a cross-section demand regression. However, since a primary contention of this paper is that one needs to use predicted hazard rates from a mobility equation to correct for bias in a demand regression, Hanushek and Quigley's approach is not feasible.

changes in each of the demand factors independently.[16]

I estimate a competing-risks hazard model for the multiple moving transitions. The model correctly treats right-censoring, where families may not have moved by the end of the sample period, allows for multiple discrete types of moves, and allows the covariates to vary with duration. I control nonparametrically for duration dependence since the theory provides no clear prior for any functional form.[17]

I apply a technique derived by Han and Hausman (1990), Sueyoshi (1987), and Meyer (1990). For person i, the hazard at duration t is defined using a proportional hazards form:

$$\lambda(t|X_{it},\beta,\theta_i)=\theta_i\lambda_{0t}\exp(X_{it}\beta) \tag{3}$$

where λ_{0t} is the baseline hazard at duration t, X_{it} is the 1xK vector of covariates such as changes in variables since the last move, and β is the Kx1 parameter vector. Individual heterogeneity enters as a multiplicative factor, θ, that shifts the baseline hazard. Discretizing λ_0, define ξ_j as the jth step in the baseline hazard, $\xi_j \equiv \int_{j-1}^{j} \lambda_{0s} ds$. Sueyoshi (1987) shows that the probability that the move occurs before duration t can be written as:

$$F(t|X,\beta)=Pr\left[\ln\sum_{k=1}^{t} \exp(X_k\beta)\xi_k \geq \epsilon+\omega\right] \tag{4}$$

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[16]This assumption merely states that a change in a variable changes the difference between desired and actual housing consumption by the same increment regardless of the changes in any of the other variables. If multiple variables change, the effects are added together, with no interactions.

[17]In addition, the annual data I use has many durations that are tied -- families that move after the same number of years of living in a particular residence -- which is not appropriate for a continuous baseline hazard.

where ϵ is distributed extreme value and ω reflects the individual heterogeneity. Denoting the term inside the square brackets by ψ_t, the likelihood of a move between $t-1$ and t is:

$$L_t(\beta,\xi|X) = \int_{\psi_{t-1}}^{\psi_t} f(\epsilon)d\epsilon = F(\psi_t) - F(\psi_{t-1}) = F\left(\ln \sum_{k=1}^{t} \exp(X_k\beta)\xi_k\right) - F\left(\ln \sum_{k=1}^{t-1} \exp(X_k\beta)\xi_k\right) \tag{5}$$

and the likelihood when the residence spell is censored at duration t is:

$$L_t(\beta,\xi|X) = \int_{\psi_t}^{\infty} f(\epsilon)d\epsilon = 1 - F(\psi_t) = 1 - F\left(\ln \sum_{k=1}^{t} \exp(X_k\beta)\xi_k\right) \tag{6}$$

where F(·) is a logit cdf if there is no individual heterogeneity and takes the following form if ω has a gamma distribution with variance σ^2:[18]

$$F(\epsilon+\omega) = 1 - (1+\sigma^2\exp(\epsilon+\omega))^{-1/\sigma^2} \tag{7}$$

Although this exposition has assumed a binary choice, whether to move or not, families actually have several possible competing types of moves from their residence spell. The competing risks are assumed to be independent.[19] Thus, when examining any possible outcome,

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[18]The gamma provides a reasonably flexible distribution and is computationally convenient since its integral is known in closed form. Ideally, one would control nonparametrically for the heterogeneity, perhaps through the use of fixed effects. However, there is currently no consistent empirical method for estimating fixed effects in a duration model and recovering a nonparametric baseline hazard although Chamberlain (1985) describes some methods for estimating fixed effects with parametric baseline hazards and it is possible to implement fixed effects in a Cox partial-likelihood framework. Instead, one can assume some flexible parametric form for the heterogeneity, such as the gamma distribution. The combination of the parametric heterogeneity and the nonparametric baseline has been found to adequately control for unobserved heterogeneity. [Han-Hausman (1990)]

[19]Multiple residence spells by the same family are also assumed to be independent.

if another outcome occurs first, the spell is treated as being censored at that point.[20]

To estimate the coefficients of this model, the parameter vectors β and ξ are chosen using standard techniques to maximize the following log likelihood for each of the possible types of moves, m:

$$\mathcal{L}_m = \sum_{i=1}^{N} \sum_{t=1}^{T_i} \gamma_{it}\delta_{it} \ln\left(F(\ln \sum_{k=1}^{t} \exp(X_{ik}\beta)\xi_{ik}) - F(\ln \sum_{k=1}^{t-1} \exp(X_{ik}\beta)\xi_{ik}) \right) + \gamma_{it}(1-\delta_{it}) \ln\left(F(1 - \ln \sum_{k=1}^{t} \exp(X_{ik}\beta)\xi_{ik}) \right) \quad \textbf{(8)}$$

The variable δ_{it} is an indicator that takes the value one if the family makes a move of type m during period t and zero if they do not move or they make some other type of move. The variable γ_{it} is equal to one if the period t is the last observed period in the residence spell.

2. Data, Sample Construction and Identifying Variation

I use a panel data set from the Panel Study of Income Dynamics (PSID) that covers the 1970-1992 period. This section describes the benefits of examining this time period, highlights the sources of identifying variation, and outlines the construction of the sample and the variables.

2.1. The sample period

[20]Relaxing this assumption for owners and computing a three-way competing risk involves computing trivariate normals and maximizing a likelihood with 120 to 180 parameters, depending on the specification.

For tax and housing research, the 1970s and 1980s are interesting decades and provide several sources of exogenous, tax-induced variation in user cost and capital gains liabilities. Although there were relatively few changes in the tax legislation during the 1970s, high inflation combined with a lack of indexing of the tax code caused bracket creep for many families. Figure 2 shows the marginal subsidy rate on the first dollar of home mortgage interest for several example families at different real income levels. During the period 1970-1980, the marginal income tax rate faced by each of these families rose. All else equal, the higher income taxes translated into greater housing subsidies for itemizers, lowering the user cost of home owning.

The 1980s witnessed several legislative changes in the tax code, beginning with the Economic Recovery Tax Act of 1981 (ERTA). ERTA lowered the top marginal tax rate on unearned income from 70 percent to 50 percent and phased in tax rate reductions for other income brackets. This raised the user cost of housing for many families, especially higher-income ones. The effect of this legislation on the marginal subsidy rates can be seen in figure 2. The other major legislative tax change in the 1980s, the Tax Reform Act of 1986 (TRA86), raised the standard deduction, lowered tax rates (especially for higher-income families), and restricted tax shelters such as owning rental property. The effect on marginal subsidy rates can be seen in Figure 2.

In the time dimension, the effect of the changes in tax rates on the user cost of owner-occupied housing is dwarfed by the impact of inflation, which shows up in the expected capital gain on the property and in the after-tax nominal interest rate. Figure 3 shows the movement of user cost during the 1970s and 1980s for four sample families. The high inflation of the 1970s raised expected capital gains, lowering the user cost. As inflation fell in the 1980s, user cost rose again. The impact of tax changes on user cost over time has a similar pattern. However, the expansion then compression of the marginal tax rate distribution in the 1970s and 1980s causes the variation in user cost among the four example families to decrease.

The components of the tax code that determine homeowners' potential capital gains tax liabilities also varied substantially during the 1970s and 1980s. For 1978, the capital gains exclusion was raised from 50 to 60 percent, lowering the effective capital gains tax rate. The capital gains tax rate fell further when statutory tax rates were lowered in ERTA. In TRA86, the capital gains exclusion was eliminated while marginal tax rates fell. The exclusion elimination and the tax rate decrease raised the capital gains tax rate for some people, though the top tax rate on capital gains was capped at 28 percent.

The amount of the one-time capital gains exclusion for homeowners has also varied over the sample period. Prior to 1978, the exclusion was only available for homeowners aged 65 and over; later the minimum age was lowered to 55. Over the period 1970 to 1976, the capital gains base was reduced by a ratio equal to $20,000 divided by the sale price of the home. In 1976, the numerator was increased to $35,000. In 1978, a $100,000 exclusion could be offset against accumulated capital gains in the home and for 1981 the exclusion amount was raised to $125,000.

2.2. Identifying variation

Several sources of variation are available to identify the effect of taxes on the demand for owner-occupied housing. One significant source of exogenous variation is the change in average marginal tax rates over time. However, other factors such as interest rates and inflation also influence the demand for housing through the user cost. It may be difficult to disentangle taxes and other factors from other housing market or national level occurrences. In the estimation that will follow, I will experiment with including year dummies in the regressions.

While neither the time dimension nor the cross section may provide clean tax variation, the interaction of the two does. Tax rates have changed differentially over time for families at different income levels. If no other factor that is correlated with income level affected changes in housing demand over the sample period, one can attribute differences in changes in demand for otherwise identical people to the differences in the changes in their tax rates.

Housing demand is affected by state as well as federal taxes. Although state taxes do not vary much over time, they do vary across individuals and are incorporated into the analysis. I also discuss controlling for the state of residence in the estimation sections.

2.3. Data and sample construction

My primary source of data is the Panel Study of Income Dynamics, a longitudinal survey that has been collected annually since 1968. Respondents are asked a number of questions, the answers to which can be used to calculate marginal tax rates and income concepts. It is also a standard source for housing demand research since it reports many relevant aspects of the family's housing status. Due to the longitudinal aspect of the data set, one can observe dynamic behavior and multiple residence spells.

I use a sample of families observed over the 1970 to 1992 time period. Table 1 details the sample construction. The initial PSID sample included a random cross-section of the population and an additional low-income sample. In 1989, a Hispanic sample was added. I use only the observations derived from the original random cross-section sample. Families were matched by the ID number of the household head from the PSID waves, yielding a starting sample of 80,787 observations, or 22,822 residence spells or fragments of spells.[21]

In several years of the PSID, the questions corresponding to crucial data items for homeowners were not asked.[22] In those cases, the data was imputed using a weighted average of the closest years when the variable was available. If the family moved once during the interval, the missing data was set equal to the closest available value in the same residence spell. However, if the family moved multiple times during the interval, at least one residence spell had no observed neighboring data to impute with and those 701 spells had to be discarded.

For the duration model, special attention must be paid to censoring. To execute the analysis, the initial year of a family's residence in a dwelling must be observed so the duration of the spell is known, that is, the spells must not be left-censored. Right censoring, or not observing how the residence spell ends, can be handled by the estimation procedure. Families are allowed to enter the sample period at any point. Multiple spells for a given family are included. Figure 4 shows some examples of censoring:

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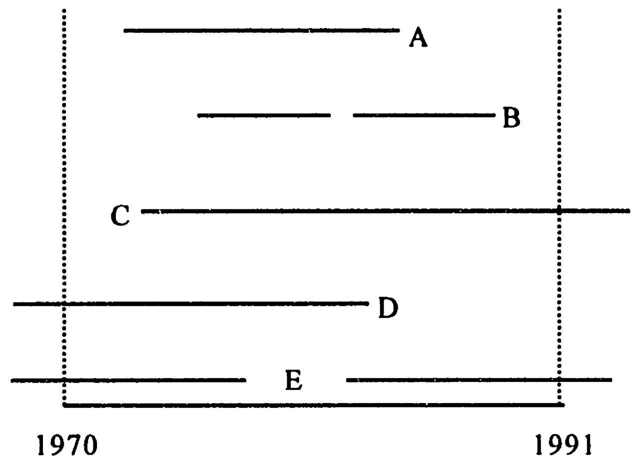
[21]For the purposes of this paper, I ignore the effect of taxes, user cost and income on household formation. Haurin, Hendershott and Kim (1994) find that there is a relationship.

[22]Property taxes paid is not available in 1978, 1988, and 1989. Mortgage principal remaining was not asked in 1973, 1974, 1975 and 1982.

Figure 4



Families A, B, and C are included in the sample, while D is not. The second spell of family E is included, but the first spell is not. Family A is not censored in any way since both the beginning and ending times of its residence spell are observed. Neither spell of family B is censored, so both are included. Family C is right-censored -- the ending date of the spell is unobserved -- but we know that they had not moved as of 1990 and use that information for consistent estimation. Family D is omitted from the sample since the beginning date of their spell is unobservable. The beginning date of family E's first spell is not observed, so it is left-censored and the entire spell is omitted. The second spell is included, however, since it is only right-censored.

Left-censoring is by far the primary reason that observations are discarded from the sample. Some homeowners, a total of 17, remain in their houses for more than 22 years and thus neither the start nor the end of their residence spell is observed even though they appear in each year of the data. 16,361 observations corresponding to spells that are both left- and right-censored are deleted. In addition, 8,166 observations on spells that are left-, but not right-, censored must be omitted. Also, 129 spells have gaps and are omitted in their entirety.[23]

Since income information in the PSID is provided for the *prior* year, the last observation on each family and any observations preceding a gap in reporting, has no such data. 6,093 observations, including all of the 1992 data, are dropped for this reason. In addition, 156 spells are removed because they have zero values for income or are missing user cost data at some point during the spell. The PSID income questions are asked about the prior year while nearly all other questions are asked about the contemporaneous year. Thus I do not observe a family's income in 1992 and, once I match the income variables to the proper year, the effective sample is from 1970 to 1991.

The final sample size is 46,128 observations on 15,980 spells, nearly evenly divided between renters and homeowners. Although the initial sample was nearly two-thirds renters, like the U.S. population, owners are more likely to be left-censored since their residence spells are longer on average.

The PSID is known to have sample attrition. If families drop out at random, the estimation is consistent since a family is right-censored whenever they leave the sample in the middle of a residence spell. However, it may be the case that families drop out of the PSID when they move. In that situation, the parameter estimates may be biased. It is difficult to predict ex ante what the direction of the bias will be: families that would have moved in response to a tax change but drop out of the sample instead will appear as right-censored when they should be counted as a move and will drive the tax coefficient towards zero. Families that would have

[23]Following a procedure suggested by Diamond and Hausman (1984), I examine the impact of the left-censoring by repeating the empirical analysis after artificially left-censoring the sample in 1975. The results are not substantively different.

moved even when there was no change in taxes but drop out of the sample instead will drive the tax coefficient away from zero since they will appear to have stayed put.

2.4. Moving transitions

Five possible transitions are considered in the mobility estimation in this paper. Every year, an owner has the option of purchasing a larger house ("trade up"), moving to a smaller house ("trade down), or becoming a renter. Renters can choose to purchase a house or can move to another rental dwelling. Lastly, a family can also choose not to move at all.[24]

A homeowning family is considered to have traded up when it reports in the following year that it has moved since the previous year and its real house value increases or has no change between the two years. A trade down occurs if the real house value decreases between the two houses. The distinction between trading up and trading down is likely to be a noisy one since self-reported house value is itself noisy.[25] For example, if families overestimate the value of their house, some moves that are actually upwards trades may appear as downward trades in the data. However, moderate changes in the definitions of trading up and down do not seem to have any

[24]Although the model is framed in terms of moving for consumption purposes, I estimate the mobility equations on all moves, including so-called "forced" moves. Since consumptive moves should not be long-distance, I excluded the 14 percent of the observations in my sample that correspond to spells that ended in moves across states. The estimated parameters of interest do not vary much between the original and the reduced samples. Also, the PSID provides the self-reported reason for a move. I separate the responses by "exogenous" (ie: "moved for job") and "endogenous" (ie: "wanted more housing") motivations and reestimate the mobility equations on both samples. Unfortunately, the sample sizes become too small to generate statistically significant estimates, but the point estimates of the key parameters do not differ much.

[25]See Goodman and Ittner (1992) for research on this subject. The authors find that homeowners overestimate the value of their houses by 6 percent on average but the inaccuracy does not change with duration.

significant qualitative effect on the results.[26]

In total, I observe just over 1,000 homeowning families trading up, about 460 trading down, and 940 homeowning families switching to becoming renters. Renters move much more frequently and many more actual moves are observed; about 2,500 transitions to home ownership, out of a base of 11,700 renting spells, are in the data.[27] Although the number of owners becoming renters seems large, many are probably temporarily renting while searching for a new house. Of the 940 families moving from ownership to renting, 343 return to ownership within two years -- the deadline for rolling over capital gains.

Figures 5 and 6 express the likelihood of the transitions as a hazard rate, or probability of moving a given duration conditional on not having moved before then. For example, in figure 5, if a homeowning family has lived in their house for five years, they have more than a four percent chance of moving to a larger house (the top section), about a one percent probability of moving to a smaller house (the middle section), and about a 3.5 percent chance of becoming a renter (the bottom section). Overall, a homeowning family has more than a 9 percent probability of moving if they have not moved in the previous five years.

In figure 6, the empirical hazards for renting families are shown. The hazard rates are much higher -- in the first year of residence, a renter will move more than half the time while an owner will move only about 15 percent of the time. For the renter, the bulk of the moves are to other rental dwellings. However, the hazard of a renter becoming a homeowner is also quite

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[26]Such changes include requiring the new house value to be lower (higher) than the old house value by a certain factor for the move to be considered as a trade down (trade up) and using different deflators to determine real house value. Using a start-of-spell to start-of-spell measure of trading up, rather than an end-of-spell to start-of-spell measure does have a small impact and is discussed in the results section.

[27]Appendix table B breaks out moves by duration.

large; even after five years in the apartment or house, the renter has more than an eight percent chance of purchasing a home.

For both owners and renters, the hazard of moving -- in any particular way or in total -- declines with duration, though the owners' hazards spike at about eight years and renters' spike between 10 and 11 years. These patterns of declining hazards support the idea that the costs to moving increase with duration so the probability of a family crossing their adjustment bounds decreases with duration. However, these patterns could be caused by family-level heterogeneity. If some families are frequent movers and some are reluctant movers, then a sample of all the families over time would contain many moves at short durations, reflecting the frequent movers, and few moves at long durations, from the reluctant movers. In other words, the declining hazards may reflect the cross-sectional heterogeneity between families rather than actual probabilities for a given family over their spell.[28]

2.5. Variable construction and means

Table 2 reports the sample means and standard deviations, broken out by home ownership. All dollar values are in real (1990) dollars. Nearly half the observations are of owners and they average five years duration in their residences. Duration is measured as the number of years since the last move, with a move in the first 12 months counting as a one year duration, the next twelve as two years, and so forth. Obviously, a simple mean is not a sufficient

[28]Absent individual heterogeneity and duration-dependent moving costs, Caballero and Engel (1993) point out that the hazard should be increasing in duration.

statistic since the lengthy tail of the duration distribution is censored.[29] Renters are observed to average 2.4 years in their residences.

After-tax income is the sum of all reported income components minus the calculated tax liability. In the early 1970s, some of the income variables are categorized and the midpoint of the categories, or 1.5 times the upper limit, are used. The sample appears to be more representative of lower-income households; the average after-tax income is just $29,788 ($1990). While this is not a problem per se, it may limit the applicability of the sample to the population as a whole. In addition, much of the interesting tax variation occurs near the upper end of the income distribution which is not well-represented here. Owners, with an average after-tax income of $37,023, have much higher incomes than renters, at $21,189. Total positive income is the sum of all nonnegative income components reported in the PSID. On average, it is about $9,000 higher than after-tax income for the entire sample.

To obtain tax liabilities and rates, income variables from the PSID for the years 1980 through 1991 are passed through the National Bureau of Economic Research's TaxSim tax calculator. This model was designed to calculate tax liabilities for filers in the Treasury's Statistics of Income Public Use Sample of tax returns, but also works with the more limited PSID data.[30] For observations during the 1970 to 1979 period, a similar tax calculator that uses only the items in the PSID data set was written by the author. Both tax calculators can compute first- and last-dollar marginal tax rates.

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[29]This is why Henderson and Ioannides (1989) and Gronberg and Reed (1991) estimate parametric forms of the duration model. With the properly fit model, they can estimate the shape of the tail and calculate a mean.

[30]The most significant omission in the PSID data is capital gains.

State tax rates for the period 1979-1991 are calculated using the NBER's State Tax Calculator. State tax rates for data from years prior to 1979 are computed using the 1979 state tax laws. Calculated state tax liabilities are used as a deduction when calculating the federal tax rate. The two tax rates are combined for an overall rate faced by the household.

Only "first dollar" tax rates, calculated setting all housing-related, and thus potentially endogenous, variables to zero, are used in this study. The potentially endogenous variables include mortgage interest paid and property tax deductions.

User cost is constructed by inserting the appropriate first-dollar tax and subsidy rates into equation 2. The loan-to-value ratio on the first dollar of housing is assumed to be the average LTV for the entire sample, 0.43. The interest rate and the rate of return on equity are both assumed to be the seven-year Treasury bill rate. The property tax rate is the average for the entire sample, 1.3 percent. Following Poterba (1992), the depreciation rate and maintenance costs are each fixed at 0.02, the risk premium for housing is 0.04, and expected inflation is a five-year moving average of inflation as measured by the CPI. Average user cost is 0.98, slightly higher for renters than for owners.

The tax value of the wasted deduction is computed as the difference between the current tax liability for a family and what the liability would be if there were no standard deduction. House value is the self-reported answer to the question, "If you were to place your house on the market today, about how much would it bring?" The average value for the owners in the PSID sample is $89,725.

The capital gains tax base for a family is the sum of all house price appreciation observed in the data set. Thus only families with multiple observed residence spells can have nonzero capital gains liabilities. I assume that the family uses any applicable capital gains exclusions and subtract the exclusion amount from the capital gains tax base. The capital gain tax liability is computed as the increase in taxes from realizing the entire capital gains tax base.

Filing status is self-reported, but families that claim to be married filing separately are labeled joint filers so the family can be considered to face one tax rate. The number of dependents is taken to be the number of children aged 18 and under. Age exemptions are assigned to family members aged 65 and over.

3. Mobility Results

In this section, I examine the impact of a number of tax, income, price, and demographic variables on families' moving transitions. I use the Han-Hausman/Sueyoshi/Meyer proportional hazards model described in equation 8, assuming that the competing risks are independent.

I proxy for the difference between current and preferred housing consumption with a function of the changes in user cost, income, capital gains, and other covariates since the first year of the residence spell:

$$z_t \approx \Upsilon(\ln(Y_t) - \ln(Y_1), \ln(UC_t) - \ln(UC_1), CG_t - CG_1, X_t - X_1) \tag{9}$$

For example, in the first year of the spell, the difference in income is identically zero since the family is at its optimal level. In the second year, the difference is taken between that year and the one before it -- first differences. In the third year of the spell, second differences are used, and so forth.[31]

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[31] The function $\Upsilon(\cdot)$ is assumed to be linear in each of the differences.

The covariates in the baseline specification include the percent change in user cost since the first year of the residence spell; the percent change in after-tax income since the first year of the spell; the capital gains tax liability that would be induced if the house were sold and no new one were purchased; the change in the tax value of the excess standard deduction since the first year of the spell; an indicator variable for the change in the family's filing status, such as becoming a head of household, getting married, becoming single, or no change; age dummies for five-year intervals from age 20 to 75 and one for 75 and over; indicator variables for change in retirement status; an indicator variable for whether the family used to have children living in the house and no longer does and vice versa; and the difference in the number of children living at home. For owners, the covariates include the family's accumulated capital gain on housing while the renter equations include an indicator variable for whether the renter was observed moving from ownership to renting within the last two years. Half of the specifications include a complete set of year dummies and the other half include the national unemployment rate.

While the logic behind including most of the covariates is obvious, the capital gains variables require some explanation. The family's accumulated capital gain on housing is included to mitigate bias in the estimated coefficient on capital gain tax liability induced by inaccurate self-reported house values. If a family overestimates its house value, and its error grows with duration so it is nearly accurate in the first year of the residence spell but gradually moves further from reality, it will appear to have a capital gain on its house but will also be more likely to be considered a trade down when it moves since it will get the purchase price of its new house more or less "right." A family that underestimates its house value and appears to have a low capital gain will be more likely to look like a trade up. This type of measurement error will generate the "wrong" sign on the capital gains tax liability variable in the "trade up" and "trade down" regressions.[32] By including accumulated capital gains as a variable in the regression, the capital gains tax liability will be identified solely from exogenous variation in capital gains tax rates and the capital gains exclusion.

For renters, the indicator variable for recently moving from ownership to renting is intended to control for searching behavior. An owner with a capital gain may become a renter but intend to purchase a new house within the capital gains rollover period. Including the searching dummy controls for the fact that owners may have to temporarily rent before finding an appropriate house. The capital gain tax liability coefficient will be identified from variation in the size of the capital gain, the capital gain tax rate, and the exclusion.

3.1. Baseline mobility results

Table 3 presents the estimated effect of key variables on the transition hazards. The variables are the percent change in user cost since the beginning of the residence spell, the percent change in after-tax income, the capital gains tax liability due if the family were to sell its house and not purchase a new one, the tax value of the excess standard deduction, and indicator variables for the change in filing status. Each of these variables are addressed in turn.

An increase in user cost is expected to lower the likelihood that a renter purchases a house since, absent asset price or rent changes, housing becomes relatively more expensive and

[32] Jeremy Stein (1993) postulates that homeowners with house price *depreciation* and high loan-to-value ratios may become locked-in to their current residence since they lack the capital and liquidity to pay off their mortgages and move. Sewin Chan (1995) finds empirical evidence of "spatial lock-in" using a data set of New York mortgages. In several different specifications, I have found statistically significant evidence of such lock-in as well. However, controlling for spatial lock-in does not appear to affect the other estimated coefficients. For the sake of (relative) brevity, I do not report the results here; they will appear in a future paper.

the renter's optimal housing consumption will shift downward relative to current consumption. In the first column, the estimated user cost coefficient is − 1.16 (0.21). Table 4 interprets the coefficients in terms of the likelihood that a family moves. In the first column of table 4, a 10 percent increase in user cost, evaluated with the estimated coefficient of -1.16, is estimated to shift the baseline hazard down by 10 percent.[33] This change would lower the average hazard of renters moving to ownership by 1.3 percentage points and translates into more than a 3.8 percentage point increase in the likelihood that a renter will not have purchased a house after five years of living in the same apartment.

A decline in user cost is expected to increase the probability that an owner purchases a larger house. In the second column the estimated coefficient of − 0.37 (0.16) implies that a 10 percent increase in user cost since the beginning of the residence spell is estimated to reduce the hazard of an owner purchasing a larger house by about 3.5 percent at every duration. Since trading up is such a low probability event -- the average hazard is about 4.3 percent -- this effect is not large in an absolute sense. A 10 percent increase in user cost, from 0.10 to 0.11 for example, would lower the average hazard by about 0.15 percentage points. This translates into an owning family being more than half a percentage point less likely to have traded up after five years.

With a decline in user cost, families on the borderline of being willing to trade up are spurred into doing so, while families who were about to trade down should no longer find the

[33]Elasticities are calculated by computing the percent difference between the predicted hazard for each family averaged over the sample of owners or renters and the averaged predicted hazard when there is a 10 percent increase in the variable for each family. In general, one standard deviation of the variables in question is greater than 10 percent.

need to be quite so pressing. Similarly, an increase in user cost should encourage owners to move to renters as the relative price of homeowning increases. The third column shows, however, that changes in user cost have no measurable effect on the hazard of trading down, while the fourth column indicates that if changes in user cost affect the likelihood that owners become renters, it is not distinguishable in the data.

The second row of the first panel of table 3 examines the impact of changes in after-tax income on families' moving hazards. As income rises, families should be more likely to move so they can consume more housing, by trading up or by becoming owners. If their income falls, families should be more likely to trade down or become renters.

For renters, the estimated coefficient of 0.33 (0.07) translates into more than a three percent increase in the hazard of purchasing a house when income increases by 10 percent, as shown in the second panel of table 4. After five years, a family with the higher income would be 1.1 percentage points more likely to have purchased a house.

In the second column of table 3, the estimated coefficient of 0.76 (0.11) implies that a 10 percent increase in income raises the average hazard that a family trades up by 7.6 percent. The opposite effect is measured for owners transiting to renter status in column four, where the same increase in income lowers average hazard that owners transit to renter status by 3.2 percent. Once again, the effect on the likelihood of a family trading down is not measurably different from zero.

The third row of table 3 explores the effect of a family's capital gains tax liability on moving. There is no measurable effect in column 2 of the capital gains tax liability on the likelihood of owners trading up, but in column 3 families with higher capital gains tax liabilities are statistically significantly less likely to trade down. For families with capital gains, a 10 percent increase in the capital gains tax liability would lower the average hazard of trading down by 1.5 percent.[34]

In the fourth column of table 3, it can be seen that owners with higher capital gain liabilities are statistically significantly less likely to move to a rental. The estimated coefficient of − 0.024 (0.012) corresponds to a one percent decline in the average hazard rate for families with capital gains when the capital gains liability increases by 10 percent. This figure is probably low since one-third of owning families that become renters are observed purchasing a new house within the two year roll-over window, and thus do not pay the entire capital gains liability, if any. Therefore, not all the population of owners switching to renters should be responsive to their capital gains liability in the data. The families that know ex ante that they will roll over their gain would not respond to changes in the capital gains tax rate and drive the estimated coefficient towards zero.

For renters, a high capital gains tax liability is an indicator that they are more likely to purchase a house. A 10 percent increase in capital gains tax liability for renters with capital gains, evaluated at the estimated coefficient in the first column of 0.035 (0.009), translates into a 1.3 percent increase in the average hazard rate for renters with capital gains. This finding is suggestive that renters that have a capital gain from a previous house that can still be rolled over

[34]When calculating the capital gain elasticities, I omit families with no capital gain on their houses since multiplying their "gain" by 1.1 would have no effect on their hazard. A more interesting statistic would be to simulate a change in the capital gains tax rate or the amount of the exclusion. These simulations are presented in section 6.

into a new residence are more likely to do so when the tax liability is larger.[35]

Unfortunately, the change in the tax value of the excess standard deduction is not measured with enough precision to be statistically different than zero, except in the owners-trading-up regression.[36] Since theory would predict that the excess deduction should only affect the owner/renter decision, this result is a bit puzzling. The estimated coefficient of −0.55 (0.11) in the "trade up" regression might be due to the fact that purchasing a new house requires refinancing a mortgage. Since the interest payments are front-loaded, an increase in the excess standard deduction should make a family less willing to take out a larger mortgage since the after-tax interest payments would be greater.

The next three lines of table 3 report the estimated effect of changes in tax filing status. Families which become heads of households are statistically significantly less likely to buy houses if they are renters and more likely to become renters if they are owners. Families which get married during their residence spell become significantly more likely to move, whether by trading up or trading down, and are more likely to purchase a house if they are currently renting. Families that were married and become single, either by death or divorce, are estimated to be more likely to trade down and much more likely to become renters from owners.

The second panel of table 3 repeats the estimation, incorporating a complete set of year

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[35]The implied economic impact from the estimated capital gains coefficients, while statistically significantly different from zero, is small. Previous research on this topic has generally failed to find any significant effect of capital gains taxes on mobility [Hoyt and Rosenthal (1990)]. However, Burman *et al* (1996), using tax return data from families that sold their homes, finds that eliminating the capital gains tax on housing would enable 25 percent of the families that moved to trade down rather than up.

[36]Interestingly, indicator variables for whether the family went from zero tax value of excess standard deduction to positive, or vice versa, enter significantly into all specifications except the renter-to-owner. This result suggests to me that the actual tax value of the excess deduction may be poorly measured.

dummies rather than the national unemployment rate. The year dummies are included to control for unobserved national-level events, such as changes in house prices or recessions, that may be correlated with the probability of moving and some of the right-hand-side variables.

Including year dummies has very little effect on the point estimates or standard errors in the first panel in table 3, except for the user cost coefficients.[37] The estimated "renters to owner" coefficient drops from −1.19 to −0.93 (0.235) and the "trade up" coefficient changes sign, but is not statistically different from zero. The coefficients in the "trade down" and "to renter" columns are still indistinguishable from zero.

If the year dummies' impact comes through controlling for variation in user cost due to changes in interest rates and expected inflation, a case can be made for leaving the year dummies out of the estimation since the effect of interest rates and expected inflation on housing demand and moving decisions should come through user cost. In that case, one would include the year dummies only to focus the estimation on the tax-induced variation. If the change in the estimated user cost coefficients is due to some other omitted variable operating in the time dimension, then including year dummies may be necessary to obtain consistent estimates.

As it turns out, year dummies alone explain about 61 percent of the variation in the user

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[37]Several alternate specifications besides including year dummies have been examined, but yield little difference from the baseline results. Including a complete set of state dummies to control for fixed effects had virtually no impact on the parameters of interest, except that the excess standard deduction variable was negative and statistically significant in the renter-to-owner regression. The state dummies were not individually statistically different from zero. When state house price indices were added to the state dummies, there was no additional change in the main parameter estimates and the estimated coefficients on the price indices were statistically significant only in the specification without year dummies.

A set of 17 dummy variables for $10,000 categories in total positive income at the time of the move was included in case the probability of moving varies with wealth. Families with higher initial TPI were more likely to move and, in general, the magnitude of the estimated coefficients increased. However, the estimated coefficient on the capital gains variable was frequently no longer significant.

cost variable, while the interest rate and expected inflation alone explain 59 percent of the variation in the user cost variable. If the year dummies are replaced with the national unemployment rate, the interest rate, and expected inflation, the estimated coefficients are nearly identical to those estimated when the year dummies are included. This evidence suggests that including year dummies controls for the time-dimension variation in user cost. However, many of the tax rate changes occur primarily in the time dimension and that variation accounted for by the time dummies. The remaining variation, differential changes in user cost over time, yields somewhat different estimated coefficients in the trade up regression.

3.2. Gamma heterogeneity results

Table 5 compares the estimated user cost coefficients from several alternative specifications to the baseline results reported in table 3, beginning with the results from incorporating gamma heterogeneity when estimating the original specifications. While taking account of the heterogeneity has some effect on the estimated user cost coefficients, it has little effect on any of the estimated coefficients for the other parameters except for some of the demographic covariates, so they are not reported. Although the estimated gamma variance parameter is frequently large, it is statistically different from zero only for the owner-to-renter regression.[38]

Overall, the estimated user cost coefficients for renters are robust to specification

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[38]I have also tried controlling for mover/non-mover heterogeneity by including an indicator variable for whether the family responded affirmatively *at the time of their move* to the question "Do you think you might move in the next couple of years?" While this variable has a large positive coefficient, including it does not change the other point estimates.

changes, while the estimated parameters for owners are more sensitive. In the first column of table 5, renters becoming owners, incorporating gamma heterogeneity has a slight depressing effect on the estimated user cost coefficients but the differences are small relative to the estimated standard errors. The estimation finds very little heterogeneity in this behavior, so the small effect on the coefficients is reasonable.

For owners trading up, the estimated user cost coefficient increases in magnitude from the baseline estimate to −0.55 (0.06). In the trading down regression, the user cost point estimate rises from −0.35 in table 3 to −0.50 (0.25). This negative estimated coefficient indicates that a higher user cost is correlated with fewer moves at all by owners, whether up or down. The estimates in column three display substantial heterogeneity. The estimate of the gamma variance is 1.99 (1.00) and the estimated user cost coefficient increases in magnitude from virtually zero to 0.30 (0.23)

When year dummies are included, the coefficient on owners trading up changes sign to 0.42 (0.24), the coefficient on trading down falls to -0.13 (0.28), and the own-to-rent coefficient declines as well. However, including year dummies also causes a 30 percent increase in the estimated income coefficient, a 30 percent decrease in the estimated coefficient on the excess standard deduction, and some loss of efficiency, suggesting that with year dummies it may be difficult to separately identify the tax-based variables.

3.3. The effect of controlling for cross-section tax variation

The tax rate is a nonlinear function of income, filing status, and the mix of types of income and deductions, and thus varies across families at any given point in time. However, as has been frequently pointed out, it is difficult to determine whether tax elasticities can be identified separately from income using the nonlinearity of the tax code in a cross-sectional regression. [Feenberg (1987)] I control for a nonlinear function of income by including a set of 17 dummy variables for $10,000 intervals in the level of after-tax income.[39] The remaining variation in user cost should be primarily due to differential changes in tax rates over time by income category. The estimated user cost coefficients are reported in the third panel of table 5. Including the dummies has little effect on the estimated user cost elasticities for owners, but reduces the estimated elasticity for renters by about half. Also, many of the estimated capital gains coefficients cease to be statistically significant.

3.4. Does incorporating duration dependence make a difference?

Thus far, I have assumed that it is necessary to control for duration dependence in the mobility regressions. Since the estimated duration dummies are statistically significant, they belong in the mobility regressions, but are the other estimated coefficients biased if duration dependence is ignored? In the last panel of table 5, the user cost coefficients that were estimated without controlling for duration are more negative than the estimates correcting for duration dependence, suggesting that accounting for duration dependence is potentially important.

4. Estimating Demand Elasticities in the Presence of Moving Costs

While immobility affects the dynamic pattern of housing consumption and possibly asset

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[39] I cannot separately identify changes in income from changes in user cost if I include a set of dummies to control nonparametrically for *changes* in income.

prices, the response of housing demand to a tax change is the ultimate determinant of dead weight loss. Economists have tried to estimate the user cost and income elasticities of demand for more than two decades and have come up with a range of findings. Rosen (1979) estimated a user cost elasticity of about – 1.0 and an income elasticity of about 0.75, while recent estimates by Hendershott, Haurin, and Kim (1994) suggest that the user cost elasticity is around -0.5.

However, most authors have not fully considered the potential biases created when only actual housing consumption, not desired housing demand, is observed. Since the level of consumption is updated only when a family moves, if the decision to move is correlated with the amount of housing a family chooses to consume, the estimated coefficients on the sample of observed owners may suffer from sample selection bias. The approach taken by Rosen (1979), Haurin (1991), and others, using predicted inverse Mills ratios from a probit equation of whether a family is an owner in a second-step demand equation for a sample of homeowners, is applicable only if moving is assumed to be frictionless. This static strategy fails to recognize the duration dependence inherent in moving and that decisions to move are based largely on changes in underlying characteristics.[40]

Much of the recent literature on housing demand has addressed a related question: when during the residence spell is it appropriate to relate a family's housing consumption to its observed characteristics? Many authors, including Henderson and Ioannides (1989) and Haurin, Hendershott, and Kim (1994), estimate a demand equation on recent movers under the

[40]Henderson and Ioannides (1989) recognize duration dependence, but for a different reason. They postulate that the fixed costs of moving are amortized over the expected length of stay in a residence, so a family that intended to stay in a house longer would find it cheaper to own. They estimate a cross-section duration model, predict expected length-of-stay, and include the predictions in a second step demand equation.

assumption that families that have recently adjusted their consumption levels reflect the true relationship between Q^* and their underlying characteristics.[41] However, when forward-looking families move, they choose to consume C percent of Q^* since they foresee a trend in their housing demand. If C varies across families in a way that is correlated with the right-hand side variables, demand estimates from a sample of recent movers cannot be interpreted as latent demand.[42] Edin and Englund (1991) lend credence to the forward-looking hypothesis by demonstrating that the residuals in a housing demand equation are smallest near the middle of a residence spell. However, one cannot determine a priori which year would make the best match.

I use a new procedure to correct for the potential dynamic sample selection and to mitigate the problems that arise when families are forward-looking. Predicted hazard rates from the various mobility equations from the previous section are included semiparametrically in a housing demand equation. This technique uses all observations on homeowners, avoiding the loss of efficiency from using a sample of movers. Unlike previous sample selection work in this area, I avoid making strong functional form assumptions by using a semiparametric method derived from Newey (1991).

There are two channels through which dynamic sample selection can occur. Families enter the sample of observed homeowners by purchasing a house -- by trading up, trading down, or ceasing to rent. If any of these decisions are correlated with the quantity of housing the family will choose to consume when it purchases its house, the potential for sample selection bias is

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[41]Several authors discuss whether it is appropriate to use a sample of recent movers. Dynarski (1985) finds no significant difference in income or price elasticities between movers and nonmovers, but finds that the estimated coefficients on the demographic characteristics vary between samples.

[42]If C is a fixed percent of Q^* or varies randomly across families, the estimated parameters will be off by a constant.

present. In addition, families are observed *in the same house* until they choose to leave, either by trading up, trading down, or becoming renters. If any of these decisions are correlated with the quantity of house the family owns, again, the potential for sample selection bias is present.

Assuming observations on multiple residence spells by the same family are independent, one can think of a family as entering the sample of homeowners when it moves into a particular house. It remains in the sample until it moves again. Since movers are families that have the largest difference between their actual and desired housing consumption, correlation between the error terms in the moving equations and the quantity equation is possible.

The quantity of housing consumed, Q_{it}, is observed only for those families that chose to move into a house at the last move and, as of duration t, have not yet moved out. A household moves if its hazard of a particular transition, λ_{trans}, at t is greater than some threshold, μ_{trans}. The time of end of the previous residence spell is denoted by time 0. Thus $\lambda(X_{i0}\beta_{trans},\xi_{trans})$ is the transition hazard from the previous residence spell at the time of the last move, or "entry hazard," and $\lambda(X_{it}\beta_{trans},\xi_{trans})$ is a contemporaneous hazard for transiting out of ownership, or "exit hazard." The subscript "trans" refers to the type of move: an owner trading up ("UP"), trading down ("DOWN"), or becoming a renter ("RENT"), or a renter becoming an owner ("OWN"). The conditions under which a family's housing consumption is observed can then be written as:

$$Q_{it} \text{ observed if: } \left\{ \begin{array}{l} \left\{ \begin{array}{r} \lambda(X_{i0}\beta_{UP},\xi_{UP})-\mu_{i0,UP}>0 \\ or\ \lambda(X_{i0}\beta_{DOWN},\xi_{DOWN})-\mu_{i0,DOWN}>0 \\ or\ \lambda(X_{i0}\beta_{OWN},\xi_{OWN})-\mu_{i0,OWN}>0 \end{array} \right. \\ AND \\ \left\{ \begin{array}{r} \lambda(X_{it}\beta_{UP},\xi_{UP})-\mu_{it,UP}<0 \\ and\ \lambda(X_{it}\beta_{DOWN},\xi_{DOWN})-\mu_{it,DOWN}<0 \\ and\ \lambda(X_{it}\beta_{RENT},\xi_{RENT})-\mu_{it,RENT}<0 \end{array} \right. \end{array} \right. \qquad \textbf{(10)}$$

The demand equation to be estimated is the quantity of housing, Q_{it}, for family i in every year that they are observed, t_i, as a function of individual covariates, R_{it}, parameter vector, α, and unobservable component, ν_{it}:

$$Q_{it} = R_{it}\alpha + \nu_{it} \tag{11}$$

If ν is correlated with any of the μ terms above, the OLS estimate of α may be inconsistent. [Heckman (1979)] The typical static selection correction includes the inverse Mills ratio from the selection equation. The dynamic analog is straightforward. The hazard models written here are essentially logits with duration dependence and an inverse Mills ratio is simply a hazard rate. If there were only one possible transition, one could mimic a Heckman correction by including the hazard from the first step mobility model in the second step quantity regression. In this case, there are six selection equations. Assuming the correlation between each error in the first step equations and the error in the second step equation are independent, six hazard terms could be included in the second step equation.

Including the hazard terms linearly requires making a restrictive assumption on the joint distribution of the error terms in the first step mobility equations and the demand equation. Instead, Newey (1990) shows that the correction term can be entered into the demand equation as a polynomial and consistency and asymptotic normality is achieved without any strong functional form assumptions on the joint error distribution.[43]

Following Newey (1990) by writing $h_{trans}(\lambda_{0,trans})=E[\nu \mid R_t, \mu_{0,trans}>-\lambda(X_0\beta_{trans},\xi_{trans})\ ]$ and $k_{trans}(\xi_{t,trans})=E[\nu \mid R_t, \mu_{t,trans}>-\lambda(X_t\beta_{trans},\xi_{trans})\ ]$, the sample-selection-corrected demand equation

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[43] Although Newey presents an example with no assumption on the error structure in the first stage equation, I assume a logit error form when estimating the hazard models.

becomes:

$$Q_t = R_t\alpha + \Theta(\lambda_{0,UP}, \lambda_{0,DOWN}, \lambda_{0,OWN}, \lambda_{t,UP}, \lambda_{t,DOWN}, \lambda_{t,RENT}) + \nu_t \tag{12}$$

Assuming the correlation between each error in the first step equations and the error in the second step equation are independent,[44] the interactions between the hazards can be eliminated and each hazard can be entered with its own polynomial:[45]

$$\begin{aligned} Q_t = R_t\alpha &+ h_{UP}(\lambda_{0,UP}) + h_{DOWN}(\lambda_{0,DOWN}) + h_{OWN}(\lambda_{0,OWN}) + \\ &k_{UP}(\lambda_{t,UP}) + k_{DOWN}(\lambda_{t,DOWN}) + k_{RENT}(\lambda_{t,RENT}) + \nu_t \end{aligned} \tag{13}$$

Using polynomials for h(·) and k(·) allow for a flexible functional form for the joint distribution between ν and the μ terms.[46] In the empirical work that follows, I include a quartic in each of the hazards.[47] Newey shows that polynomials in the entry and exit hazards are likely

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[44] Empirically, incorporating interactions makes no difference and adds significant complexity when computing the standard errors.

[45] Since the joint distributions of ν and the μ terms are unknown, h(·) and k(·) have unknown functional forms. If each first step error is jointly normal with the second step error, and the first step mobility equations are estimated as ordered probits instead of logits, a linear h(·) or k(·) corresponds to the Heckman correction.

[46] Numbering the possible moving transitions from one to six, the estimated asymptotic variance can be computed in the following way [Newey (1990), p.5], where $\hat{h}(\lambda) \equiv \hat{p}^K(\lambda)'\hat{\zeta}$ is the estimate of h(λ) from a first-step mobility regression, $\hat{Z}_t \equiv (R_{it}', \hat{p}^K(\hat{\lambda}_{t,UP})', \hat{p}^K(\hat{\lambda}_{t,DOWN})', \hat{p}^K(\hat{\lambda}_{t,OWN})')'$ is a vector of right-hand-side variables for the second step demand equation, $\hat{\Xi} \equiv [\hat{\alpha}', \hat{\zeta}']$ is the parameter vector from the demand equation, $\hat{B} \equiv [\hat{\beta}, \hat{\xi}]$ is the parameter vector from a first step mobility equation, and $\hat{H} \equiv \sum_{t=1}^{n} \hat{Z}_t[\partial\hat{h}(\hat{\lambda}_t)/\partial\lambda]\partial\lambda(X_t, \hat{\mathrm{B}})/\partial \mathrm{B}'/n$ if tr=1,2,3 and $\hat{H} \equiv \sum_{t=1}^{n} \hat{Z}_t[\partial\hat{k}(\hat{\lambda}_t)/\partial\lambda]\partial\lambda(X_t, \hat{\mathrm{B}})/\partial \mathrm{B}'/n$ if tr=4,5,6:

$$\hat{V}(\hat{\alpha}) = [I_\alpha, 0](\hat{Z}'\hat{Z}/n)^{-1}\{\sum_{t=1}^{n} \hat{Z}_t\hat{Z}_t'(y_t - \hat{Z}_t'\hat{\Xi})^2/n + \sum_{tr=1}^{6} \hat{H}_{tr}\hat{V}(\hat{B}_{tr})\hat{H}_{tr}'\}(\hat{Z}'\hat{Z}/n)^{-1}[I_\alpha, 0]'$$

The variance in this case is a combination of the White heteroskedasticity correction (the first term in braces) plus the *sum* of terms which account for the generated regressors from the mobility estimation.

[47] One could use a computationally-expensive cross-validation technique to determine the optimal order for each of the polynomials. However, a quartic seems to be an acceptable compromise between functional flexibility and loss of efficiency.

to be identified even without any exclusion restrictions, variables in X that are not in R, as long as the hazard functions are not linear in $X\beta$.[48] In any case, the tax value of the excess standard deduction will be an exogenous excluded variable in my estimation. The entry hazards are further identified since they are predetermined in the prior residence spell.

5. Housing Demand Results

The demand equations estimated in this section all take a similar form. The equation to be estimated is:

$$\log(Q_{it}) = \alpha + \beta_1 \log(User\,Cost_{it}) + \beta_2 \log(Income_{it}) + \beta_3 CG\,Tax\,Liability_{i0} + \beta_4 Capital\,Gains_{i0} + \\ \beta_5 Filing\,Status_{it} + \beta_6 Exit\,Hazards_{it} + \beta_7 Entry\,Hazards_{it} + \beta_8 Age_{it} + \beta_9 X_{it} + \epsilon_{is} \tag{14}$$

where Q is self-reported real house value, income is after-tax disposable income, filing status is a vector of indicator variables for married and head of household (single is omitted), exit hazards are the hazards of leaving ownership for renter status or trading up or down, entry hazards are indicator variables or hazards for renters becoming owners or owners trading up or down, year is a complete set of year dummies, and age is a set of indicator variables for five-year age categories. The vector of remaining covariates, X, includes time-varying covariates such as an indicator variable for whether the family has children, the number of children the family has, and retirement status, and time invariant dummy variables for race and gender of the family head. The capital gains tax liability and accumulated capital gains variables are set equal to the values

[48]For identification, the polynomial in the hazard term cannot be linearly dependent with the other variables on the right-hand-side of the demand equation. Nonlinearities in the hazards from the first step can be exploited for identification without making restrictive assumptions about the joint error distribution.

at the end of the family's prior spell, if one is observed. The subscript i refers to the individual, t to the calendar year, and s to the residence spell. The standard errors take account of the first stage estimation as detailed in section 4.

5.1. No selection correction

As a baseline, I begin by estimating a housing demand equation with no hazard correction on the entire sample of 25,051 home owners. In the first column of table 6, the user cost elasticity is estimated to be $-.49$ with a standard error of 0.04.[49] For comparison, Rosen (1979) estimates a user cost elasticity of about -1.0 and Henderson and Ioannides (1989) find a price elasticity of about -0.5.[50]

The income elasticity is estimated to be about 0.61 (0.03). This elasticity is well within the range that has been found in the previous literature; for instance, Rosen (1979) estimates an income elasticity of 0.75 and Hoyt and Rosenthal (1990), using a nonlinear budget set methodology, estimate an income elasticity of 0.10.

Families with higher capital gains tax liabilities at the time of their move, controlling for the level of capital gains, consume more housing. This result is consistent with families trading up or not trading down to avoid paying capital gains. Families with higher accumulated capital gains at the time of their move also consume more housing, perhaps because they are wealthier or to avoid capital gains taxation.

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[49] Henderson and Ioannides (1989) point out that user cost may decrease with expected duration. However, including an indicator variable for whether the family thought it would move within a couple of years at the time of its last move has no effect on the estimated user cost coefficient.

[50] The difference between my estimates and those in Rosen (1979) is attributable to his using a translog cross-section specification in 1970 versus my log-log panel specification over a different time period.

5.2. Dynamic sample selection corrections with exit hazards

Since exit hazards can be computed for every observation in the sample, I begin by incorporating exit hazards into the demand regressions.[51] In the second column of table 6, each exit hazard is included linearly. However, there is virtually no effect on the other estimated parameters in table 6 except the estimated income coefficient, which rises by six percent.

The third column in table 6 incorporates the exit hazards in the regression as quartics to account for the unknown distribution of the error terms between the mobility and demand equations. When the exit hazards are included in this way, the estimated user cost coefficient declines about 10 percent to -0.44 (0.04), but the standard errors are large enough that the decrease is not statistically significant. The estimated income coefficient increases by more than 10 percent to 0.68 (0.03), about twice the standard error.

The trade up hazard is expected to have a negative estimated coefficient since a higher than average probability of trading up is correlated with a higher than average excess demand for housing. Conversely, the trade down hazard should have a positive estimated coefficient. The coefficient on the owner-to-renter hazard should have a positive sign since owners who want to switch to renting are consuming too much housing relative to what they demand and the renters that are omitted from the estimation demand less housing, on average, than owners.

As expected, in the second column of table 6, the trade up hazard has a negative

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[51]Hazards are computed using the first stage mobility results estimated excluding the capital gains variables since they are derived from changes in house value, the left-hand-side variable in the demand equation. Predicted hazards are calculated using a discrete time formula:

$$\frac{F(\ln(\sum_{i=1}^{t} \exp(X_i\beta)\xi_i)) - F(\ln(\sum_{i=1}^{t-1} \exp(X_i\beta)\xi_i))}{1 - F(\ln(\sum_{i=1}^{t-1} \exp(X_i\beta)\xi_i))}$$

coefficient, -1.46 (0.47), indicating that families that are more likely to trade up are consuming less housing than they would like. The trade down coefficient has the opposite sign from what the theory would suggest, and is estimated to be -2.41 (0.79). The owner-to-renter hazard is positive and statistically significant, 1.55 (0.28), suggesting that families with a higher hazard of exiting homeownership are currently consuming more housing relative to their other characteristics. I do not report the estimated coefficients on the quartic hazards in the third column of table 5, but the sum of the terms in each of the transition hazards is significantly different from zero.

These results indicate that taking account of the sample selection issue will be important when simulating the effect of tax changes but it does not change the interpretation of the partial user cost elasticity of demand. Interestingly, previous research has generally failed to find that a standard Heckman correction either affects the estimates of the other coefficients or enters the demand equation significantly.[52]

5.3. Adding entry hazards as a dynamic sample selection correction

The second panel in table 6 incorporates entry hazards. Since entry hazards are only observed for families with multiple consecutive residence spells and the initial spells are unusable, the sample size decreases substantially to 15,394.[53] To provide a new baseline, I

[52]Edin and Englund (1991) find that a sample selection correction using a cross-section of Swedish data does not enter with statistical significance. Henderson and Ioannides (1989) find mixed results with a two-step Heckman correction but find statistical significance when the selection and demand equations are estimated jointly using maximum likelihood.

[53]There is a second sample selection issue here -- if there is something unobservably different about families with more frequent multiple moves, the estimated parameters may not be generally applicable.

reestimate the demand regression without any sample selection correction. The estimated user cost coefficient for this sample is −0.52 (0.07), virtually the same as for the full sample. The other estimated coefficients change little.

As before, including linear hazard corrections has only a small effect on the other parameters. The pattern of the exit hazards is not different from the previous panel and the estimated coefficients on the "trade down" and to "renter hazards" are statistically significantly different from zero.

When the entry and exit hazards are included as quartics, the user cost coefficient drops 20 percent to −0.43 (0.05), the same estimated coefficient as with the larger sample and just the exit hazards. The estimated income coefficient rises to 0.70 (0.04).

The estimated coefficients on the entry hazards are what one would expect. In the second column of panel 2, families that had a higher hazard of trading up at the time of their move consume more housing in their current spell. The opposite is true for families that had a higher hazard of trading down. The hazard of renters becoming owners has no apparent effect on the amount of housing consumption in the current spell. Of the quartic hazards in the third column, all but the trade down exit and entry hazards are statistically significant.

5.4. Controlling for year effects

The first panel of table 7 compares the estimated user cost elasticities from the demand model excluding and including year effects. The results excluding year dummies are repeated from table 6. If year dummies are included in the regression, the point estimates increase in magnitude by about one third and the estimated standard errors triple. The effect of incorporating the sample selection correction has the same pattern as in the baseline specification and induces about a 25 percent decline in the estimated user cost coefficient, but the difference is not statistically significant. Including year dummies has little effect on the other estimated coefficients and the sample selection corrections enter with similar estimated coefficients as when year dummies are included.

5.5 Controlling for cross-sectional income variation

In the estimation so far, several types of variation have been used to identify the effect of user cost on the demand for owner-occupied housing. Two of these -- tax rates changing differentially by income class and differences in state tax rates -- are arguably clean sources of variation. The remaining variation, that some people have higher incomes than others in the cross-section, may be suspect. First, it may be difficult to separately identify tax rate effects from income effects. Additionally, the long-run elasticity concept estimated using a cross section may be quite different than the response to a policy change. Finally, if unobserved individual heterogeneity can be written as a nonlinear function of income, one would like to control for that.

In order to reduce the variation from cross section income differences, I include 17 real income dummies to control nonparametrically for a nonlinear relation between income and quantity of owner-occupied housing demanded. Thus the user cost elasticity is identified by variation within income class -- differences in user cost among families with the same income. Although some of the variation may still be due to the cross section, I expect that most of the user cost variation is now due to tax rate changes and differences in state tax rates. The results for user cost are reported in table 7. The estimated user cost elasticity declines only slightly from the baseline specification without year dummies, to -0.39 (0.03). Adding year dummies has no additional impact on the estimated coefficient. When the entry and exit hazards are included, they have the usual signs. Although the hazards come in significantly, they have no impact on the estimated user cost coefficient.

5.6. Using recent movers

Reestimating the model using a sample of recent movers will provide estimates of the effect of the parameters on the choice of C, the target level of housing consumption relative to housing demand. In addition, since a regression on movers alone is less efficient than using a sample of all homeowners, seeing if there is a difference between the estimates is an important sensitivity check.

Table 8 reports the estimates from running the baseline demand equation on a sample of recent movers. The estimated user cost elasticity, -0.42 (0.06), is lower than the -0.49 in the first column of table 7, but the size of the standard errors makes it hard to distinguish between them. Neither the linear nor the quartic entry hazard corrections have any effect on the point estimates and only the linear "trading up" and "trading down" coefficients enter statistically significantly. The exit hazard is not included since it is a constant — every family is in its first year of residence.

There is virtually no difference between the estimated user cost coefficient for recent movers and the corrected user cost coefficient on the full sample, -0.43, suggesting that the choice of the sample does not matter much when estimating the user cost elasticity of demand. There are several possible explanations for this result. At the time of a move, a family may choose C close to Q^*, either due to liquidity constraints, myopia, or that Q^* does not change much over time. Or, C is just a level shift of Q^* and is not affected differentially by user cost. In either case, this result is consistent with the small effect of sample selection on the user cost elasticity.

6. Simulation Results

In this section, the baseline mobility and housing demand results are combined to provide a dynamic view of the effect of a policy change on the home ownership rate and quantity of housing consumed. The dynamic effect of two policies are simulated: the elimination of the mortgage interest and property tax deductions such as might take place under a flat-tax system and the elimination of capital gains taxation on housing.

It should be emphasized from the start that these policy simulations reflect the *partial* equilibrium effects on housing consumption. While a general equilibrium treatment of a policy change would account for a possible offsetting price response and change in pre-tax interest rates, I assume those effects do not occur. Instead, these simulations are intended to make interpretation of the dynamic consumption response easier, lend insight into the behavior of the aggregate adjustment cost function $\kappa(\bullet)$, and demonstrate the behavior of part of a general equilibrium system.

The dynamic pattern of consumption response does provide some insight into the response of asset prices over time. While the estimates in section 5 detail the response of housing demand to a tax change, not all of the demand response translates into changes in consumption. One would expect a large asset price effect and a small consumption effect shortly after a tax shock and over time the consumption effect should grow and the asset price effect should diminish.

In each simulation, the same procedure is followed. First, a baseline projection is calculated for a 20 year period. Then the policy change is coded and a new projection is estimated. The difference between the two is the partial equilibrium effect due to the policy change.

To compute a projection, I use a cross-section of all the families in the sample in 1990. Each family is followed for twenty years. Each year, a uniform random number is drawn and matched with the estimated transition hazards from the mobility model to determine whether the family moves or remains in their house. If the family purchases a new house, the estimates from the housing demand model determine the quantity of house consumed. The family's duration in their residence is incremented if they do not move or reset to one if they do. All the other variables are held constant. This procedure is replicated 5,000 times to estimate each family's dynamic housing consumption behavior. The results are aggregated together to obtain a projection for the entire sample.

Since families are not aged and there are no "deaths" or "births" in the simulations, the results probably understate the true impact of a policy. Many of the largest effects come through renters transiting to owners, but once renters become owners it is unlikely that they return to renting. Without an annual crop of new first-time home buyers in the simulation, that channel for affecting housing decisions is minimized.

6.1. Elimination of the mortgage interest deduction

Figures 7A and 7B depict the dynamic housing consumption response to the elimination of the mortgage interest and property tax deductions. Eliminating the deductions in 1990, the sample year, constitutes about an 8 percent increase in user cost, from 0.122 to 0.132. The solid black line shows that absent price and interest rate changes the average house value for families that own declines by 2.5 percent within 20 years, but more than half the decline occurs in the first five years. The thick dashed line shows that the homeownership rate declines by more than one percent within four years, then rebounds slightly. On a base homeownership rate of 65 percent, the decline is nearly an entire percentage point -- a large drop relative to movements in the 1970s and 1980s. Combining quantity and homeownership, the average house value declines by about 3.5 percent after 20 years, a -0.44 elasticity of housing consumption.

In figure 7B, the changes in moving rates due to the elimination of the deductions is shown. The rate at which renters become owners drops about 6 percent and remains at that level, explaining the observed decline in homeownership rates. Owners are slightly less likely to become renters and as time passes and there is a smaller pool of owners, the number of moves out of ownership declines since there are fewer owners to move. The number of owners trading up and trading down fall as well.

6.2. Elimination of capital gains taxes on housing

Figures 8A and 8B repeat the exercise with the elimination of capital gains taxes on housing. The average statutory capital gains tax rate falls from 0.22 to 0 and the average capital gains liability drops from $5,537 to 0. In marked contrast to the findings in Burman et al (1996).

this policy has little effect on the housing market.[54] Average house value declines by 1 percent, due to the finding that capital gains keep families from trading down as much as they otherwise would have. The homeownership rate is virtually unchanged. The rate at which families trade down declines only 0.5 percent for about 14 years, then rebounds. The rate of owners becoming renters is virtually unchanged.

7. Conclusion

The tax code has a significant effect on both housing demand and housing consumption. For mobility, changes in user cost strongly affect the transition from renter to owner, similar to the finding in Engelhardt (1993) that higher house prices dissuade first-time homebuyers. I also find mixed evidence that higher user cost may reduce the likelihood that an owner trades up and increase the likelihood that an owner becomes a renter. After-tax income nearly always affects the likelihood of the moving transitions.

Higher capital gains liabilities are found to discourage owners from trading down or moving permanently from owner to renter. These estimated parameters are tightly measured, in contrast to the previous studies on capital gains and housing. The implied effect on the likelihood of moving is small, unlike the findings in Burman et al (1996).

Applying the dynamic sample selection corrections in this context met with limited success. Although the corrections are significantly correlated with the level of housing demand, including them does not statistically significantly affect the other point estimates. Part of the

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[54]Burman et al (1996) find that 4 percent of movers would choose to trade down rather than up if the capital gains tax on housing were eliminated, a 25 percent increase in the rate of trading down.

reason for this outcome is that housing consumption does not diverge much from housing demand over the residence spell.

The corrected user cost elasticities of demand range from -0.4 to -0.5 in the preferred specifications. These estimates are lower than in many early housing studies but are in line with results found in some recent papers. These lower estimated elasticities imply that the steady-state dead-weight loss only one third of the amount calculated in Poterba (1992). After-tax income elasticities are estimated to be around 0.6.

Finally, the dynamic response of housing consumption to eliminating the mortgage interest and property tax deductions is shown to reduce the average quantity of house consumed by 3.5 percent over 20 years and to lower the aggregate homeownership rate by almost one percentage point. This slow consumption adjustment implies a different revenue stream for the government than what has been previously assumed. Since consumption takes a while to adjust, any expected revenue gain or loss from a policy change would be mitigated.

The finding that housing consumption responds slowly and dynamically to changes in the tax code suggests several avenues for future research. Of primary importance is how slow housing consumption adjustment interacts with the asset price effect of a change in tax policy. Combining the demand estimation presented here with empirical work on housing supply elasticities and an asset-price model of the housing market could generate a more general equilibrium model of the housing market that could be used to project a time path for both price and consumption responses to a tax change. Distinguishing the degree to which a tax change is capitalized into house prices is important for several reasons. First, the estimates of the efficiency consequences of a tax change depend on the relative responses of demand and price.

The standard estimate of the dead-weight loss declines if prices capitalize tax changes rather than consumption adjusting, but Skinner (1996) points out that a sudden change in asset prices creates its own distortion. Second, the incidence of the tax change may depend on the degree to which prices capitalize a tax change since an increase in housing prices, for example, would represent a windfall for the homeowning portion of the population.

Delving further into families' moving behavior would also be informative. Since families lose utility from being further from their desired housing consumption, policies that reduce the costs to moving, such as eliminating capital gains on housing or increasing the deductibility of moving costs, may create efficiency gains. Quantifying these effects requires estimating a utility-based model of moving behavior.

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Table 1: Sample Construction

	Observations		Spells	
	Minus	Total	Minus	Total
Original number:		**80,787**		**22,822**
Questions not asked in some years, data not imputable	2,261	78,526	701	22,121
Families with no moves over 22 years	374	78,152	17	22,104
Other left- and right-censored spells	16,361	61,791	1,772	20,332
Left-censored spells	8,166	53,625	1,744	18,588
Last year observed for a family	6,093	47,532	2,323	16,265
Spells with gaps	336	47,196	129	16,136
Spells with missing values for user cost	12	47,184	2	16,134
Spells with missing or zero values for income	1,056	46,128	154	15,980
Final number:	**34,659**	**46,128**	**6,994**	**15,980**
Owners:	26,930	25,051		
Renters:	7,729	21,007		

Table 2: Sample Means

Variable	Entire Sample	Owners	Renters
Age of Head	39.4 (16.1)	42.6 (14.9)	35.7 (16.6)
Number of Kids	0.95 (1.18)	1.13 (1.21)	0.72 (1.11)
Percent Owners	0.54 (0.50)		
Years Since Move	3.9 (3.8)	5.24 (4.30)	2.4 (2.3)
After-tax Income	29,788 (23,580)	37,023 (27,185)	21,189 (14,227)
Total Positive Inc.	38,734 (38,383)	49,032 (45,937)	26,496 (20,988)
User Cost	0.098 (0.024)	0.095 (0.023)	0.101 (0.026)
Wasted Deduction	665 (423)	726.6 (446.2)	592 (382)
Marginal Tax Rate	0.22 (0.24)	0.26 (0.11)	0.16 (0.33)
% Δ User Cost	5.1 (21.0)	7.9 (25.0)	2.0 (14.1)
% Δ Income	2.5 (38.1)	2.9 (39.7)	2.1 (36.2)
Δ Wasted Deduction	-30.5 (280.3)	-46.9 (335.6)	-11.1 (193.3)
House Value		89,725 (77,208)	
Property Tax Rate		1.3 (1.8)	
LTV Ratio		0.43 (0.38)	
Deductible Interest		3,471 (3,936)	
Foregone Equity		5,052 (5,923)	
CG Tax Liability if >0, % > 0	7,904 (14,757)	8,071 (14,996)	4,699 (8,414)
Filing Status:			
% Single	45.6	31.5	62.4
% Married	49.7	66.3	30.0
% Head of Household	4.7	2.2	7.7
# Observations	46,128	25,051	21,077

Notes: Standard deviations are in parentheses. All dollar amounts are in real 1990 dollars. Data are from the Panel Study of Income Dynamics, 1970-1991. The construction of user cost, total income, and wasted deduction is described in the text. LTV ratio is the size of the homeowner's outstanding mortgage as a percentage of the value of the house. Deductible interest is the dollar amount of mortgage interest that the homeowner could claim as an itemized deduction. Foregone equity is the opportunity cost of investing capital in a house rather than in 7-year Treasury bills.

Table 3: The Impact of Covariates on Transition Hazards

	No Year Dummies				Year Dummies			
	Renters	Owners			Renters	Owners		
	To Owner	Trade Up	Trade Dn	To Renter	To Owner	Trade Up	Trade Dn	To Renter
% Δ User Cost	-1.16 (0.21)	-0.37 (0.16)	-0.35 (0.26)	-0.05 (0.20)	-0.90 (0.24)	0.19 (0.19)	-0.19 (0.31)	-0.05 (0.24)
% Δ Income	0.33 (0.07)	0.76 (0.11)	-0.02 (0.14)	-0.34 (0.10)	0.36 (0.07)	0.84 (0.11)	0.01 (0.15)	-0.35 (0.11)
CG Tax Liability[1]	0.035 (0.009)	-0.005 (0.007)	-0.021 (0.007)	-0.024 (0.012)	0.038 (0.009)	-0.003 (0.007)	-0.020 (0.007)	-0.026 (0.012)
Δ Excess Deduction[1]	-0.07 (0.15)	-0.55 (0.11)	-0.05 (0.18)	-0.16 (0.14)	-0.07 (0.15)	-0.44 (0.12)	-0.05 (0.19)	-0.10 (0.15)
Head of Household	-0.73 (0.29)	-0.17 (0.45)	-0.14 (0.73)	1.50 (0.29)	-0.76 (0.29)	-0.17 (0.45)	-0.22 (0.74)	1.46 (0.29)
Now Married	0.48 (0.24)	1.01 (0.27)	0.97 (0.43)	0.17 (0.47)	0.51 (0.24)	1.03 (0.27)	0.96 (0.43)	0.07 (0.47)
Now Single	-0.58 (0.43)	-0.46 (0.45)	1.36 (0.36)	2.73 (0.23)	-0.50 (0.43)	-0.34 (0.46)	1.38 (0.37)	2.71 (0.23)
Log($\mathcal{L}$)	-7.306	-4.145	-2.232	-3.700	-7.282	-4.091	-2.211	-3.646

Notes: Standard errors are in parentheses. N=25,051 for owners, 21,077 for renters. Estimates are calculated using a semiparametric method due to Han-Hausman/Sueyoshi/Meyer from PSID data spanning 1970-1991. See text for details. Changes are measured relative to the first year of residence. Age dummies for five-year intervals, the change in the number of children in the family, the change in whether the family has children, and dummies for the change in retirement status are included but not reported. Total capital gains is included for owners and a dummy variable for renters that were owners in the previous spell and that have not exited the capital gains rollover period are included but not reported. The first panel includes the national unemployment rate and the second panel includes a complete set of year dummies. The omitted filing status category is "No change in filing status." [1]Capital gains tax liability and the change in the excess deduction are measured in thousands.

Table 4: The Effect of Covariates on the Likelihood of Moving

	Renters	Owners		
	To Owner	Trade Up	Trade Down	To Renter
Average hazard rate:[1]	12.93	4.32	1.90	3.92
A 10 percent increase in user cost leads to a...				
% Δ in the avg. hazard rate	-9.99	-3.48	-3.43	-0.47
Δ in the avg. hazard rate	-1.29	-0.15	-0.06	-0.02
Δ in the probability of moving in the first 5 years	-3.80	-0.70	-0.33	-0.09
A 10 percent increase in after-tax income leads to a...				
% Δ in the avg. hazard rate	3.02	7.64	-0.15	-3.23
Δ in the avg. hazard rate	0.39	0.33	0.00	-0.12
Δ in the probability of moving in the first 5 years	1.11	1.51	-0.01	-0.61
A 10 percent increase in capital gains tax liability leads to a...				
% Δ in the avg. hazard rate	1.28	-0.27	-1.54	-0.96
Δ in the avg. hazard rate	0.45	-0.01	-0.02	-0.03
Δ in the probability of moving in the first 5 years[2]	0.56	-0.04	-0.12	-0.16

Notes: Estimates are calculated as the difference in the average predicted hazard over the sample using the estimates reported in table 3. The hazard is the probability of moving at time t conditional on not having moved before that time. The capital gains numbers reported are for only those families with positive capital gains tax liabilities. [1]The average hazard rate is reported for the entire sample. Renters with positive capital gains tax liabilities have a 35.19 average hazard rate for purchasing a house. [2]For renters, the figure reported is for moving within the first *two* years.

Table 5: The Effect of Alternate Specifications on the Estimated User Cost Coefficient

	Renters	Owners		
	To Owner	Trade Up	Trade Down	To Renter
Baseline Estimation (from table 3):				
No Year Effects	-1.16 (0.21)	-0.37 (0.16)	-0.35 (0.26)	-0.05 (0.20)
Year Effects	-0.90 (0.24)	0.19 (0.19)	-0.19 (0.31)	-0.05 (0.24)
Gamma Heterogeneity:				
No Year Effects	-1.09 (0.22)	-0.55 (0.15)	-0.50 (0.25)	0.30 (0.23)
Year Effects	-0.83 (0.60)	0.42 (0.24)	-0.13 (0.28)	0.18 (0.26)
Many Income Dummies:				
No Year Effects	-0.65 (0.20)	-0.44 (0.15)	-0.32 (0.26)	-0.02 (0.22)
Year Effects	-0.41 (0.21)	0.05 (0.18)	-0.16 (0.30)	-0.01 (0.26)
No Duration Dependence:				
No Year Effects	-1.29 (0.20)	-0.53 (0.15)	-0.64 (0.20)	-0.40 (0.17)
Year Effects	-1.03 (0.23)	0.06 (0.18)	-0.42 (0.27)	-0.44 (0.21)

Notes: Reported coefficients are for the percent change in user cost since the beginning of the residence spell. Standard errors are in parentheses. N=25,051 for owners, 21,077 for renters. Estimates are calculated using a semiparametric method due to Han-Hausman/Sueyoshi/Meyer from PSID data spanning 1970-1991. See text for details. The baseline hazard in the first three panels is estimated nonparametrically. All other variables from table 3 are included but not reported. The specifications without year effects include the national unemployment rate, otherwise a complete set of year dummies is included.

Table 6: Housing Demand Regressions Using Mobility First Stage to Correct for Sample Selection and Disequilibrium Bias

	Exit Hazards			Entry and Exit Hazards		
	None	Linear	Quartic	None	Linear	Quartic
Log User Cost	-0.49 (0.04)	-0.47 (0.04)	-0.44 (0.04)	-0.52 (0.07)	-0.48 (0.06)	-0.43 (0.05)
Log Income	0.61 (0.03)	0.65 (0.03)	0.68 (0.03)	0.61 (0.05)	0.65 (0.04)	0.70 (0.04)
CG Tax Liability[1]	0.019 (0.006)	0.019 (0.006)	0.017 (0.006)	0.017 (0.007)	0.016 (0.006)	0.014 (0.006)
Capital Gains[1]	0.003 (0.001)	0.003 (0.001)	0.003 (0.001)	0.004 (0.001)	0.004 (0.001)	0.004 (0.001)
Exit Hazards:						
Trade Up		-1.46 (0.47)	[10.56] {0.00}		-0.93 (0.55)	[5.38] {0.02}
Trade Down		-2.41 (0.79)	[3.12] {0.08}		-3.39 (0.96)	[0.64] {0.42}
To Renter		1.55 (0.28)	[21.57] {0.00}		1.60 (0.34)	[31.91] {0.00}
Entry Hazards:						
Trading Up					0.78 (0.48)	[4.64] {0.03}
Trading Down					-3.03 (1.04)	[0.96] {0.33}
Becoming Owner					0.02 (0.15)	[7.20] {0.01}
Adj. R2	0.3785	0.3847	0.3931	0.3806	0.3904	0.4025
N	25,051	25,051	25,051	15,394	15,394	15,394

Notes for Table 6: Standard errors are in parentheses and are corrected for heteroskedasticity and for the generated first step hazards. The χ^2 statistic for the test of the null hypothesis that the sum of the quartic terms is equal to zero is reported in square brackets. The corresponding p-value is reported in curly brackets. The capital gains tax liability and capital gains variables are set equal to the value at the end of the previous spell, if capital gains are observed, zero otherwise. The left-hand-side variable is log house value. In the first panel, the sample consists of annual observations on homeowning families from the PSID over the period 1970 to 1991. In the second panel, only families where two or more consecutive spells are observed are used and the first spell is discarded, so the period covers 1970-1991. Hazards are computed from the baseline mobility model excluding the capital gains variables. Age dummies for five-year intervals, race dummies, the number of children in the family, filing status dummies, a retired dummy, the national unemployment rate, a national house price index, and indicator variables for gender and the presence of children are included but not reported. [1]Capital gains and the corresponding tax liability are measured in thousands.

Table 7: The Effect of Controlling Nonparametrically for Time and Income on Housing Demand Regressions

	Exit Hazards			Entry and Exit Hazards		
	None	Linear	Quartic	None	Linear	Quartic
Baseline Estimation:						
No Year Effects	-0.49 (0.04)	-0.47 (0.04)	-0.44 (0.04)	-0.52 (0.07)	-0.48 (0.06)	-0.43 (0.05)
Year Effects	-0.71 (0.13)	-0.71 (0.11)	-0.64 (0.10)	-0.81 (0.19)	-0.78 (0.16)	-0.62 (0.15)
Many Income Dummies:						
No Year Effects	-0.41 (0.03)	-0.40 (0.03)	-0.39 (0.03)	-0.39 (0.04)	-0.38 (0.05)	-0.36 (0.05)
Year Effects	-0.38 (0.08)	-0.39 (0.08)	-0.39 (0.08)	-0.36 (0.10)	-0.37 (0.10)	-0.35 (0.10)

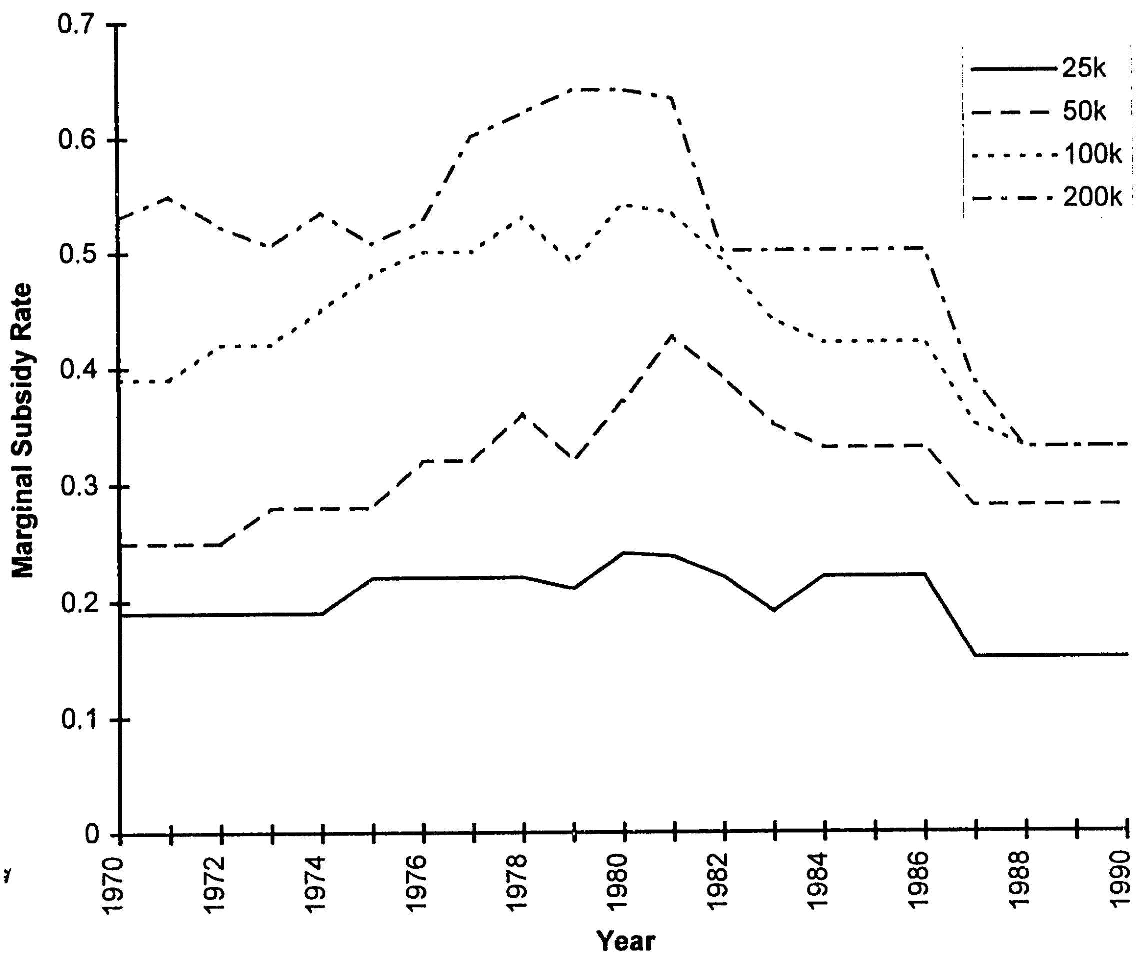
Notes: Standard errors are in parentheses and are corrected for heteroskedasticity and the generated first step hazards. The χ^2 statistic for the test of the null hypothesis that the sum of the quartic terms is equal to zero is reported in square brackets. The corresponding p-value is reported in curly brackets. The left-hand-side variable is log house value. In the first panel, the sample consists of annual observations on homeowning families from the PSID over the period 1970 to 1991. In the second panel, only families where two or more consecutive spells are observed are used and the first spell is discarded, so the period covers 1970-1991. The other covariates listed in table 5 are included.

Table 8: Housing Demand Regressions for Recent Movers
Using Mobility First Stage to Correct for Sample Selection and Disequilibrium Bias

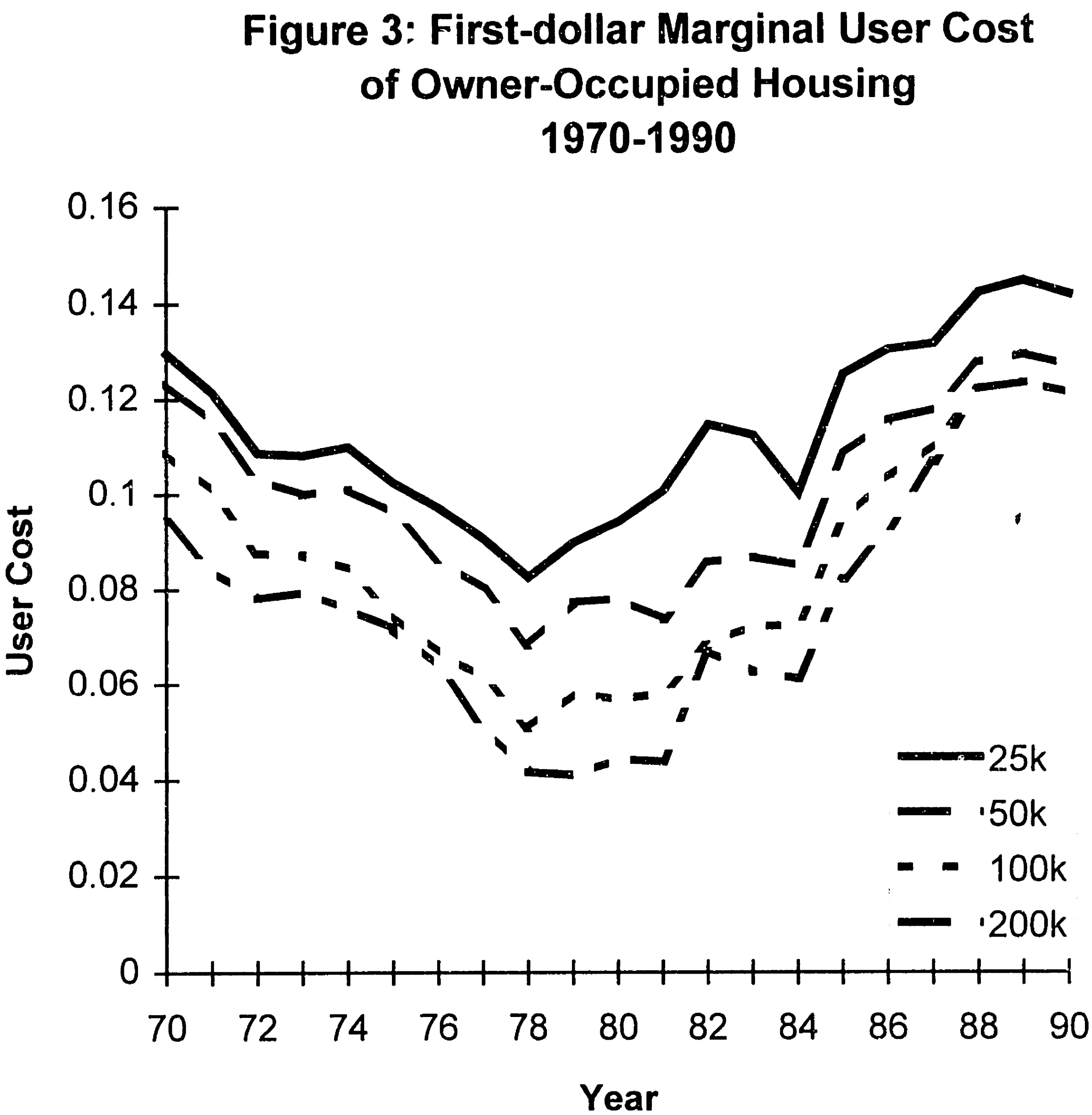
	Exit Hazard Sample	**Entry Hazard Sample**		
	None	None	Linear	Quartic
Log User Cost	-0.42 (0.06)	-0.43 (0.07)	-0.43 (0.07)	-0.42 (0.07)
Log Income	0.65 (0.02)	0.66 (0.03)	0.65 (0.03)	0.64 (0.03)
CG Tax Liability[1]	0.012 (0.004)	0.010 (0.004)	0.011 (0.004)	0.009 (0.004)
Capital Gains[1]	0.003 (0.001)	0.004 (0.001)	0.003 (0.001)	0.004 (0.001)
Entry Hazards:				
Trading Up			0.73 (0.36)	[1.66] {0.20}
Trading Down			-4.09 (1.10)	[0.03] {0.87}
Becoming Owner			-0.04 (0.16)	[0.00] {0.99}
Adj. R2	0.3641	0.3748	0.3780	0.3794
N	4,041	3,178	3,178	3,178

Notes: Standard errors, corrected for heteroskedasticity and generated hazards, are in parentheses. The χ^2 statistic for the test of the null hypothesis that the sum of the quartic terms is equal to zero is reported in square brackets. The corresponding p-value is reported in curly brackets. The capital gains tax liability and capital gains variables are set equal to the value at the end of the previous spell, if capital gains are observed, zero otherwise. The left-hand-side variable is log house value. In the first panel, the sample consists of annual observations on homeowning families in the first year of their residence spells from the PSID over the period 1970 to 1991. In the second panel, only families where two or more consecutive spells are observed are used and the first spell is discarded, so the period covers 1971-1991. Hazards are computed from a first stage mobility model estimated as in table 3 except excluding the capital gains variables. Age dummies for five-year intervals, race dummies, the number of children in the family, filing status dummies, a retired dummy, the national unemployment rate, a national level house price index, and indicator variables for gender and the presence of children are included but not reported.

Figure 2: First-dollar Marginal Subsidy Rates on Home Mortgage Interest for Several Example Families, 1970-1990



First-dollar subsidy rates on home mortgage interest deductions are reported for a married couple with two children. Real annual wage and salary income is allowed to vary from $25,000 to $200,000. The family also has an additional 10% of wage income in the form of taxable dividend or interest income. The families are assumed to be itemizers and, for the purposes of computing state taxes, are said to be living in Massachusetts.



First-dollar user costs are reported for a married couple with two children. Real annual wage and salary income is allowed to vary from $25,000 to $200,000. The family also has an additional 10% of wage income in the form of taxable dividend or interest income. The families are assumed to be itemizers and, for the purposes of computing state taxes, are said to be living in Massachusetts. The property tax rate is assumed to be 3%, the mortgage rate and interest rate on foregone interest income are both assumed to be 7%, and the house value is assumed to be three times wage and salary income. Expected house appreciation is a five-year moving average of the CPI.

Figure 5: Kaplan-Meier Empirical Hazards for Owners' Moving Transitions

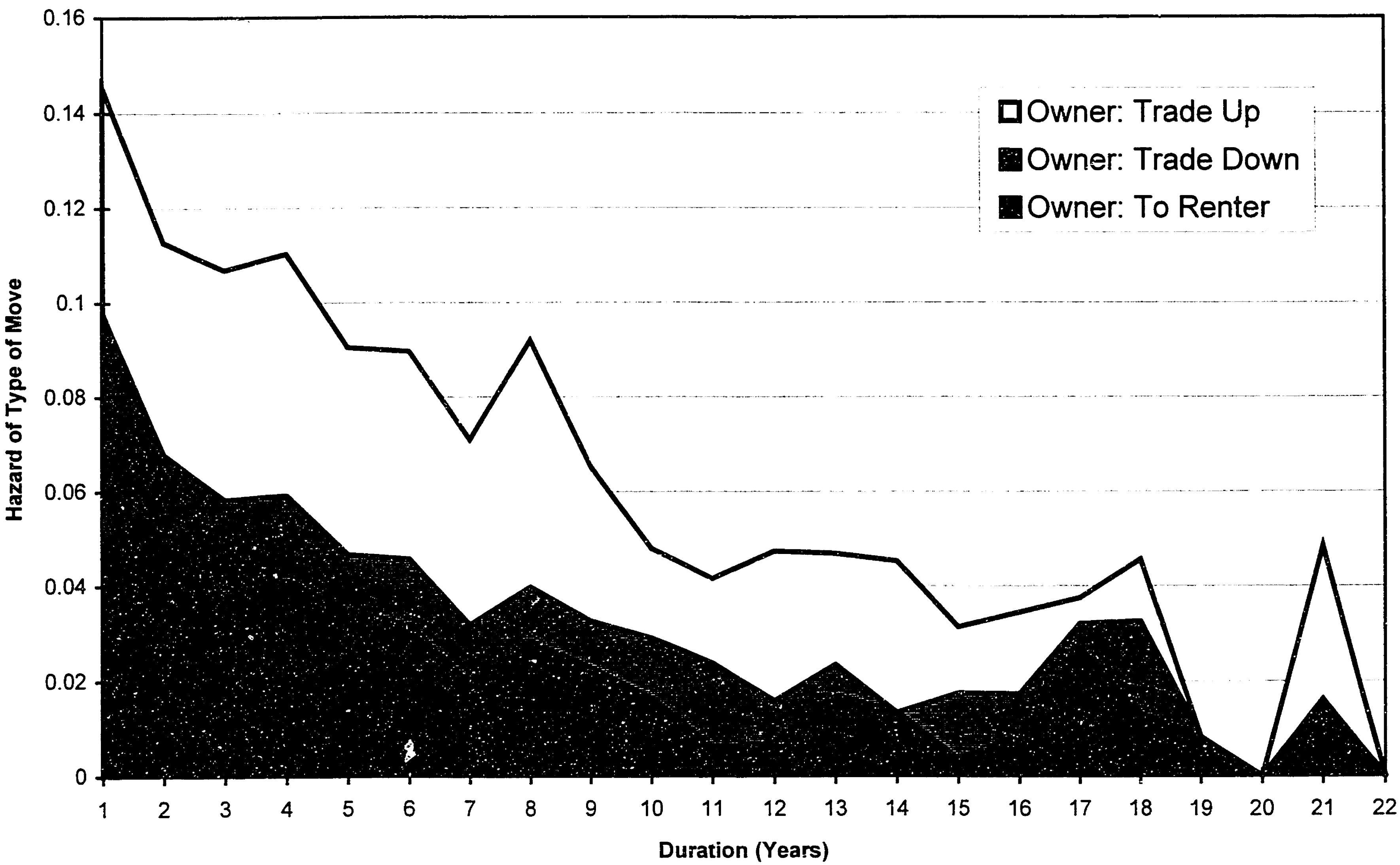


Figure 6: Kaplan-Meier Empirical Hazards for Renters' Moving Transitions

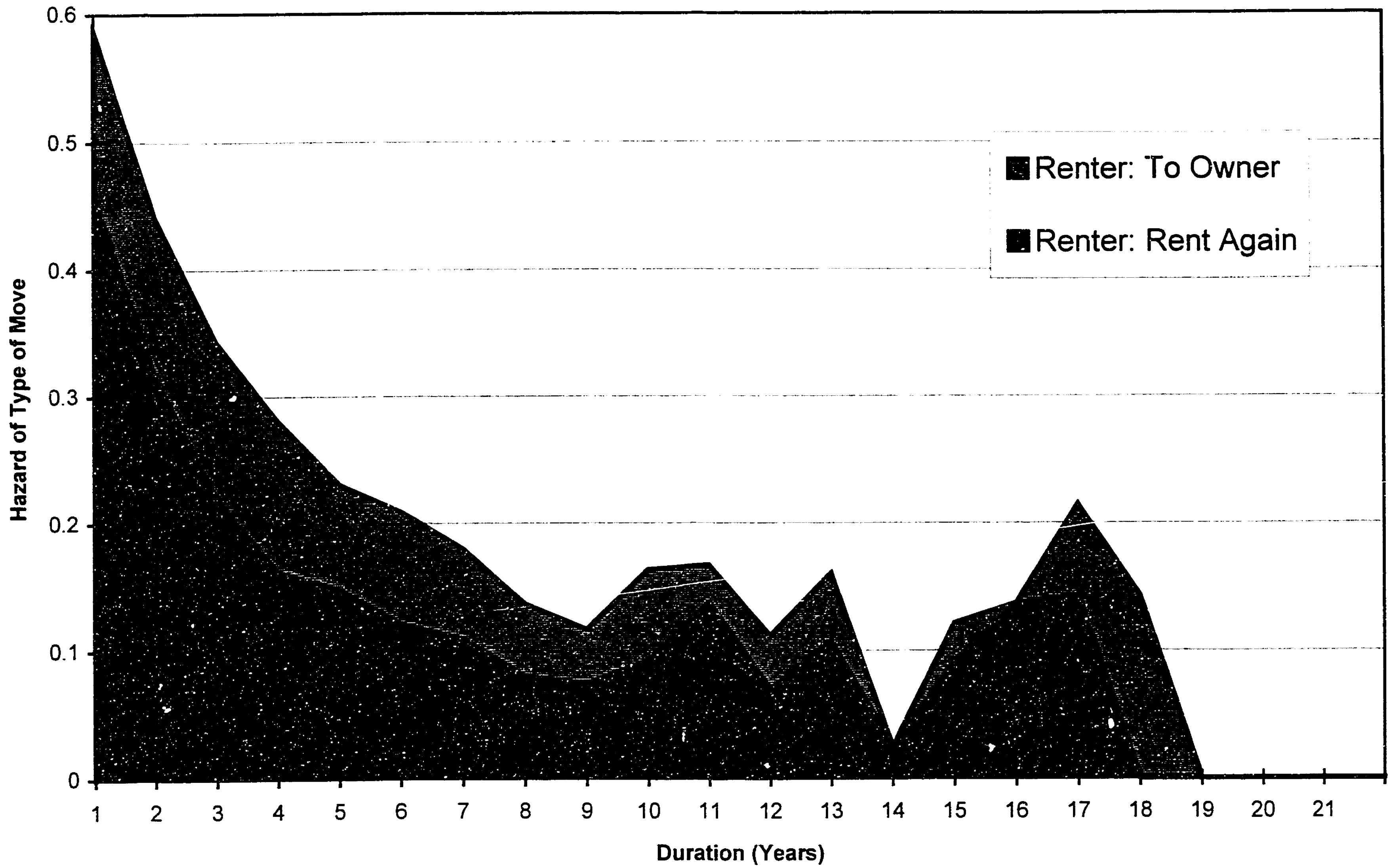


Figure 7A: Changes in Values and Percent Owners Each Year Due to Eliminating the Mortgage Interest and Property Tax Deductions

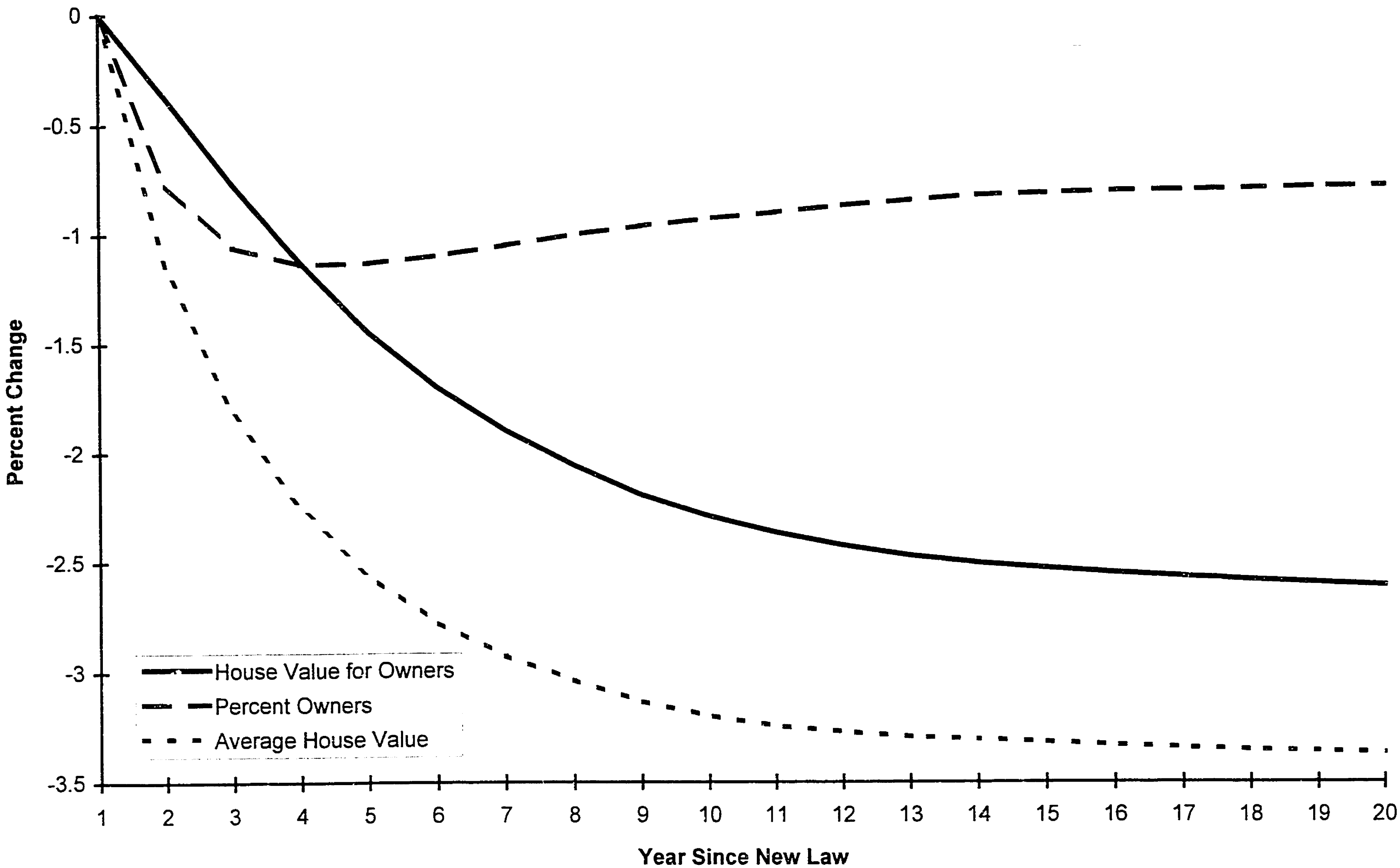


Figure 7B: Changes in Percent Moving Each Year Due to Eliminating the Mortgage Interest and Property Tax Deductions

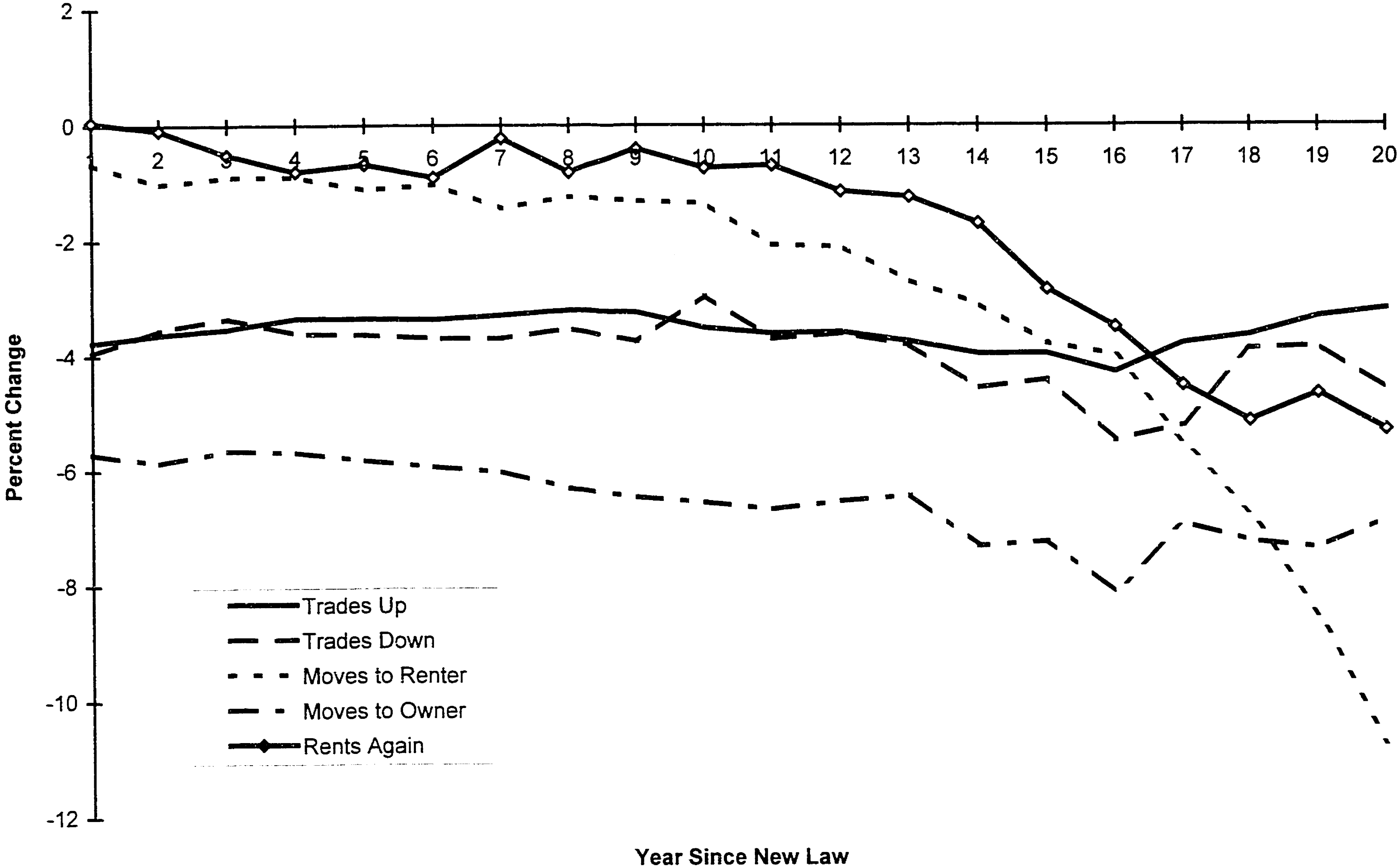


Figure 8A: Changes in House Values and Percent Owners Each Year Due to Eliminating the Capital Gains Tax on Owner-Occupied Housing

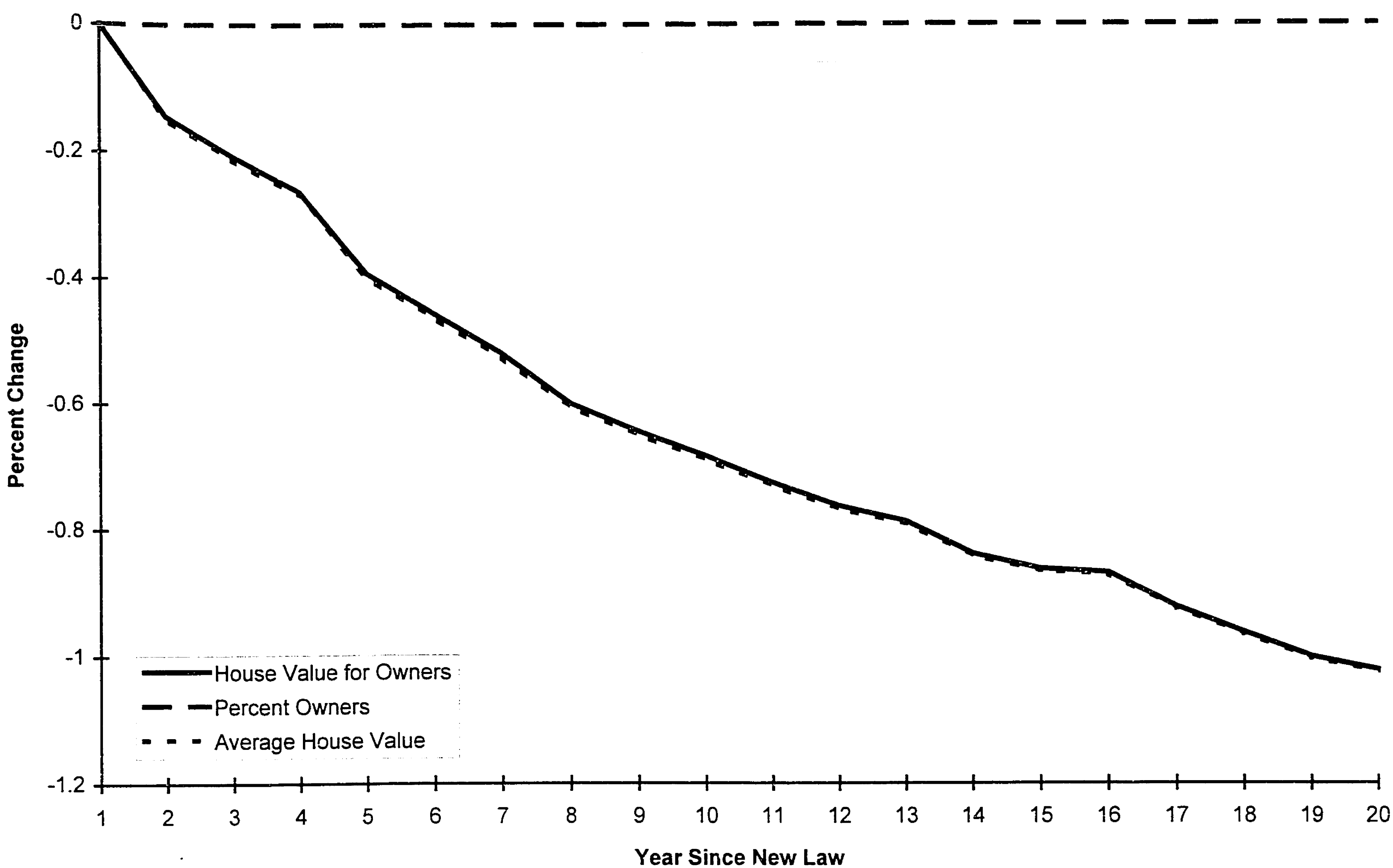
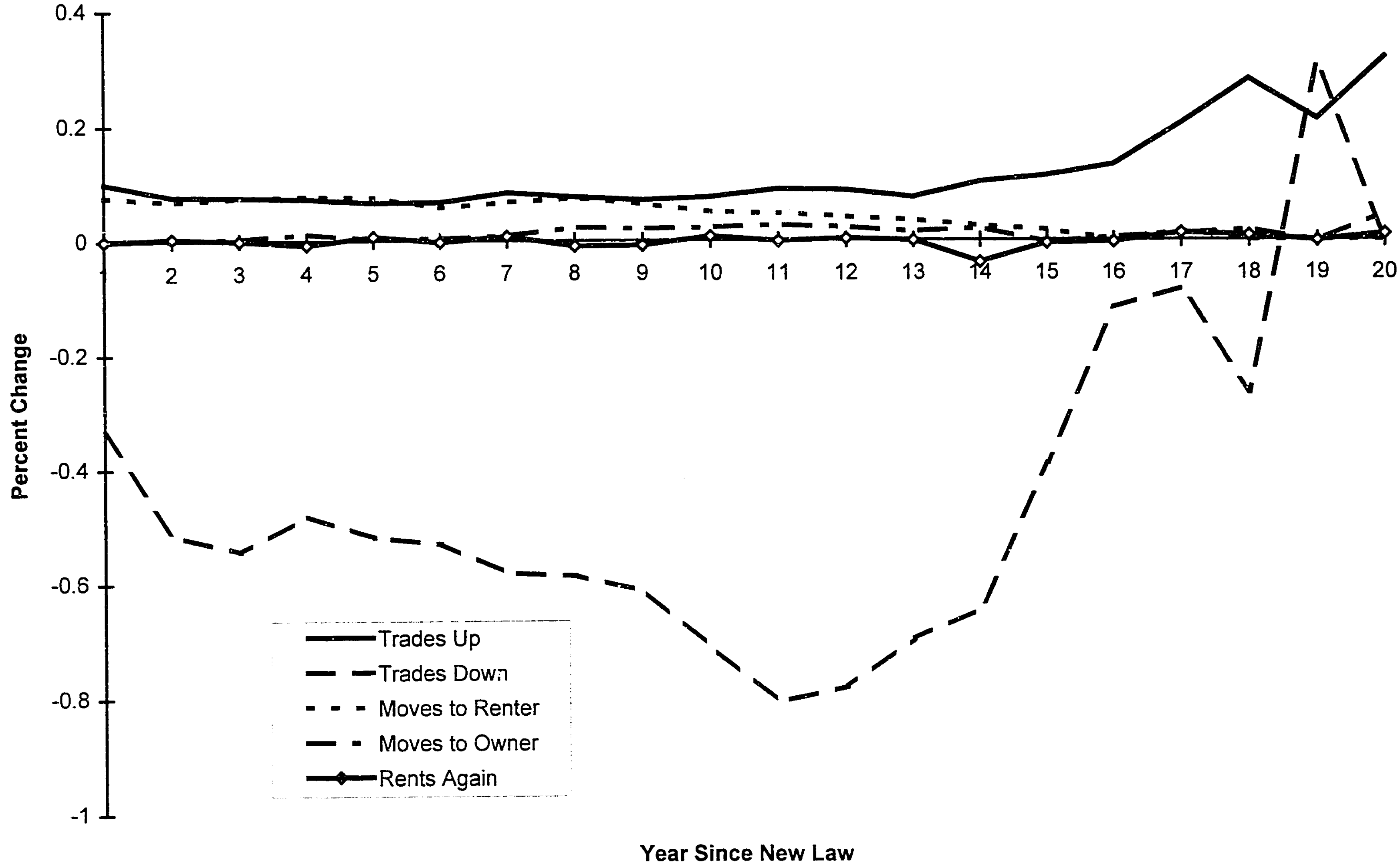


Figure 8B: Changes in Percent Moving Each Year Due to Eliminating the Capital Gains Tax on Owner-Occupied Housing



Appendix Table A: Sample Breakdown by Year

Year	Number of Observations	Percent Owners	Average Duration
1970	528	27.3	3.6
1971	884	32.9	4.5
1972	1,117	40.0	5.0
1973	1,264	40.1	5.9
1974	1,430	41.8	6.2
1975	1,550	42.7	6.3
1976	1,776	46.1	6.4
1977	1,911	48.4	6.6
1978	1,989	50.9	7.1
1979	2,163	54.5	7.2
1980	2,278	56.1	7.3
1981	2,361	57.1	7.5
1982	2,414	55.8	7.6
1983	2,487	55.9	7.5
1984	2,526	56.8	7.5
1985	2,592	57.4	7.4
1986	2,622	58.3	7.3
1987	2,654	58.5	7.1
1988	2,647	58.4	7.0
1989	2,641	59.7	6.9
1990	2,813	60.6	6.1
1991	2,877	60.5	5.5
Total:	**45,524**	**49.9**	**6.8**

Appendix Table B-1: Owners' Moving Transitions in PSID Data

Duration (Years)	Number of Owners Who: Trade Up	Trade Down	To Renter	Number of Owners Censored	Owners Risk Set
1	224	170	293	322	4,672
2	167	64	187	274	3,663
3	147	61	114	215	2,971
4	126	45	102	198	2,434
5	87	23	70	186	1,963
6	71	23	51	137	1,597
7	51	17	26	121	1,315
8	57	13	31	106	1,100
9	30	9	21	63	893
10	15	10	12	77	770
11	12	12	4	75	656
12	16	5	4	71	553
13	11	3	8	74	457
14	12	0	5	70	363
15	4	3	1	44	276
16	4	2	2	35	224
17	0	1	5	26	181
18	2	3	2	25	149
19	0	0	1	32	117
20	0	0	0	24	84
21	2	0	1	36	60
22	0	0	0	21	21
Total:	**1,038**	**464**	**940**	**2,230**	**24,519**

Notes: Calculated from PSID data spanning 1970-1991. See text for details.

Appendix Table B-2: Renters' Moving Transitions in PSID Data

Duration (Years)	Number of Renters Who Move: To Owner	Number of Renters Who Move: To Rental	Number of Renters Censored	Renters Risk Set
1	1,427	5,137	543	11,171
2	511	1,287	255	4,064
3	248	441	137	2,011
4	142	192	90	1,185
5	63	113	66	761
6	46	63	46	519
7	26	40	37	364
8	15	21	39	261
9	8	14	26	186
10	10	13	16	138
11	3	14	13	99
12	3	5	8	69
13	3	6	6	53
14	0	1	6	38
15	1	3	6	31
16	0	3	5	21
17	1	2	4	13
18	0	0	0	6
19	0	0	1	6
20	0	0	1	5
21	0	0	4	4
Total:	**2,507**	**7,355**	**1,309**	**21,005**

Notes: Calculated from PSID data spanning 1970-1991. See text for details.

Appendix Table C: The Effect of Right- and Left-Censoring on the Estimated Tax Parameters in the Mobility Model

	Renters	Owners		
	To Owner	Trade Up	Trade Down	To Renter
Left-censored before 1975:				
% Δ User Cost	-1.00 (0.27)	-0.19 (0.25)	-0.62 (0.44)	-0.29 (0.28)
% Δ Income	0.37 (0.08)	0.85 (0.14)	-0.14 (0.10)	-0.46 (0.14)
CG Tax Liability[1]	0.035 (0.009)	-0.007 (0.007)	-0.017 (0.007)	-0.034 (0.014)
Δ Excess Deduction[1]	-0.14 (0.18)	-0.58 (0.12)	-0.10 (0.22)	-0.12 (0.15)
Log($\mathcal{L}$)	-5,685	-3,232	-1,697	-2,970
Observations:	16,665	18,338	18,338	18,338
Right-censored after 8 years duration:				
% Δ User Cost	-1.12 (0.22)	-0.52 (0.19)	-0.48 (0.32)	-0.23 (0.22)
% Δ Income	0.34 (0.07)	0.69 (0.11)	0.08 (0.16)	-0.34 (0.11)
CG Tax Liability[1]	0.034 (0.009)	-0.006 (0.007)	-0.020 (0.009)	-0.025 (0.013)
Δ Excess Deduction[1]	-0.08 (0.15)	-0.46 (0.12)	-0.05 (0.19)	-0.08 (0.15)
Log($\mathcal{L}$)	-7,188	-3,656	-1,967	-3,354
Observations:	20,386	20,092	20,092	20,092

Notes: Standard errors are in parentheses. Estimates are calculated using a semiparametric method due to Han-Hausman/Sueyoshi/Meyer from PSID data spanning 1970-1991. In the first panel, residence spells begun before 1975 are deleted. In the second panel, all spells are artificially right-censored after 8 years duration. Changes are measured relative to the first year of residence. Age dummies for five-year intervals, the change in the number of children in the family, the change in whether the family has children, filing status dummies, the national unemployment rate, and dummies for the change in retirement status are included but not reported. Total capital gains is included for owners and a dummy variable for renters that were owners in the previous spell and that have not exited the capital gains rollover period are included but not reported. [1]Capital gains tax liability and the change in the excess deduction are measured in thousands.

Appendix Table D: Complete Set of Estimates from the Baseline Mobility Model

	Renters		Owners	
	To Owner	Trade Up	Trade Down	To Renter
% Δ User Cost	-1.16	-0.37	-0.35	-0.26
	(0.21)	(0.16)	(0.26)	(0.17)
% Δ Income	0.33	0.76	-0.02	-0.17
	(0.07)	(0.11)	(0.14)	(0.27)
CG Tax Liability (000s)	0.035	-0.005	-0.021	-0.024
	(0.009)	(0.007)	(0.007)	(0.012)
Capital Gains (000s)		-0.002	0.007	-0.001
		(0.001)	(0.001)	(0.002)
Searching Dummy	1.15			
	(0.07)			
Δ Wasted Ded (000s)	0.00	-0.55	-0.05	-0.16
	(0.00)	(0.11)	(0.18)	(0.14)
Head of Household	-0.73	-0.17	-0.14	1.50
	(0.28)	(0.45)	(0.73)	(0.29)
Now Married	0.48	1.01	0.97	0.17
	(0.24)	(0.27)	(0.43)	(0.46)
Now Single	-0.58	-0.46	1.36	2.73
	(0.43)	(0.45)	(0.36)	(0.23)
Age Dummies:				
25 to 29	0.36	-0.09	-0.68	-0.63
	(0.06)	(0.14)	(0.21)	(0.13)
30 to 34	0.40	-0.26	-0.37	-0.60
	(0.07)	(0.14)	(0.20)	(0.13)
35 to 39	0.34	-0.25	-0.42	-0.74
	(0.09)	(0.15)	(0.21)	(0.15)
40 to 44	0.30	-0.30	-0.30	-0.60
	(0.11)	(0.16)	(0.23)	(0.16)
45 to 49	0.23	-0.75	0.07	-1.15
	(0.13)	(0.20)	(0.25)	(0.20)
50 to 54	0.14	-0.78	-0.07	-0.85
	(0.14)	(0.22)	(0.25)	(0.21)
55 to 59	0.14	-0.90	-0.18	-0.94
	(0.15)	(0.25)	(0.26)	(0.22)
60 to 64	0.04	-1.08	0.11	-1.53
	(0.16)	(0.26)	(0.25)	(0.27)
65 to 69	-0.49	-1.26	-0.82	-1.42
	(0.20)	(0.29)	(0.36)	(0.28)
70 to 74	-0.61	-1.01	-0.37	-1.11
	(0.21)	(0.31)	(0.34)	(0.27)
75 and Over	-0.79	-0.82	0.13	-0.37
	(0.16)	(0.27)	(0.27)	(0.19)

Now Retired	0.21	0.24	0.44	0.35
	(0.24)	(0.27)	(0.28)	(0.24)
No Longer Retired	0.04	-0.53	-1.36	0.90
	(0.30)	(0.57)	(1.01)	(0.32)
Now Has Children	0.42	0.53	0.01	-0.26
	(0.15)	(0.12)	(0.25)	(0.17)
No Longer Has Children	-0.33	0.27	0.10	-0.17
	(0.28)	(0.21)	(0.26)	(0.27)
Δ # of children	0.23	0.14	0.04	0.12
	(0.07)	(0.05)	(0.08)	(0.08)
Unemploy. Rate	-0.05	0.04	0.02	0.06
	(0.02)	(0.02)	(0.04)	(0.02)
Baseline Hazard:				
1	0.301	0.050	0.038	0.075
	(0.033)	(0.010)	(0.008)	(0.016)
2	0.284	0.044	0.018	0.058
	(0.020)	(0.009)	(0.004)	(0.012)
3	0.315	0.047	0.020	0.044
	(0.021)	(0.010)	(0.004)	(0.009)
4	0.306	0.049	0.018	0.051
	(0.023)	(0.010)	(0.004)	(0.009)
5	0.226	0.042	0.018	0.044
	(0.026)	(0.010)	(0.003)	(0.008)
6	0.249	0.042	0.013	0.042
	(0.024)	(0.010)	(0.003)	(0.008)
7	0.221	0.038	0.011	0.026
	(0.025)	(0.009)	(0.003)	(0.006)
8	0.191	0.051	0.010	0.039
	(0.024)	(0.012)	(0.002)	(0.009)
9	0.163	0.033	0.008	0.033
	(0.027)	(0.009)	(0.002)	(0.008)
10	0.280	0.020	0.010	0.024
	(0.037)	(0.007)	(0.003)	(0.008)
11	0.137	0.019	0.015	0.009
	(0.019)	(0.007)	(0.004)	(0.004)
12	0.208	0.035	0.007	0.012
	(0.028)	(0.011)	(0.002)	(0.005)
13	0.032	0.003	0.000	0.001
	(0.005)	(0.001)	(0.000)	(0.001)

Notes: Standard errors are in parentheses. N=25,051 for owners, 21,077 for renters. Estimates are calculated using a semiparametric method due to Han-Hausman/Sueyoshi/Meyer from PSID data spanning 1970-1991. Changes are measured relative to the first year of residence. The omitted categories for filing status, retirement, and has kids are each "no change." The omitted age category is "20-24." Baseline hazard coefficients are transformed to reflect hazard rates and their standard errors are computed using the delta method.

Appendix Table E: Complete Set of Estimates from the Demand Model

	Exit Hazards			Entry and Exit Hazards		
	None	Linear	Quartic	None	Linear	Quartic
Log User Cost	-0.49 (0.04)	-0.47 (0.04)	-0.44 (0.04)	-0.52 (0.07)	-0.48 (0.06)	-0.43 (0.05)
Log Income	0.61 (0.03)	0.65 (0.03)	0.68 (0.03)	0.61 (0.05)	0.65 (0.04)	0.70 (0.04)
CG Tax Liability[1]	0.019 (0.006)	0.019 (0.006)	0.017 (0.006)	0.017 (0.007)	0.016 (0.006)	0.014 (0.006)
Capital Gains[1]	0.003 (0.001)	0.003 (0.001)	0.003 (0.001)	0.004 (0.001)	0.004 (0.001)	0.004 (0.001)
Married	-0.04 (0.03)	0.00 (0.03)	-0.01 (0.03)	-0.05 (0.03)	0.01 (0.04)	0.00 (0.04)
Head of Household	-0.21 (0.08)	-0.24 (0.08)	-0.33 (0.08)	-0.33 (0.08)	-0.34 (0.07)	-0.40 (0.08)
Retired	0.11 (0.04)	0.13 (0.04)	0.14 (0.04)	0.08 (0.07)	0.11 (0.07)	0.12 (0.07)
Age dummies:						
25 to 29	0.39 (0.04)	0.42 (0.04)	0.50 (0.04)	0.39 (0.05)	0.36 (0.06)	0.43 (0.06)
30 to 34	0.52 (0.04)	0.54 (0.05)	0.63 (0.05)	0.47 (0.06)	0.45 (0.06)	0.52 (0.07)
35 to 39	0.58 (0.05)	0.61 (0.05)	0.73 (0.05)	0.54 (0.06)	0.53 (0.07)	0.63 (0.07)
40 to 44	0.61 (0.05)	0.62 (0.05)	0.73 (0.06)	0.55 (0.06)	0.53 (0.07)	0.63 (0.08)
45 to 49	0.60 (0.05)	0.63 (0.06)	0.74 (0.07)	0.53 (0.07)	0.56 (0.08)	0.66 (0.09)
50 to 54	0.62 (0.05)	0.63 (0.06)	0.70 (0.06)	0.47 (0.07)	0.51 (0.08)	0.58 (0.09)
55 to 59	0.68 (0.05)	0.70 (0.06)	0.77 (0.07)	0.57 (0.08)	0.62 (0.09)	0.70 (0.10)
60 to 64	0.67 (0.06)	0.71 (0.06)	0.81 (0.07)	0.47 (0.08)	0.56 (0.09)	0.67 (0.11)
65 to 69	0.69 (0.06)	0.67 (0.07)	0.72 (0.08)	0.53 (0.10)	0.54 (0.11)	0.61 (0.12)
70 to 74	0.66 (0.07)	0.65 (0.08)	0.70 (0.08)	0.44 (0.12)	0.44 (0.13)	0.53 (0.14)
75 and Over	0.67 (0.07)	0.67 (0.08)	0.67 (0.08)	0.49 (0.11)	0.52 (0.11)	0.58 (0.13)
Unemploy. Rate	-0.010 (0.004)	-0.011 (0.005)	-0.012 (0.004)	-0.013 (0.006)	-0.014 (0.007)	-0.014 (0.006)
National Price Index	0.004 (0.001)	0.003 (0.001)	0.002 (0.001)	0.004 (0.001)	0.002 (0.001)	0.002 (0.001)

Gender	-0.01	0.04	0.05	0.10	0.17	0.19
	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)
Race:						
Black	-0.21	-0.21	-0.20	-0.19	-0.18	-0.17
	(0.05)	(0.05)	(0.05)	(0.08)	(0.08)	(0.08)
Hispanic/	0.21	0.21	0.21	0.11	0.11	0.11
Other	(0.06)	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)
Have	0.005	0.018	0.024	0.025	0.024	0.030
Children	(0.028)	(0.028)	(0.028)	(0.033)	(0.033)	(0.033)
# of	-0.004	-0.007	-0.007	-0.020	-0.024	-0.024
Children	(0.012)	(0.012)	(0.012)	(0.014)	(0.013)	(0.013)
Constant	2.75	2.59	2.26	2.73	2.71	2.08
	(0.28)	(0.28)	(0.28)	(0.41)	(0.39)	(0.43)
Exit Hazards:						
Trade Up		-1.46	-9.98		-0.93	-8.80
		(0.47)	(1.91)		(0.55)	(8.27)
Trade Up^2			81.50			74.43
			(19.99)			(23.17)
Trade Up^3			-238.47			-213.38
			(76.58)			(87.45)
Trade Up^4			219.29			189.34
			(85.10)			(95.44)
Trade Down		-2.41	6.43		-3.39	6.03
		(0.79)	(7.25)		(0.96)	(8.27)
Trade $Down^2$			-459.50			-401.99
			(299.38)			(361.05)
Trade $Down^3$			7,128.34			5,396.59
			(4,775.36)			(5,965.90)
Trade $Down^4$			-36,087.55			-23,018.12
			(23,702.93)			(30,556.28)
To Renter		1.55	6.70		1.60	5.68
		(0.28)	(1.06)		(0.34)	(1.22)
To $Renter^2$			-18.35			-12.45
			(7.38)			(8.47)
To $Renter^3$			9.68			-0.66
			(17.34)			(19.41)
To $Renter^4$			7.69			13.83
			(11.96)			(12.76)

Entry Hazards:	
Trade Up	2.38
	(2.50)
Trade Up^2	-11.86
	(20.73)
Trade Up^3	35.11
	(58.06)
Trade Up^4	-39.41
	(49.25)
Trade Down	2.88
	(18.99)
Trade Down^2	-331.22
	(848.46)
Trade Down^3	6,370.18
	(14,313.68)
Trade Down^4	-33,718.14
	(75,617.81)
To Owner	1.51
	(2.16)
To Owner^2	-2.70
	(13.03)
To Owner^3	-8.99
	(30.93)
To Owner^4	19.24
	(24.46)

Notes: Standard errors are in parentheses and are corrected for heteroskedasticity and for the generated first step hazards. The capital gains tax liability and capital gains variables are set equat to the value at the end of the previous spell, if capital gains are observed, zero otherwise. The left-hand-side variable is log house value. In the first panel, the sample consists of annual observations on homeowning families in the PSID over the period 1970 to 1991. In the second panel, only familes where two or more consecutive spells are observed are used and the first spell is discarded, so the period covers 1970-1991. Hazards are computed from the baseline model estimated without the capital gains variables. The omitted filing status category is "single," the omitted race category is "white," and the omitted age category is "20-24." N=25,051 in the first panel and 15,394 in the second. [1]Capital gains and the corresponding tax liability are measured in thousands.

Chapter 2:

Are Tax Reforms
Capitalized into House Prices?

Although the effects of aggregate house price changes on the economy may be dramatic, the empirical link between U.S. tax policy and the price of housing is not well understood. Owner-occupied housing is subsidized in the U.S. relative to other assets since the return on the house — the rent an owner would charge someone to live in his house instead of living there himself — is untaxed and mortgage interest and property taxes are deductible from taxable income. In addition, due to generous roll-over and exclusion provisions, the capital gain on a house is virtually untaxed. If the favorable tax treatment of owner-occupied housing were scaled back, the additional expense may be capitalized into a lower asset price.

Determining how changes in the tax treatment of owner-occupied housing are capitalized into house prices is important because systemic movements in the price of owner-occupied housing can have substantial effects, both on families' balance sheets and in terms of economic efficiency. The median family of retirement age has 40 percent of its net worth tied up in its home and younger families hold an even larger proportion of their wealth in their houses. An unexpected house price decline could significantly reduce a highly-leveraged young homeowner's wealth and also decrease his mobility if his house price falls enough that he cannot afford to pay off his mortgage in order to move. [Stein (1993), Chan (1995)] For older families, the decrease in wealth accompanying a house price decline comes at a time when they cannot increase labor earnings or savings to offset it. [Hoynes and McFadden (1994)] Changes in house prices also affect whether families become first-time homebuyers and may influence the decision to save for a down payment. [Engelhardt (1994)]

The efficiency loss from the tax subsidy to owner-occupied housing is sensitive to the house price elasticity. Since changes in house prices are transfers of wealth from current homeowners to or from future ones, an aggregate price shift may have a potentially significant effect on dead-weight loss because the government uses distortionary taxes to raise revenue and the return on housing is untaxed. Skinner (1996) points out that an increase in income tax rates, which would raise house prices, would increase dead-weight loss since current homeowners would receive a windfall capital gain that is virtually untaxed in the current system while future homeowners would save proportionally more through houses since they are more expensive. Since returns on houses are untaxed, this implies that the government's tax base dwindles in the future, lowering efficiency. Skinner estimates that the efficiency loss from the current tax system through this channel alone is $120 billion if the price elasticity of housing with respect to changes in the tax code is equal to one. This particular efficiency loss would decline with the capitalization rate.

During the 1970s and 1980s, several macroeconomic and legislative changes affected the value of the subsidy to owner-occupied housing: high inflation during the 1970s, combined with marginal tax rate brackets that were not indexed for inflation, led to marginal tax rates ratcheting upwards for most families as nominal incomes rose; legislative changes during the 1980s reduced marginal tax rates, especially for high income taxpayers, and raised standard deductions, reducing the number of families that itemized on their tax returns.

While many studies [e.g., Rosen (1979)] have shown that changes in this subsidy affect the demand for owner-occupied housing, it is not clear how much of the response reflects a change in the asset price of the house versus a change in the quantity of real housing consumed. Capozza, Green, and Hendershott (1996), using pooled-cross-section variation in the net-of-tax mortgage rate and property tax rates across 63 MSAs in 1970, 1980 and 1990, contend that any change to the subsidy would be fully capitalized into the asset price of housing. Abraham and Hendershott (1994) find a house price elasticity of -0.47 on the net-of-tax interest rate, using time-dimension variation over the 1977-1992 period. Poterba (1991) is unable to find a relation between the change in the average marginal tax rate and the change in constant-quality house prices using data for 39 cities in the 1980s. Estimating the true elasticity is quite important: an assumption of full price capitalization implies that eliminating the mortgage and property tax deductions would lead to a 15 percent decline in the average house price, or a $1.2 trillion drop in the value of the aggregate owner-occupied housing stock. [Capozza, Green, and Hendershott (1996)]

This paper exploits the fact that marginal tax rates vary by *family* at any point in time due to differences in income and demographic characteristics and, over the 1970 to 1991 time period, have changed differentially by income. If a higher-income family tends to purchase houses in a different market on average — more expensive homes, for example — than a lower-income family, then a house belonging to a family that undergoes a larger change in tax rates should exhibit a greater change in price. As long as one examines changes in price conditional on a family's not moving, the quantity of house consumed is held constant and the price response can be separately identified. This identification strategy avoids the primary failure of the previous literature: by computing tax rates at the city or national level,a significant portion of the identifying tax variation is lost through aggregation.

Using family-level longitudinal data drawn from the Panel Study of Income Dynamics covering 1970-1991, I find short-run capitalization rates of as much as -0.198 (0.028) into self-reported house value when I control for the differential effect of the widening income distribution in the U.S. on house prices and account for the fact that families endogenously choose to move out of their houses, creating sample-selection bias in family-level data. Scaled so the average changes in self-reported house value match changes in the state house price index, the estimated capitalization rate rises to -1.383 (0.196).

Section 1 discusses how a researcher can identify house price changes in response to tax policy using family-level data and why this approach is potentially an improvement over using location-based variation. Section 2 briefly presents the capitalization equation to be estimated and outlines ways to avoid difficulties with using self-reported house value. Section 3 discusses the data sets used for this analysis and notes how the key variables are constructed. Section 4 reports the results for a baseline capitalization equation, augments it by considering the effect of changes in the income distribution on house prices, and controls for the sample selection bias caused by endogenous moving. It also attempts to examine the dynamic pattern of the house price response. Finally, section 5 concludes.

1. Identifying House Price Changes in Family-level Data

In asset market equilibrium, the return a family would require from holding a house would have to be the same as the return it would receive from any other asset with identical risk characteristics. [Poterba (1984)] This condition can be expressed by:

$$R = P_H[(1-\tau_{HMI})(\alpha i+\tau_p) + (1-\tau_{int})(1-\alpha)r + \delta + m + \beta - \pi^e] \tag{1}$$

where R is the rent the homeowner would have to pay to live in his house and P_H is the market price of the house. The terms in brackets are the user cost of owner occupied housing — the marginal annual flow cost of having a house. The first term reflects the after-tax cost of debtfinancing a house: if α percent of the house's value is financed with debt and mortgage interest is paid at rate i, the out-of-pocket interest expenditure is reduced by the tax subsidy to home mortgage interest, τ_{HMI}, if the homeowner itemizes on his tax return. Property taxes, τ_p, are deductible at the same rate. The second term formalizes the notion that the equity used to finance the house could be earning an alternate rate of return, r, if it were invested elsewhere. That foregone return would have been taxed at the rate for interest or dividend income, τ_{int}. The next three terms correspond to the depreciation rate for the house, δ, the maintenance costs, m, and the risk premium required to invest in a house, β. The last term, π^e, is the expected rate of capital gain on the house — a higher rate of appreciation lowers the annual cost of maintaining the asset.

Denoting the terms in brackets by "UC," equation (1) can be rewritten as:

$$\frac{R}{P_H} = UC \qquad \textbf{(2)}$$

If there is an aggregate shift in the user cost of owner-occupied housing, equilibrium rent and/or house prices must adjust so the equality in (2) continues to hold. Rent is determined by the quantity of housing stock in the market relative to the level of demand, so in the immediate run an increase in user cost, for example, may be capitalized into a lower asset price of owner-occupied housing or into a higher rent for a given structure. Lower house prices relative to construction costs would reduce home building over time, raising rent and causing house prices to rebound.

While a number of authors have presented theoretical models of house price dynamics in response to a proportional, universal change in user cost [Poterba (1984), DiPasquale and Wheaton (1996)], the predictions are qualitative in nature and the results vary substantially depending on the assumptions about how families form expectations about future house prices. Determining the house price elasticity with respect to changes in the user cost requires empirical investigation.

Traditional theoretical models assume that there is only one housing market and one type of (infinitely divisible) house. It is difficult to econometrically identify a reduced-form equation from such a model since the predictions concern aggregate price movements in the national housing market. The best exogenous variation would be from inflation and a researcher would have to be concerned that other omitted factors that were changing over time may be yielding spurious results.

However, there are many distinct housing markets in the U.S., distinguished by factors such as location and quantity or quality. Houses in San Diego, for example, are a market for a different constituency than ones in Boston. Large houses, on sizeable plots of land, have a different set of potential buyers than small ones on quarter-acre lots. Expensive homes in towns with excellent school systems are available to a different set of families than less-expensive homes in a town where the school system has a lower graduation rate.

The latter two examples emphasize that housing markets can be distinguished on the basis of quantity of housing services provided. Rosen (1979), and the many papers that followed, showed that families with high permanent incomes or wealth would constitute the consumer base for high-quantity houses and lower-income families would tend to purchase smaller, lower-quality houses.

Although some factors, such as construction costs, do not vary across housing markets distinguished on the basis of quantity, the tax treatment of owner-occupied housing is quite different across such markets, assuming that larger houses have high-income clienteles. Not only do higher-income families have higher tax rates and are more likely to itemize on their tax returns and thus have a higher subsidy rate, tax rates have varied *differentially* by income category over the last two decades. Figure 1 shows the progression of tax rates for four example families (assumed to be itemizing on their tax returns) with different real incomes over 1970-1990. During the 1970s, high inflation combined with unindexed marginal tax brackets led to "bracket creep," where effective marginal tax rates rose for a given real income since rising nominal incomes ratcheted families into higher tax brackets. During the 1980s, legislative changes reduced tax rates, especially for higher-income taxpayers. The Economic Recovery Tax Act of 1981 (ERTA) lowered the top marginal tax rate from 70 to 50 percent and phased in tax rate reductions over three years for other brackets. The Tax Reform Act of 1986 (TRA86) again lowered the top marginal income tax rates from 50 to 33 percent, although the top effective capital gains tax rate rose from 20 to 28 percent.

If families of different income levels form clienteles for different types of houses, or match into houses based on their incomes and other factors that determine tax rates (such as the number of dependent children), houses in markets where the prospective buyers undergo larger changes in their tax rates should exhibit correspondingly larger changes in price. Assuming that a particular homeowning family is representative of the prospective buyers for its current house, a house owned by a family that has larger changes in tax rates should have larger changes in its own house price. Thus one way to empirically measure the response of house prices to changes in tax rates is to look for a correlation in *individual* level data.

This empirical test is particularly appropriate for measuring the price capitalization of the tax treatment of owner-occupied housing since it utilizes the greatest possible amount of tax variation and avoids assumptions about distributions of income and house prices by location. The econometrician can observe the actual (usually self-reported) house value for each family, rather than using the median house value, or an index, for a location. Tax rates can be computed on the family level, rather than computing the average for a location. One can be certain that the house in question matches with the tax characteristics when family-level data are used, rather than assuming the median house belongs to the family with the average marginal tax rate.

Most significantly for identifying price effects, one can control for the quantity of housing a family chooses by conditioning on the family not moving. Changes in value for a given house within a family's residence spell, absent renovations and changes in location amenities, reflect only price changes since the quantity of housing consumed must be constant. This approach avoids the complications of constructing constant-quality price indices or assuming that average quantity consumed in a location is constant over time.

This approach is significantly different than the methodology used in most of the previous literature. Assuming that different cities or MSAs constitute distinct housing markets yields a source of identifying variation that has been exploited by several authors. Capozza, Green and Hendershott (1996) estimate equation (2) directly using a pooled cross-section of 63 MSAs observed in 1970, 1980, and 1990. The authors use median rent and median house price in each MSA to construct the rent/price ratio and incorporate each component of the user cost separately. While they include a dummy variable for each year, they do not control for MSA-level fixed or random effects. Their results are striking: the estimated coefficient on the net-of-tax mortgage rate is 1.12 (0.24) and on the net-of-tax property tax rate is 1.08 (0.19), indicating that the rent/price ratio more than fully capitalizes locational differences in user cost. Since researchers have failed to find any empirical relation between changes in user cost and rent [e.g., Blackley and Follain (1996)], the authors conclude that user cost changes will be completely capitalized into house prices.

Other authors use variation over time. In two papers, Abraham and Hendershott (1992, 1994) use the marginal tax rate interacted with the interest rate as a control variable. Although they have data for about 30 cities, the variation in this variable comes from changes over the 14-year time span since the tax rate is calculated on the national level. The authors estimate the coefficient on the after-tax interest rate to be on the order of -0.5 in various specifications.

Other authors have examined the specific case of property tax capitalization. In particular, Rosen (1982) studies the effect of the passage of California's Proposition 13, which limited local property taxes, on mean house prices in 64 towns in the San Francisco area. This event is a particularly useful one to study since the state of California used its budget surplus to prevent cutbacks in local services. Rosen finds evidence of strong capitalization effects: a one percent increase in property tax rates leads to a seven percent decline in mean appraised house value.

While the location-based approach may be appropriate for determining the effect of local factors such as construction cost and property taxes on house prices, it is not the most compelling strategy for examining the relationship to taxes. In order to implement the location-based approach, one has to make strong assumptions about the distribution of tax rates, income, and housing quantity within an MSA. More precisely, these papers use a median or repeat-sale house price index and relate that to median or average income and to a local or national mean after-tax interest rate.

Two authors apply a similar empirical strategy to the one I use in this paper. Poterba (1991) presents tabulations of the relative appreciation rates of trade-up homes and starter homes from 1977 through 1989 and also compares high- and low-priced homes during the 1970s. Poterba shows that trade-up homes appreciated more rapidly during 1977-1980 and more slowly during 1983-1986, matching relative movements in user cost driven by inflation and taxes. He also finds that high-priced homes appreciated more rapidly than low-priced homes during the 1970s and the differential rate of appreciation was highest when inflation was greatest. However, Poterba's tabular approach does not control for changes in income or provide easily interpretable elasticity estimates.[1]

Although Mayer (1993) is most interested in explaining differential house price volatility across classes of houses, his methodology has some relation to the estimation I undertake. As a measure of house prices, Mayer (1993) uses the Case/Shiller repeat-sale index for four cities and, following Poterba (1991), divides the houses into three price tiers. He then relates the differential price appreciation between the top and bottom tiers to the differential income growth rates and the differential user cost growth rates. Mayer's estimated user cost coefficient, while not significantly different from zero, has the expected sign, -0.144 (0.094), and implies much lower capitalization rates than previously found in the literature. Unfortunately, the estimated coefficient on the income variable also has a negative sign, the opposite of what one would expect, although it is statistically indistinguishable from zero: -0.199 (0.300). Unlike Mayer and

[1]When Poterba uses variation across cities and includes an explicit tax variable, the estimate is not significantly different from zero.

Poterba, my estimation strategy uses variation across an income and house price continuum rather than assigning arbitrary house value categories.

It is clear that the empirical literature does not speak with one voice, or very loudly at all, on a parameter that is very important for the economy and for policy. Taking a different empirical tack might hold some promise.

2. Empirical Implementation

Taking logs of equation (2), the equilibrium relationship for family *i* living in house *j* in period *t* is:

$$\ln(P_{H_{jt}}) - \ln(R_{jt}) = -\ln(UC_{it}) + \epsilon_{ijt} \tag{3}$$

and the relationship to be estimated is therefore:

$$\ln(P_{H_{jt}}) = \beta \ln(UC_{it}) + \gamma X_{ijt} + \epsilon_{ijt} \tag{4}$$

where the X_{ijt} proxy for $\ln(R_{jt})$ and also include covariates that could affect house prices but which are not captured in the simple model outlined in section II. These covariates will be discussed later.

In family-level data, P_H is not usually observed. Instead, families report the market value of their house, $P_H \cdot Q_H$, where Q_H is the quantity of housing services the family is consuming.[2] In addition, one might be concerned that there are house-specific unobservable qualities that affect

[2]In the estimation in this paper, I assume that Q_H is constant within a residence spell. Clearly, depreciation, renovations, or additions could lead to within-spell changes. Although I will not be able to observe these changes in my data, there would be bias only if within-spell changes in Q_H were correlated with changes in house prices. If this were true, it seems reasonable that families would be more likely to renovate when house prices were high and let the house depreciate when prices were low. Then, the calculated change in P_H from differencing out the constant portion of Q_H would be too large, biasing the estimated coefficients away from zero.

the reported price for the house; that is, $\epsilon_{ijt} = \zeta_j + \eta_{ijt}$. To isolate the price response, I control for the quantity of housing consumed and the stochastic ζ_j term by taking differences within observed residence spells, estimating the following equation conditional on each family not changing houses:

$$\Delta \ln(P_{H_{jt}}) = \beta \Delta \ln(UC_{it}) + \gamma \Delta X_{ijt} + \Delta \eta_{ijt} \tag{5}$$

Since the interest rate component of the user cost is potentially endogenous, I instrument for the change in log user cost with two family-level federal and state tax variables, the percent change in $1-\tau_{\text{HMI}}$ and the percent change in $1-\tau_{\text{int}}$. For example, an exogenous increase in demand for houses may drive up house prices but would also heighten demand for mortgage debt, driving up interest rates and raising the user cost. This endogeneity should generate a positively biased estimate of β since a higher user cost would be correlated with higher house prices. Since changes in the tax code are arguably not driven by conditions in the housing market, the tax variation provides a way of identifying the effects of changes in user cost on house prices.

Instrumenting with tax rates also avoids bias due to measurement error in user cost. The property tax rate is constructed as property taxes paid divided by the self-reported house value. If the house value is reported with error, for example too low, the property tax component of user cost will be large by construction while the left-hand-side variable, house value, will be small. Thus, the estimated user cost coefficient would be biased toward a more negative value as a result of the measurement error. As long as the tax rates are exogenous to the self-reported house value, which they are since they are first-dollar measures, instrumenting will circumvent this bias.

3. Data and Variable Construction

My primary source of data is the Panel Study of Income Dynamics (PSID), a longitudinal survey that has been collected annually since 1968. This family-level data set queries the respondents on several topics crucial for estimating housing models, including self-reported house value, timing of moves, and income disaggregated by source. Due to the longitudinal aspect of the data set, one can obtain multiple observations on house value while making sure that the family remains in the same residence.

I use two samples of families observed over the 1970 to 1992 time period, with the distinction being that the first sample imposes that the first year of a family's residence in a particular house be observed for that residence spell to be included.[3] Table 1 details the construction of the two samples. The initial PSID sample included a random cross-section of the population and an additional low-income sample. In 1989, a Hispanic sample was added. I use only the observations derived from the original random cross-section sample. Families were matched by the ID number of the household head from the PSID waves, yielding a starting sample of 80,787 observations, or 22,822 residence spells or fragments of spells. When the sample is restricted to homeowners, the number of available observations drops to 51,981.

In several years of the PSID, the questions corresponding to crucial data elements for homeowners were not asked.[4] In those cases, the missing variable was imputed using a weighted average of the closest years when the data was available. If the family moved once during the

[3]The PSID does not ask when the family first moved into a house, so a family must be observed for its entire residence spell to determine length-of-stay.

[4]Property taxes paid is not available in 1978, 1988, and 1989. Mortgage principal remaining was not asked in 1973, 1974, 1975, and 1982.

interval, the missing data was set equal to the closest available value in the same residence spell. However, if the family moved multiple times during the interval, at least one residence spell had no observed neighboring data and the 2,261 observations in those spells had to be discarded.

For some portions of the analysis, differences are taken relative to the beginning of the residence spell. Thus in the first sample, any left-censored spells — where the first year of residence is not observed — were discarded. All the observations on 17 homeowners were deleted because they remained in their houses for more than 22 years — they were already living there in 1970 and had not moved before 1992. 14,737 more observations corresponding to spells where neither the first nor last years of residence were observed but which did not last the entire sample period were also deleted. In addition, 5,716 observations on spells that were left-censored only were omitted.

Of the remaining residence spells, 176 have gaps in the consecutive observations and are omitted in their entirety. Since income information in the PSID is provided for the *prior* year, the last observation for each family cannot be used. 2,913 observations, including all of the 1992 data, are dropped for this reason; the resulting sample period covers 1970-1991. In addition, 751 spells are removed because they have zero values for income or are missing user cost data at some point during the residence spell.

The final sample size for the first sample is 25,051 observations on homeowners. The second sample does not eliminate the left-censored spells or the spells with gaps, allowing 18,839 more observations to remain, for a total of 43,880.

Table 2 reports the means for the two samples. The columns show the mean level for each variable and different measures of changes in the variable — the percent change since the beginning of the residence spell, the percent change since the previous year if the family has not moved during that period, and the percent change since three years prior, again conditioning on the family not moving.

House value is the self-reported answer to the question, "if you were to place your house on the market today, about how much would it bring?" All dollar values are deflated to 1990 real amounts using the GDP deflator. The average house value is slightly smaller for the full sample and the house price growth since the beginning of the residence spell is much greater than for the short differences.

The user cost variable is the user cost as a proportion of the last dollar the family spends on its house. In equation (1), the subsidy rate on the last dollar of home mortgage interest and property taxes, τ_{HMI}, is computed by passing the relevant income, deduction, and demographic variables for each family through the National Bureau of Economic Research's TaxSim federal and state tax calculators for 1979 to 1991 and through a federal tax calculator written by the author for 1970 to 1979. The tax computation accounts for the deductibility of state taxes on the federal tax return. If it is calculated that the homeowner would take the standard deduction rather than itemize, τ_{HMI} is set to zero. The loan-to-value ratio, α, is determined from questions asked in the PSID. The mortgage interest rate, i, and the market rate of return, r, are both assumed to be the Treasury 7-year bill rate. The property tax rate is the sample average for homeowners in the PSID. The last-dollar tax rate on interest income, τ_{int}, is computed in the same manner as the subsidy rate on home mortgage interest. Following Poterba (1992), depreciation and maintenance costs are each assumed to be 2 percent per year, the risk premium is 4 percent per year, and the expected capital gain, π^e, is a five-year moving average of CPI growth. For both samples, the average marginal user cost is computed to be 0.096.

Two net-of-tax rates are reported: one corresponding to $1-\tau_{HMI}$ and the other to $1-\tau_{int}$. Each uses a "first-dollar" concept, the tax rate assuming no housing-related income or deduction items. The first-dollar concept is exogenous whereas the "last-dollar" rate will be affected by how much owner-occupied housing a family chooses to consume. If, absent the housing-related deductions, the homeowner would not be an itemizer, the first-dollar τ_{HMI} is set to zero. The net-of-tax rate on interest income is slightly lower than the one for home mortgage interest for precisely that reason.

The income concept used is after-tax disposable income. All income components, both positive and negative, in the PSID are summed and the calculated first-dollar tax liability is subtracted.[5] The average income in the "small" sample is \$37,023 and \$33,655 in the full sample.

The remaining variables in table 2 describe the family's position in the income distribution. For each year in the sample period, I obtained the total family income variable and the CPS weights from the Mare-Winship Current Population Survey extracts. I matched the pre-tax real income at the time the family moved into their current house for each observation in my sample to the corresponding year in the CPS and calculated the income-weighted percent of the

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[5]Unfortunately, many income variables in the PSID are top coded in nominal terms. In addition, the extent of the top coding is reduced periodically as more space is allocated to some data elements. It turns out that very few families are subject to the top coding. When I repeated the estimation after artificially top coding all the income variables in constant real terms beginning when the number of digits increased (i.e., if the number of digits rose from four to five, I artificially top coded the five-digit variables at \$9,999 real dollars) the results were not noticeably different. A more subtle issue is that the PSID income variables became more disaggregated over the life of the survey. For example, "total transfer income" was broken down into components. If families tend to report more income when asked in more detail about each component, incomes will appear to rise over time. If the magnitude of this phenomenon differs by income, perhaps since poorer families are more likely to have more sources of transfer income, my results could be affected.

income distribution in that year with a higher family income than the PSID observation. I also calculated the percent of the population in the CPS income distribution with an income within 10 percent of the PSID family's income at the beginning of its current residence spell. On average, in the small sample 12.7 percent of the population was within plus or minus 10 percent of the observed family's income while 57.5 percent of total family income was earned by families with a higher income than the observed family. In the full sample, a smaller percentage of the population had incomes close to the observed PSID incomes and the PSID families were lower in the income distribution.

4. The House Price Capitalization of Tax Changes

This section presents the results from the estimation of equation (5). I begin by testing how well changes in self-reported house value seem to track other measures of house price inflation. I then turn to estimating how much of the tax-induced changes in user cost were capitalized into house prices and investigate how the effect varies over the sample period. Finally, I examine how changes in the income distribution influence house prices and determine the effect of self-selection bias in a family's decision to move on the estimation.

4.1. Do self-reported house values reflect price fluctuations?

Measuring the price level for houses is quite difficult. Repeat-sales indices suffer from selection bias for properties that turn over multiple times. Constant-quality indices estimated from hedonic regressions are prone to omitted variable bias. Assessments, too, are troublesome since the objective of the assessor is not necessarily to measure the market value of a home and he may also lack complete information.

In most family-level data, house value is a figure provided by the survey respondent. While most researchers would concede that self-reported house value is a noisy measure of true house price, the key question is whether the measurement error is systematically correlated with any of the measured variables. Goodman and Ittner (1992), in a study using a cross-section of the American Housing Survey, find that homeowners overvalue their houses by 6 percent on average but are unable to find a correlation between the mismeasurement and other variables.

Random variation in the 6 percent measurement error in self-reported house value, since it is the left-hand-side variable in equation (5), would just lead to larger standard errors. However, a possibility not addressed by Goodman and Ittner is that some homeowners may be slow to update their reported values since it takes some time for market information to trickle down to families that are not actively trying to buy or sell a home. Another possibility is that families do not realize the full magnitude of a price change until they actually try to sell their house. In either case, the measured change in price, if calculated as a difference from the time the family moved in to the residence, would only be a fraction of the true change.

For the purposes of this paper, it is important to determine whether a change in the overall house price level is reflected in self-reported house values. If not, any relation found when estimating equation (5) would be spurious. If self-reported house values capture only a percentage of a change in price level, knowing that percentage will aid in interpreting the results.

A simple test of whether families on average correctly revise their self-reported valuations of their houses in response to changes in the market price for homes is whether families in areas with higher true house price appreciation report larger increases in house values.

I estimate the following equation:

$$\%\Delta P_{H,R_{jt}} = \theta(\%\Delta P_{H,true_{kt}}) + \xi_t + \omega_{jt} \tag{6}$$

where the left-hand-side variable is the percent change in the self-reported house value by a family in a given house, j, observed at time t. To measure the "true" percentage house price change, I use the percent change in the Freddie Mac repeat-sales index for the state of residence, k.[6] Since the Freddie Mac series begins in 1975, I shorten the sample period to 1975 to 1991. Year effects, ξ_t, are included to control for price changes in the time dimension.

If families perfectly internalize changes in market prices for houses, we would expect to estimate a coefficient of one for θ. However, that may not occur even with completely rational survey respondents. The PSID families may not be representative of the houses that sold multiple times in the state. If the houses owned by the PSID families had a different average level of appreciation than the average repeat sale in a state, one would not expect a coefficient of one. There are a couple of ways in which the PSID sample frame could generate a result that is different than one. First, the PSID undersamples high-income families. Mayer (1993) points out that prices for houses owned by high-income families are more volatile than for the lower-income segment of the market. Too few high income families in the PSID sample will thus cause the average self-reported house value to vary less than the average market price, yielding a coefficient lower than one. Second, if the geographical distribution of PSID respondents within states tends to be skewed to high- or low-volatility areas, the estimated θ could be different from

[6]Using state-level house price indices is a crude measure of geographically distinct markets. The PSID also has county-level identifiers but for confidentiality reasons one needs to apply to obtain them. Once I receive the county identifiers, I will match families into MSAs and use MSA-level price indices.

one. Finally, houses that sell repeatedly may not be representative of prices in the market so the repeat-sales index does not measure the "true" price. Even with these caveats, an estimated coefficient on θ that is close to one should indicate that families "get it right" on average when they report changes in house values. An estimate that is significantly less than one is an indication that families are either slow to update their valuations or are unwilling to revise their estimates of their house value to the same degree that the market has actually moved.

The results from estimating equation (6) are reported in table 3. Two different definitions of "percent change" are examined — the percent change between the current period, t, and the time the family first moved into the house, c, and simple differences between t and t-s. Since house value updating may occur with a lag, I try taking differences of various lengths to see if the fit improves.[7] Equation (6) is estimated both with year effects, to test whether families in states with relatively higher house price appreciation report correspondingly larger increases in their house values, and excluding the year dummies, to test whether families increase their reported house values over time at the same rate that house prices in their state are rising. For the specification that examines differences from the beginning of the residence spell, dummy variables for the current year and the year that the family moved into the house are included to completely control for changes in the time dimension. When a fixed-length difference is used, only year dummies are necessary.

In the top panel of table 3, using percent changes since the start of the residence spell tracks the state house price index well when year dummies are not included. The estimated

[7]There is also potentially an errors-in-variables problem. Taking differences with longer or shorter gaps may improve the signal-to-noise ratio. See Griliches and Hausman (1986).

coefficient, θ, is 0.641 (0.013), indicating that an average homeowner will raise his self-reported house value by 6.4 percent if the house prices in the state rise by 10 percent. Most of the accuracy of the response comes from the time dimension variation — the owner increases his self-reported house value over time at nearly the same rate that actual house prices rise. In the second column, year dummies are included to see what happens with the "cleaner" test and the estimated response rate drops to 0.145 (0.021). If house prices rise by 10 percent more in one state than in another, the average homeowner in the former state will report an increase in house value that is 1.5 percent greater than if he lived in the other state.

In the lower panel, fixed differences of various lengths are examined, both for the small sample and for the full sample. In both, the estimated response rate appears to increase until the third difference row, at which point it levels off. For that reason, I will use the third differences in the rest of this paper even though it requires reducing the number of observations by nearly 50 percent. In the third differences case, the estimated response rate ranges from 0.259 (0.019) for the full sample excluding year effects to 0.136 (0.024) for the full sample with year dummies.

These results indicate that families may smooth house price volatility in their minds. Therefore, the results that will be obtained should reflect a lower bound on the true price capitalization effects. It also suggests that one could "scale up" by the rate of self-reported house value updating to obtain an upper bound. Suppose that we discovered that a 10 percent decline in user cost would cause a homeowner to raise his self-reported house value by one percent. Since a one percent increase in self-reported house value was generated by a 7.4 percent increase in house prices (using 0.136 as the estimate of θ), the true capitalization rate must be 74 percent rather than 10 percent. The reciprocal of the estimates in table 3 could provide the scale factor to convert the self-reported house value capitalization into an upper bound true house price capitalization.

4.2. Baseline capitalization rates

Table 4 reports estimates of equation (5) using differences since the beginning of the residence spell. In this specification, the only covariate other than user cost is the percent change in the family's income. I include this variable so the percent change in user cost is identified from exogenous legislative changes in the tax rate, holding income constant, rather than from a nonlinear transformation of changes in family income. Since there is an income effect in housing demand, changes in family income may also independently affect house prices.

The first column in the top panel of table 4 estimates equation (5) over the entire 1970 to 1991 sample period and exploits the time dimension variation. The estimated capitalization rate for changes in user cost is −0.211 (0.020), so a 10 percent rise in user cost due to a change in the tax code would lead to a 2.1 percent decline in the average self-reported house price. A similar increase in after-tax income is estimated to cause a 0.51 (0.09) percent increase in self-reported house price.

Looking down the first column of table 4, panels two and three report results from using two different controls for time dimension variation. Panel two incorporates a full set of indicator variables for the year of the move and the current year, controlling nonparametrically for all variation in the time dimension. Panel three includes the percent change in the state house price index under the hypothesis that any average (by state) change in market or family characteristics would be captured by the state house price index, leaving variation about the mean. Using the state house price index control gains efficiency since it allows for identification from sampling variation. For example, suppose the PSID sample is not perfectly representative of each state. Using time dummies will absorb the sample average variation, leaving the estimates unidentified if there is not much variation about the annual means. By including a state house price index, I control for all time dimension changes that affect the housing market on average, but gain some identification from the fact that the PSID sample may be slightly different from the national sample and thus has a sample average that is different from the state average as measured by the house price index. For example, if the PSID sample has lower taxes than average in Wisconsin but higher in North Dakota, the estimated coefficients are partly identified from the differences from the average if the state house price index is used but not if time dummies are used.

In panels two and three, the estimated user cost coefficient gets weaker while the estimated income coefficient grows larger when controls for the time dimension are included. An obvious culprit is that much of the tax-induced variation occurs as average effects in the time dimension.[8] The bulk of the income variation, on the other hand, is due to uncorrelated changes within families rather than from aggregate, proportional shocks and thus is enhanced by the inclusion of time effects.

Oddly, the user cost capitalization rate estimated when time dummies are included is positive, 0.061 (0.035), although not statistically significantly different from zero. Examining the second and third columns of table 4, which splits the sample into the pre-1986 and 1986-and-on periods, provides some insight. Before 1986, the estimates suggest higher price

[8]This is especially true in the PSID which does not have many high-income families in the sample to provide identification from differential tax rate changes.

capitalization; with no time dimension controls the estimated user cost elasticity is -0.688 (0.034), using the state house price index yields an estimated elasticity of -0.135 (0.034), and including time dummies generates an elasticity of -0.096 (0.045). However, the user cost estimates for the TRA86 period, when tax rates dropped for high-income taxpayers, are uniformly positive. Utilizing the time dimension variation mitigates the positive coefficient somewhat. The underlying data show that, over this period, while average real house prices fell with average tax rates, house prices for houses owned by high-income families rose relative to those owned by low-income families. This result is exactly the opposite of what changes in tax rates should have encouraged, and generating the pattern in table 4. This finding suggests that there is an omitted variable that varies differentially by income over time in a way that is correlated with house prices and tax rates. The next two subsections examine some possible explanations.

4.3. The effect of the widening income distribution on house prices

One widely-reported economic fact of the late 1980s was that the distribution of income in the U.S. began to widen dramatically. The increasing gap could have significant implications for house prices. It is well-established that families with higher incomes prefer to consume more housing, so a widening income gap should drive up prices for high-end homes as demand outstrips the current housing stock.

For the limited purposes of this study, I postulate two simple models of how houses are allocated to potential buyers in the market. The first is a model where the stock of houses fills from the top down and consists of an infinitely divisible quality continuum with a fixed total quantity. The price for a given house would be affected by the number of families desiring to purchase a house with at least that quality and how much house quantity they want to buy. Therefore, the income-induced price pressure for a house owned by a particular family could be summarized as a function of the proportion of the total family income in the economy held by families with incomes greater than the family under consideration.[9]

Another possible model is that houses are finite and not divisible, but can be ordered by a function of quantity and quality which could be expressed as a "fundamental" price. Houses of a particular price appeal to families with appropriate incomes. For example, one rule of thumb is that families purchase houses that cost three times their annual incomes. Therefore families with $100,000 annual incomes would constitute a market for houses that cost about $300,000. A $50,000 income family would never be in the same housing market. In this model, price pressure comes in market segments, so a boom in $100,000 income families only raises the prices of $300,000 houses, though the category boundaries are blurry. For a given family, their house price should rise if the proportion of the population with incomes near their income rises.

Table 5 reports the results from incorporating covariates to control for the widening income distribution when estimating equation (5), using percent changes in the state house price

[9]Note that the relevant family income for the observed family was its income *when it moved into the house* since the prospective buyers are all about to move into that same house. For example, if a family's income fell, one would not want to say that the house they are living in suddenly has a lower-income market, especially since families adjust their housing consumption infrequently. In fact, if a family's income fell and they would want to consume less housing, they would move to a different house and a new family that had an income comparable to the old family's initial income would move in. All income distribution variables used in this paper are computed using the family's real income at the time of its move compared to the income distribution in the year of the observation.

index as the control for variation in the time dimension.[10] The first panel includes a control for the top-down hypothesis: a measure of the percent change in the percent of the total income in a year that was earned by families with greater incomes than the observed family.[11] The estimated self-reported house price capitalization rates drop substantially, to −0.113 (0.022) for the entire sample period, −0.189 (0.040) for the pre-1986 period, and the sign becomes negative for the 1986-on period, although the estimated coefficient of −0.40 (0.056) is not statistically significant. The income distribution variable has the expected positive coefficient, which does not vary much for the pre- or post-1986 periods. The estimated coefficient for the entire sample period is 0.242 (0.026), so if the percent of total family income in the economy held by families with higher incomes than the observed family's increases from 50 percent to 55 percent, that family's house price should rise by nearly 2.5 percent.

The second panel tests the second hypothesis, that the density of the income distribution around a family determines changes in house prices, by including a variable that measures the percent change in the percent of the U.S. population in a year that has family income within 10 percent (plus or minus) of the observed family's income. Adding this covariate has a smaller negative effect on the estimated user cost coefficient and has a small but statistically significant

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[10]Even though the previous regressions controlled for changes in income for the observed family, they did not control for changes in the income distribution. The observed family's change in income provides information about the movements in income for families that hold a particular class of houses, but it cannot tell us anything about whether more or fewer families have that level of income. For that reason, incorporating even high-order polynomials (up to degree 9) in change in income has virtually no effect on the income distribution coefficient estimates. In fact, it does not affect the user cost coefficient estimates either.

[11]I have also tried including a measure of the "bottom up" hypothesis — that the housing market fills from below. The distribution variable was the percent change in the percent of the population that had incomes below the observed family. Using this measure generated similar results as when I used the "top down" measure, although with the opposite sign, of course.

effect on house prices: the estimated income distribution coefficient for the entire sample period is 0.049 (0.014).

The last panel tests a combination of the two income distribution hypotheses. Incorporating both income distribution variables significantly lowers the estimated user cost coefficient. Over the entire sample period, the estimate drops to -0.140 (0.024). The total effect on the estimated user cost coefficient is much greater in the 1986-on period — the 1986-on coefficient drops by 0.174 versus a 0.060 drop for pre-1986 — substantiating the hypothesis that the late-80s widening income distribution generated some of the unexpected results.

When the two income distribution variables are included together, the top-down variable has a positive coefficient that is even larger than before, estimated to be 0.357 (0.037) for the entire sample period. However, the estimated coefficient on the "number of income neighbors" variable becomes negative, suggesting that there may be some colinearity between the two. It is not surprising that the two variables are closely related since the constructions overlap to some degree.[12]

4.4. Using a sample selection correction to control for endogenous moving

One potential endogeneity problem arises when families move out of their houses. An exogenous increase in demand for owner-occupied housing may raise house prices but may also induce more families to move since most families increase their housing consumption by switching residences. If families are more likely to move when house prices are rising for unobserved reasons, the estimated mean will be biased towards zero since higher values of the

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[12]Including an interaction term for the two income distribution variables has no effect.

left-hand-side variable would be less likely to be included in the data set. An unobserved demand shift is not the only possible explanation for this truncation problem. Stein (1993) shows that families with high leverage rates could be less likely to move if house prices fell since they might not have enough net worth to pay off their mortgages and would be "locked-in" to their houses. Chan (1995) finds considerable evidence of this spatial lock-in effect in mortgage data. For estimating equation (5), if families with price declines are more likely to remain in the data set the estimated mean will be biased since the distribution of $\Delta\eta$ will not be symmetrically truncated and the estimates of β and γ may be biased as well.

Endogenous moving can be described as a sample-selection problem similar to the one described in Heckman (1979), . A homeowning family remains in its current house if the predicted probability of moving does not exceed the family's stochastic willingness-to-move threshold:

$$g(Z_{ijt}\delta) \leq \nu_{ijt} \tag{7}$$

The Z variables include $\Delta\ln(UC)$ and the ΔX variables, as well as instruments for the difference between $\Delta\ln(P_H)$ and the change in price required for the family to decide to move. Sinai (1997) shows that equation (7) can be written as conditions on the predicted hazard rate, or probability of moving conditional on not having moved prior to the observed duration, for each competing type of move by a homeowner — trading up to a larger house, trading down to a smaller one, or becoming a renter. The homeowner must not have executed any of the three types of possible moves to still be observed in the house. Continuing to use equation (7) as shorthand for these three conditions being true, the estimated coefficients from equation (5) are potentially biased if the expectation of $\Delta\eta$ conditional on the family not moving is not zero. Taking expectations of equation (5):

$$
\begin{aligned}
&\mathrm{E}[\Delta\ln(P_{H_{it}})\,|\,\Delta\ln(UC_{it}),\Delta X_{ijt}, g(Z_{ijt})-\nu_{ijt}\leq 0] = \\
&\beta\,\mathrm{E}[\Delta\ln(UC_{it})] + \gamma\,\mathrm{E}[\Delta X_{ijt}] + \mathrm{E}[\Delta\eta_{ijt}|g(Z_{ijt})-\nu_{ijt}\leq 0]
\end{aligned}
\tag{8}
$$

The last expectation term is not zero if $\Delta\eta$ is correlated with ν. Since it is omitted from the regression, the estimated constant term, or conditional mean change in house price, would be inconsistent. If this omitted expectation term is correlated with the other right-hand-side variables, the estimates of β and γ could be biased as well.

Newey (1988) shows that $\mathrm{E}[\Delta\eta_{ijt}|g(Z_{ijt})-\nu_{ijt}\leq 0]$ can be approximated semiparametrically by a series expansion $h(g(Z_{ijt}))$, allowing for a flexible, unspecified joint distribution between η and ν. As in Sinai (1997), a polynomial in each of the three predicted hazard rates can be incorporated into the price equation, (5), to correct for the potential sample selection bias. The equation to be estimated would then be:

$$
\begin{aligned}
&\Delta\ln(P_{H_{it}}) = \beta\,\Delta\ln(UC_{it}) + \gamma\,\Delta X_{ijt} + \\
&h_{UP}(\lambda_{UP}(Z_{ijt}\delta_{UP})) + h_{DOWN}(\lambda_{DOWN}(Z_{ijt}\delta_{DOWN})) + h_{RENT}(\lambda_{RENT}(Z_{ijt}\delta_{RENT})) + \mu_{ijt}
\end{aligned}
\tag{9}
$$

Families may drop out of the sample in a non-random manner by choosing to move between houses. If the decision to move is endogenous, the estimates of equation (5) may be biased. Since the work by Stein (1993) and Chan (1995) suggests that families with house price declines are less likely to move, one would expect that the estimated mean in a house price capitalization regression would be biased downwards. One would also expect the user cost coefficient to be biased toward zero; since families with more elastic demand are more likely to move and would also be likely to have larger price responses, the families that are observed not moving are the ones with smaller price and quantity elasticities.

Table 6 reports the results from estimating equation (9) — controlling for endogenous moving by incorporating a semiparametric sample-selection correction. In this case, the first stage (selection) equation was estimated following Sinai (1997). A competing risks duration model was estimated for three possible types of moves: owners trading up to larger houses, trading down to smaller ones, or leaving homeownership and becoming a renter.[13] Predicted hazard rates were generated for each type of move and incorporated as sample selection correction terms into the house price capitalization regression. The terms were included both linearly and as a quartic.

The first column of table 6 repeats the baseline estimation for the entire sample period, using the percent change in the state house price index to control for time dimension variation, and including both measures of changes in the income distribution. The second column incorporates the predicted moving hazards in a linear form. The estimated user cost coefficient drops slightly, to −0.167 (0.025), which is the predicted direction. The estimated coefficients on all three predicted hazards of leaving homeownership are negative, consistent with the notion that the sample selection problem biases the estimated mean downward. In the linear specification, only the estimated coefficients on the trade up and trade down hazards are significant, at −0.859 (0.112) and −2.430 (0.518) respectively.

The third column incorporates the predicted hazards as quartics to minimize assumptions

[13]The right-hand-side variables were % Δ user cost, % Δ after-tax income, Δ excess deduction, age dummies, a set of dummies for change in marital status, dummies for change in retirement status, dummies for the change in whether the family has children, the change in the number of children, % Δ state house price index, and a set of duration dummies to estimate a nonparametric baseline hazard.

on the joint distribution of the stochastic terms in the selection equations and in the price capitalization equation. The estimated user cost coefficient falls further, to −0.198 (0.028). The estimated coefficients on the quartic hazards are not reported, just the corresponding F-statistic and p-values for the null hypothesis that the sum of the four hazard terms equals zero. The trade up and own-to-rent hazards are both strongly statistically significant in this specification.

4.5. Interpreting the capitalization rates

In the final specification, the estimated capitalization rate of changes in the user cost into self-reported house value is −0.198 (0.028). While self-reported house value is what a family considers when it makes portfolio decisions, this capitalization rate does not reflect the family's true wealth — that is, how much it could sell the house for.

To obtain an estimate of the capitalization of user cost into the price level, and for comparison with previous work, one could scale up this estimate by a scaling factor from table 3. Although we cannot estimate a scaling factor when using the state house price index as a control variable, the scaling factor for the "small" sample with year effects is 6.987 and should be an upper bound for the appropriate scaling factor since it assumes the worst-case scenario for how well self-reported house values reflect true house price changes. Thus the upper-bound capitalization rate, scaled to match the state house price index, is −1.383 (0.196). From table 4, scaling the estimates with no year effects (by a factor of 1.560) yields a capitalization rate of −0.329 (0.031).

4.6. What are the dynamics of the house price response?

Although the scaled estimated house price capitalization rates that emerge from this estimation are greater than one, they are well within the range of conceivable parameter values. One likely explanation for the large point estimates is the dynamic pattern of the house price response. Asset price models of the housing market, such as the one in Poterba (1984), tend to predict overshooting: very large initial price jumps followed by a rebound. Overshooting when the equilibrium capitalization rate is high could lead to initial price elasticities that are much greater than one.

In the estimation thus far, the entire dynamic path of house prices is not given equal weight. Since more observations are available for the periods immediately after a tax change than for periods farther out in time, the average estimated response is for the short-term. In the data, the average residence spell lasts 5.24 years, not accounting for right censoring, and that figure is a high upper bound on the weighted average period over which the house price response is estimated. Given those facts, it is not surprising that high capitalization rates are found.

Not all models of house prices generate immediate overshooting, however. DiPasquale and Wheaton (1996) discuss models that exhibit cyclical price behavior. Mankiw and Weil (1989) show that in a model where predictable changes in housing demand will occur slowly, prices will jump initially and continue to increase to a peak, after which the prices will slowly fall. It is important, therefore, to distinguish between these differing models with empirical work.

Estimating the dynamics of house price responses places high demands on the data. In table 7, I estimate a distributed lag of percent changes in user cost and income on the percent change in house value. Due to the data length and quality, I only use four lags, so the idea is to determine from the initial response whether the overshooting model or some more gradual model should apply. As usual, I instrument for the percent change in user cost with the percent change in the net-of-tax rates.

The estimation of table 7 differs from the previous tables in several ways. First, instead of looking at differences since the last move, I use third differences. The two-period gap was determined to be optimal by an inspection of table 3. I also use the full sample, not the "small" sample that was used previously. Finally, if I cannot construct a particular lagged difference because an observation is missing, I set the value of that difference to zero.[14]

The results are mixed. The first column of table 7, which does not include explicit controls for time dimension variation, seems to lend credence to a model of gradual price adjustment. The amount of the price capitalization increases over the first four periods and declines slightly in the last period. When the percent change in the state house price index is included, the point estimates from using the full sample period support the overshoot hypothesis: prices jump in the first lag and rebound after that. If the sample is restricted to the pre-1986 period, the point estimates both with and without controls for the time dimension support a gradual price adjustment. However, none of the coefficients are estimated tightly enough to reject any reasonable hypothesis of house price dynamics. In all specifications, an increase in income continues to raise prices even four periods after the shock.

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[14]One should model the endogeneity of spell length to be able to assign the zero value in this way. I assume the endogeneity is minimal since the sample selection correction had such a small effect.

5. Conclusion

In this paper, I have applied family-level data to the study of a market phenomenon — how house prices respond to a change in user cost. This unusual approach afforded a number of advantages: I could observe high-frequency changes in house value, controlling for quantity of real housing consumed by conditioning on a family not moving; I was able to utilize significantly more tax variation than previous studies, especially the fact that marginal tax rates varied differentially by income level over time; and I could ensure the consistency and exogeneity of my estimation by using tax changes as an instrument without making assumptions about the relation between the median house in a location and the average tax rate observed there.

House price capitalization of tax-induced changes in the user cost of owner-occupied housing is strikingly high, estimated to be -0.198 (0.028) into self-reported house value and -1.383 (0.196) when scaled up to match the state house price index. Although this figure is an upper bound since it assumes the worst-case scenario for how well self-reported house values reflect true house price changes and it corresponds to a short-term price response rather than an equilibrium concept, near-perfect price capitalization is a reasonable conclusion.

While this rate of price capitalization is higher than the amount found in earlier work that used non-tax-induced variation, a number of facts suggest that it is likely to be the appropriate figure. First, the estimates in the previous literature are probably biased upwards due to the endogeneity of interest rates, just as the point estimates in this paper are more positive if one does not instrument using tax rates.[15] Second, the one other paper in this area that focuses on the impact of taxes on house prices, Capozza, Green, and Hendershott (1995), also finds complete

[15]See Appendix B.

price capitalization. Finally, a high level of price capitalization is consistent with the results in Sinai (1997) which is unable to find much effect of changes in user cost on homeowners' consumption of real quantities of owner-occupied housing. That is, even substantial user cost changes do not induce much variation in families' decisions to move, which they must do to significantly change their housing consumption.

However, a number of puzzles remain. The behavior of house prices around TRA86 is a conundrum. Tax rates dropped much more for high income families than for low income families, yet I find little house price response or effects that appear to go the wrong way. Interestingly, in his 1991 paper, Poterba compares price appreciation for trade-up relative to starter homes and finds that trade-up homes appreciated faster over the 1986-1989 period; as in this paper, that is the unexpected direction. Also, if changes in user cost are fully offset by price changes, why does Sinai (1997) find strong evidence that renters are less likely to become homeowners if user costs rise?

Finally, the dynamics of house price responses to aggregate shocks is still not well-identified in empirical work. Although the theoretical models have clean, if competing, predictions, it is difficult to distinguish between them in the data.

It is apparent that a considerable amount of research remains to be done before the asset price side of the owner-occupied housing market is understood. In this paper, I have assumed that families cannot renovate their houses. However, building additions and renovating is growing in popularity as open plots of land are becoming scarce. Some of the measured capitalization effect may simply be due to improvements. More study on investing in a family's current house is needed.

How houses are allocated in the market and how prices are set is another area ripe for investigation. In this paper, I assumed two loose ways by which competition for a limited supply of houses could affect house prices and found that changes in the income distribution are important determinants of relative house price changes but did not pursue the issue. However, this topic is important because it suggests that changes in the micro-level distribution of characteristics in the population may affect house prices, not just the national average characteristic as is typically assumed. There are also interesting implications for house price volatility by income class, as Mayer (1993) examines.

Finally, a better empirical understanding is needed of the interactions of the different segments of the owner-occupied housing market. A study that links the demand for owner-occupied housing with price effects, construction, and vacancies would be an asset to understanding the implications of policy and macroeconomic changes for the housing sector and is the subject of future research.

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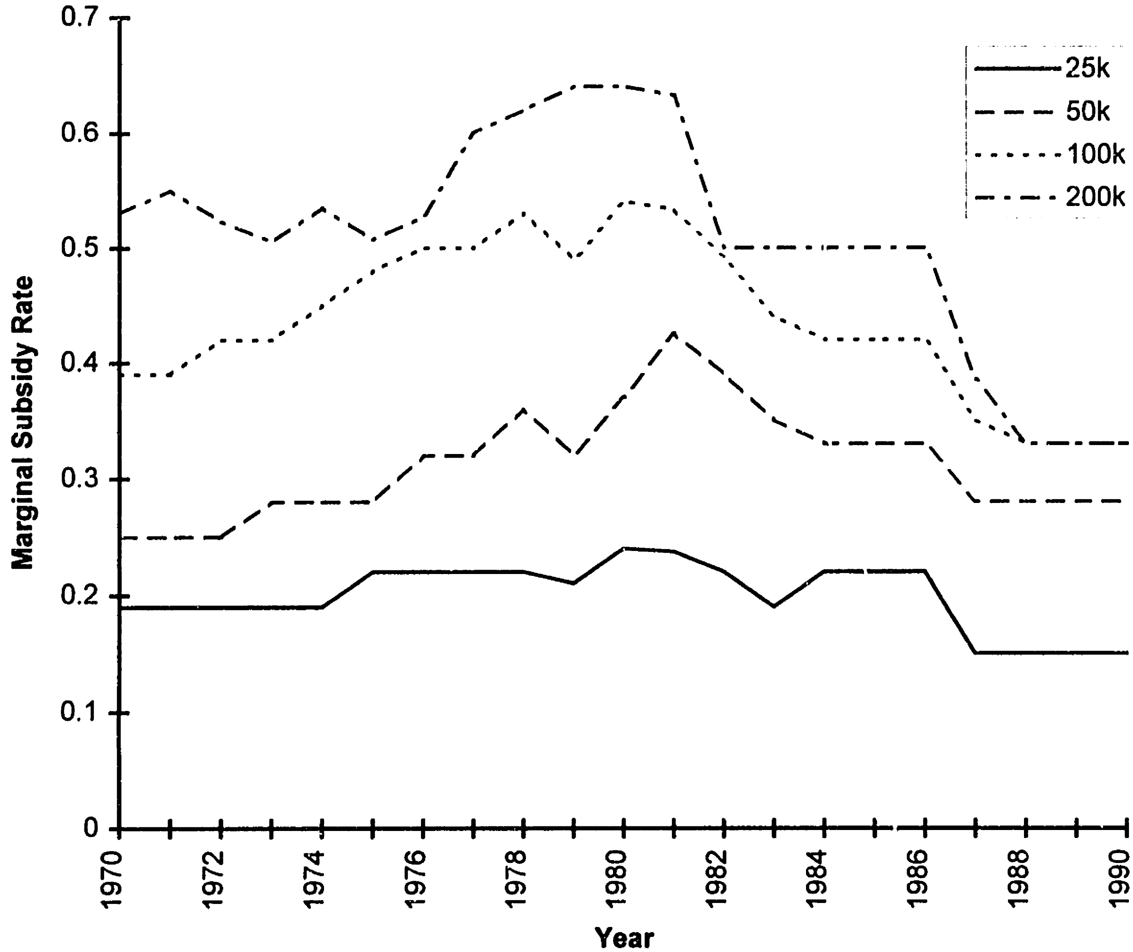
Figure 1: First-dollar Marginal Subsidy Rates on Home Mortgage Interest for Several Example Families, 1970-1990



First-dollar subsidy rates on home mortgage interest deductions are reported for a married couple with two children. Real annual wage and salary income is allowed to vary from $25,000 to $200,000. The family also has an additional 10% of wage income in the form of taxable dividend or interest income. The families are assumed to be itemizers and, for the purposes of computing state taxes, are said to be living in Massachusetts.

Table 1: Sample Construction

	"Small" Sample		Full Sample	
	Minus	Total	Minus	Total
Original number of observations:		**80,787**		**80,787**
Renters	28,806	51,981	28,806	51,981
Questions not asked in some years, data not imputable	2,261	49,720	2,261	49,720
Families with no moves over 22 years	374	49,346		
Other left- and right-censored spells	14,737	34,609		
Left-censored spells	5,716	28,893		
Last year observed for a family	2,913	25,980	4,387	45,333
Spells with gaps	176	25,804		
Spells with missing values for user cost	2	25,802	2	45,331
Spells with missing or zero values for income	751	25,051	1,451	43,880
Final number:	**55,736**	**25,051**	**36,907**	**43,880**

Notes: Sample is constructed from the Panel Study of Income Dynamics, 1970-1992. The "small" sample eliminates any observations corresponding to left-censored residence spells or spells with gaps.

Table 2: Sample Means

	"Small" Sample				Full Sample		
	Level	%Δ since start of spell	%Δ -- First differences	%Δ -- Third differences	Level	%Δ -- First differences	%Δ -- Third differences
House value	89,725 [77,208]	27.4 [48.7]	-0.08 [30.9]	0.7 [38.2]	82,820 [70,543]	0.2 [33.4]	1.7 [40.2]
Marginal user cost	0.096 [0.028]	7.4 [26.2]	2.1 [15.8]	8.5 [21.9]	0.096 [0.029]	0.8 [15.2]	4.4 [22.7]
Net-of-tax rate on HMI	0.74 [0.11]	0.8 [11.9]	0.5 [8.8]	1.9 [11.6]	0.76 [0.12]	0.3 [10.7]	1.2 [11.6]
Net-of-tax rate on int.	0.72 [0.13]	0.7 [13.0]	0.4 [10.6]	1.9 [13.3]	0.75 [0.14]	0.3 [10.7]	1.3 [13.5]
After-tax income	37,023 [27,185]	2.9 [39.7]	0.8 [37.1]	2.6 [40.6]	33,655 [26,354]	-0.1 [39.1]	-0.2 [43.4]
% of obs. within 10% of income	12.7 [5.8]	2.3 [19.8]	-0.1 [13.0]	-0.8 [18.9]	9.5 [5.8]	-1.1 [36.7]	-4.7 [46.6]
% of inc. above in distribution	57.5 [26.8]	7.4 [16.0]	1.4 [8.7]	4.5 [12.3]	75.6 [29.2]	0.7 [6.5]	2.3 [9.1]
Number of observations	25,048	24,955	20,279	13,546	43,877	36,903	26,678

Notes: Standard deviations are in square brackets. Percentage values have been multiplied by a factor of 100 to convert them to percents for the purpose of this table. Dollar values are in real (1990) dollars. "Small" sample refers to the sample where the first year of residence in a house must be observed.

Table 3: How Well Do Changes in Self-reported House Value Track House Prices?

Percent Change in Self-reported House Value	Estimated Coefficient on Percent Change in the State House Price Index					
	"Small" Sample			Full Sample		
	No Year Effects	Year Effects	Number of Obs.	No Year Effects	Year Effects	Number of Obs.
Percent differences since the start of the residence spell:						
Alone	0.641 (0.013)	0.145 (0.021)	14,422			
With State Effects	0.711 (0.014)	0.125 (0.025)	14,422			
Annual percent differences:						
De-meaned	0.038 (0.012)	0.141 (0.022)	18,337	0.090 (0.007)	0.128 (0.016)	35,565
First	0.160 (0.040)	0.042 (0.045)	14,422	0.208 (0.029)	0.068 (0.035)	31,032
Second	0.204 (0.030)	0.102 (0.034)	11,408	0.247 (0.020)	0.093 (0.025)	27,412
Third	0.212 (0.027)	0.145 (0.031)	9,009	0.269 (0.017)	0.127 (0.022)	24,367
Fourth	0.199 (0.027)	0.154 (0.029)	7,073	0.273 (0.016)	0.136 (0.020)	21,753
Fifth	0.183 (0.027)	0.160 (0.029)	5,542	0.262 (0.015)	0.141 (0.019)	19,534
Sixth	0.176 (0.028)	0.157 (0.030)	4,313	0.269 (0.015)	0.155 (0.019)	16,742
Seventh	0.172 (0.031)	0.159 (0.032)	3,324	0.278 (0.015)	0.165 (0.020)	14,351

Notes: Standard errors are in parentheses. "Small" sample refers to the sample where the first year of residence in a house must be observed. The construction of this sample is described in Table 1. The left-hand-side variable is the percent change in self-reported house value using the PSID over the period 1975 to 1991. The right-hand-side variable is the percent change in the Freddie-Mac repeat sale state house price index. The definition of percent change used is labeled in the leftmost column. All differences are taken within residence spells to hold constant the quantity of housing consumed.

Table 4: IV Regression -- The Effect of Changes in User Cost on House Prices

	1970-1991	Pre-1986	1986-On
No Year Dummies:			
% Δ User cost[1]	-0.211 (0.020)	-0.688 (0.034)	0.073 (0.047)
% Δ After-tax income	0.051 (0.009)	-0.048 (0.013)	0.064 (0.013)
Adjusted R^2	0.0019	0.0343	0.0090
Current and Initial Year Dummies:			
% Δ User cost[1]	0.061 (0.035)	-0.096 (0.045)	0.269 (0.057)
% Δ After-tax income	0.083 (0.008)	0.068 (0.013)	0.088 (0.011)
Adjusted R^2	0.3532	0.3255	0.3885
State House Price Index:			
% Δ User cost[1]	-0.033 (0.017)	-0.135 (0.034)	0.102 (0.039)
% Δ After-tax income	0.072 (0.007)	0.060 (0.012)	0.072 (0.011)
% Δ State house price index	0.802 (0.009)	0.842 (0.016)	0.733 (0.013)
Adjusted R^2	0.2990	0.2861	0.3128
# of observations	20,299	11,759	8,540

Notes: Standard errors are in parentheses. Time dimension controls are in italics. Left-hand-side variable is % Δ in self-reported house value. All differences are taken relative to the first year of the residence spell and are only calculated *within* a spell (that is, conditional on not moving). The first year of each residence spell is excluded from the sample. [1]% Δ User Cost is instrumented by % Δ (1-tax rate).

Table 5: IV Regression -- The Effect of Controlling for Changes in the Income Distribution on House Price Capitalization

	House Market "Fills from Above"			House Markets Affected by "Neighbor" Pressure			Mix of Hypotheses		
	Entire Period	Pre-1986	1986-On	Entire Period	Pre-1986	1986-On	Entire Period	Pre-1986	1986-On
% Δ User cost[1]	-0.113 (0.022)	-0.189 (0.040)	-0.040 (0.056)	-0.041 (0.017)	-0.131 (0.034)	0.088 (0.042)	-0.140 (0.024)	-0.195 (0.041)	-0.072 (0.058)
% Δ After-tax income	0.064 (0.007)	0.051 (0.012)	0.064 (0.011)	0.071 (0.007)	0.063 (0.012)	0.070 (0.011)	0.060 (0.007)	0.050 (0.013)	0.065 (0.011)
% Δ in % of income above[2]	0.242 (0.026)	0.293 (0.041)	0.216 (0.043)				0.357 (0.037)	0.318 (0.052)	0.438 (0.064)
% Δ in % pop. within ±10% income[3]				0.049 (0.014)	0.090 (0.020)	0.004 (0.024)	-0.110 (0.021)	-0.021 (0.026)	-0.227 (0.036)
% Δ State house price index	0.764 (0.010)	0.809 (0.019)	0.707 (0.013)	0.797 (0.009)	0.839 (0.017)	0.732 (0.013)	0.755 (0.021)	0.807 (0.019)	0.690 (0.013)
Adjusted R^2	0.3020	0.2893	0.3175	0.2982	0.2873	0.3112	0.3033	0.2892	0.3221
# of observations	20,217	11,691	8,526	20,241	11,713	8,528	20,211	11,687	8,524

Notes: Standard errors are in parentheses. Left-hand-side variable is % Δ in self-reported house value. The % Δ state house price index controls for variation in the time dimension. All differences are taken relative to the first year of the residnece spell and are only calculated *within* a spell (that is, conditional on not moving). The first year of each residence spell is excluded from the sample. [1]% Δ User Cost is instrumented by % Δ (1-tax rate). [2]% Δ in % of income above is the percent change in the amount of total family income in that year that is above the family's income in the income distribution. [3]% Δ if % pop. with ±10% income is the percent change in the percent of families in a year's income distribution that have incomes within 10 percent of the family's income.

Table 6: Controlling for Endogenous Moving with a Semiparametric Sample Selection Correction

	No Correction	Linear Hazards	Quartic Hazards
% Δ User cost[1]	-0.140 (0.024)	-0.167 (0.025)	-0.198 (0.028)
% Δ After-tax income	0.060 (0.007)	0.075 (0.008)	0.051 (0.009)
Trade up hazard		-0.859 (0.112)	[8.79] {0.003}
Trade down hazard		-2.430 (0.518)	[0.03] {0.863}
Own-to-rent hazard		-0.070 (0.077)	[12.69] {0.000}
% Δ State house price index	0.764 (0.010)	0.710 (0.012)	0.599 (0.017)
Adjusted R^2	0.3033	0.3066	0.3146

Notes: Standard errors are in parentheses. In columns 2 and 3, the standard errors are not corrected for including the sample selection terms. The left-hand-side variable is % Δ in self-reported house value. All differences are taken relative to the first year of the residence spell and are only calculated *within* a spell (that is, conditional on not moving). Predicted hazards are computed from a competing risks duration model with a nonparametric baseline hazard corresponding to one in Sinai (1997). The first year of each residence spell is excluded from the sample. The sample period thus covers 1971-1991, and includes 20,211 observations. The % Δ state house price index controls for variation in the time dimension. Other variables that are included but not reported are the two income distribution variables from table 5. [1]% Δ User cost is instrumented by % Δ (1-tax rate).

Table 7: Dynamics of Housing Price Responses Using a Distributed Lag

	Full Sample Period		Pre-1986	
% Δ User cost[1]	-0.023 (0.038)	0.008 (0.038)	-0.139 (0.047)	-0.098 (0.046)
% Δ User cost L1	-0.055 (0.050)	-0.040 (0.050)	-0.142 (0.060)	-0.112 (0.060)
% Δ User cost L2	-0.028 (0.054)	0.002 (0.054)	0.059 (0.063)	0.074 (0.064)
% Δ User cost L3	-0.021 (0.058)	0.001 (0.058)	-0.075 (0.072)	-0.064 (0.072)
% Δ User cost L4	0.018 (0.050)	0.011 (0.050)	-0.112 (0.065)	-0.109 (0.064)
% Δ After-tax income	0.030 (0.008)	0.035 (0.008)	0.018 (0.011)	0.026 (0.011)
% Δ After-tax income L1	0.029 (0.010)	0.030 (0.010)	0.007 (0.013)	0.012 (0.013)
% Δ After-tax income L2	0.008 (0.012)	0.012 (0.012)	0.016 (0.014)	0.017 (0.014)
% Δ After-tax income L3	0.022 (0.013)	0.026 (0.013)	0.022 (0.015)	0.023 (0.015)
% Δ After-tax income L4	0.026 (0.012)	0.024 (0.012)	-0.002 (0.015)	-0.001 (0.014)
% Δ % income above in dist'n	-0.057 (0.034)	-0.060 (0.034)	-0.032 (0.042)	0.001 (0.042)
% Δ % of pop. within ±10%	0.015 (0.007)	0.014 (0.007)	0.019 (0.008)	0.017 (0.008)
% Δ State price index		0.261 (0.022)		0.267 (0.041)
F-test sum of user cost	15.62 {0.0001}	0.36 {0.5486}	61.98 {0.0000}	29.52 {0.0000}
F-test sum of income	58.98 {0.0000}	72.81 {0.0000}	8.08 {0.0045}	12.63 {0.0004}
Adjusted R^2	0.0096	0.0138	0.0224	0.0257

Notes: Standard errors are in parentheses. Left-hand-side variable is % Δ self-reported house value. Changes are third-differences within a residence spell. If a difference cannot be computed due to sample restrictions, its value is set to zero. "L" refers to the number of periods the variable has been lagged. The sample size is 26,709 for the full sample period, 17,322 for the pre-1986 period. [1]% Δ user cost is instrumented by % Δ (1-tax rate).

Appendix Table A: First-stage Regressions of Net-of-tax Rates on User Cost (Corresponds to Table 4)

	1970-1991	Pre-1986	1986-On
No Year Dummies:			
% Δ Net-of-tax rate on HMI	1.026 (0.023)	0.955 (0.031)	0.594 (0.031)
% Δ Net-of-tax rate on Interest	0.514 (0.021)	0.418 (0.026)	0.495 (0.028)
% Δ After-tax income	0.112 (0.004)	0.059 (0.006)	0.056 (0.005)
Adjusted R^2	0.4085	0.3476	0.3034
Current and Initial Year Dummies:			
% Δ Net-of-tax rate on HMI	0.617 (0.011)	0.716 (0.015)	0.495 (0.016)
% Δ Net-of-tax rate on Interest	0.286 (0.009)	0.267 (0.012)	0.307 (0.014)
% Δ After-tax income	-0.008 (0.002)	-0.013 (0.003)	-0.000 (0.003)
Adjusted R^2	0.8807	0.8525	0.8363
State House Price Index:			
% Δ Net-of-tax rate on HMI	1.043 (0.023)	0.784 (0.030)	0.672 (0.030)
% Δ Net-of-tax rate on Interest	0.507 (0.021)	0.424 (0.025)	0.429 (0.027)
% Δ After-tax income	0.114 (0.004)	0.029 (0.006)	0.058 (0.005)
% Δ State house price index	0.028 (0.004)	-0.179 (0.007)	0.134 (0.005)
Adjusted R^2	0.4096	0.3858	0.3572
# of observations	20,299	11,759	8,540

Notes: Standard errors are in parentheses. Time dimension controls are in italics. Left-hand-side variable is % Δ in user cost. All differences are taken relative to the first year of the residence spell and are only calculated *within* a spell (that is, conditional on not moving). The first year of each residence spell is excluded from the sample. The F-statistic for the test of the null hypothesis that the sum of the estimated coefficients on the net-of-tax variables is equal to zero is always greater than 3,450 with a corresponding p-value of <0.0000.

Appendix Table B: House Prices and Changes in User Cost, Alternate Specifications

	Not Instrumenting	Third Differences
No Year Dummies:		
% Δ User cost[1]	-0.091 (0.013)	-0.081 (0.021)
% Δ After-tax income	0.064 (0.008)	0.024 (0.006)
Adjusted R^2	0.0062	0.0066
Nonparametric Year Controls:		
% Δ User cost[1]	-0.225 (0.022)	0.022 (0.030)
% Δ After-tax income	0.041 (0.007)	0.039 (0.007)
Adjusted R^2	0.3584	0.0180
State House Price Index:		
% Δ User cost[1]	-0.054 (0.011)	-0.012 (0.023)
% Δ After-tax income	0.069 (0.007)	0.035 (0.006)
% Δ State house price index	0.801 (0.009)	0.268 (0.019)
Adjusted R^2	0.2991	0.0119
# of observations	20,301	26,709

Notes: Standard errors are in parentheses. Time dimension controls are in italics; nonparametric year controls are initial and current year dummies for the first column, just year dummies for the second column. Left-hand-side variable is % Δ in self-reported house value. First column differences are taken relative to the first year of the residence spell and the second-column uses third differences. Differences are only calculated *within* a spell (that is, conditional on not moving). The first year of each residence spell is excluded from the sample. [1]% Δ User cost is instrumented by % Δ (1-tax rate), except in the first column which is not instrumented at all.