Tax Policy, Housing Markets, and Elderly Homeowners

by

Hui Shan

B.S. Economics and Biophysics
University of Michigan, 2003

Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in Economics
at the
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
June 2008

Author . . . .
Department of Economics
May 15, 2008

Certified by . . . .
James M. Poterba
Mitsui Professor of Economics
Thesis Supervisor

Certified by . . . .
William C. Wheaton
Professor of Economics
Thesis Supervisor

Accepted by . . . .
Peter Temin
Elisha Gray II Professor of Economics
Chairman, Department Committee on Graduate Students

Tax Policy, Housing Markets, and Elderly Homeowners

by

Hui Shan

Submitted to the Department of Economics
on May 15, 2008, in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy in Economics

Abstract

This dissertation consists of three essays studying the impact of tax policy on housing markets and elderly homeowners. Chapter One examines the potential lock-in effect of capital gains taxation on home sales, using the Taxpayer Relief Act of 1997 (TRA97) as a policy instrument. Before 1997, homeowners were subject to capital gains taxation when they sold their houses unless they purchased replacement homes of equal or greater value. Since 1997, homeowners can exclude $500,000 of capital gains when they sell their houses. Using zip-code level housing price indices and sales data from 1982 to 2006 on single-family houses in 16 affluent towns within the Boston metropolitan area, I find that TRA97 reversed the lock-in effect for houses with low and moderate capital gains. However, the semiannual home sale rate of houses with capital gains above $500,000 declined after TRA97, suggesting that TRA97 generated an unintended lock-in effect for houses with capital gains over the maximum exclusion amount.

Chapter Two studies the relationship between property taxes and elderly mobility. This is the first study using an instrumental variable approach to address the endogeneity problem associated with property taxes in analyzing elderly mobility. Using household-level panel data from the Health and Retirement Study (HRS) and a newly-collected dataset on state-provided property tax relief programs, I find evidence suggesting that higher property taxes raise mobility rates among elderly homeowners. Eligibility for relief programs lowers mobility rates, and the impact of these programs appears to vary with program types, program generosity, and implementation strategy.

Chapter Three investigates the effect of property taxes on elderly homeowners labor supply decisions, using similar data and empirical strategy employed in Chapter Two. I examine both the extensive margin - whether elderly homeowners' delay retirement or reenter the labor force in the face of rising property taxes, and the intensive margin - whether elderly homeowners work longer hours when property taxes increase. I find little evidence that property taxes have a significant impact on elderly labor supply.

Thesis Supervisor: James M. Poterba
Title: Mitsui Professor of Economics

Thesis Supervisor: William C. Wheaton
Title: Professor of Economics

Acknowledgments

I am deeply indebted to Jim Poterba for his insightful advice, unwavering support, and invaluable guidance. I have been most fortunate to learn from him as his student, his RA, his TA, and his advisee during the past five years. As accomplished and successful as he is, he shows great care and genuine respect for everyone who interacts with him. I wish to express my admiration and appreciation to him by striving to do excellent work and to be a respectable person in my career.

My two other advisors, Bill Wheaton and Jerry Hausman, earned my eternal gratitude. Sitting in Bill's office and discussing research ideas with him have been one of the most intellectually stimulating experiences for me at MIT. His boundless knowledge in housing economics and his constant encouragement have benefited me enormously. Jerry is one of the most brilliant economists that I know. His witty and incisive comments and his dedication to teaching and advising have made my study at MIT the most rewarding.

Special thanks go to Amy Finkelstein who taught me how to do quality empirical research when I worked as her RA after my first year at MIT. Her enthusiasm and passion for economic research have been truly inspiring to graduate students like me. Many professors at MIT have also generously offered me advice and assistance, including Josh Angrist, David Autor, Victor Chernozhukov, Peter Diamond, Jon Gruber, Michael Greenstone, Panle Jia, and Whitney Newey.

My undergraduate advisor at University of Michigan, Joel Slemrod, introduced me to the fascinating research world of taxation. His encouragement and guidance inspired me to study public finance at MIT. Over the past a few years, I had the opportunity to interact with many economists outside MIT, including Nathan Anderson, Jon Bakija, Katharine Bradbury, Karl Case, Daphne Kenyon, Kim Rueben, and Bob Tannenwald. This thesis benefited tremendously from their thoughtful comments and suggestions.

My friends at MIT and NBER deserve special mention. Tonja Bowen Bishop provided innumerable constructive comments on my research. Gathering around our

NBER carrels and chatting with her about economics, politics, and everything else were my favorite study breaks. I am grateful to Amanda Kowalski for making working hard fun. Her positive attitude, her work ethic, and our "Friday Lunches" enabled me to embrace the inevitable ups and downs in graduate school more gracefully and confidently. I am fortunate to know and coauthor with David Powell, a bright economist and a gifted stand-up comedian. His warm personality and great sense of humor are most endearing. I thank Filippo Balestrieri, Neil Bhutta, Tatiana Didier, Rongzhu Ke, Jin Li, and Chris Smith for their friendship and for making my years in Cambridge pleasant and memorable.

Financial support from a number of sources has helped me complete my thesis. My first two years at MIT were funded by The Yung Chen Memorial Fund. My last two years at MIT were supported by the National Institute on Aging, Grant Number P01-AG05842, and by The Lincoln Institute of Land Policy. For assistance with Chapter Two and Three, I thank Mohan Ramanujan at NBER for technical assistance with using HRS restricted data.

I would like to thank my parents and brother for their unconditional love and support. In every step of my life, they have always believed in me and been proud of me. Never having the privilege of going to college when they were young, my parents taught my brother and me only by example. For all the sacrifices that they made so that we could have a better future, I owe the world to them.

Finally, my greatest thanks are to my husband, Hao Zhou. His extraordinary patience and immeasurable love have made the past five years the happiest in my life. Without him, this thesis would not have been possible. I dedicate this thesis to him.

Contents

Introduction **8**

1 Effect of Capital Gains Taxation on Home Sales: Evidence from the Taxpayer Relief Act of 1997 **17**

1.1 Introduction 17

1.2 Background 20

1.2.1 Tax Law 20

1.2.2 Theoretical Predictions 23

1.2.3 Previous Studies 28

1.3 Data 31

1.4 Empirical Strategy and Estimation Results 38

1.4.1 Non-Parametric Approaches 38

1.4.2 A Difference-in-Differences Framework 42

1.4.3 Robustness Checks and Extensions 47

1.4.4 Estimate the Tax Elasticity of Home Sales 54

1.5 Conclusion 57

Bibliography **59**

2 Property Taxes and Elderly Mobility **71**

2.1 Introduction 71

2.2 Background and Previous Research 75

2.3 Data 79

2.3.1 HRS Household Level Panel Data 79
2.3.2 Data on Property Tax Relief Programs 82
2.4 Empirical Strategy and Results 88
2.4.1 Using Relief Benefits as Instruments 89
2.4.2 Using Variations in Housing Appreciation Rates 99
2.4.3 Do Liquidity Constraints Matter? 102
2.5 Program Evaluations 104
2.6 Conclusion 108

Bibliography **111**

3 Property Taxes and Elderly Labor Supply **125**
3.1 Introduction 125
3.2 Background 128
3.3 Data Description 132
3.4 Empirical Strategy and Estimation Results 138
3.4.1 Property Taxes and Retirement Decisions 138
3.4.2 Property Taxes and Reentry Decisions 142
3.4.3 Property Taxes and Working Hours 144
3.4.4 Robustness Checks and Extensions 146
3.5 Conclusion 150

Bibliography **153**

List of Figures

1-1 TRA97 Flow Chart 22
1-2 Tax Liability as a Function of Capital Gains 24
1-3 Tax Differences and Replacement Home Value for Homeowners with Capital Gains over $500K 27
1-4 Semiannual Price Appreciation Rates 33
1-5 Nominal Median Value of Single Family Houses 34
1-6 Histogram of Taxable Capital Gains 37
1-7 Local Polynomial Regression of the Raw Data 40
1-8 Sale Probability by Capital Gains Categories 41

2-1 Median Property Tax and House Value 76
2-2 Conditional Average Benefits by State 87

3-1 Labor Force Participation of Male Homeowners 134
3-2 Labor Force Participation of Female Homeowners 135
3-3 Empirical Retirement Hazard Rate of Homeowners 136

List of Tables

1.1 Town Characteristics 61

1.2 Number of Parcels by Town 62

1.3 Percent of Parcels in Sample 63

1.4 Sale Prices and Numbers 64

1.5 Summary Statistics 65

1.6 Main Results 66

1.7 Robustness Checks 67

1.8 Effect of TRA97 over Time 68

1.9 Tax Elasticity of Home Sales 69

2.1 Mobility and Homeownership 115

2.2 Summary Statistics 116

2.3 Benefit Formula Examples 117

2.4 Property Tax Relief Program Benefits 118

2.5 Main Results 119

2.6 Probit, Random Effect Probit, and LPM 120

2.7 Robustness Checks 121

2.8 ETR Interacted with h 122

2.9 Program Evaluation 123

3.1 PDV of Hypothetical Property Tax Increases 156

3.2 Summary Statistics of Analysis Samples 157

3.3 Retirement Estimation Results 160

3.4 Reentry Estimation Results 162
3.5 Working Hour Estimation Results 164
3.6 Robustness Checks - Female Retirement Sample 166
3.7 Robustness Checks - Reentry Samples 167
3.8 Robustness Checks - Female Working Hour Sample 168
3.9 Property Tax Effect on Homeowners of Different Age Groups 169

Introduction

In the United States, federal income tax system subsidizes investment in owner-occupied housing through four major provisions. First, imputed rent implicitly received by homeowners is untaxed. Second, mortgage interest payments are deductible for homeowners who choose to itemize their deductions. Third, capital gains on owner-occupied houses are largely untaxed. Lastly, property tax payments are deductible for homeowners who choose to itemize their deductions. Because housing wealth is often the most important wealth component for many American homeowners, these tax provisions may have significant economic impact on housing markets and homeowners. This thesis consists of three empirical studies that provide new evidence on the link between housing tax policy and homeowners' behavioral responses. In particular, the first paper considers the effect of housing capital gains taxation on homeowners' decisions to sell their homes. The second and third papers study how rising property taxes may have affected elderly homeowners in their mobility and labor supply decisions.

The Taxpayer Relief Act of 1997 (TRA97) is one of the most sweeping reforms in the history of housing capital gains taxation. Before 1997, homeowners were subject to capital gains taxation when they sold their houses unless they resort to the "roll-over rule" or the "age-55 rule." The roll-over rule allowed home sellers to postpone their capital gains provided that they purchase replacement homes of equal or greater value. The age-55 rule allowed home sellers of age 55 or above to claim a one-time exclusion of $125,000. Since 1997, home sellers can exclude $500,000 of capital gains and such exclusions can be used as often as every two years. Previous research on

TRA97 has solely relied on the age-55 rule for identification due to data limitations in public surveys.

In Chapter One, I use zip-code level housing price indices and sales records on single-family houses in 16 affluent towns within the Boston metropolitan area between 1982 and 2006 to construct a unique panel of single-family houses. By exploiting the cross-sectional variation in accumulated capital gains and the arguably exogenous change in exclusion levels introduced by TRA97, I am able to identify the effect of capital gains taxation on home sales. Both non-parametric analysis and regression results show that the home sale rate increased after 1997 for homeowners with capital gains between \$0 and \$500,000, suggesting that TRA97 reversed the lock-in effect of capital gains taxes for these homeowners. In contrast, home sale rate declined after 1997 for homeowners with capital gains above \$500,000, suggesting that TRA97 may have generated an unintended lock-in effect for these homeowners by eliminating the roll-over rule.

This paper is the first study that examines the effect of TRA97 on home sales for houses with capital gains over \$500,000. It is also the first study that estimates the tax elasticity of home sales using post-TRA97 data. Because capital gains exclusions are defined in nominal terms rather than in real terms, a growing number of homeowners start to find themselves with more than \$500,000 housing capital gains, especially during housing market booms similar to the one we experienced in the early 2000s. Moreover, capital gains tax rates may rise significantly after the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA) expires in 2011, which will increase tax liabilities for home sellers with capital gains over \$500,000. Thus, the findings of this paper have important implications to housing market dynamics in the future.

The housing market boom during late 1990s and early 2000s has caused sharp increases in residential property taxes in the United States. Housing-rich but income-poor elderly homeowners often complain about rising tax burdens, and anecdotal evidence suggests that some move to reduce their tax burden. There has been little systematic analysis, however, of the link between property tax levels and the mobility rate of elderly homeowners. Chapter Two of this thesis investigates this link using household-level panel data from the Health and Retirement Study (HRS) and a newly-collected dataset on state-provided property tax relief programs. These relief programs generate variation in property tax burdens that is not due solely to arguably endogenous local community choices about taxes and expenditure programs. The findings of this paper provide robust evidence that higher property taxes raise mobility among elderly homeowners. A reduced form analysis of mobility rates and property tax relief programs suggests that eligibility for relief programs lowers mobility rates. The impact of relief programs appears to vary with program types, program generosity, and implementation strategy.

Using the same data and empirical strategies as in Chapter Two, Chapter Three of this thesis empirically tests whether rising property taxes have led elderly homeowners to increase their labor supply. This is the first study that estimates the wealth effect of property tax on elderly homeowners' labor supply behavior. To obtain a comprehensive picture of elderly homeowners' labor supply response to rising property taxes, I examine both the extensive margin - whether elderly homeowners delay retirement or reenter the labor force to pay for higher property taxes, and the intensive margin - whether elderly homeowners work longer hours when property taxes increase. Across various sub-samples and alternative specifications, I find little evidence that property taxes have a significant impact on elderly homeowners' decisions to retire, to reenter the labor force, or to increase working hours. Taken together, findings in Chapter Two and Chapter Three imply that elderly homeowners have chosen to lower their property tax burdens by moving to lower tax areas or downsizing rather than by increasing their labor supply.

Chapter 1

Effect of Capital Gains Taxation on Home Sales: Evidence from the Taxpayer Relief Act of 1997[1]

1.1 Introduction

In the United States, capital gains are generally taxed upon realization and appreciated assets enjoy basis step-up when transferred by bequest. Economists have long recognized the potential lock-in effect of capital gains taxation in financial markets. However, very few empirical studies have examined the lock-in effect of capital gains taxation in housing markets. The Taxpayers Relief Act of 1997 (TRA97) has generated the most drastic changes in the tax treatment of housing capital gains since the late 1970s, and therefore, serves as a natural experiment for researchers to study the impact of capital gains taxation on housing markets. For example, Bier, Maric and Weizer (2000), Farnham (2006), Cunningham and Engelhardt (2007), and Biehl

[1]I thank Joe Nugent, Karen MacTavish, Knorr Maryanne, David Stiff, Tim Warren Jr, and especially Alan Pasnik and Jim Shaughnessy for generously providing me with data and patiently answering my questions.

and Hoyt (2008) use TRA97 as a policy instrument to examine the effect of housing capital gains taxation on homeowners' moving decisions.

Prior to TRA97, homeowners had to pay capital gains taxes when they sold their houses unless they resorted to the "roll-over rule" or the "age-55 rule." The roll-over rule allowed a home seller to postpone his capital gains taxes provided that he bought another home of equal or greater value within two years. The age-55 rule allowed home sellers of age 55 or older to claim a one-time exclusion of $125,000 against their capital gains. TRA97 abolished both the roll-over rule and the age-55 rule. Instead, homeowners can exclude $500,000 (or $250,000 for single filers) capital gains when they sell their houses after TRA97, and they can potentially claim such an exclusion as often as every two years.

Existing studies on TRA97, including Farnham (2006), Cunningham and Engelhardt (2007), and Biehl and Hoyt (2008), have found that capital gains taxes during the pre-TRA97 period locked in many homeowners and that TRA97 released such lock-in effects. These studies, however, have two major limitations. First, because survey datasets that are publicly available often do not have sufficient information on house values for researchers to infer accumulated capital gains, most existing studies on TRA97 have relied on the age-55 rule for identification. Second, even when it is possible to impute accumulated capital gains, nationally representative surveys usually do not capture enough high-end houses for researchers to study homeowners with capital gains above $500,000. For example, Farnham (2006) uses 1989-2003 American Housing Survey (AHS) data where the median house value is only $101,257 and the median capital gains are only $34,856 in 2000 dollars.

Due to these data limitations, several important aspects of TRA97 remain unaddressed or understudied by the existing literature, including whether TRA97 differentially affected homeowners with different levels of accumulated capital gains, how the repeal of the roll-over rule affected homeowners with capital gains over $500,000, and what the tax elasticity of home sales has been after 1997. These unanswered questions have important economic and policy implications for a number of reasons. First, capital gains exclusions are defined in nominal terms and a growing number of homeowners start to find themselves with more than $500,000 housing capital gains, especially during the 2000-2005 housing market boom. Second, capital gains tax rates may increase after the Jobs and Growth Tax Relief Reconciliation Act of 2003 (JGTRRA) expires in 2011, which can potentially affect housing markets nationwide. Third, tens of millions of baby-boomers are entering retirement age and considering selling their big houses to reduce housing consumptions. Capital gains taxes will become relevant to many of them since they tend to have lived in their homes for decades and have accumulated sizable capital gains.

In this paper, I construct a panel of single-family houses using zip-code level semiannual housing price indices and 1982-2006 sales records in 16 affluent towns within the Boston metropolitan area. Because the sales data originally come from local registries of deeds, they are not subject to top-coding, and they are more accurate than self-reported housing values found in most survey datasets. To identify the effect of capital gains taxation on home sales, I exploit the cross-sectional variation in accumulated capital gains and the arguably exogenous change in exclusion levels introduced by TRA97. I also exploit legislative changes in capital gains tax rates in 2001 and 2003 to estimate the tax elasticity of home sales during the post-TRA97 period. This paper is the first study to look at how TRA97 affects houses with capital gains exceeding $500,000. It is also the first study to estimate the tax elasticity of home sales using post-TRA97 data.

A number of interesting findings emerge from this paper. First, the semiannual home sale rate increased after TRA97 among homeowners with capital gains between $0 and $500,000, suggesting that these homeowners were locked in by housing capital gains taxes before 1997 and TRA97 reversed such a lock-in effect. Second, for houses with capital gains over $500,000, the semiannual home sale rate declined after TRA97. This finding suggests that TRA97 may have unintentionally locked in homeowners with capital gains exceeding the maximum exclusion level. Furthermore, the releasing effect of TRA97 on homeowners with capital gains between $0 and $500,000 appears to be short-lived, whereas the locking-in effect of TRA97 on homeowners with capital gains above $500,000 appears to be long-lasting. Lastly, estimation results on the tax elasticity of home sales during the post-TRA97 period suggest that a $10,000 increase in capital gains taxes lowers semiannual home sale rate by 0.16-0.25 percentage points.

The rest of this paper proceeds as follows. Section 2 introduces the background on housing capital gains taxation and TRA97, illustrates how TRA97 may affect home sales, and gives an overview of the existing literature. In section 3, I describe the data used in this paper. I then explain my empirical strategies, discuss estimation results, and show robustness checks and extensions to the main model in section 4. The last section concludes.

1.2 Background

1.2.1 Tax Law

TRA97 greatly simplified the tax treatment of housing capital gains. Before 1997, a home seller was subject to capital gains taxation if the selling price net of selling expenses exceeded the adjusted basis of the home. The adjusted basis is defined as purchase price plus purchase costs (e.g. settlement fees and closing costs) and the cost of improvements and additions.[2] However, if the home seller bought a replacement home of equal or greater value within a four-year window, which started two years before and ended two years after the date of sale, he would postpone the capital

[2]According the IRS rules, the cost of improvements and additions can be added to the adjusted basis, whereas the cost of repairs cannot. IRS publication 523 has more details on the distinction.

gains taxes.[3] If the replacement home value was between the purchase price and the selling price of the current home, the difference between the replacement home value and the selling price of the current home would result in immediate taxes, and the difference between the replacement home value and the purchase price of the current home would be postponed. The amount of postponed capital gains would be subtracted from the basis of the newly purchased replacement home. This tax provision, unofficially called the "roll-over rule," had been in the Internal Revenue Code since 1951.

In addition to the roll-over rule, which provided preferential tax treatment for home sellers who bought more expensive replacement homes, the Internal Revenue Code also featured preferential tax treatment for older home sellers before TRA97. Beginning in 1964, homeowners aged 65 and over who had lived in their homes for at least five years during the past eight years could claim a once-in-a-lifetime exclusion of up to $20,000 against taxable capital gains.[4] The maximum exclusion amount was raised to $35,000 in 1976. In 1978, the age requirement was lowered to 55, the residence requirement was changed from living in the home for at least five out of previous eight years to three out of previous five years, and the maximum exemption amount was raised to $100,000.[5] Finally, in 1981, the maximum exclusion amount was raised to $125,000. This "age-55 rule" remained unchanged until TRA97.[6]

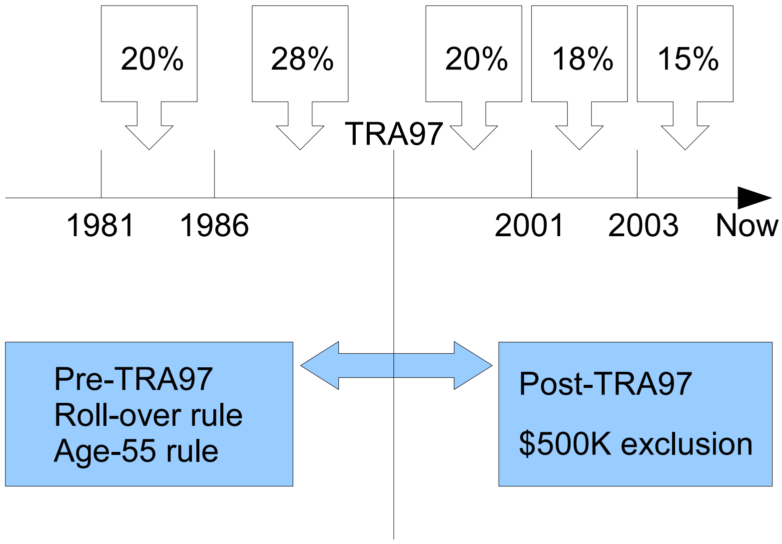
[3] In fact, IRS Publication 523 explicitly says that "Generally, you *must* postpone tax on the gain on the sale of your main home if you buy and live in a new main home within the replacement period and it costs at least as much as the adjusted sales price of the old home."

[4] The exclusion amount equaled the total capital gain if the sale price was less or equal to $20,000. For homes selling for more, the excludable portion was calculated by multiplying the capital gain by the ratio of $20,000 to sale price.

[5] This $100,000 exclusion did not depend on the sale price.

[6] This one-time exclusion was $125,000 for both single filers and married joint filers. Married separate filers, however, had a one-time exclusion of only $62,500. In addition, the exclusion could only be used once in a lifetime and no balance could be carried forward for a future sale.

Figure 1-1: TRA97 Flow Chart



TRA97 was signed into law on August 5, 1997. Effective for sales after May 6, 1997, it fundamentally altered the tax treatment of housing capital gains. First, TRA97 eliminated the roll-over rule. After 1997, the tax treatment of housing capital gains no longer depended on whether a home seller bought a replacement home or the value of the replacement home. Second, it eliminated the age-55 rule. Older home sellers now face the same tax treatment as their younger counterparts. Third, it allowed home sellers to exclude $500,000 (or $250,000 for single filers) housing capital gains if they have owned and lived in their homes for at least two years during the past five. There is no limit on how many times one can claim such exclusions during one's lifetime, as long as the ownership and use tests are met. Finally, TRA97 lowered the long-term capital gains tax rates from 15% and 28% to 10% and 20%.[7] Figure

[7]Capital gains tax rates have been changed many times since 1981. Before the Tax Reform Act of 1986, the top marginal tax rate was 20%. The Tax Reform Act of 1986 raised it to 28%, although effective tax rates exceeded 28% for many high-income taxpayers because of interactions with other tax provisions. TRA97 reduced capital gains tax rates and introduced a separate rate schedule for long-term gains. Beginning May 7, 1997, the top rate on long-term capital gains was 20%. Beginning

1-1 summarizes the key changes in housing capital gains taxation between 1981 and now.

1.2.2 Theoretical Predictions

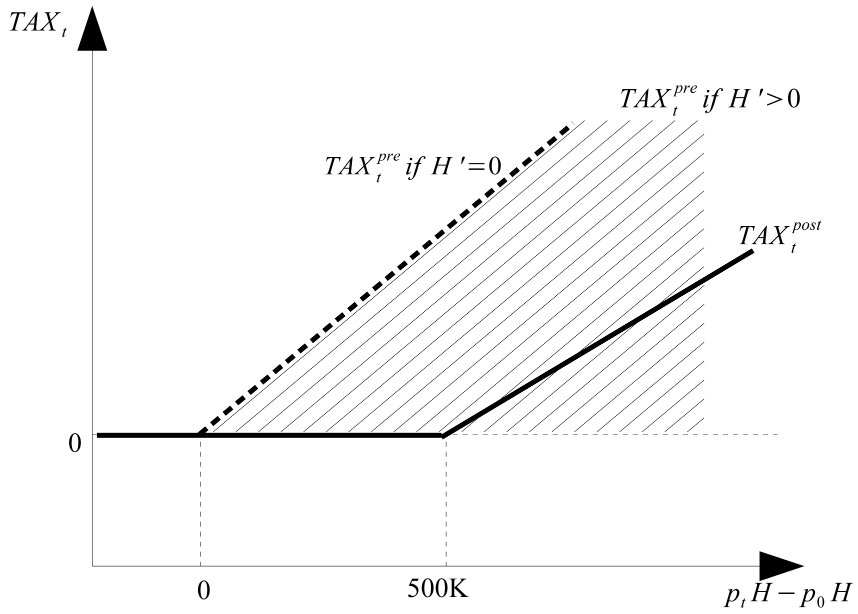
To evaluate the impact of TRA97 on home sales, we need to analyze how homeowners who have accumulated different levels of capital gains and who desire to purchase different replacement homes are affected by TRA97 differently. Suppose a homeowner bought his house at time 0 when the per-unit housing price was p_0. Let H denote the amount of housing purchased by this homeowner. At time t, the per-unit housing price is p_t, and the homeowner considers selling his house. In the event that he sells his house at time t, he would like to purchase a replacement home of quantity H' at price p_t. If his replacement home is actually a rental housing unit, $H' = 0$. For ease of exposition, I make two simplification assumptions. First, I assume this homeowner is younger than 55 or he has used the one-time capital gains exclusion under the pre-TRA97 tax regime if he is 55 or older. Under this assumption, we can ignore the age-55 rule for the moment. Second, I assume away purchase expenses and selling expenses when imputing capital gains. Without imposing these assumptions, the qualitative conclusions drawn in this section remain the same, but the notation would have been far more complicated.

Given the tax law described above, this homeowner's tax liability under the pre-TRA97 tax regime is

$$Tax_t^{pre} = \begin{cases} \tau_t^{pre}(p_t H - p_0 H) & \text{if } p_t H' \le p_0 H \\ \tau_t^{pre}(p_t H - p_t H') & \text{if } p_0 H < p_t H' < p_t H \\ 0 & \text{if } p_t H' \ge p_t H \end{cases}$$

in 2001, the top rate on assets held for at least five years was 18%. The Jobs and Growth Tax Relief Reconciliation Act of 2003 lowered the top capital gains tax rate to 15%.

Figure 1-2: Tax Liability as a Function of Capital Gains



where τ_t^{pre} is the capital gains tax rate faced by the homeowner under the pre-TRA97 tax law. Similarly, his tax liability under the post-TRA97 tax regime is

$$Tax_t^{post} = \begin{cases} 0 & \text{if } p_tH - p_0H \leq \$500K \\ \tau_t^{post}(p_tH - p_0H - 500K) & \text{if } p_tH - p_0H > \$500K \end{cases}$$

where τ_t^{post} is the capital gains tax rate faced by the homeowner under the post-TRA97 tax law.

Figure 1-2 illustrates the difference between Tax_t^{pre} and Tax_t^{post} graphically. The bold solid line represents Tax_t^{post}, which does not depend on replacement home values p_tH'. Tax_t^{post} is zero before capital gains $p_tH - p_0H$ reach \$500,000. As $p_tH - p_0H$ continues to rise above \$500,000, Tax_t^{post} increases linearly in $(p_tH - p_0H - 500K)$

with a slope of τ_t^{post}. On the other hand, capital gains taxes depend on replacement home value $p_t H'$ before TRA97. When the homeowner with positive capital gains chooses not to purchase a replacement home, $H' = 0$ and Tax_t^{pre} is a linear function of $p_t H - p_0 H$ with a slope of τ_t^{pre}. The dotted line represents Tax_t^{pre} in this case. As H' increases, the value of his replacement home, $p_t H'$, increases accordingly, which leads Tax_t^{pre} to shift parallelly to the right in Figure 1-2. The upward-sloping segment of Tax_t^{pre} is steeper than that of Tax_t^{post} because TRA97 reduced capital gains tax rates and hence, $\tau_t^{post} < \tau_t^{pre}$.

All else equal, higher capital gains tax liabilities raise moving costs and reduce the probability of home sales. To predict how TRA97 would affect home sales is equivalent to comparing Tax_t^{post} with Tax_t^{pre} for homeowners with different capital gains and different desired replacement homes. If $Tax_t^{post} - Tax_t^{pre}$ is positive, it means that TRA97 raised tax burdens and home sale rates should decline after 1997. On the other hand, if $Tax_t^{post} - Tax_t^{pre}$ is negative, it suggests that TRA97 reduces tax burdens and home sale rates should increase after 1997.

For homeowners with capital gains between \$0 and \$500,000,

$$Tax_t^{post} - Tax_t^{pre} = \begin{cases} -\tau_t^{pre}(p_t H - p_0 H) & \text{if } p_t H' \leq p_0 H \\ -\tau_t^{pre}(p_t H - p_t H') & \text{if } p_0 H < p_t H' < p_t H \\ 0 & \text{if } p_t H' \geq p_t H \end{cases}$$

In this case, $Tax_t^{post} - Tax_t^{pre}$ is unambiguously negative, suggesting that homeowners with capital gains in this range are more likely to sell their homes after TRA97, *ceteris* *paribus.* For homeowners with capital gains above \$500,000,

$$Tax_t^{post} - Tax_t^{pre} =$$
$$\begin{cases} (\tau_t^{post} - \tau_t^{pre})(p_tH - p_0H) - \tau_t^{post} \cdot 500K & \text{if } p_tH' \leq p_0H \\ (\tau_t^{post} - \tau_t^{pre})p_tH + \tau_t^{pre}p_tH' - \tau_t^{post}(p_0H + 500K) & \text{if } p_0H < p_tH' < p_tH \\ \tau_t^{post}(p_tH - p_0H - 500K) & \text{if } p_tH' \geq p_tH \end{cases}$$

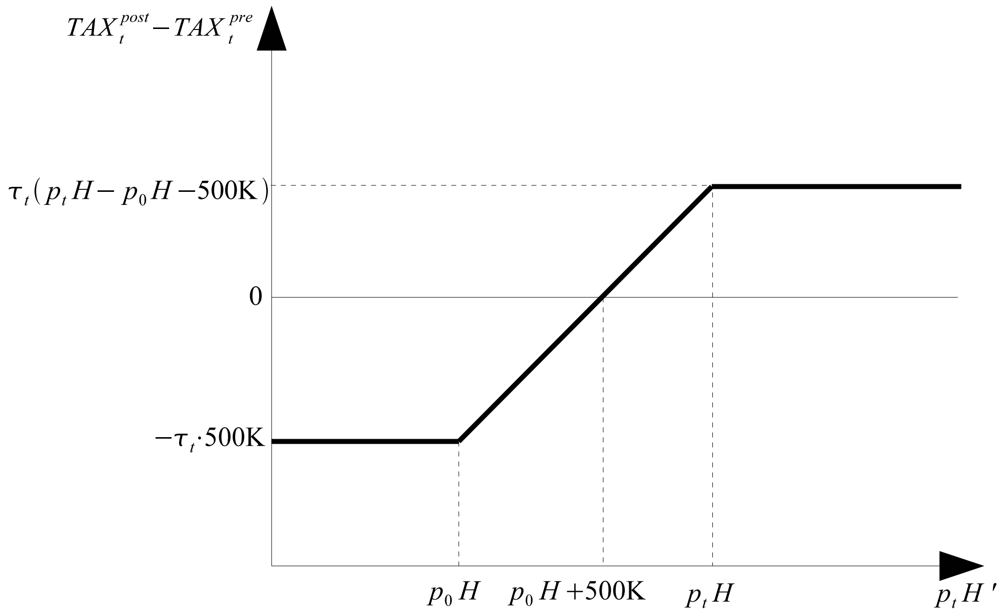
To simplify the discussion, I assume $\tau_t^{post} = \tau_t^{pre} = \tau_t$ and rewrite the above equation:

$$Tax_t^{post} - Tax_t^{pre} = \begin{cases} -\tau_t \cdot 500K & \text{if } p_tH' \leq p_0H \\ \tau_t(p_tH' - p_0H - 500K) & \text{if } p_0H < p_tH' < p_tH \\ \tau_t(p_tH - p_0H - 500K) & \text{if } p_tH' \geq p_tH \end{cases}$$

The sign of $Tax_t^{post} - Tax_t^{pre}$ in this case is ambiguous because it depends on the replacement home value p_tH'. For example, if the replacement home value is sufficiently low, then $Tax_t^{post} - Tax_t^{pre}$ is negative and home sale rates would be higher after TRA97 among these homeowners. However, if the replacement home value is sufficiently high, $Tax_t^{post} - Tax_t^{pre}$ becomes positive and home sale rates would decline after TRA97.

Figure 1-3 depicts the relationship between $Tax_t^{post} - Tax_t^{pre}$ and p_tH' graphically. It shows that, *a priori*, we cannot predict how TRA97 would affect home sale rate for homeowners with capital gains above \$500,000. If most of these homeowners move to significantly less expensive replacement homes after selling their houses, then their tax burdens are lower under the post-TRA97 regime and home sale rates would increase after TRA97. In contrast, if most homeowners with large capital gains prefer living in relatively expensive replacement homes, then their tax burdens are actually higher under the new tax regime, and TRA97 would cause home sale rates to decline. The intuition behind this observation comes from the tradeoff between

Figure 1-3: Tax Differences and Replacement Home Value for Homeowners with Capital Gains over \$500K



the elimination of the roll-over rule and the enactment of a more generous exclusion provision. On one hand, the \$500,000 exclusion would reduce the amount of capital gains taxes owed, and hence, would reverse the lock-in effect of capital gains taxes. On the other hand, the elimination of the roll-over rule implies that a home seller could always avoid paying any tax by purchasing an equally or more expensive house before 1997, but after 1997 he must pay tax on the portion of capital gains exceeding \$500,000 no matter what. If the latter effect overcomes the former effect, TRA97 might unintentionally lock in homeowners with capital gains over \$500,000, and we might observe a drop in home sale rates after 1997 among these people.

In summary, the above analysis predicts that TRA97 would increase home sale rates among homeowners with capital gains between \$0 and \$500,000. The effect of TRA97 on home sales among homeowners with capital gains above \$500,000 is ambiguous in theory, and thus, only empirical investigation can determine the sign and magnitude of the effect of TRA97 on these homeowners. Note that the above analysis refers to married homeowners. For single homeowner, the exclusion level is \$250,000, and theoretical predictions differ for single homeowners with capital gains between \$0 and \$250,000 from those with capital gains over \$250,000.

1.2.3 Previous Studies

The pre-TRA97 capital gains taxation had been criticized for its complexity and potentially large distortions of homeowners' mobility and housing consumption decisions. Using 1970-1981 Panel Study of Income Dynamics (PSID) data, Newman and Reschovsky (1987) showed that that the annual mobility rate of homeowners 55 to 64 years old increased after the 1978 reform, which raised the exclusion level from \$35,000 to \$100,000 and lowered the age requirement from 65 to 55. Hoyt and Rosenthal (1990) first recognized that the roll-over rule generated "kinks" in home sellers' budget sets and encouraged them to consume more housing than they otherwise would have. Such kinks were ignored by previous studies on housing demand such as King (1980) and Rosen (1979). Using 1981 AHS data, Hoyt and Rosenthal (1990) estimated the price elasticity of housing demand with non-linear budget sets. Hoyt and Rosenthal (1992) performed policy simulations using the estimation results of their previous paper. Their simulations suggest that by increasing capital gains tax rates, the Tax Reform Act of 1986 enhanced the importance of the capital gains kinks in home sellers' budget sets and therefore produced a larger efficiency loss. Using 1993 IRS Statistics of Income tax return data, Burman, Wallace and Weiner (1996) showed that the tax raised little revenue: A total of \$50.5 billion housing capital gains were reported on Form 2119 in 1993, but \$18 billion was not taxable because of the age-55 rule and $30 billion was not taxable because of the roll-over rule. The authors estimated a sequential-choice model and found that capital gains taxes had a large and significant impact on the decision to buy or to rent. They also found that capital gains taxes were a large but only marginally significant deterrent to moving down (i.e. moving to less expensive houses), but they did not find any effect of taxes on the amount of housing demanded by home sellers who chose to move down. Furthermore, the authors argued that the housing capital gains taxation at the time was regressive because it treated more favorably home sellers who could afford expensive replacement houses, and the compliance cost was high because of record-keeping and complex rules. Sinai (1998) applied a competing-risk duration model on 1970-1992 PSID data to estimate the effect of capital gains taxes on homeowners' mobility decisions. Unlike Burman, Wallace and Weiner (1996), he found that capital gains taxes had a statistically significant but small impact on the likelihood of moving, the choice of owning versus renting, and the choice of moving up versus down.

To my knowledge, only four papers have studied the impact of TRA97 on residential mobility. Using deed transfer data from nine years before to 17 months after TRA97 in four Ohio metropolitan areas, Bier, Maric and Weizer (2000) found no evidence that the probability of moving down increased after 1997. Instead, they found that moving up dominated all four areas. Farnham (2006) used 1983-2003 AHS data to study how the elimination of the age-55 rule affected residential mobility. He found evidence suggesting that homeowners under age 55 were locked in before TRA97 and the passage of TRA97 boosted residential mobility among the previously locked-in households. Cunningham and Engelhardt (2007) used 1996 and 1998 Current Population Survey (CPS) data to compare the mobility rates of homeowners just above age 55 (i.e. 56-58) and homeowners just below (i.e. 52-54). They found that the repeal of the age-55 rule raised residential mobility of the 52-54 year olds by 22-31 percent from the mean annual mobility rate of 4 percentage points. Biehl and Hoyt (2008) used 1995-1996, 1998-1999, and 2002-2004 AHS data to conduct a similar analysis. They found that TRA97 increased mobility rates of homeowners aged 50-54 relative to homeowners aged 55-65, but such a release effect dissipated a few years after the passage of TRA97.

Overall, most existing studies on TRA97 have focused exclusively on the elimination of the age-55 rule. It is understandable that they have done so because of data limitations. For example, CPS does not have information on house values. AHS top codes house values at $300,000, which makes it impossible for researchers to investigate the impact of TRA97 on houses with capital gains over $500,000. Moreover, housing values in survey datasets are self-reported. Goodman and Ittner (1992) and Kiel and Zabel (1999) show that individual homeowners do not report their housing values accurately in survey datasets. Even if such measurement errors in self-reported values are random and do not systematically correlate with mobility outcomes, they may still cause attenuation bias in empirical analysis. As a result, there has been no research examining the mobility response to TRA97 of homeowners with capital gains over $500,000. Nor have there been any studies that estimate the tax elasticity of home sales using post-TRA97 data.

This paper fills these gaps in the literature. Instead of using public surveys where the unit of observation is typically a homeowner, this paper uses a unique panel of housing units constructed from zip-code level semiannual housing price indices and 1982-2006 sales records on single-family houses in 16 affluent towns within the Boston metropolitan area. These sales records are originally taken from local registries of deeds and therefore are accurate and not subject to top-coding. This paper exploits the variation in exclusion levels before and after TRA97 - $125,000, $250,000, and $500,000 - to identify the effect of capital gains taxation on home sale rates. Both nonparametric and regression analysis are used to study such an effect. It also exploits legislative changes in capital gains tax rates introduced in 2001 and 2003 to estimate the tax elasticity of home sales in the post-TRA97 period. Because of the innovations in both data and identification strategies, this paper complements the existing studies on TRA97 and sheds new light on the lock-in effect of capital gains taxation in housing markets.

1.3 Data

The backbone of the data analyzed in this paper is the sales records provided by *The Warren Group*. These are single-family house sales in 16 cities and towns within the Boston metropolitan area from 1982 to 2006. The 16 cities and towns are Belmont, Brookline, Cambridge, Carlisle, Cohasset, Concord, Dover, Lexington, Lincoln, Needham, Newton, Sherborn, Sudbury, Wellesley, Weston, and Winchester. I selected these places because their 2006 median single-family house sale prices were over $625,000, they do not have active real estate markets for second homes, and they are all located in the Middlesex and Norfolk counties. Homeowners living in these cities and towns are mostly high-income and well-educated individuals. Table 1.1 shows that 61-83% of individuals 25 years and older who live in these places have at least a Bachelor's degree, compared with the average of 33% in Massachusetts. The 1999 median household income of homeowners in these places was around $100,000.[8] Another important feature of these 16 cities and towns is that the number of single-family houses has been roughly constant in each jurisdiction. Table 1.2 shows no significant changes in the stock of single-family houses during the past 15 years in these 16 cities and towns.

The sales data have two components. The first contains exhaustive records on single-family house sales between 1987 and 2006.[9] In other words, if there was a

[8] Brookline and Cambridge had much lower median household income because of the large renting population in these two cities.

[9] Because the Newton data were extracted in 2006 and the rest were extracted in 2007, I only

single-family house in any of the 16 cities and towns that was sold anytime between 1987 and 2006, the sale record would appear in the data. The raw data have a total of 78,599 sales of 48,240 single family houses. Each record has information on parcel ID, parcel location, sale date, sale price, buyer name, seller name, current assessment value, house characteristics such as lot size, living area, year built, total number of rooms, number of bedrooms, number of bathrooms, and building style.[10] The second component contains sales records from 1982 to 1986. This dataset was compiled initially by a company that was later acquired by *The Warren Group*. As a result, it only has information on parcel location, sale date, sale price, buyer name, and seller name. Moreover, it is unclear whether the sales records are exhaustive and whether a sale was on a single-family house. The raw data have a total of 36,103 sales recorded.

To separate sales of single-family houses from sales of other properties in the 1982-1986 dataset, I requested local assessment data from all 16 cities and towns. Based on the assessment data provided by local assessors' offices, I constructed a "universe" of single-family houses for the 16 cities and towns. This constructed universe has a total of 80,978 parcels, and it contains information on parcel ID, parcel location, parcel zip code, current assessment value, and lot size.[11] Merging this universe dataset with the 1982-1986 sales records by parcel location, I identified 11,458 sales of single-family houses. Then I combined the 1982-1986 data with the 1987-2006 data. After a series of data cleaning procedures, I obtained a sales dataset with a total of 82,884 sales records on 50,369 parcels.[12] Around 57% of these parcels were sold only once between

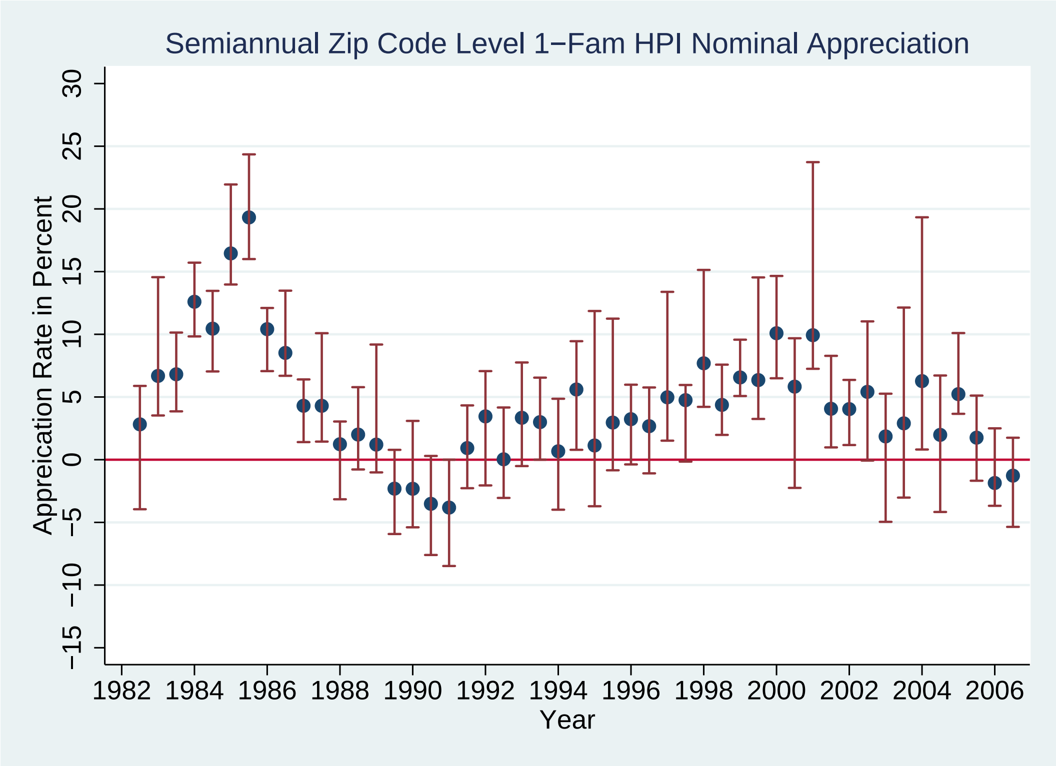
have sales between January 1, 1987 and June 23, 2006 for the city of Newton. For the other 15 cities and towns, I have all sales between January 1, 1987 and December 31, 2006.

[10]Parcel and parcel ID are terms used in assessment practice. In this paper, a parcel means a single-family house. A parcel ID is a unique ID that is attached to a single-family house. The current assessment value refers to the FY2006 assessment value for the city of Newton and FY2007 for the other 15 cities and towns.

[11]I tried to obtain additional housing characteristics such as living area, year built, total number of rooms, number of bedrooms, number of bathrooms, and building style. But because each of these variables was missing in at least one town's assessment data, I had to exclude them from the single-family house universe dataset.

[12]Such procedures include dealing with sales between non-individual parties (e.g. financial insti-

Figure 1-4: Semiannual Price Appreciation Rates



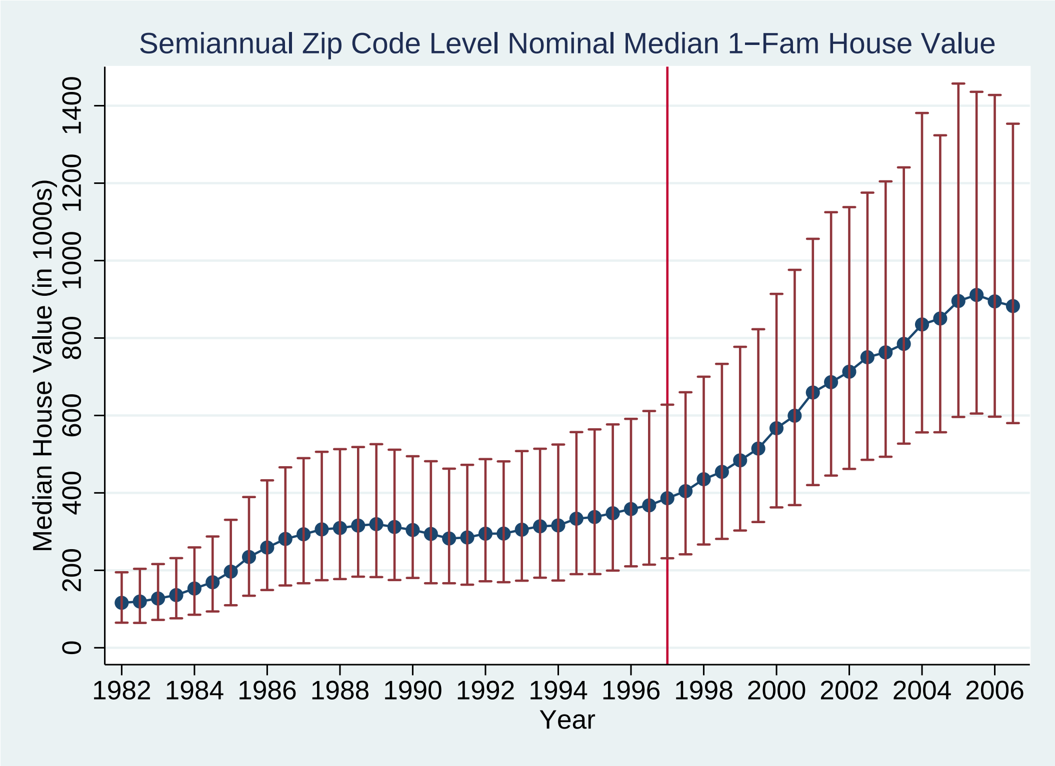
Note: Each vertical bar represents the semiannual single-family nominal housing appreciation rates of 26 zip code areas at a certain time. The circle on each vertical bar indicates the mean of the 26 housing appreciation rates.

1982 and 2006, while the remaining 43% of them were sold more than once during the sample period. Table 1.3 breaks down the parcels in the universe dataset and the sales data by city and town. It shows that approximately 62% of all single-family houses in the 16 cities and towns are in the sales data. Table 1.4 displays the mean and median sale prices and the number of sales by year.[13] The relatively high prices in the post-1997 period will allow me to examine the effect of TRA97 on homeowners with capital gains above $500,000.

tutions, trusts, builders, and developers), multiple sales on the same date, sales with suspiciously low prices, and other unusual cases.

[13] I do not convert the prices into real dollars because capital gains tax exclusions are in nominal terms.

Figure 1-5: Nominal Median Value of Single Family Houses



Note: Each vertical bar represents the median single-family nominal house values of 26 zip code areas at a certain time. The circle on each vertical bar indicates the mean of the 26 median house values.

Once I had sales data with precise purchase prices, I used 1982-2006 zip-code level semiannual housing price indices, which were provided by *Fiserv Lending Solutions*, to impute nominal capital gains for these houses in subsequent years at half-year intervals. Such indices, also called *Case-Shiller Home Price Indices*, are constructed using repeated sales data on single-family houses.[14] Figure 1-4 shows the semiannual housing appreciation rates for the 26 zip codes in the 16 cities and towns during the sample period. The vertical bars connect the maximum and minimum appreciation

[14]Case-Shiller HPI and the OFHEO (Office of Federal Housing Enterprise Oversight) HPI are the two major housing price indices in the United States. The methodology behind these two indices are very similar, but they rely on different underlying data: Case-Shiller uses purchase prices from county records and OFHEO uses conforming mortgage data from Fannie Mae and Freddie Mac. Because houses in my dataset have relatively high values and OFHEO does not include sales in which jumbo mortgages are used, Case-Shiller HPI is more appropriate in this application.

rates, and the circles indicate appreciation rates averaged across the 26 zip codes at any given time. From 1982 to 2006, the cities and towns under study experienced significant ups and downs in the single-family housing market. Moreover, there appears to be substantial heterogeneity in housing appreciation rates across zip codes at any given time. Combined with the policy shocks brought forward by TRA97, such housing market movements provide useful variations for me to study the effect of capital gains taxation on home sales. Using median assessment values of FY2007 and 1982-2006 zip-code level housing value appreciation rates, I extrapolated median assessment values for all years between 1982 and 2006 by zip code at half-year intervals. Figure 1-5 displays these extrapolated median house values. The vertical bars connect the maximum and minimum median values, and the circles indicate median values averaged across the 26 zip codes at any given time. The average median house value increased from \$116,000 in 1982 to \$883,000 in 2006 in nominal terms. There also appears to be substantial heterogeneity in median house values across zip codes at any given time.

Putting together purchase prices from the sales data and semiannual appreciation rates of the corresponding zip code area, I imputed current prices for each parcel at half-year intervals for all subsequent years before the next sale. For example, if a parcel was sold in the first half-year of 1990 at price P^0_{1990} and then was sold again in the second half-year of 2000 at price $P^0_{2000.5}$, I would derive current prices $\{P^1_{1990.5}, P^1_{1991}, P^1_{1991.5}, ..., P^1_{2000}, P^1_{2000.5}\}$ by applying 1990-2000 semiannual appreciation rates to P^0_{1990}. Similarly, I would derive current prices $\{P^1_{2001}, P^1_{2001.5}, P^1_{2002}, ..., P^1_{2006}, P^1_{2006.5}\}$ by applying 2000-2006 semiannual appreciation rates to P^0_{2000}. In the end, I created a panel of single-family houses where each observation is a parcel-time combination, each observation has information on purchase price and current price, and time is measured in the unit of half-year. By law, selling expenses can be subtracted from selling prices when calculating taxable capital gains. Because home sellers usually pay 6% of selling prices to realtors as commission fees, I define taxable capital gains (TCG) as[15]

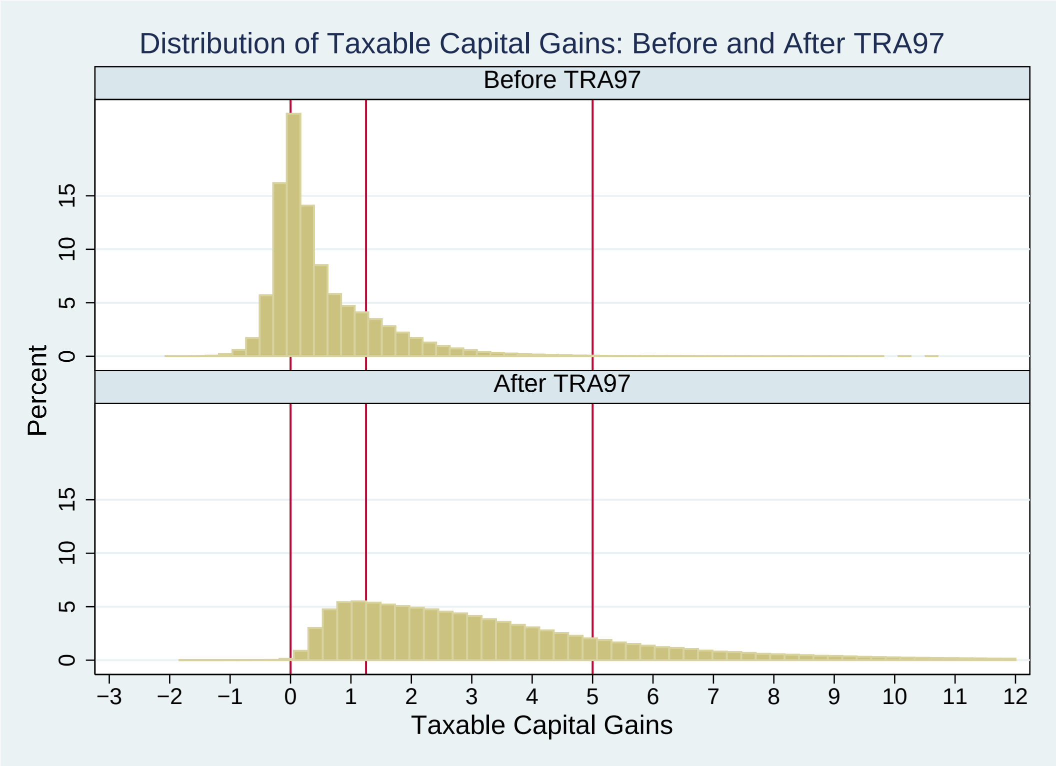
$$TCG = CurrentPrice - PurchasePrice - 0.06 \times (CurrentPrice).$$

In addition, because TRA97 required homeowners to have owned and lived in the house for two years out of the previous five to qualify for capital gains exclusions, I dropped observations that are within two years of the most recent sale for the post-TRA97 records. To prevent extreme cases from driving the estimation results, I also dropped observations where house prices are over \$5 million or the difference between the current price and the purchase price is below -\$100,000 or above \$3 million. The final analysis sample has 1.16 million observations on 46,403 unique parcels.

Figure 1-6 shows histograms of TCG before and after TRA97. There are two key differences between these two histograms. First, due to the housing market boom between the late 1990s and mid 2000s, only a tiny fraction of parcels had negative capital gains after 1997. Second, only a small fraction of parcels had capital gains over \$125,000 before 1997. Even fewer parcels had capital gains above \$500,000 before TRA97. In contrast, a large number of parcels have accumulated more than \$500,000 capital gains after TRA97. Both the housing market boom and the fact that the \$500,000 capital gains exclusion level is written in nominal terms rather than being indexed by inflation contribute to this pattern. When comparing home sale rates of houses with capital gains above \$500,000 before and after TRA97, we need to be cautious because the houses that had more than \$500,000 capital gains before 1997 may be very different from their counterparts during the post-TRA97 period. In the regression analysis shown later in this paper, I control for as many factors as the dataset allows and control for them as flexibly as possible. But the

[15] I define TCG in nominal terms because tax rules are written in nominal terms.

Figure 1-6: Histogram of Taxable Capital Gains



lack of observations with huge capital gains before 1997 poses a serious challenge to any empirical strategy estimating the impact of TRA97 on houses with capital gains above \$500,000.

Table 1.5 shows summary statistics of some key variables for the pre-TRA97 and post-TRA97 periods separately. The average semiannual home sale rate in the sample is 2.4 percentage points during the pre-TRA97 period, and it is the same as the sale rate during the post-TRA97 period. The number of observations is approximately evenly split between the pre-TRA97 period and the post-TRA97 period. Consistent with the pattern described in Figure 1-6, Table 1.5 indicates that only 0.2% of the pre-TRA97 observations are in the category with capital gains over \$500,000, whereas 20.5% of the post-TRA97 observations are in that same category. A mere 0.1% of the post-TRA97 observations have negative capital gains. There is no significant change in the average lot size before and after TRA97. The average semiannual housing appreciation rate after 1997 is 3.1% in real terms, which is significantly higher than the average semiannual housing appreciation rate of 1.0% before 1997.

1.4 Empirical Strategy and Estimation Results

In this section, I first employ non-parametric approaches to illustrate the impacts of TRA97 on home sale rates for homeowners with different levels of accumulated capital gains. Then I use a simple difference-in-differences regression framework to estimate the magnitude of such impacts. I also perform robustness checks and study the dynamic aspect of the TRA97 effect. Lastly, I exploit legislative changes in the top capital gains tax rate to estimate the tax elasticity of home sales in the post-TRA97 period.

1.4.1 Non-Parametric Approaches

As discussed earlier in this paper, TRA97 eliminated capital gains taxation on home sales for homeowners with capital gains below $500,000. Thus, we expect home sale rates of these homes to increase after 1997. The effect of TRA97 on homes with capital gains above $500,000 is ambiguous. On one hand, TRA97 legislated a very generous exclusion of $500,000. On the other hand, it took away the roll-over rule that enables home sellers to avoid paying any tax at the time of a sale. Before formulating a rigorous regression model, it is instructive to use non-parametric approaches to compare home sale rates for houses with different levels of capital gains before and after TRA97. The idea is to let the data speak for themselves without imposing any functional form assumptions.

As shown in Figure 1-2, tax liabilities are continuous functions of housing capital gains in both the pre-TRA97 and the post-TRA97 periods, even though the first derivative of tax liabilities with respect to capital gains is discrete. Because home selling is a binary decision, non-parametric smoothing techniques such as a local polynomial regression are useful to illustrate the relationship between home sale probabilities and taxable capital gains. A local polynomial regression is similar to a kernel regression. While a kernel regression estimates the weighted mean locally, a local polynomial regression estimates a weighted polynomial function locally. In a kernel regression, we minimize

$$\sum_i K\left(\frac{x_i - x_0}{h}\right)(y_i - m_0)^2$$

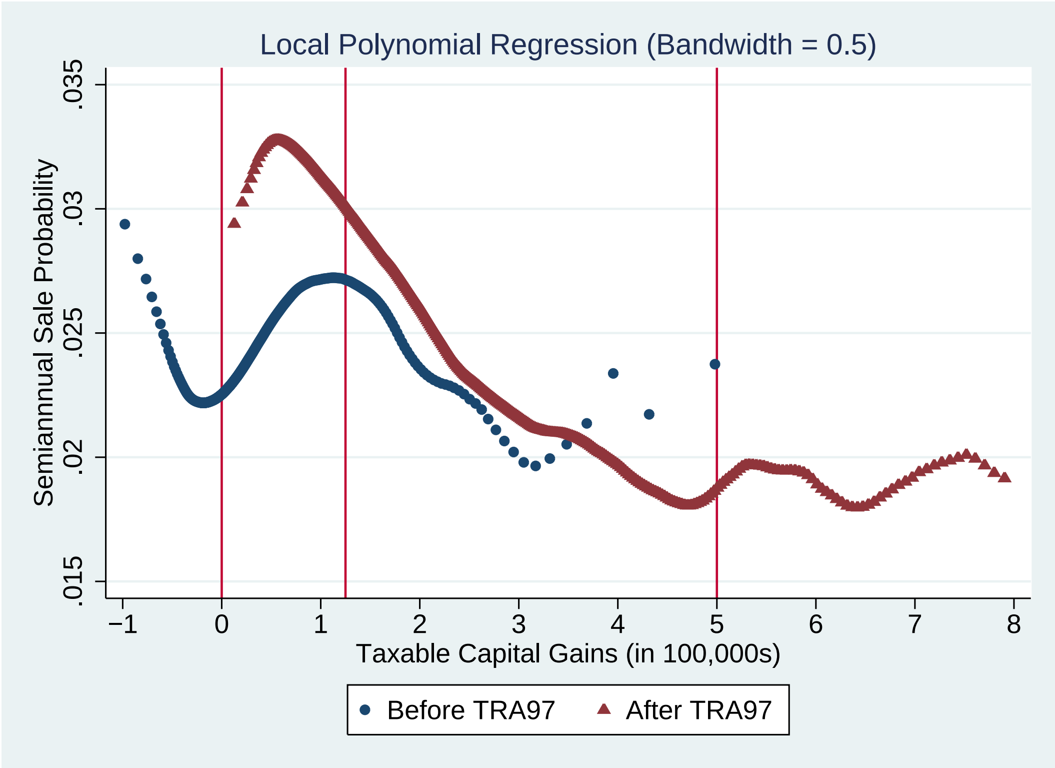
with respect to m_0, where $K(\cdot)$ is a kernel weighting function and h is the bandwidth. In a local polynomial regression, we minimize

$$\sum_i K\left(\frac{x_i - x_0}{h}\right)\left(y_i - a_0 - a_1(x_i - x_0) - ... - a_p\frac{(x_i - x_0)^p}{p!}\right)^2$$

with respect to $(a_0, a_1, ..., a_p)$, where p is preferably an odd number. Fan and Gijbels (1996) list many attractions of local polynomial regressions, including that they have better bias properties than kernel regressions.

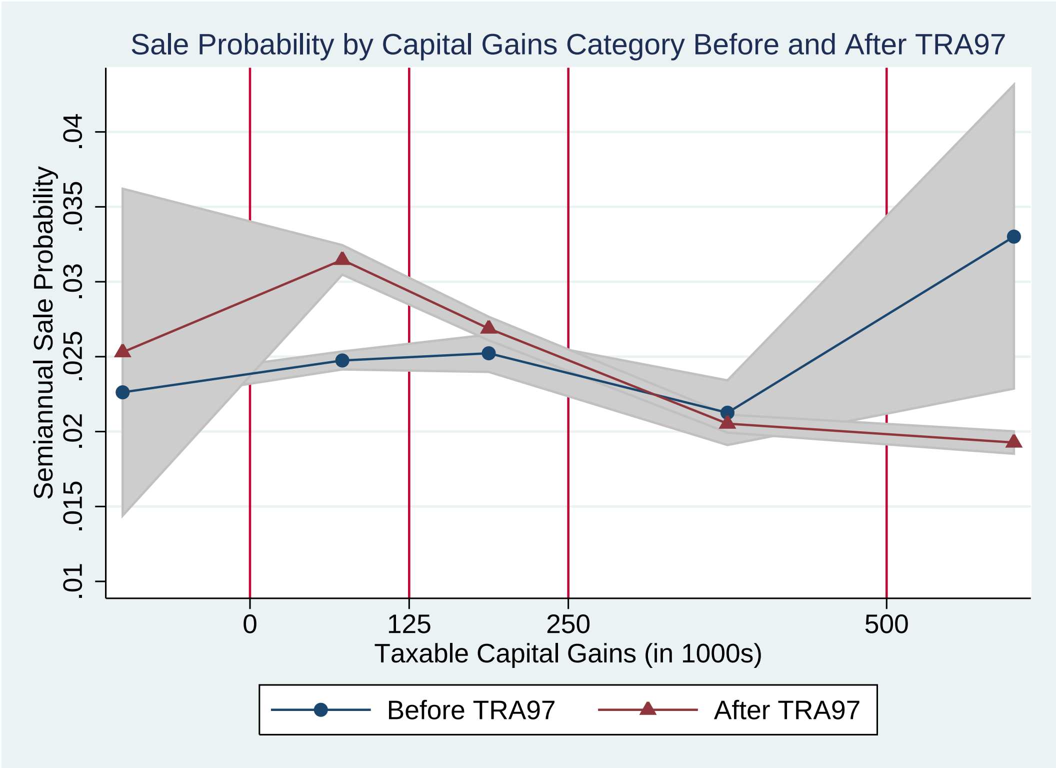
I fit a local cubic polynomial model on the pre-TRA97 data and the post-TRA97 data separately, using the alternative Epanechnikov kernel function and a bandwidth of \$500,000. Figure 1-7 shows the estimation results. The scattered circles represent the smoothed home sale probabilities at various capital gains levels before TRA97, and the scattered triangles represent those after TRA97. Because only a tiny fraction of parcels had capital gains over \$500,000 before TRA97 or had negative capital gains after TRA97, I can compare only parcels with capital gains between \$0 and \$500,000 in the local polynomial framework. Figure 1-7 presents several interesting patterns. First, home sale rates and taxable capital gains have an inverse U-shape

Figure 1-7: Local Polynomial Regression of the Raw Data



relationship within the $0 to $500,000 capital-gain range. When a homeowner has lived in the house only for a short period of time, his accumulated capital gains tend to be small and he is also unlikely to move since he has adjusted his housing consumption recently. When the homeowner has stayed in the house for a long period of time, his accumulated capital gains are usually large. But to the extent that there is heterogeneity among homeowners in their moving propensities, living in the same house for an extended period of time may indicate that this homeowner has a distaste for moving. Thus, the home sale rate of this homeowner is low, as suggested by the "mover-stayer" model in the literature. Second, the local polynomial regression result suggests that home sale rates are higher after TRA97 for homes with relatively low capital gains, but the order reverses right before capital gains reach $400,000. This pattern is roughly consistent with the theoretical prediction that TRA97 increases the

Figure 1-8: Sale Probability by Capital Gains Categories



home sale rate of houses with capital gains between \$0 and \$500,000. Third, it shows that home sale rates increase slightly near the \$500,000 exclusion level during the post-TRA97 period. Such a pattern may imply the presence of the "churning" behavior - namely, homeowners sell their homes to reset tax basis when they accumulate \$500,000 taxable capital gains after 1997.

To compare home sale rates before and after TRA97 for parcels with capital gains below \$0 or above \$500,000, I also impute simple means of sale rates by capital gains categories for the pre-TRA97 and post-TRA97 period respectively. Because the maximum exclusion amount was \$125,000 before 1997 and \$500,000 (for married couples) or \$250,000 (for singles) after 1997, I focus on five capital gains categories: less than \$0, \$0 to \$125K, \$125K to \$250K, \$250K to \$500K, and over \$500K. Figure 1-8 displays the results, where circles represent the pre-TRA97 data and triangles represent the post-TRA97 data. Shaded areas cover the range between two standard errors above and two standard errors below the point estimates. Thus, overlapping gray areas imply that the point estimate using the pre-TRA97 data is not statistically different from the point estimate using the post-TRA97 data for that particular capital gains category. Note that the standard errors of estimated semiannual home sale rates for houses with capital gains over $500,000 before 1997 and for houses with negative capital gains after 1997 are very large due to the lack of many observations in these categories. In fact, there are only 830 observations that have negative capital gains during the post-TRA97 period, compared with 158,598 such observations during the pre-TRA97 period. Similarly, there are only 1,242 observations with over $500,000 capital gains before 1997, whereas there are 134,463 such observations after 1997.

The results shown in Figure 1-8 suggest that home sale rates of parcels with capital gains between $0 and $125,000 increased after 1997 and the difference is statistically significant, which is consistent with our prediction. Interestingly, for parcels with capital gains above $500,000, sale rates declined after 1997 and the difference is also statistically significant. This finding suggests that for homeowners with extraordinary capital gains, the effect of eliminating the roll-over rule may have outweighed the effect of providing the $500,000 exclusion. Lastly, for parcels with negative capital gains or with capital gains between $125,000 and $500,000, average sale rates after TRA97 are indistinguishable from those before TRA97 in this simple non-parametric framework.

1.4.2 A Difference-in-Differences Framework

Even though the non-parametric results seem to suggest that TRA97 had an important impact on home sales and the impact varied with capital gain levels, they do not take into account other factors that may have driven the difference in home sale rates between before TRA97 and after TRA97. To control for these possible confounding factors and isolate the TRA97 effect, I use a simple difference-in-differences regression model.

Because housing is both a consumption good and an investment good, I model the home sale probability as

$$\text{Prob}(Sale_{it}) = F\left(\frac{U_{it}^{*} - U_{it}}{U_{it}^{*}}, MC_{it}, r_t, h_t\right) \tag{1.1}$$

where $Sale_{it}$ is the binary outcome variable that equals one if homeowner i sells his home between time t and $t+1$. Because housing is lumpy and costly to adjust, a homeowner may not be able to change his housing consumption constantly. Thus, his utility from consuming the present house, which presumably was bought years ago, may be well below the utility from consuming a house that he would choose to live in if housing consumption could be adjusted in a costless manner. The first term within the $F(\cdot)$ function measures how the homeowner's utility from consuming the present home, U_{it}, compares with his utility from consuming his ideal home, U_{it}^{*}. The larger the difference between U_{it} and U_{it}^{*}, the more likely he will sell his home between time t and $t+1$ and change his housing consumption bundle (i.e. $F_1 > 0$). The second term within the $F(\cdot)$ function measures the moving cost associated with changing one's housing consumption. The higher the moving cost, the less likely the homeowner will sell the house (i.e. $F_2 < 0$). Because housing is also an investment good, both the expected housing appreciation rate, h_t, and the expected return to alternative investment opportunities, r_t, will influence the homeowner's selling decision. In this paper, I assume adaptive expectation and h_t is measured by the real housing appreciation between time $t-1$ and time t.

Capital gains taxes affect home selling decisions by increasing the moving cost, MC_{it}. I specify MC_{it} as

$$MC_{it} = G(X_{it}, Z_{it}, Tax_{it}) \tag{1.2}$$

where X_{it} are homeowner i's characteristics and Z_{it} are characteristics of the present home. For example, moving costs may be higher for bigger houses or larger households. Tax_{it} is the amount of capital gains taxes homeowner i will have to pay if he sells his home between time t and $t+1$. Thus, the higher Tax_{it} is, the higher the moving cost becomes, and the lower the sale probability will be.

$$\frac{\partial \text{Prob}(Sale_{it})}{\partial Tax_{it}} = F_2 \cdot \frac{\partial MC_{it}}{\partial Tax_{it}} < 0 \tag{1.3}$$

TRA97 introduced arguably exogenous changes to tax treatment of housing capital gains, and such changes were different for houses with different levels of capital gains. Therefore, I can use these changes to identify the effect of capital gains taxes on home sales. Specifically, I estimate the following Probit model[16]

$$\begin{aligned}
\text{Prob}(Sale_{ict}) = \Phi \Bigg(& \alpha_0 + \sum_{k=1}^{23} \alpha_k \cdot \mathbf{1}(TCG_{ict} \in C_k) + \beta_1 \cdot \mathbf{1}(TCG_{ict} \leq 0) \cdot After_t \\
& + \beta_2 \cdot \mathbf{1}(0 < TCG_{ict} \leq 125K) \cdot After_t + \beta_3 \cdot \mathbf{1}(125K < TCG_{ict} \leq 250K) \cdot After_t \\
& + \beta_4 \cdot \mathbf{1}(250K < TCG_{ict} \leq 500K) \cdot After_t + \beta_5 \cdot \mathbf{1}(TCG_{ict} > 500K) \cdot After_t \\
& + \gamma_1 h_{ict} + \gamma_2 log(lotsize)_{ict} + \delta_c + \theta_t \\
& + \sum_{j=1}^{4} \rho_{1j} (RPP_{ict})^j + \sum_{j=1}^{4} \rho_{2j} (RCP_{ict})^j + \sum_{j=1}^{4} \rho_{3j} (T_{ict})^j \Bigg)
\end{aligned} \tag{1.4}$$

where $Sale_{ict}$ indicates whether homeowner i in city c sells his house between time t and $t+1$. Function $\mathbf{1}(\cdot)$ returns one if the condition expressed in the parenthesis is true and zero otherwise. $After_t$ indicates whether the observation is in the post-TRA97 period. Because I do not observe homeowners' characteristics in my dataset, I use "time since purchase" - T_{ict} - as a proxy for the difference between the utility from consuming the desired house and the utility from consuming the present house

[16]I estimate a Probit model instead of a linear probability model because the average home sale rate is only 0.023 and very far from 0.5. A linear probability model may be biased in this case.

(i.e. $\frac{U_{it}^{*}-U_{it}}{U_{it}^{*}}$). The intuition is that the longer one stays in one's home, the more likely that he has experienced shocks that affect housing demand (e.g. change in family size). I use the real purchase price of the present home, RPP_{ict}, as a proxy for permanent household income. The rationale is that permanent household income is a key determinant of housing demand - households with higher incomes can afford more expensive homes. I also use the real current price, RCP_{ict}, as a proxy for wealth. To control for these three variables as flexibly as possible, I put their polynomials to the fourth order in the regression model as explanatory variables. In addition, $log(lotsize)_{ict}$ measures how large the parcel is. Because larger parcels imply higher moving cost, we expect γ_1 to be negative. h_{ict} is the real housing appreciation from time $t-1$ to time t. δ_c and θ_t stand for city fixed effects and year fixed effects, respectively.

Essentially, I want to compare home sale rates before and after TRA97 for five capital gains categories: $TCG_{ict} \leq 0$, $0 < TCG_{ict} \leq 125K$, $125K < TCG_{ict} \leq 250K$, $250K < TCG_{ict} \leq 500K$, and $TCG_{ict} > 500K$. Instead of controlling just for these five capital gains categories, I assign capital gains into 23 categories: less than -\$50K, -\$50K-\$0, \$0-\$25K, \$25K-\$50K, \$50K-\$75K, \$75K-\$100K, \$100K-\$125K, \$125K-\$150K, \$150K-\$175K, \$175K-\$200K, \$200K-\$250K, \$250K-\$300K, \$300K-\$350K, \$350K-\$400K, \$400K-\$450K, \$450K-\$500K, \$500K-\$600K, \$600K-\$700K, \$700K-\$800K, \$800K-\$900K, \$900K-\$1M, \$1M-\$1.5M, and more than \$1.5M. In this way, I allow for more flexibility in estimating the baseline effect of capital gains on home sales in the pre-TRA97 period. These 23 categories are expressed as $\{C_k, k = 1, 2, ..., 23\}$ in equation (1.4). As discussed before, TRA97 should have no impact on houses with negative capital gains since no taxes were due for these houses throughout the sample period. TRA97 unambiguously reduced tax liabilities and should increase home sale rates if capital gains were between \$0 and \$500,000. The effect of TRA97 is *a priori* ambiguous on houses with capital gains above \$500,000. In summary, we expect

$\beta_1 = 0$, $\beta_2 > 0$, $\beta_3 > 0$, $\beta_4 > 0$, and $\beta_5 \lesseqgtr 0$. To the extent that there are many single homeowners in the sample, β_4 may be smaller in magnitude than β_2 and β_3 because single homeowners can only exclude $250,000 capital gains.

Table 1.6 displays the estimation results with standard errors clustered at parcel level. The numbers shown in the table are the marginal effects of corresponding explanatory variables evaluated at means. For ease of exposition, I show these marginal effects in percentage terms. In column (1), I include only year dummies, the 23 TCG category dummies, and interactions of TRA97 with the five TCG category dummies as explanatory variables. As predicted, the estimated coefficient $\hat{\beta}_1$ is statistically indistinguishable from zero at conventional confidence level, whereas $\hat{\beta}_2$, $\hat{\beta}_3$ and $\hat{\beta}_4$ are all positive and statistically significant. These results suggest that TRA97 raised sale rates of parcels with capital gains between $0 and $500,000, but did not change sale rates of parcels with negative capital gains. On the other hand, the estimated coefficient on the interaction between TRA97 and capital gains exceeding $500,000, $\hat{\beta}_5$, is negative and statistically significant. It implies that for parcels with capital gains over $500,000, the elimination of the roll-over rule reduced sale rates by more than the increase in sale rates induced by the large exclusion level. Therefore, we observe a net decrease in sale rates among these parcels after TRA97.

In column (2), I add more controls to the regression model, including real semi-annual housing appreciation rate, log of lot size, polynomials of real purchase price, polynomials of real current price, polynomials of time since purchase, and city dummies. The estimated coefficient on log of lot size is negative and statistically significant, suggesting that larger houses have lower sale rates, possibly due to higher moving costs. The estimated coefficient on housing appreciation rate is also negative and statistically significant, which is consistent with the notion that housing is partly an investment good. For example, during a housing market boom, adaptive expectation implies that homeowners extrapolate the housing price movement and expect the boom to continue. Therefore, they often hold on to their homes and defer sales. Similar to the estimation results shown in column (1), $\hat{\beta}_1$ is statistically indistinguishable from zero, $\hat{\beta}_2$ and $\hat{\beta}_3$ are positive and statistically significant, and $\hat{\beta}_5$ is negative and statistically significant. $\hat{\beta}_4$ remains positive but is no longer statistically significant.

Column (3) has the same specification as column (2) except that I control for city$\times$year fixed effects to allow for city-year specific shocks to the housing market. All estimated coefficients remain almost identical to the results obtained using the column (2) specification. The estimated marginal effects of the interaction terms suggest that TRA97 increased semiannual sale rates by 0.33-0.54 percentage points for parcels with capital gains between \$0 and \$500,000, representing a 13-22 percent increase from the average sale rate during the pre-TRA97 period. In contrast, TRA97 reduced sale rates of parcels with more than \$500,000 capital gains by 0.79 percentage points, representing a 24 percent decline from the average sale rate during the pre-TRA97 period. Taken together, the evidence shown in this section suggests that TRA97 reversed the lock-in effect of capital gains taxes for houses with low or moderate capital gains. However, it may have generated an unintended lock-in effect for houses with very large capital gains due to the elimination of the roll-over rule.

1.4.3 Robustness Checks and Extensions

In the previous section, I find that TRA97 appears to have an important effect on home sales. In particular, home sale rates of parcels with capital gains between \$0 and \$500,000 increased and home sale rates of parcels with capital gains above \$500,000 decreased after TRA97. In this section, I present a set of robustness checks and examine how the effect of TRA97 evolved over time.

Potential Measurement Errors in Taxable Capital Gains

The empirical strategies used in this paper depend on taxable capital gains being accurately measured. Compared with most public surveys, the data used in this paper have much better measures of taxable capital gains because they are imputed using actual sale records and zip-code level housing price indices. Nevertheless, to the extent that home improvements and renovations affect taxable capital gains, the data used in this paper may still contain measurement errors since I do not observe these improvement and renovation activities. To ascertain that the estimates shown above are not driven by such measure errors in taxable capital gains, I carry out several robustness checks.

First, I drop houses that are likely to have had significant improvements and renovations during the sample period. Recall that the dataset is a panel of single-family houses where I observe actual sale prices if they are sold and I also observe assessment values at the end of the sample period. Suppose a parcel was first purchased in the first half-year of 1990 at price P^0_{1990} and then was sold in the second half-year of 2000 at price $P^0_{2000.5}$. I impute the values of this parcel at each point of time since 1990 using zip-code level semiannual housing appreciation rates. In other words, I use P^0_{1990} as the base to obtain house values $\{P^1_{1990.5}, P^1_{1991}, P^1_{1991.5}, ..., P^1_{2000}, P^1_{2000.5}\}$. Similarly, I use $P^0_{2000.5}$ as the base to obtain house values $\{P^1_{2001}, P^1_{2001.5}, P^1_{2002}, ..., P^1_{2006}, P^1_{2006.5}\}$. If the actual sale price in the second half-year of 2000, $P^0_{2000.5}$, is very different from the imputed house value at that time, $P^1_{2000.5}$, it is likely that the parcel experienced significant modifications between 1990 and 2000. To reduce biases introduced by such measurement errors, I drop all observations on this parcel for years between 1990 and 2000. In addition, if the FY2007 assessment value of this parcel, P^{av}_{FY2007}, is very different from the imputed house value at the end of the sample period, $P^1_{2006.5}$, it is likely that the parcel experienced major changes between 2000 and 2006. In this case, I drop observations on this parcel for all years between 2000 and 2006.

Second, I change the cut-off point for the \$500,000 capital gains exclusion. Suppose that a homeowner purchased the house at P_t^0, renovated the house during the years when he lived in it, and then sold the house to someone else at price $P_{t'}^0$. According to IRS rules, the cost of renovation can be subtracted from the sale price $P_{t'}^0$ when calculating the taxable capital gains. But the added value to the house due to renovation also appreciated between the time of renovation and t', which contributes to actual taxable capital gains. Because I do not observe the timing of the renovation and the amount of money this homeowner spent on renovating the house, the predicted taxable capital gains of this house at time t' are lower than the actual taxable capital gains. In other words, the homeowner may have accumulated \$500,000 capital gains before t' even though my calculation suggests that his accumulated capital gains reached \$500,000 at time t'. To correct such a discrepancy, I change the cut-off point from \$500,000 to \$450,000 as a robustness check.

Panel A in Table 1.7 displays the estimation results of the robustness checks described above. In column (2), I drop observations if actual sale prices and predicted sale prices differ by 100% or if FY2007 assessment values and predicted values at the end of the sample period differ by 100%. Before this procedure, the correlation between predicted values and actual values was 0.80. It increased to 0.91 after this procedure. Column (2) shows that the estimated marginal effects of TRA97 on home sales are very similar to the main results obtained from the original sample. In column (3), I drop observations when actual sale prices and predicted sale prices differ by 50% or when FY2007 assessment values and predicted values at the end of the sample period differ by 50%. After this procedure, the correlation between predicted values and actual values increased further to 0.95. In this case, the estimated marginal effects of TRA97 change somewhat in magnitude, but they are not statistically different from the main results shown in column (1). Column (4) shows the estimated marginal effects when the cut-off point is changed from \$500,000 to \$450,000. The results remain largely the same. The results of robustness checks shown in panel A of Table 1.7 suggest that the measurement error problem associated with taxable capital gains is unlikely to be severe and the main findings shown in the previous section are robust to potential measurement errors.

Alternative Sample and Specification

The results shown in Table 1.6 may have over-estimated the effect of TRA97 if homeowners in 1996 anticipated the passage of TRA97. If they indeed knew that TRA97 was going to pass, homeowners who intended to sell their homes might have delayed selling their homes until after May 7, 1997 if their capital gains were relatively low. Alternatively, if they had huge capital gains, they might have accelerated selling their homes to take advantage of the roll-over rule. Under these circumstances, the findings presented in Table 1.6 would be artificial rather than real. To deal with such "anticipation" effects, I drop the 1996 observations and re-estimate equation (1.4). The estimation results are shown in column (2) of panel B in Table 1.7. The estimated coefficient $\hat{\beta}_1$ is still not statistically different from zero. $\hat{\beta}_2$, $\hat{\beta}_3$ and $\hat{\beta}_4$ are all positive and statistically significant. $\hat{\beta}_5$ is negative and also statistically significant. The magnitudes of the marginal effects are similar to the main results shown in column (1). These estimation results point in the direction that the passage of TRA97 was likely to be unexpected, as suggested by Dai, Maydew, Shackelford and Zhang (2006).

The dataset analyzed in this paper contains houses that were sold between 1982 and 2006. If houses that were first sold before TRA97 are systematically different from houses that were first sold after TRA97, the results shown in the previous section may be driven by such compositional changes. To deal with this concern, I estimate the model using houses that were sold at least once before TRA97. This procedure makes sure that the pre-TRA97 and post-TRA97 observations in the sample are the same parcels. Column (3) of panel B in Table 1.7 presents the estimation results. The estimated marginal effects are remarkably similar to the results obtained using the original sample, suggesting that it is unlikely that systematic differences exist between houses whose first observed sales occurred before TRA97 and those whose first observed sale occurred after TRA97.

In estimating equation (1.4), I control for polynomials of real purchase prices, real current prices, and time since purchase up to the fourth order. To allow for more flexibility in these polynomial controls, I include polynomials up to the sixth order as a robustness check. Column (4) of panel B in Table 1.7 displays the estimation results of this more flexible specification. The estimated marginal effects of TRA97 are virtually identical to the results obtained when I control for polynomials only to the fourth order, suggesting that fourth order polynomials provide ample flexibility for these control variables to affect home sale rates in the main specification.

Dynamic Effect of TRA97

The effect of TRA97 on home sales during the years immediately following 1997 may be different from the effect of TRA97 many years after the law change. In fact, Biehl and Hoyt (2008) find intriguing evidence of the dissipating effect of TRA97. Comparing mobility of homeowners over and under 55 before and after TRA97, they show that TRA97 reversed the lock-in effect of capital gains taxes for homeowners under age 55. However, they find that such an impact of TRA97 disappeared in a few years after 1997, suggesting that the effect of TRA97 was temporary rather than permanent. To investigate the short-term effect of TRA97, I estimate equation (**??**) using only data within a narrow window of the law change.

In column (2) of Table 1.8, I limit the sample to 18 months before and 18 months after TRA97.[17] The estimated marginal effects suggest that home sale rates of parcels

[17] Note that the variable $(TCG \leq 0) * TRA97$ is dropped for collinearity reasons in this smaller

with capital gains between \$0 and \$500,000 increased within 18 months of the law change. The magnitudes of these effects are similar to the main findings shown in column (1). Home sale rates of parcels with capital gains above \$500,000 also appear to increase, but the effects are not statistically significant. In column (3), I limit the sample to 3 years before and 3 years after TRA97. For parcels with capital gains between \$0 and \$500,000, the estimated marginal effects of TRA97 are very similar to the results obtained using the 18-month window. However, the estimated effect of TRA97 on parcels with capital gains over \$500,000 becomes essentially zero when using the 3-year window. In summary, it appears that the unlocking-effect of TRA97 on parcels with capital gains between \$0 and \$500,000 manifested shortly after the law change. In contrast, the short-run effect of TRA97 on parcels with capital gains over \$500,000 seems to be insignificant.

To further investigate the long-term versus short-term effect of TRA97 on home sales, I estimate the following model where the long-term effect of TRA97 is allowed to be different from the short-term effect of TRA97.

$$\begin{aligned}
&\text{Prob}(Sale_{ict}) = \Phi\Bigg(\alpha_0 + \sum_{k=1}^{23}\alpha_k \cdot \mathbf{1}(TCG_{ict} \in C_k) + \beta_1^{early} \cdot \mathbf{1}(TCG_{ict} \leq 0) \cdot Early_t \\
&+\beta_1^{later} \cdot \mathbf{1}(TCG_{ict} \leq 0) \cdot Later_t + \beta_2^{early} \cdot \mathbf{1}(0 < TCG_{ict} \leq 125K) \cdot Early_t \\
&+\beta_2^{later} \cdot \mathbf{1}(0 < TCG_{ict} \leq 125K) \cdot Later_t + \beta_3^{early} \cdot \mathbf{1}(125K < TCG_{ict} \leq 250K) \cdot Early_t \\
&+\beta_3^{later} \cdot \mathbf{1}(125K < TCG_{ict} \leq 250K) \cdot Later_t + \beta_4^{early} \cdot \mathbf{1}(250K < TCG_{ict} \leq 500K) \cdot Early_t \\
&+\beta_4^{later} \cdot \mathbf{1}(250K < TCG_{ict} \leq 500K) \cdot Later_t + \beta_5^{early} \cdot \mathbf{1}(TCG_{ict} > 500K) \cdot Early_t \\
&+\beta_5^{later} \cdot \mathbf{1}(TCG_{ict} > 500K) \cdot Later_t + \gamma_1 h_{ict} + \gamma_2 log(lotsize)_{ict} + \delta_c + \theta_t \\
&+\sum_{j=1}^{4}\rho_{1j}(RPP_{ict})^j \sum_{j=1}^{4}\rho_{2j}(RCP_{ict})^j + \sum_{j=1}^{4}\rho_{3j}(T_{ict})^j\Bigg) \qquad (1.5)
\end{aligned}$$

where $Early_t$ is an indicator variable that equals one if t falls within an initial period after TRA97, and $Later_t$ is an indicator variable that equals one if t falls

sample. Therefore, β_1 cannot be estimated in practice.

out of the initial period after TRA97. If TRA97 unlocked homeowners with capital gains between \$0 and \$500,000 but the effect is transitory, then we would expect ($\beta_i^{early}, i = 2, 3, 4$) to be positive and ($\beta_i^{later}, i = 2, 3, 4$) to be zero. Similarly, if TRA97 unintentionally locked in homeowners with capital gains over \$500,000 but the effect is transitory, then we would expect β_5^{early} to be negative and β_5^{later} to be zero.

Column (4) and column (5) of Table 1.8 display the estimation results of equation (1.5) where the initial period is defined as 18 months and 3 years after TRA97, respectively.[18] The estimates presented in column (4) and column (5) show two interesting patterns. First, the estimated coefficients ($\hat{\beta}_i^{early}, i = 2, 3, 4$) are indeed positive and statistically significant, whereas the estimated coefficients ($\hat{\beta}_i^{later}, i = 2, 3, 4$) are much smaller in magnitude and statistically indistinguishable from zero. This pattern suggests that TRA97 raised selling probabilities among homeowners with capital gains between \$0 and \$500,000 who were presumably locked in by capital gains taxes prior to TRA97. Such an unlocking effect of TRA97, however, was achieved within a short initial period after the passage of TRA97. After this initial period, there was no evidence that home sale rates under the new tax regime were significantly higher compared to the pre-TRA97 period. Second, the estimates of short-term effect of TRA97 on houses with capital gains above \$500,000, $\hat{\beta}_5^{early}$, are statistically insignificant. In contrast, the estimates of the long-term effect, $\hat{\beta}_5^{later}$, are negative, statistically significant, and large in magnitude compared with $\hat{\beta}_5^{early}$. This pattern suggests that sale rates of houses with massive capital gains responded little to TRA97 right after 1997. In the long-run, however, TRA97 appeared to reduce sale rates of these houses significantly. Such a delayed effect of TRA97 may be because homeowners were not fully informed about the implications of TRA97 immediately after the law change.[19]

[18]Note that the variables $\mathbf{1}(TCG_{ict} \leq 0) \cdot Early_t$ and $\mathbf{1}(TCG_{ict} \leq 0) \cdot Later_t$ are dropped for collinearity reasons. Therefore, β_1^{early} and β_1^{later} cannot be estimated in practice.

[19]Anecdotal evidence suggests that, as of now, many homeowners still do not fully understand the

In summary, the robustness checks shown in this section lend support to the main findings that TRA97 increased home sale rates of parcels with capital gains between \$0 and \$500,000 and reduced home sale rates of parcels with capital gains over \$500,000. When I allow the effect of TRA97 to vary over time, I find evidence suggesting that the release effect of TRA97 on parcels with relatively low capital gains was completed within a short period of time after the law change, suggesting that this effect was transitory. For parcels with extraordinary capital gains, however, the effect of TRA97 was initially insignificant but became stronger in later years, suggesting that the unintended lock-in effect of TRA97 on houses with capital gains over \$500,000 could remain relevant in future years.

1.4.4 Estimate the Tax Elasticity of Home Sales

TRA97 lowered the top tax rate on long-term capital gains from 28% to 20%. In 2001, the top rate was further reduced to 18% for capital gains on assets held for five years or longer. Since 2003, long-term capital gains have been taxed with a maximum rate of 15%. In this section, I use the legislative changes in the top capital gains tax rate during the post-TRA97 period to estimate the tax elasticity of home sales.[20]

To estimate $\frac{\partial \text{Prob}(Sales_{ict})}{\partial Tax_{ict}}$, I need to impute Tax_{ict} for every homeowner in the sample. Since I do not have homeowner characteristics and income data to infer actual marginal tax rates, some assumption on homeowners' marginal tax rates is necessary to impute their housing capital gains tax liabilities. As shown in Table 1.1, homeowners in the 16 cities and towns studied in this paper are mostly high-income individuals, so it is reasonable to assume that they face the top capital gains tax rate. This assumption allows me to calculate the amount of taxes that a homeowner would

\$500,000 capital gains exclusion provision.

[20]Estimating the tax elasticity of home sales for the pre-TRA97 data is very difficult because capital gains tax liabilities depended on age of the seller and value of the replacement home, neither of which is observed in my data.

owe if he were to sell his house within the next six months:

$$Tax_{ict} = Real(\tau_t \cdot \max(0, TCG_{ict} - 500,000)), \quad \begin{cases} \tau_t = 0.20 & \text{for 1998 - 2000} \\ \tau_t = 0.18 & \text{for 2001 - 2002} \\ \tau_t = 0.15 & \text{for 2003 - 2006} \end{cases}$$

where function $Real(\cdot)$ converts nominal dollar amounts into real 2000 dollar amounts. Panel A in Table 1.9 displays the summary statistics of such imputed Tax_{ict}. Because the first \$500,000 capital gains can be excluded at the time of a sale and because almost 80% of observations during the post-TRA97 period have capital gains below \$500,000, only 20% of observations have non-zero Tax_{ict}. The average capital gains taxes are \$8,205 for the full post-TRA97 sample and \$39,936 for non-zero observations.

To estimate the elasticity of home sales with respect to capital gains taxes, I use the Probit model

$$\begin{aligned} \text{Prob}(Sale_{ict}) = \Phi \Bigg(& \alpha_0 + \sum_{k=1}^{23} \alpha_k \cdot \mathbf{1}(TCG_{ict} \in C_k) + \lambda Tax_{ict} + \gamma_1 h_{ict} \\ & + \gamma_2 log(lotsize)_{ict} + \delta_c + \theta_t + \sum_{j=1}^{4} \rho_{1j} (RPP_{ict})^j + \sum_{j=1}^{4} \rho_{2j} (RCP_{ict})^j \\ & + \sum_{j=1}^{4} \rho_{3j} (T_{ict})^j \Bigg) \end{aligned} \tag{1.6}$$

where the key parameter to be estimated is λ. Panel B in Table 1.9 presents the results from estimating equation (1.6). In column (1), I include all observations in the sample and find the estimated coefficient on capital gains taxes, $\hat{\lambda}$, negative and statistically significant. The estimated marginal effect suggests that a \$10,000 increase in capital gains tax liabilities reduces semiannual home sale rates by 0.16 percentage points, representing a 7% decline from the average semiannual sale rate of 2.4 percentage points. Because legislative changes in top rate do not generate variations in tax liabilities if capital gains are below the exclusion level of $500,000, it is useful to examine the tax elasticity among houses with strictly positive capital gains taxes. Column (2) reports the estimation result when observations with zero taxes are dropped. The estimated marginal effect becomes larger. The magnitude suggests that a $10,000 increase in tax liabilities reduces semiannual sale rates by 0.25 percentage points, a 13% decrease from the average semiannual sale rate of these houses.

The estimates of the reduced-form tax elasticity of home sale rates allow us to do policy simulations and to infer the impact of hypothetical changes in housing capital gains taxation on home sales. For example, according to the 2004 Survey of Consumer Finances (SCF), 3.26% of homeowners in the U.S. would be subject to capital gains taxes if they were to sell their homes at the time of survey. Among these homeowners, the median tax liability is approximately $30,000, assuming a 15% capital gains tax rate. The estimate shown in column (2) of Table 1.9 suggests that if we eliminate capital gains taxes on housing altogether, the semiannual home sale rate would increase by 0.75 percentage points among the 3.26% homeowners who have positive tax liabilities. Another interesting scenario is when the JGTRRA expires in 2011 and the top capital gains tax rate may increase from the current 15% to 20%. Using the 2004 SCF statistics, this change in top tax rate would increase tax liabilities by $10,000 for the median homeowner among the 3.26% homeowners who have capital gains over $500,000. The estimate shown in column (2) of Table 1.9 suggests that the semiannual home sale rate would decrease by 0.25 percentage points as a result among these homeowners. In summary, even though we need to be cautious when making out-of-sample predictions using the estimates shown this section, it is helpful to have a tightly estimated elasticity of home sales with respect to housing capital gains taxes for many back-of-envelope calculations.

1.5 Conclusion

TRA97 introduced the largest change in decades to the tax treatment of housing capital gains in the United States. While researchers have started to use it as a policy instrument to identify the lock-in effect of capital gains taxes in housing markets, existing empirical studies have rarely looked beyond the age-55 rule due to data limitations. In this paper, I collect data from various sources, including local assessment records, zip-code level housing price indices, and sales data on single-family houses, to construct a unique panel of houses where housing capital gains can be imputed more accurately than possible in most survey datasets. Instead of relying on the age-55 rule, I identify the tax lock-in effect by exploiting the cross-sectional variation in accumulated capital gains and the exogenous change in exclusion levels brought forward by TRA97. This paper is the first study, to my knowledge, to examine the effect of TRA97 on houses with capital gains over $500,000. It is also the first to estimate the tax elasticity of home sales using post-TRA97 data.

I find robust evidence suggesting that TRA97 reversed the lock-in effect of capital gains taxes for houses with capital gains between $0 and $500,000. After 1997, the semiannual sale rate of these houses increased by 0.33-0.54 percentage points, representing a 13-22 percent increase from the average semiannual sale rate during the pre-TRA97 period. However, TRA97 appeared to have generated an unintended lock-in effect on houses with capital gains above $500,000. The semiannual sale rate of these houses declined by 0.79 percentage points after 1997, equivalent to a 24 percent decrease from the average semiannual sale rate during the pre-TRA97 period. This empirical finding suggests that although TRA97 raised home sale rates by allowing for a large capital gains exclusion, it also reduced home sale rates of houses with massive capital gains through the elimination of the roll-over rule. Overall, homeowners who had accumulated more than $500,000 capital gains became less willing to sell their houses after TRA97. Furthermore, the unlocking effect of TRA97 on houses with relatively low capital gains dissipated shortly after 1997, but the unintended lock-in effect of TRA97 on houses with massive capital gains appears to be long-lasting. I also estimated the tax elasticity of home sales during the post-TRA97 period, using legislative changes in top capital gains tax rates. The estimation results suggest that a $10,000 increase in tax liability reduces semiannual sale rates by 0.16-0.25 percentage points, a 7-13 percent decline from the average level. These estimates are useful for simulations of hypothetical reforms such as eliminating taxes on housing capital gains or increasing the top capital gains tax rate.

This paper brings new evidence to the literature on the lock-in effect of capital gains taxation in housing markets. Nevertheless, the field calls for more research to fully understand the welfare impact of TRA97. First, this paper does not take into account any general equilibrium effect potentially generated by TRA97. By reducing taxes on housing capital gains, TRA97 reduced the user cost in the housing market, which could have increased housing investment at the expense of non-housing investment. In addition, anecdotal evidence has suggested that TRA97 might be responsible for the rapid growth of second home markets in recent years. Such interesting and important topics await future research. Second, the dataset analyzed in this paper is a panel of houses instead of a panel of households. Thus, I do not observe where people moved to once they sold their houses. We need high quality longitudinal data on households to quantify the distortion of capital gains taxation in the pre-TRA97 tax regime and to understand the extent to which TRA97 reversed the lock-in effect of housing capital gains.

Bibliography

[1] Bier, Thomas, Ivan Maric, and Winifred Weizer. "A Preliminary Assessment of the New Home Seller Capital Gains Law." *Housing Policy Debate* 11 (3) 2000, 645-673.

[2] Biehl, Amelia and William Hoyt. "The Taxpayer Relief Act of 1997 and Homeownership: Is Smaller Now Better?" *National Tax Joural* forthcoming.

[3] Burman, Leonard, Sally Wallace, and David Weiner. "How Capital Gains Taxes Distort Homeowners' Decisions?" Proceedings of the National Tax Association 89th Annual Conference, November 1996.

[4] Cunningham, Christopher and Gary Engelhardt. "Housing Capital-Gains Taxation and Homeowner Mobility: Evidence from the Taxpayer Relief Act of 1997." *Journal of Urban Economics* forthcoming.

[5] Dai, Zhonglan, Edward Maydew, Douglas Shackelford, and Harold Zhang. "Capital Gains Taxes and Asset Prices: Capitalization or Lock-In?" NBER Working Paper 12342, 2006.

[6] Fan, Jianqing and Irene Gijbels. "Local Polynomial Modelling and Its Applications." London, Chapman and Hill, 1996.

[7] Farnham, Martin. "Residential Mobility, the Capital Gains Tax, and the 'Taxpayer Relief Act of 1997'." University of Michigan Dissertation Charpter.

[8] Goodman, John and John Ittner. "The Accuracy of Home Owners' Estimates of House Value." *Journal of Housing Economics* 2 (4) 1992, 339-357.

[9] Hoyt, William and Stuart Rosenthal. "Capital Gains Taxtation and the Demand for Owner-Occupied Housing." *The Review of Economics and Statistics* 72 (1) 1990, 45-54.

[10] Hoyt, William and Stuart Rosenthal. "Owner-Occupied Housing, Capital Gains, and the Tax Reform Act of 1986." *Journal of Urban Economics* 32 (2) 1992, 119-139.

[11] Kiel, Katherine and Jeffrey Zabel. "The Accuracy of Owner-Provided House Values: The 1978-1991 American Housing Survey." *Real Estate Economics* 27 (2) 1999, 263-298.

[12] King, Mervyn. "An Econometric Model of Tenure Choice and Demand for Housing as a Joint Decision." *Journal of Public Economics* 14 (2) 1980, 137-160.

[13] Newman, Sandra and James Reschevsky. "Federal Policy and the Mobility of Older Homeowners." *Journal of Policy Analysis and Management* 6 (3) 1987, 402-416.

[14] Rosen, Harvey. "Housing Decisions and the U.S. Income Tax: An Econometric Analysis." *Journal of Public Economics* 11 (1) 1979, 1-24.

[15] Sinai, Todd. "Taxation, User Cost, and Household Mobility Decisions." Wharton Real Estate Center Working Paper #303, University of Pennsylvania, 1998.

Table 1.1: Characteristics of the 16 MA Cities/Towns

City/Town	Bachelor's	Graduate or Professional	Median HH Income 1999	Ownership Percentage
MA	19.5%	13.7%	50,502	62
Belmont	26.4%	36.7%	80,295	61
Brookline	31.7%	45.3%	66,711	45
Cambridge	26.7%	38.5%	47,979	32
Carlisle	44.3%	39.1%	129,811	94
Cohasset	40.0%	20.7%	84,156	85
Concord	31.4%	34.7%	95,897	81
Dover	43.5%	34.3%	141,818	95
Lexington	26.8%	42.2%	96,825	83
Lincoln	28.5%	40.7%	79,003	61
Needham	31.3%	33.5%	88,079	81
Newton	29.1%	38.9%	86,052	70
Sherborn	39.0%	36.7%	121,693	93
Sudbury	34.4%	37.5%	118,579	92
Wellesley	34.7%	41.2%	113,686	83
Weston	30.1%	45.0%	153,918	86
Winchester	32.2%	32.7%	94,049	81

Notes: Data are from MA Stata Data Center. Bachelor's and Graduate or Professional refer to educational attainment for the population 25 years and older in 2000. Ownership Percentage is owner-occupied housing units as a percentage of all occupied housing units in 2000.

Table 1.2: Number of Single-Family Houses over Years

City/Town	FY1992	FY2000	FY2007
Belmont	4,484	4,519	4,520
Brookline	4,320	4,410	4,534
Cambridge	3,434	3,550	3,732
Carlisle	1,364	1,533	1,627
Cohasset	2,080	2,165	2,243
Concord	4,427	4,626	4,640
Dover	1,572	1,712	1,741
Lexington	8,682	8,821	8,917
Lincoln	1,415	1,492	1,510
Needham	8,045	8,252	8,326
Newton	16,876	16,891	16,909
Sherborn	1,272	1,331	1,320
Sudbury	4,566	5,141	5,341
Wellesley	7,101	7,206	7,254
Weston	3,113	3,301	3,340
Winchester	5,308	5,467	5,581

Notes: Data are from MA Department of Revenue.

Table 1.3: Percent of Single-Family Houses in the Sales Data

City/Town	Total Number of Houses	Number of Houses in Sales Data	% of Houses in Sample
Belmont	4,520	2,669	59
Brookline	4,534	2,813	62
Cambridge	3,710	2,256	61
Carlisle	1,561	1,034	66
Cohasset	2,243	1,381	62
Concord	4,445	2,756	62
Dover	1,740	1,221	70
Lexington	8,902	5,437	61
Lincoln	1,510	880	58
Needham	8,098	4,851	60
Newton	16,910	10,131	60
Sherborn	1,320	894	68
Sudbury	5,321	3,839	72
Wellesley	7,254	4,770	66
Weston	3,339	2,080	62
Winchester	5,580	3,357	60
Total	80,987	50,369	62

Notes: Data on total number of single-family houses are constructed from local assessment records.

Table 1.4: Sale Prices and Number of Sales by Year

Year	Mean	Median	N
1982	137,084	123,500	1,405
1983	152,902	135,000	2,444
1984	186,019	164,900	2,405
1985	240,716	217,000	2,365
1986	308,295	272,000	2,345
1987	332,980	285,000	3,542
1988	361,837	300,000	3,160
1989	357,911	300,000	2,936
1990	342,556	287,500	2,612
1991	315,569	267,000	3,601
1992	322,201	275,000	3,955
1993	337,797	290,000	3,906
1994	359,474	311,250	3,960
1995	386,454	330,000	3,517
1996	407,836	348,000	3,845
1997	443,288	374,850	4,016
1998	487,614	410,000	4,246
1999	541,459	450,000	4,333
2000	659,927	532,125	3,848
2001	728,247	587,000	3,144
2002	747,042	615,000	3,650
2003	806,318	665,000	3,469
2004	893,483	720,500	3,990
2005	969,502	785,000	3,569
2006	969,513	781,000	2,621
Total			82,884

Notes: Sale prices are shown in nominal terms. The number of sales in 2006 is low because I drop Newton sales in 2006. The number of sales between 1982 and 1986 is low because the 1982-1986 sales data may not be exhaustive. See the Data section for details.

Table 1.5: Summary Statistics of Key Variables

	Pre-TRA97 (N=507,424)		Post-TRA97 (N=654,474)	
	Mean	SD	Mean	SD
Semiannual Sale Dummy	0.024	0.153	0.024	0.153
Taxable Capital Gains (TCG)	49,616	88,931	344,707	278,541
Real Purchase Price	346,159	201,876	366,904	241,624
Real Current Price	435,740	228,898	733,265	423,058
Real Housing Appreciation Rate	0.010	0.046	0.031	0.037
Log(lot size)	9.678	0.968	9.734	0.986
($TCG \leq 0$)	0.313	0.464	0.001	0.036
($0 < TCG \leq 125K$)	0.525	0.499	0.192	0.394
($125K < TCG \leq 250K$)	0.125	0.331	0.259	0.438
($250K < TCG \leq 500K$)	0.035	0.184	0.342	0.474
($TCG > 500K$)	0.002	0.049	0.205	0.404

Notes: Taxable Capital Gains (TCG) are measured in nominal terms. Lot size is measured in unit of square footage. Housing appreciation rates refer to semiannual appreciation rates. Real dollars refer to 2000 dollars.

Table 1.6: Effect of TRA97 on Home Sales - Main Results

	(1)	(2)	(3)
(TCG≤0)*TRA97	1.20	0.53	0.60
	(0.75)	(0.62)	(0.64)
(0<TCG≤25K)*TRA97	0.71***	0.54***	0.54***
	(0.14)	(0.13)	(0.14)
(125K<TCG≤250K)*TRA97	0.62***	0.44**	0.45**
	(0.16)	(0.15)	(0.16)
(250K<TCG≤500K)*TRA97	0.39*	0.29	0.33
	(0.19)	(0.19)	(0.19)
(TCG>500K)*TRA97	-0.83**	-0.82**	-0.79**
	(0.29)	(0.29)	(0.30)
Housing Appreciation Rate		-1.89**	-1.99**
		(0.58)	(0.64)
Log(lot size)		-0.43***	-0.44***
		(0.03)	(0.03)
23 TCG Category Dummies	Y	Y	Y
Real Purchase Price Polynomials	N	Y	Y
Real Current Price Polynomials	N	Y	Y
Time since Purchase Polynomials	N	Y	Y
Year Dummies	Y	Y	N
City Dummies	N	Y	N
City*Year Dummies	N	N	Y
N	1,161,442	1,161,442	1,152,127
Pseudo-R^2	0.006	0.013	0.013

Notes: All columns are Probit regressions with outcome variable equal to 1 if the house was sold in the next half-year. Marginal effects are expressed in percentage for ease of exposition. Polynomials are controlled to the 4th order. Standard errors shown in parentheses are clustered at house level. * significant at 0.05 level, ** significant at 0.01 level, *** significant at 0.001 level.

Table 1.7: Effect of TRA97 on Home Sales - Robustness Checks

A. Measurement Errors in TCG

	(1) Original Sample	(2) Difference < 100%	(3) Difference < 50%	(4) Cut-off at 450K
(TCG≤0)*TRA97	0.53	0.39	0.25	0.53
	(0.62)	(0.62)	(0.65)	(0.62)
(0<TCG≤125K)*TRA97	0.54***	0.63***	0.68***	0.54***
	(0.13)	(0.14)	(0.15)	(0.13)
(125K<TCG≤250K)*TRA97	0.44**	0.42**	0.36*	0.44**
	(0.15)	(0.15)	(0.17)	(0.15)
(250K<TCG≤500K)*TRA97	0.29	0.23	0.02	0.33
	(0.19)	(0.19)	(0.20)	(0.19)
(TCG>500K)*TRA97	-0.82**	-0.79*	-1.08**	-0.64*
	(0.29)	(0.33)	(0.35)	(0.27)
N	1,161,442	1,105,591	972,902	1,161,442
Pseudo-R^2	0.013	0.014	0.015	0.013

B. Alternative Sample and Specification

	(1) Original Sample	(2) Drop 1996	(3) House Sold Before 1997	(4) 6th Order Polynomials
(TCG≤0)*TRA97	0.53	0.52	1.79	0.50
	(0.62)	(0.62)	(1.10)	(0.62)
(0<TCG≤125K)*TRA97	0.54***	0.51***	0.53***	0.52***
	(0.13)	(0.13)	(0.13)	(0.13)
(125K<TCG≤250K)*TRA97	0.44**	0.45**	0.43**	0.44**
	(0.15)	(0.15)	(0.15)	(0.15)
(250K<TCG≤500K)*TRA97	0.29	0.47*	0.28	0.34
	(0.19)	(0.20)	(0.19)	(0.19)
(TCG>500K)*TRA97	-0.82**	-0.76*	-0.77**	-0.81**
	(0.29)	(0.33)	(0.29)	(0.29)
N	1,161,442	1,097,625	1,063,510	1,161,442
Pseudo-R^2	0.013	0.013	0.014	0.013

Notes: All columns are Probit regressions with outcome variable equal to 1 if the house was sold in the next half-year. Marginal effects are expressed in percentage for ease of exposition. Other control variables are housing appreciation rate, log of lot size, 23 TCG category dummies, purchase price polynomials, current price polynomials, time since purchase polynomials, city dummies, and year dummies. Polynomials are controlled to the 4th order unless indicated otherwise. Standard errors shown in parentheses are clustered at house level. * significant at 0.05 level, ** significant at 0.01 level, *** significant at 0.001 level.

Table 1.8: Effect of TRA97 on Home Sales - Extensions

	(1) Original Sample	(2) 18-Month Window	(3) 3-Year Window	(4) Early = 18 Months	(5) Early = 3 Years
(TCG≤0)*TRA97	0.53	-	-		
	(0.62)	-	-		
(0<TCG≤125K)*TRA97	0.54***	0.66***	0.59***		
	(0.13)	(0.15)	(0.15)		
(125K<TCG≤250K)*TRA97	0.44**	0.58*	0.41*		
	(0.15)	(0.28)	(0.20)		
(250K<TCG≤500K)*TRA97	0.29	0.28	0.39		
	(0.19)	(0.38)	(0.28)		
(TCG>500K)*TRA97	-0.82**	0.50	-0.01		
	(0.29)	(1.02)	(0.61)		
(0<TCG≤125K)*Early				0.52***	0.51***
				(0.14)	(0.14)
(0<TCG≤125K)*Later				0.03	0.11
				(0.52)	(0.53)
(125K<TCG≤250K)*Early				0.37*	0.47**
				(0.18)	(0.16)
(125K<TCG≤250K)*Later				-0.08	-0.11
				(0.50)	(0.50)
(250K<TCG≤500K)*Early				0.66*	0.54*
				(0.28)	(0.22)
(250K<TCG≤500K)*Later				-0.25	-0.34
				(0.50)	(0.48)
(TCG>500K)*Early				0.13	-0.36
				(0.55)	(0.35)
(TCG>500K)*Later				-1.21**	-1.30***
				(0.41)	(0.39)
N	1,161,442	181,704	1,161,442	421,457	1,161,442
Pseudo-R2	0.013	0.008	0.013	0.009	0.013

Notes: All columns are Probit regressions with outcome variable equal to 1 if the house was sold in the next half-year. Marginal effects are expressed in percentage for ease of exposition. Other control variables are housing appreciation rate, log of lot size, 23 TCG category dummies, purchase price polynomials, current price polynomials, time since purchase polynomials, city dummies, and year dummies. Polynomials are controlled to the 4th order. Standard errors shown in parentheses are clustered at house level. * significant at 0.05 level, ** significant at 0.01 level, *** significant at 0.001 level.

Table 1.9: Estimate the Tax Elasticity of Home Sales using Post-TRA97 Data

A. Summary Statistics

	N	Mean($)	Median($)	SD($)
Taxes (including zeros)	654,474	8,205	0	24,914
Taxes (excluding zeros)	134,463	39,936	26,224	41,881

B. Estimation Results

	(1) All Obs	(2) Drop Zeros
Capital Gains Taxes (in $10,000s)	-0.155*	-0.254**
	(0.073)	(0.091)
N	654,474	134,463
Pseudo-R^2	0.011	0.008

Notes: All columns in Panel B are Probit regressions with outcome variable equal to 1 if the house was sold in the next half-year. Marginal effects are expressed in percentage for ease of exposition. Other control variables are housing appreciation rate, log of lot size, 23 TCG category dummies, purchase price polynomials, current price polynomials, time since purchase polynomials, city dummies, and year dummies. Polynomials are controlled to the 4th order. Standard errors shown in parentheses are clustered at house level. * significant at 0.05 level, ** significant at 0.01 level, *** significant at 0.001 level.

Chapter 2

Property Taxes and Elderly Mobility[1]

2.1 Introduction

During the late 1990s and early 2000s, the housing market in the United States experienced a remarkable boom. As housing prices increased, property taxes rose significantly in many parts of the country. Increases in property taxes have drawn attention from both the general public and policy makers. The public and politicians are particularly concerned that elderly homeowners who live on fixed incomes will be driven out of their homes because they can no longer afford increasing property taxes. In response, many states are looking for new ways of providing property tax relief to elderly homeowners. Although policy makers have assumed that rising property taxes cause elderly homeowners to move, researchers have provided little empirical evidence of such a link.

Apart from its policy implications, studying property taxes' effect on elderly mobility is also of great economic importance. The simplest version of the life-cycle

[1] I thank David Baer for his assistance with property tax relief program data.

model, which assumes away capital market imperfection, transaction costs, bequest motives, and uncertainty, predicts that utility-maximizing agents accumulate wealth while working and deplete wealth after retirement. If elderly homeowners view their housing wealth as a part of retirement savings to be used for general consumption, then we would expect elderly homeowners to trade down and consume their housing wealth after retirement. However, studies including Feinstein and McFadden (1989) and Venti and Wise (1989, 1990, 2001) find little evidence of downsizing behavior among elderly homeowners in the absence of precipitating shocks such as health decline and loss of spouse. Because residential mobility is directly linked to housing adjustment and downsizing decisions, studying how factors such as property taxes affect elderly mobility may help us build richer models to describe household life-cycle saving and consumption patterns.

Despite its policy and economic significance, the question whether property taxes have caused elderly homeowners to move is difficult to address empirically for two reasons. First, reliable household-level measures of property tax payments and mobility outcomes are scarce. Hence, many earlier studies use aggregated measures such as property tax per capita and state to state or county to county migration flows. These studies include Cebula (1974), Clark and Hunter (1992), Dresher (1994), Conway and Houtenville(2001), and Duncombe et al (2003). Second, property taxes are likely to be endogenous to individuals' moving decisions. For example, elderly homeowners who value local public services (e.g. nice parks, low crime rates, and new senior centers) are likely to live in areas with high property taxes that provide superior services. Such tastes for local public services are also likely to correlate with mobility outcomes. Because individual taste is not observable to econometricians, studies such as Seslen (2005) that fail to instrument for property taxes suffer from omitted variable bias.

In this paper, I use the 1992 to 2004 waves of the Health and Retirement Survey (HRS) panel data. This dataset has household-level measures of property tax payments and mobility outcomes in addition to extensive information on demographics and socio-economic characteristics. I use two empirical strategies to identify the causal effect of property taxes on elderly mobility. First, I exploit the variation in state-provided property tax relief programs and use simulated relief benefits to instrument for property tax payments. Such simulated relief benefits contain only the variation in program rules and depend exclusively on state, year, and age of homeowners. More generous relief programs reduce property tax payments of eligible homeowners, and these state-provided programs are arguably exogenous to individual homeowners' unobserved tendency to move. Therefore, simulated relief benefits serve as a valid instrument for property taxes in studying elderly mobility.

Second, I use the variation in effective property tax rates and housing value appreciation rates to study whether higher property taxes cause elderly homeowners to move. The thought experiment is to compare two observably identical homeowners, one living in a place with a high effective property tax rate and the other living in a place with a low effective property tax rate. When housing values in both places go up, the person who lives in the area with a high effective tax rate will experience a larger increase in property taxes. Thus, he would be more likely to move if property taxes affect homeowners' mobility. The key identification assumption here is that in the absence of a property tax effect, mobility in areas with high effective tax rates responds to rising property values the same way as mobility in areas with low effective tax rates, after controlling for observable characteristics. For example, if increases in housing wealth affect mobility, this identification strategy assumes that such a wealth effect is symmetric for homeowners in both areas with high effective tax rates and areas with low effective tax rates.

I find that higher property taxes have a significant impact on elderly homeowners' moving decisions. My central instrumental variable estimates suggest that a \$100 increase in annual property taxes causes the two-year mobility rate to increase by 0.76

percentage points, which represents an eight percent increase from a baseline two-year mobility rate of nine percent. The result is robust to various model specifications. Individuals living in areas where the effective property tax rate is high and housing values have been appreciating rapidly are the most affected. Moreover, I present suggestive evidence that liquidity constraints may play a role in the effect of property taxes on elderly mobility.

I also find that state-provided property tax relief programs reduce eligible homeowners' moving probability. Programs that cap annual property tax payments seem to have the most pronounced effect on elderly mobility. Eligible relief benefits from homestead exemptions, homestead credits, and circuit-breakers programs needs to reach certain threshold to generate any impact on elderly mobility. Programs that are implemented by state personal income tax credits do not appear to reduce mobility. These findings may be useful to policy-makers in designing cost-effective and efficient relief mechanisms. This paper's findings also provide indispensable evidence for normative welfare analysis of the impact of property taxes and property tax relief programs on elderly homeowners.

This paper proceeds as follows. The next section outlines the background and reviews previous research on property taxes and elderly mobility. Section 3 then describes the data used in this paper. In section 4, I explain the empirical strategies that I use to identify the effect of property taxes on elderly mobility. I also show estimation results using these strategies. In section 5, I evaluate various property tax relief programs from the policy perspective. The last section concludes and provides directions for future research.

2.2 Background and Previous Research

In 2004, property tax collections in the U.S. exceeded \$300 billion. Property taxes are responsible for approximately 72% of all local tax revenues, representing the most important tax revenue source for local governments.[2] The housing market boom of the late 1990s and early 2000s led to significant increases in residential property taxes. Figure 2-1 shows that from 2000 to 2005, median house value rose by 47% in real terms and median property tax payments by homeowners increased by 30%.[3]

Rising property taxes may be particularly burdensome to elderly homeowners. Following notations in Poterba (1992), the user cost faced by a homeowner under the current U.S. tax system can be written as

$$uc = \begin{cases} (1-\tau_{inc})[\tau_p + \alpha i + (1-\alpha)r] + m + \delta - \pi^H & \text{for itemizers} \\ \tau_p + \alpha i + (1-\tau_{inc})(1-\alpha)r + m + \delta - \pi^H & \text{for non-itemizers} \end{cases}$$

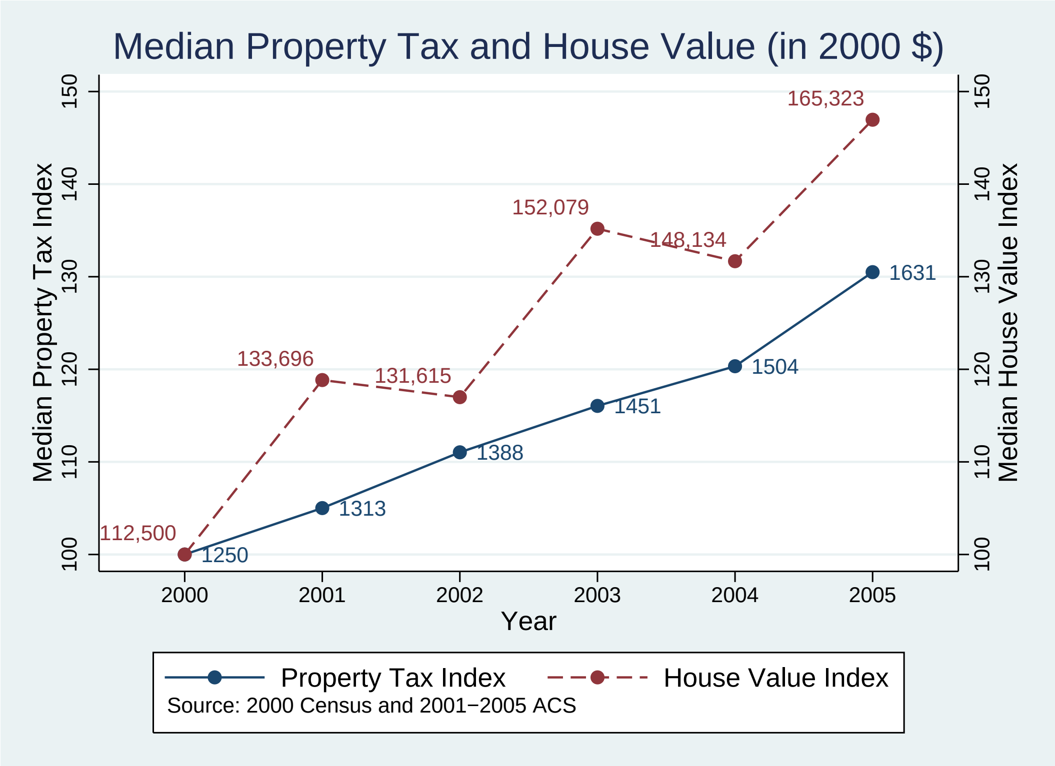
where τ_{inc} is the homeowner's marginal income tax rate, τ_p is the effective property tax rate, α is the loan to value ratio on the house, i is the mortgage interest rate, r is the interest rate on alternative investment opportunities, m is the maintenance cost rate, δ is the true economic depreciation rate, and π_H is the housing value appreciation rate. One measure of property tax burden on homeowners is the ratio of property tax rates to user costs. For itemizers and non-itemizers, the ratios are

$$burden = \begin{cases} \dfrac{(1-\tau_{inc})\tau_p}{(1-\tau_{inc})[\tau_p + \alpha i + (1-\alpha)r] + m + \delta - \pi^H} & \text{for itemizers} \\ \dfrac{\tau_p}{\tau_p + \alpha i + (1-\tau_{inc})(1-\alpha)r + m + \delta - \pi^H} & \text{for non-itemizers} \end{cases}$$

[2] See Bradley (2005) and NCSL (2005).

[3] A number of factors could have contributed to the increase in residential property tax payments, including unfunded federal mandates, reduction in state aid to local governments, changes in the cost of providing local public services, and relative appreciation rates of residential versus non-residential properties. Although interesting in its own right, it is beyond the scope of this paper to determine which factors explain the property tax increases the most.

Figure 2-1: Median Property Tax and House Value



According to the 2004 Survey of Consumer Finances, the median homeowner of age 65 or above is a non-itemizer who faces a marginal tax rate of 15% and who has paid off his mortgages. In contrast, the median homeowner of age below 65 is an itemizer with a marginal tax rate of 25% and a loan to value ratio of 0.5. Assuming that $\tau_p = 0.01$, $i = 0.08$, $r = 0.05$, $m = 0.02$, $\delta = 0.02$, and $\pi^H = 0.03$, we have $burden_{nonelderly} = 0.12$ and $burden_{elderly} = 0.19$. If we assume elderly homeowners spend less on home maintenance than non-elderly homeowners as suggested by Davidoff (2007), the property tax burden on elderly homeowners would appear even higher than that on non-elderly homeowners.

State and local governments may be concerned that elderly homeowners in the face of rising property tax burdens decide to relocate to areas with low property taxes. Given that around half of property tax revenues are used to finance public schools and that elderly homeowners usually do not consume school services, elderly homeowners may find that the local public services that they receive are not worth their costs. In response, they decide to readjust their demand for housing consumption bundles and relocate to areas with both fewer public services and lower property taxes. Precisely because elderly homeowners in general consume fewer public services but expand the state and local tax base, they are attractive to state and local governments except when they reach the end of their lives and demand expensive medical care services through Medicaid. They are even called "pure gold" in Longino and Crown (1989). As discussed in Mackey and Carter (1994), many states in the U.S. provide a wide range of tax preferences to entice elderly migrants.

Alternatively, increasing property taxes may raise mobility rates among elderly homeowners through liquidity constraints. Because the elderly typically rely on fixed incomes such as Social Security benefits and pension benefits, and because many of them do not have many liquid assets, rising property taxes may cause elderly homeowners to be liquidity-constrained. Even if an elderly homeowner has great psychological attachment to his house and prefers not to move as long as he can afford it, significant increases in property taxes may eventually cause the homeowner to liquidate his housing wealth. This liquidity constraint mechanism and the demand readjustment mechanism mentioned earlier have very different welfare implications as to whether property tax relief programs should be provided by state and local governments.

A few papers investigate property taxes and elderly mobility using household-level data. The studies closest to this paper are Farnham and Sevak (2006) and Seslen (2005). The former study is a test of a life-cycle Tiebout model using the 1992-2000 HRS data and local fiscal data. It finds that cross-state, empty-nest movers experience reduced exposure to local school spending and property taxes. Although their study examines both property taxes and elderly mobility, Farnham and Sevak (2006) addresses the question from a different angle than the current study. Their study focuses on testing whether property tax payments decline after an elderly homeowner makes a move, whereas my study asks whether rising property taxes induce elderly homeowners to move. Moreover, their paper presents a correlation study, while my paper tackles the causality question using instrumental variable strategies.

Seslen (2005) examines the effect of property taxes on elderly homeowners' downsizing decisions in a competing risk framework. Using the Retirement History Survey from 1969 to 1979, she finds little evidence that property taxes affect elderly homeowners' decisions to move and to liquidate their housing wealth. Thus, she concludes that property tax relief programs are likely to solely transfer resources to the wealthy without achieving the goal of protecting the needy. Although Seslen (2005) employs sophisticated econometric tools, the data she studies were collected about 30 years ago, and they may not bear on the current situation. She uses self-reported property tax payments as the key explanatory variable, but she ignores the potential endogeneity problem where some unobserved factor drives both property tax payments and mobility decisions. Finally, Seslen (2005) has neither geographic information nor relief program details, so she cannot evaluate the impact of these state-provided relief programs.

My paper advances the prior literature in several ways. First, I use the HRS data, a nationally representative panel of elderly households that contains rich information on individual and household characteristics, including actual annual property tax payments. The panel structure also provides an opportunity for me to look at the dynamic relationship between the last period's property tax payments and the next period's mobility outcomes, which is impossible to do with cross-sectional data. Second, during my sample period, the United States experienced significant increases in property taxes. The recent trend of rising property taxes provides a good opportunity to study the effect of property taxes on elderly homeowners' moving decisions.

Third, I obtained access to the HRS restricted geographic identifiers and collected data on state-provided property tax relief programs for the past 15 years. With these data, I am able to calculate the amount of eligible property tax relief benefits for each household in each survey year. Lastly, I address the potential endogeneity problem using instrumental variable approaches. To my knowledge, this is the first study to examine the causal effect of property taxes on elderly mobility and to measure property tax relief benefits at the household level. The innovations in both data and estimation methodology allow this paper to present more compelling evidence than currently exists on the effect of property taxes on elderly mobility.

2.3 Data

2.3.1 HRS Household Level Panel Data

The Health and Retirement Study (HRS) is biannual panel data of the elderly and near-elderly in the United States. At present, seven waves of the survey (1992-2004) have been released to researchers. HRS includes households from four different cohorts.[4] The original HRS cohort consists of individuals born between 1931 and 1941. They appear in all seven waves of my sample. The AHEAD cohort (born before 1924) was interviewed in 1993 first and then in 1995. Since 1998, the AHEAD cohort has been interviewed concurrently with the HRS cohort biannually. In 1998, two other cohorts were added to the sample: the "Children of the Depression" (CODA) cohort (born between 1924 and 1930), and the "War Baby" (WB) cohort (born between 1942 and 1947). Hence, these two cohorts appear only in the last four waves (1998-2004) in my sample.

[4]In 2004, a fifth cohort, Early Boomers (born between 1948 and 1953), was added to HRS. Because households in this cohort have only been interviewed once and I need at least two adjacent surveys to study whether the last period's property taxes affect mobility in the next period, I exclude them from my analysis.

In addition to the publicly available HRS data, I obtained restricted access to household level geographic identifiers. These identifiers allow me to identify the state of residence for each household at each survey interview time. The state identifier is crucial in my analysis because it links households with the state-provided property tax relief programs for which they are eligible. Because of the ambiguity associated with mobility for people living in mobile homes, I exclude them from my analysis. Because farms and ranches may be treated as agricultural rather than residential properties for property tax purposes, I also exclude people living on farms or ranches from my sample. Households residing in mobile homes or on farms and ranches combined constitute around 10 percent of the entire HRS sample. I also dropped individuals who are newly separated or divorced because mobility becomes complicated for these individuals. Newly separated or divorced homeowners represent less than 1 percent of the sample.

Except for the very first survey conducted on each household, every subsequent survey asks respondents whether they have moved since their last survey interview. I use respondents' answers as my mobility measure. I contacted HRS staff to confirm that this mobility measure is a valid and consistent measure across waves. Panel A of Table 2.1 displays the two-year mobility rates of the HRS cohort households from 1992 to 2004. In earlier years when those respondents were relatively young, their two-year moving probability was around 7%. Toward the end of the panel, the probability increases to 12%. In contrast, the average one-year mobility rate among homeowners of age below 65 is about 10% during the 1990s and early 2000s.[5] Panel B of Table 2.1 shows that homeownership rates of HRS cohort households stay steady at around 80% during the 12-year sample period. Panel C of Table 2.1 presents a tenure transition matrix for all moves made by HRS cohort households between 1992 and 2004. Over 80% of homeowners remain homeowners after they relocate, and

[5] Author's calculation using the PSID data.

70% of renters stay renters after they move. In summary, Table 2.1 shows evidence consistent with the conclusion drawn by Venti and Wise (2001) that mobility rates among elderly homeowners are very low, and that elderly homeowners do not seem to trade down and consume their housing wealth in the absence of precipitating shocks.

In all seven waves, respondents were requested to report the amount of property taxes paid on their primary residence during the past year. I assume the self-reported property tax payments are the actual payments *after* all relevant exemptions, rebates or refunds provided by relief programs have been applied. Such an assumption is crucial for the first-stage regression in my IV strategy. For programs where participation is automatic and property tax bills are mailed to homeowners after benefits have been netted out, this assumption seems justified. For programs where homeowners receive rebate checks soon after paying property taxes, it is unclear whether respondents report their before-relief property tax payments or after-relief property tax payments. For programs that are implemented by state personal income tax credits, respondents are likely to report their before-relief benefits for two reasons. First, relief benefits are usually received long after homeowners have paid their property taxes. Second, property tax relief benefits may appear less salient on state personal income tax returns. For example, filers may view property tax credits that they claim against income tax liabilities as *income tax* relief benefits rather than *property tax* relief benefits. Recent studies including Chetty, Looney and Kroft (2007) and Finkelstein (2007) suggest that tax salience could have a significant impact on behavior. Regression analysis shown later in this paper confirms that respondents in states that use income tax credits to grant property tax relief benefits do not report lower property tax payments when they are eligible for more generous relief benefits. Therefore, I exclude in my main regression analysis states where relief benefits are granted by tax credits on state personal income tax returns.[6] The dropped observations represent about 25%

[6]These states are District of Columbia, Massachusetts, Michigan, Missouri, Montana, New Jersey,

of the sample.

Table 2.2 presents the summary statistics of demographic and socio-economic variables. Note that only about 17% of moves in the sample are cross-state moves, which implies a 3.8% five-year cross-state mobility rate (i.e. $9 \times 0.17 \times (5/2) = 3.8$). This rate is very similar to what other studies on elderly migration find.[7] Given that the majority of moves are within-state relocations, results produced by studies focusing on cross-state mobilities could be misleading.

2.3.2 Data on Property Tax Relief Programs

Background on Property Tax Relief Programs

As of 2005, all 50 states and District of Columbia have some form of property tax relief programs for homeowners, especially for low-income and elderly homeowners. Many of these programs were first established well before my sample period started.[8] Broadly speaking, there are four categories of relief programs. The first includes *Homestead Exemptions and Credits*. This is the most widely used form of property tax relief. Homestead exemption programs usually reduce assessed property value by a certain amount.[9] Homestead credit programs either refund a certain percentage of taxes due or provide a fixed credit to qualifying homeowners.[10] These homestead

New Mexico, New York, Oklahoma, Rhode Island, Vermont, and Wisconsin. I do not exclude states that use rebate checks to implement relief programs because the sample size would drop significantly and asymptotic theory no longer applies when there are only a few states left in the sample and standard errors are clustered at the state level.

[7]Woo (2005) states that the five-year cross-state mobility rate among elderly homeowners is 4.2% in the Census data and 4.0% in the Current Population Survey data.

[8]Homestead exemptions are believed to be a by-product of the Great Depression of the 1930s. Circuit-breakers were first legislated in the 1960s and 1970s. Limits were put in place in the late 1970s and early 1980s when high inflation rates caused property tax bills to spiral out of control and eventually resulted in property tax revolts.

[9]For example, a homeowner of age 65 or above in Kentucky was allowed to exclude $29,400 from the assessed value of his main residence for property tax purposes in 2005.

[10]For example, Massachusetts state statue Clause 17D and 41C grant a $175 homestead credit and a $500 homestead credit respectively to homeowners of age 70 or above who satisfy certain income, assets, and residence requirements.

exemption and credit programs usually require homeowners to file an application to local property tax authorities.

The second category is *Circuit-Breakers*. Some of these programs are for homeowners only, and others are for homeowners as well as renters. Since these programs are designed to help people who need assistance, benefits are typically a decreasing function of income. Circuit-breaker programs can work either on a sliding scale or through a threshold mechanism. For example, District of Columbia has a circuit-breaker program where homeowners whose income is $20,000 or less can receive up to a $750 tax credit using a threshold mechanism. In 2007, Idaho's circuit-breaker program refunded up to $1320 for homeowners of age 65 or above with income below $28,000 using a sliding scale mechanism.[11]

The third category is *Property Tax Deferral Programs*. These programs allow qualified homeowners, typically low-income elderly homeowners, to defer property tax payments at a low interest rate. Effectively, they become a lien against the taxpayer's house. When the homeowner sells the house or dies, deferred taxes must be paid when the estate is settled. Deferral programs are considered by academics the most targeted and cost-effective way of providing property tax relief. Nevertheless, very few qualified homeowners take up such programs in practice. Anecdotal evidence suggests that elderly homeowners are reluctant to put a lien on their houses. This is consistent with the observation that very few elderly homeowners purchase reverse mortgages in the United States.

The last broad category is *Property Tax Limits*. Property tax limits include rate limits, assessment limits, revenue rollbacks, expenditure limits, and property tax freezes. Depending on the state, any one or a combination of the above limits can be

[11]The key difference between homestead credit programs and circuit-breakers is that although homestead credit programs may use income as a qualification criterion, their benefit levels do not vary with income. On the other hand, benefits are explicitly a decreasing function of income for circuit-breakers. For this reason, circuit-breakers are considered better targeted at low and moderate-income individuals.

used. Proposition 13 in California and Proposition 2.5 in Massachusetts are among the most prominent examples of property tax limits. Although almost all states have property tax limits of one kind or another, many of these programs do not guarantee that individual homeowners' property tax bills will not go up significantly from year to year. Because property taxes are the product of taxable values and tax rates, the amount of property taxes homeowners pay will be limited only when both assessment values and tax rates are limited. Rate limits or assessment limits alone are insufficient in curbing property tax growths. Moreover, states usually allow for override and bonded indebtedness so that local governments can still increase property taxes. For example, if non-elderly homeowners want to spend more on schools, they may approve an override, in which event the elderly will face rising property taxes. However, two kinds of limits apply to individual homeowners: "assessment value freezes" and "property tax freezes."[12]

Participation rates of property tax relief programs vary across states and programs. In mid-1990s, the American Association of Retired Persons (AARP) obtained numbers of program participants from various state administering offices and estimated participation rates for these programs.[13] The median estimated participation rate among the eligible is the highest for homestead exemptions - around 90%. In contrast, the median estimated participation rate is only about 40% for homestead credits and circuit-breakers and less than 1% for deferral programs. It is puzzling why participation rates for homestead credits and circuit-breakers are so low among elderly households. Some have suggested that social stigma and program complexity may play a role.[14]

[12]For example, in Illinois, homeowners of age 65 and older with income less than $40,000 may receive a freeze on their equalized assessed real property value. In Texas, school property taxes do not increase once a homeowner reaches age 65. The former is classified as an assessment value freeze and the latter is classified as a property tax freeze.

[13]See Baer (1998).

[14]See ACIR (1975).

Since state-provided property tax relief programs are extremely complicated and vary tremendously across states, I focus on three types of relief programs in this paper.[15] The first type includes homestead exemptions, homestead credits, and circuit-breakers. Relief benefits from these programs can be quantified for individual homeowners. The second and the third types refer to assessment value freezes and property tax freezes, respectively. For these two types, it is difficult to quantify their benefits for individual homeowners. Hence, I use dummy variables to indicate whether a homeowner is eligible for "assessment value freezes" or "property tax freezes."

Data Collection on Property Tax Relief Programs

First, I collected descriptive information from a range of publications by the U.S. Advisory Commission on Intergovernmental Relations (ACIR), the American Association of Retired Persons (AARP) and the National Conference of State Legislatures (NCSL) from 1990 to 2005. Then I compiled and organized such information by state and year. In my effort to confirm changes in these state programs over years and to resolve inconsistencies reported in various ACIR, AARP, and NCSL publications, I read state statutes that define these programs in legal terms. I searched for historical local news on property tax relief program changes. I studied program application forms, homeowners' brochures, and Q&As on state and/or local government websites. I contacted Connecticut, Delaware, Georgia, Florida, Hawaii, Illinois, Indiana, Louisiana, Maine, Maryland, Massachusetts, Mississippi, Nevada, New Jersey, North Dakota, Texas, Utah, Virginia, and Wyoming state and/or local governments for further explanation and confirmation of program details.

After obtaining accurate program descriptions, I calculated eligible benefits as a

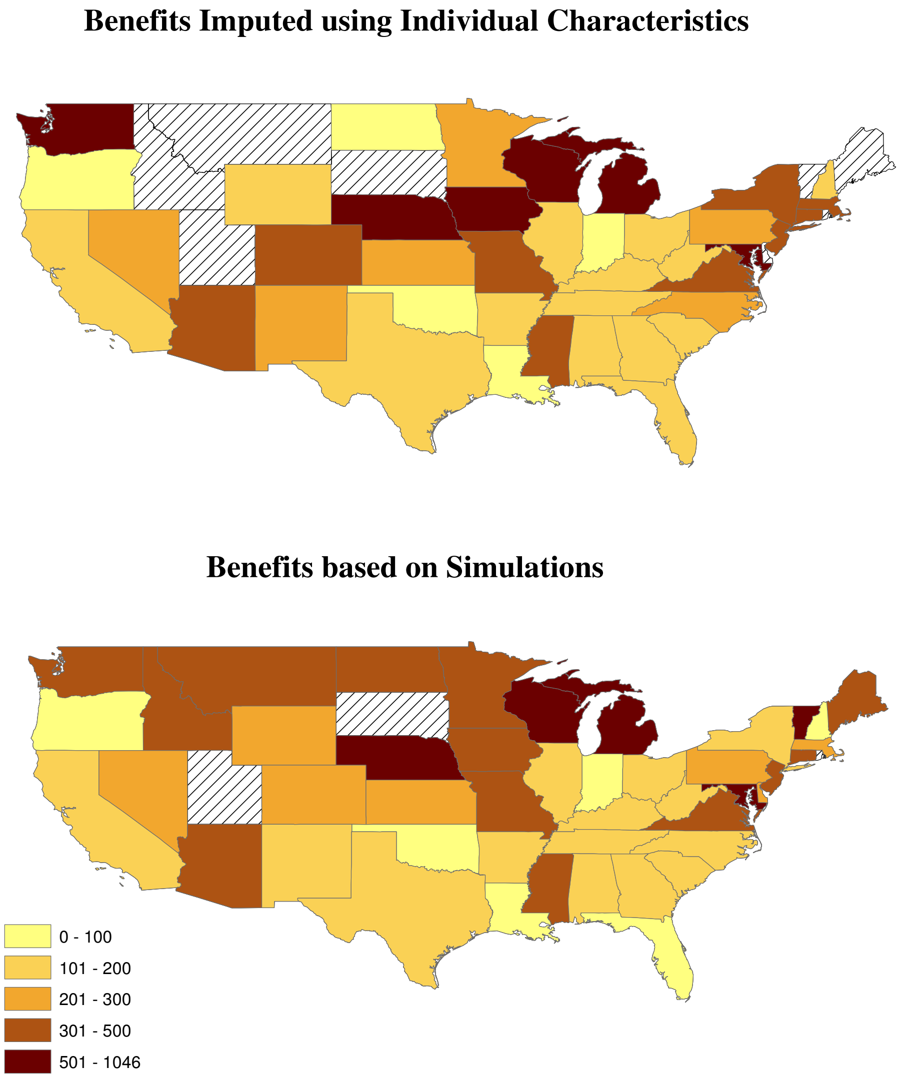
[15] I do not consider programs that provide exactly the same amount of benefits to everyone because of the lack of within-state variation. Deferral programs are ignored because participation rates are too low. Limits that affect a jurisdiction but not necessarily individual homeowners are not considered. I also exclude local option programs that vary significantly across localities due to data collection difficulties.

function of state of residence, year, age, income, house value, Social Security income, marital status, household size, and wealth. This calculation generates three output variables: the amount of benefits from homestead exemption, homestead credit, and circuit-breaker programs that a homeowner is eligible for, whether eligible for an "assessment value freeze" program, and whether eligible for an "property tax freeze" program. Such output parameters can be calculated for any homeowner in the U.S. in any year between 1990 and 2004.

Table 2.3 shows year 2000 formulas used in calculating property tax relief benefits for the ten states with the most observations in my sample. The amount of eligible benefits are calculated for a hypothetical married homeowner of age 65 with an annual total household income of \$20,000, Social Security income of \$10,000, and house value of \$100,000. The amount of eligible benefits for this hypothetical homeowner varies from zero in Pennsylvania to \$1,000 in New Jersey. The formulas shown in Table 2.3 suggest that eligible benefits vary considerably across states, and they are sensitive to individual characteristics such as income and house value.

The first two columns in Table 2.4 show the percentage of homeowners eligible for relief benefits and the average benefits conditional on eligibility by age groups, income quintiles, and house value quintiles. A number of interesting patterns emerge. The percentage of homeowners eligible for relief benefits increases monotonically in age and decreases monotonically in income and housing value. Conditional on eligibility, average benefits decrease in income. Given that property taxes relief programs target low-income and elderly homeowners, such patterns are expected. The conditional average benefits increases monotonically in housing value. This pattern is likely due to the threshold design of circuit-breakers. For example, if a circuit-breaker refunds property taxes exceeding 3% of a homeowner's income, then homeowners living in more expensive houses would receive higher refunds *ceteris paribus*. The conditional average benefits do not appear to change monotonically in age. This pattern is likely

Figure 2-2: Conditional Average Benefits by State



Note: States shown with stripes means that either no households in my sample comes from these states or the total number of households from that state in my sample is below 10. Since state identifiers are data with restricted access, I cannot show them for confidentiality reasons.

caused by complicated correlations between age and household characteristics. For example, the younger elderly homeowners tend to have higher income, which leads to lower relief benefits. The younger elderly homeowners also tend to live in more expensive houses, which leads to higher relief benefits.

The first part of Figure 2-2 plots conditional average benefits by state. South Dakota and Utah are missing from the map because no homeowners in the sample are from these two states. Delaware, Idaho, Maine, Montana, and Vermont each have fewer than 10 households in the sample. Because access to state identifiers is restricted, I cannot show them on this map for confidentiality reasons. This map also shows that District of Columbia, Iowa, Maryland, Michigan, Nebraska, Washington and Wisconsin have the highest average conditional benefits.

2.4 Empirical Strategy and Results

In this section, I present the empirical models and estimation results in studying the effect of property taxes on elderly mobility. Specifically, I first use property tax relief benefits as instruments to identify the impact of property taxes on elderly mobility. Then I use variations in housing value appreciation rates across geographic areas and years to identify such impact. I also explore whether liquidity constraints play a role in property taxes' effect on elderly homeowners' moving decisions.

2.4.1 Using Relief Benefits as Instruments

Probit and IV Probit Estimation

To investigate whether property taxes have an impact on elderly mobility, I start with the following probit model:[16]

$$\text{Prob}(Move_{ist} = 1) = \Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t) \tag{2.1}$$

where $Move_{ist}$ is a binary indicator for whether household i in state s moved between time t and $t+1$, ζ_s denotes state fixed effects, δ_t denotes year fixed effects, and the covariate vector $\mathbf{X}_{ist}$ includes income quintile indicators, house value quintile indicators, financial wealth quintile indicators, gender, race, household size, number of living children, whether married, whether newly widowed, education categories (i.e. less than high school, high school graduates, some college, and college graduates), whether currently working, whether newly retired, whether spouse is currently working, whether spouse is newly retired, whether hospitalized between the last interview and the current interview,[17] age dummies, and spouse age dummies. The key variable of interest in equation (2.1) is Tax_{ist}, property tax payments by household i in state s at time t. If higher property taxes cause elderly homeowners to move, then we expect β_1 to be positive.

The first column in Table 2.5 displays estimation results of equation (2.1). To make the results interpretable, I show marginal effects of independent variables by calculating the marginal effect for each household and then averaging them across all households. To be consistent with results presented later in this section, standard

[16] I use a probit model here because the mean of the dependent variable is far from 0.5. A linear probability model (LPM) may be biased when the dependent variable is close to zero or one, and will produce predictions beyond the range of zero to one. Results shown later suggest that both probit and LPM generate similar estimation results.

[17] For the first wave in 1992, HRS asked whether the individual was hospitalized in the past year. From the second wave on, HRS asked whether the individual was hospitalized since the last interview.

errors shown in parenthesis are bootstrapped by 500 random draws with replacement. I implement a block-bootstrap scheme to make certain that observations are clustered at state level in estimating standard errors.[18]

The estimated effect of property taxes is positive but insignificant both statistically and economically. The magnitude suggests that a \$100 increase in property taxes is associated with a mere 0.0065 percentage points increase in two-year mobility rates. It is unsurprising that the probit estimate of β_1 is small and insignificant since property taxes are likely to be endogenous to elderly homeowners' moving decisions. For instance, if there exists heterogeneity among elderly homeowners in their tastes for local public services, then homeowners who desire good local public services may choose to stay in areas that provide excellent local public services. Since such services are financed partially by property taxes, homeowners in these areas also pay high property taxes. Therefore, the unobserved tastes for local public services are correlated with both property tax payments and mobility outcomes, which causes the probit estimate of β_1 to be biased. An appropriate instrumental variable strategy has to be used to circumvent such an endogeneity problem and to generate an consistent estimate of β_1.

Since eligibility for higher relief benefits means lower property tax payments, one potential candidate as an instrument for property taxes is eligible benefits for property

[18] Bertrand, Duflo and Mullainathan (2004) point out that estimated standard errors would be too small without recognizing such correlations in regression analysis. Such underestimated standard errors often lead to incorrect rejections of the null hypothesis. Two sources of correlation exist in the data studied in this paper: the correlation between observations of the same household over time and the correlation between different households in the same state. Because some households move across state borders, clustering at the state level alone may not produce consistent estimates of standard errors. Without imposing an arbitrary and restrictive structure on the variance-covariance matrix of the error term, I experimented with a multi-way clustering method suggested by Colin, Gelbach and Miller (2006). In practice, the standard errors estimated using multi-way clustering turn out to be almost identical to the standard errors estimated by clustering only at the state level. Given that implementing the multi-way clustering method in a bootstrapping framework is very computationally demanding and that the multi-way clustering method does not seem to produce any noticeable differences, I cluster standard errors only at the state level in all results presented in this paper.

tax relief programs. Recall that my property tax relief program Benefit Calculator imputes $Benefit_{ist}$ (i.e. the amount of eligible benefits from homestead exemptions, homestead credits, and circuit-breakers) for each household in each survey year. $Benefit_{ist}$, however, is a nonlinear function of state, age, year, household income, house value, marital status, Social Security benefits, pension benefits, household size, and total assets. To the extent that any of these factors influences elderly homeowners' moving decisions through channels other than property taxes, $Benefit_{ist}$ would correlate with both property taxes and unobserved moving tendencies, and hence, would violate the exclusion restriction. For example, homeowners who receive high Social Security benefits and pension benefits may have strong ties with local labor markets, which reduces their moving probabilities. In other words, $Benefit_{ist}$ has two sources of variation: the variation caused by relief program rules and the variation stemming from individual characteristics. The latter source of variation may be endogenous and cause probit estimates to be biased. To deal with such an endogeneity problem, I use a simulated IV approach.[19]

To simulate program generosity in state s in year t for homeowners of age a, I take the national sample of homeowners of age a who responded to HRS in year t and run them through state s's relief programs. The weighted average eligible benefits for these homeowners becomes the simulated measure of program generosity for state s in year t for homeowners of age a. Essentially, I measure state program generosity using a national representative sample that does not correlate with any individual homeowner's characteristics, but only with the exogenous variation in state, age, and

[19]The idea of simulated IV can be dated back to Hausman and Wise (1976), Rosen (1976), and Hausman (1981) in labor supply studies. Currie and Gruber (1996a, b) and Cutler and Gruber (1996) build on this idea and name it the "simulated IV approach". Since then, this empirical strategy has become increasingly popular among empirical studies. Hoxby and Kuziemko (2004) and Engelhardt and Kumar (2007) are recent applications of the simulated IV approach.

year. Mathematically, $\widetilde{Benefit}_{ist}$ is constructed as follows:

$$\widetilde{Benefit}_{ist} = \frac{\sum\limits_{k \neq i} \mathbf{B}^{st}(W_{kt}, Z_{kt})\mathbf{1}(Z_{kt} = Z_{it})}{\sum\limits_{k \neq i} \mathbf{1}(Z_{kt} = Z_{it})} \tag{2.2}$$

where Z_{kt} is the age of individual k at time t. W_{kt} consists of relief program eligibility determinants, some of which may be endogenous. $\mathbf{B}^{st}(\cdot)$ is the benefit formula specific to state s at time t. $\mathbf{1}(\cdot)$ is a binary function that returns one if the statement in the parentheses is true and zero otherwise. The above equation essentially takes everyone who shares the same age as individual i at time t, calculates their eligible benefits assuming that they all live in the state where individual i lives, and averages eligible benefits across all these people. To improve small sample properties, I exclude individual i when calculating $\widetilde{Benefits}_{ist}$.

$\widetilde{Benefits}_{ist}$ isolates the exogenous variation due to relief program rules from the potentially endogenous variation due to individual homeowners' socio-economic characteristics. Comparing column (2) with column (4) of Table 2.4 illustrates this point. Even though the conditional average benefit calculated using individual characteristics has no clear relationship with age, the simulated conditional average benefit increases monotonically in age, reflecting the fact that property tax relief programs are more generous for the oldest homeowners. In addition, column (2) shows that the conditional average benefit calculated using individual characteristics decreases monotonically in income and increases monotonically in house value. In contrast, the simulated conditional average benefit does not exhibit any relationship with either income or house value, suggesting that the simulated benefit measure $\widetilde{Benefits}_{ist}$ is rid of variations stemming from individual characteristics. In summary, the simulated benefits contain only the variation in program rules and depend exclusively on state, age, and year by construction. Even though a homeowner's unobserved tendencies to move can be correlated with factors that determine his benefit eligibility, such unobserved tendencies to move are orthogonal to relief program rules and thus, orthogonal to simulated benefits.

The second map in Figure 2-2 plots simulated conditional average benefits by state. South Dakota and Utah are missing from the map because no homeowners in the sample are from these two states. Comparing the two maps in Figure 2-2, we observe that simulated eligible benefits are highly correlated with eligible benefits calculated using individual characteristics. Nevertheless, they are noticeably different from each other. For example, the conditional average benefits based on individual characteristics are higher in Colorado than in Minnesota and North Dakota, while the simulated conditional average benefits are higher in Minnesota and North Dakota than in Colorado. $Benefit_{ist}$ is measured using residents in state s, whereas $\widetilde{Benefit}_{ist}$ is measured using a national representative sample. To the extent that residents in state s is different from the national representative sample, it is unsurprising for the two maps in Figure 2-2 to exhibit different patterns.

I also use two other relief benefit eligibility measures, $ValueFreeze_{ist}$ (i.e. whether eligible for an assessment value freeze program) and $TaxFreeze_{ist}$ (i.e. whether eligible for a property tax freeze program) to instrument for property tax payments. Because eligibilities for assessment value freeze programs and property tax freeze programs depend only on state, age, year, and household income, and because state, age, year, and household income are arguably exogenous covariates, $ValueFreeze_{ist}$ and $TaxFreeze_{ist}$ satisfy the exclusion restriction and may be used as valid instruments for property tax payments.[20]

To implement the simulated IV strategy in a probit framework, I use the two-step estimator suggested by Rivers and Vuong (1988).[21] Beside computational ease, the

[20] I also tried simulating $ValueFreeze_{ist}$ and $TaxFreeze_{ist}$ and using the simulated eligibility measures as instruments for property taxes. The estimation results remain the same.

[21] The Rivers-Vuong two-step approach is a limited information procedure. Thus, it is less efficient

Rivers-Vuong two-step IV approach has another appealing feature. The usual probit t-test on $\hat{v}$, which is a consistent estimate of the first-stage error term, is a valid test of the null hypothesis that Tax_{ist} is exogenous. Such a test is equivalent to the Hausman specification test suggested by Hausman (1978). Because I use a two-step procedure to estimate the IV probit model, standard errors need to be adjusted accordingly. I choose to obtain consistent estimates of standard errors by bootstrapping in lieu of the delta-method for two reasons. First, bootstrapping is computationally easier to implement. Second, bootstrapping provides higher-order refinements while the delta-method is only a first-order approximation (Horowitz (2001)).

Column (2) of Table 2.5 shows the IV probit estimation results. The estimated marginal effect of property taxes is both statistically and economically significant. The point estimate suggests that a $100 increase in annual property tax payments induces the two-year mobility rate to increase by 0.76 percentage points. Given that the baseline two-year mobility rate among elderly homeowners is 9%, the IV probit estimate implies that a $100 increase in annual property taxes induces mobility to rise by 8 percent. Moreover, the coefficient on $\hat{v}$ is statistically different from zero at 0.01 level, rejecting the null hypothesis that Tax_{ist} is exogenous and confirming the necessity of an IV strategy.

Table 2.5 also shows that the instruments used here - $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze$ - are quite strong in the first-stage regression. The first-stage F-stat is 43 and the concentration parameter is 126. Stock, Wright and Yogo (2002) suggest that the rule of thumb for detecting weak instruments is to check whether the first-stage F-stat exceeds 10. Hansen, Hausman and Newey (2006) conclude that a concentration parameter of 30 or above suggests that there is no weak instruments problem. By either standard, $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze$ are strong

than the conditional maximum likelihood estimation (MLE). In practice, I find MLE computationally difficult, and iterations do not converge.

instruments for Tax_{ist}.

The estimated marginal effects of other covariates are mostly consistent with our expectation and the previous literature's findings. For instance, homeowners who are currently working are less likely to move. Homeowners who are recently widowed have higher moving probabilities. Large families are less prone to move, supposedly due to high moving costs. Negative health shocks and number of living children are associated with higher mobility rates, which echoes the finding by Silverstein and Angelelli (1998) that older parents engage in return migration in order to live closer to children from whom they receive care.

Allowing for Household Heterogeneity - Random Effects Probit Model

An advantage of using panel data is that we can take into account unobserved individual heterogeneity. To simplify notations, I denote y_{it} as the mobility outcome, $\mathbf{x}_{it}$ as a vector of all explanatory variables, and c_i as the time-constant unobserved household effects. The probit model assumes

$$P(y_{it} = 1|\mathbf{x}_{it}, c_i) = \Phi(\mathbf{x}_{it}\theta + c_i), \qquad t = 1, ..., T$$

The density of $(y_{i1}, ..., y_{iT})$ can be written as

$$\begin{aligned} f(y_{i1}, ..., y_{iT}|\mathbf{x}_i, c_i; \theta) &= \prod_{t=1}^{T} f(y_{it}|\mathbf{x}_{it}, c_i; \theta) \\ &= \prod_{t=1}^{T} \Phi(\mathbf{x}_{it}\theta + c_i)^{y_{it}}[1 - \Phi(\mathbf{x}_{it}\theta + c_i)]^{1-y_{it}} \end{aligned}$$

Ideally, we would prefer estimating a fixed effects probit model where no assumption is made about the distribution of c_i, and c_i are estimated as parameters along with θ. Unfortunately, the data used in this paper have a small T and a large N. The fixed effects probit model fails to produce a consistent estimate of θ due to the incidental parameters problem. Nevertheless, if we are willing to assume the conditional distribution of c_i, we can still estimate a random effects probit model.

The traditional random effects probit model imposes the assumption that c_i are normally distributed conditional on $\mathbf{x}_i$:

$$c_i|\mathbf{x}_i \sim \text{Normal}(0, \sigma_c^2)$$

Because $\{c_i\}$ are unobserved, they cannot appear in the likelihood function explicitly. Instead, we find the joint distribution of $(y_{i1}, ..., y_{iT})$ by integrating out c_i. Under the conditional normality assumption of c_i,

$$\begin{aligned} f(y_{i1}, ..., y_{iT}|\mathbf{x}_i; \theta, \sigma_c) &= \int_{-\infty}^{\infty} \left[\prod_{t=1}^{T} f(y_{it}|\mathbf{x}_{it}, c_i, \theta) \right] \frac{1}{\sigma_c} \phi\left(\frac{c}{\sigma_c}\right) dc \\ &= \int_{-\infty}^{\infty} \left[\prod_{t=1}^{T} \Phi(\mathbf{x}_{it}\theta + c_i)^{y_{it}} [1 - \Phi(\mathbf{x}_{it}\theta + c_i)]^{1-y_{it}} \right] \frac{1}{\sigma_c} \phi\left(\frac{c}{\sigma_c}\right) dc \end{aligned}$$

The log-likelihood function for the entire sample of N households can be maximized with respect to θ and σ_c.

Columns (3) and (4) in Table 2.6 present the estimation results of the random effects probit model and that of the two-step IV random effects probit model. The marginal effects are very close to those estimated by the probit and IV probit models. The point estimate of the IV random effects probit model suggests that a \$100 increase in annual property tax payments causes the two-year mobility rate to go up by 0.73 percentage points. The estimated coefficient on $\hat{v}$ is statistically different from zero at 0.01 level, suggesting that Tax_{ist} is endogenous to mobility outcomes. As a robustness check, OLS and 2SLS estimation results are shown in columns (5) and (6). The LPM also generates similar results.

Robustness Checks across Sub-samples and Specifications

In this section, I estimate the effect of property taxes on elderly mobility using different sub-samples and different model specifications. The purpose of this exercise is to check whether my findings are driven by some peculiar sub-population and whether the estimated results are robust to various regression specifications. I first exclude the AHEAD cohort. The AHEAD cohort were born before 1924 and are the oldest cohort in the sample. Even though I have included in the main specification a full set of age dummies and year dummies, which effectively controls for cohort fixed effects, coefficients on all covariates have been restricted to be the same across cohorts. If the oldest elderly move for reasons completely different from those of the younger elderly, they may respond to property taxes and relief programs distinctively from other cohorts. The regression results excluding the AHEAD cohort are shown in columns (3) and (4) of panel A in Table 2.7. The IV probit point estimate is similar to the one obtained when the AHEAD cohort is included.

Next, I drop households living in California because Proposition 13 creates a very unusual institutional setting. Proposition 13 in California was adopted in 1978. It limits property tax rate at 1% and requires assessment values grow no more than 2% per year unless the house is sold and re-assessment is carried out. Wasi and White (2005) find that Proposition 13 has a lock-in effect on homeowners in California. In the late 1980s, two amendments to Proposition 13 were passed. They allow any homeowner of age 55 or above who move to another house of equal or less market value within the same county to pay property taxes on the previous house' assessment value. Ferreira (2005) use a regression discontinuity strategy to show that mobility rates of 55-year-old homeowners are 25% higher than those of 54-year-old homeowners in California after those amendments were enacted. Proposition 13 may cause elderly homeowners in California to respond differently to property taxes and relief programs than elderly homeowners in other states. The regression results without California observations are shown in columns (5) and (6) of panel A in Table 2.7. They appear to be very similar to the results obtained for the entire sample.

I then investigate whether the results I find are driven by a small fraction of households who made multiple moves during the sample period. Specifically, I drop households who moved three or more times between 1992 and 2004.[22] Columns (7) and (8) of panel A in Table 2.7 present the probit and IV probit estimation results. The reported marginal effects remain unchanged, suggesting that the estimated marginal effect of property taxes on elderly mobility is not driven by frequent movers.

By construction, the variation in the simulated benefits comes from state, age, and year. To ascertain that the results I have found in my main regressions do not originate from uncontrolled two-way interactions between state, age and year, I add two-way interaction fixed effects in the property tax regression. The first two columns of panel B in Table 2.7 display the original estimation results without controlling for any two-way interactions. In columns (3) and (4), I add controls for state×year fixed effects. In columns (5) and (6), I add controls for year×age fixed effects. In columns (7) and (8), I add controls for state×age fixed effects.

In the case of adding state×age fixed effects, around 1,500 additional fixed effects are controlled for in the regression model. Not surprisingly, estimated standard error becomes considerably larger and the marginal effect is significant only at 0.10 level. Nevertheless, the estimated coefficient on property tax payments always remains positive and of roughly the same magnitude.

In summary, estimation results from the probit model, the random effects probit model, and the LPM all suggest that property taxes play an important role in elderly homeowners' moving decisions. The results do not appear to be driven by some

[22] I am aware that this procedure selects the sample based on the dependent variable. The sole purpose of this exercise is to check whether the estimated marginal effect of property taxes on elderly mobility changes significantly once we exclude frequent movers.

peculiar sub-sample and are robust to adding two-way interaction fixed effects. These estimation results also demonstrate that it is essential to recognize that property taxes are endogenous to the probability of moving. The central IV estimate suggests that a \$100 increase in property taxes induces the two-year mobility rate to rise by 0.76 percentage points, representing a 8% increase from the baseline average two-year mobility rate of 9 percent. Such a large estimated impact of property taxes may be a manifestation of the local average treatment effects (LATE) formulated by Imbens and Angrist (1994). The instruments used to identify the causal effect of property taxes - simulated benefits, eligibility for assessment value freeze program, and eligibility for property tax freeze program - affect property taxes only of homeowners who both are eligible for and actually take up property tax relief. Because property tax relief programs are designed to assist low-income and elderly homeowners, and because people who actually take up these programs tend to be more sensitive to property taxes, it is not surprising that the IV estimates are large in magnitude. The estimates shown here measure the mobility response to property taxes among the compliers (i.e. eligible homeowners who actually receive benefits from property tax relief programs), so one must be cautious when generalizing these results to the overall population.

2.4.2 Using Variations in Housing Appreciation Rates

In the previous section, I used eligible benefits for property tax relief programs as instruments and find that higher property taxes induce elderly homeowners to move. To the extent that these instruments affect property tax payments only of homeowners who are both eligible for and actually enroll in property tax relief programs, the effect found may be specific to such homeowners. In this section, I use a different empirical strategy to identify the effect of property taxes on mobility rates of all elderly homeowners. Specifically, I make use of the intuition that homeowners living in places with high Effective Tax Rates (i.e. the property tax payment to house value ratio) and high housing appreciation rates will be the most affected by rising property taxes. If increasing property taxes induce elderly homeowners to move, individuals subject to high $ETR_{ist} \times h_{mt}$ are more likely to move than those subject to low $ETR_{ist} \times h_{mt}$, where h_{mt} is the housing appreciation rate for Metropolitan Statistical Area (MSA) m at time t.[23] In particular, I estimate the following probit model:

$$\text{Prob}(Move_{ist} = 1) = \Phi(\gamma_1 ETR_{ist} \times h_{mt} + \gamma_2 ETR_{ist} + \gamma_3 h_{mt} + \mathbf{X}_{ist}\mathbf{\Pi} + \alpha_s + \delta_t) \quad (2.3)$$

If property taxes are important in elderly homeowners' moving decisions, then we would expect γ_1 to be positive. The identification assumption underlying this difference-in-differences framework is that, in the absence of a property tax effect, increases in housing prices have the same effect on low-ETR areas as on high-ETR areas. This assumption is very strong because it assumes that ETR varies across places for reasons exogenous to individual homeowners. It precludes situations where increases in property tax revenues are driven purely by homeowners' demand for more and better local services.

Note that individual level variables ETR_{ist} may be endogenous for the same reasons that property tax payments may be endogenous. Hence, estimates of equation (2.3) are inconsistent without an appropriate instrumental variable strategy. I use state-year level median effective tax rates ETR_{st}, and the interaction between ETR_{st} and h_{mt} to instrument for their individual level counterparts.[24] The regression out-

[23]For homeowners who do not live in MSAs, I use state-level housing appreciation rates instead. About 75% of the sample live in MSAs.

[24]The housing appreciation rates h_{mt} are from the Office of Federal Housing Enterprise Oversight (OFHEO). OFHEO publishes quarterly House Price Index (HPI) by state and by MSA. I adjust HPI for inflation using CPI to obtain real housing appreciation rates h_{mt}. The data I used to construct state level median effective tax rates ETR_{st} are from two sources. The first is the 2000 Census, which provides median effective tax rate by state. The second is the "Residential Property Tax Rates in the Largest City in Each State" published in the census Statistical Abstract series. Assuming the time trend of ETR in these largest cities coincides with the time trend of median ETR

comes are reported in columns (1) and (2) of Table 2.8.

The results in Table 2.8 show a number of interesting patterns. The estimated marginal effect of $ETR_{ist} \times h_{mt}$ in the IV probit model is positive and statistically significant, suggesting that higher property taxes induces elderly homeowners to relocate.[25] The estimated marginal effect of h_{mt} is negative and marginally significant. The variable h_{mt} measures how much housing values have changed from last year to this year. If homeowners extrapolate housing price movements, they would expect housing value to continue increasing in a housing market boom and to continue declining in a housing market bust. γ_3 being negative implies that in times when housing prices keep rising, homeowners decide to delay moving and cash in the expected capital gains later. The Hausman test appears to reject the exogeneity assumption and suggests that an instrumental variable approach is necessary for consistent estimates. In addition, the LPM generates very similar results.

I interpret the estimated marginal effects using the following example: Take ETR_{ist} to be 0.01 and h_{mt} to be 5% as the benchmark scenario. These numbers are chosen because they are close to the sample medians. Consider an increase in annual housing appreciation rate from 5% to 10%. The direct housing appreciation effect, $\gamma_3 h_{mt}$, implies a decrease in the two-year mobility rate by 1.3 ($= -0.262 \times 0.05$) percentage points. The indirect property-tax-increase effect, $\gamma_1 ETR_{ist} \times h_{mt}$, suggests a rise in the mobility rate by 1.2 ($= 0.235 \times 0.05 \times 0.01 \times 100$) percentage points. Therefore, the net effect of an increase in housing appreciation rate from 5% to 10% is a decrease of 0.1 ($= -1.3 - 1.2$) percentage points in the two-year mobility rate. Now take ETR_{ist} to be 0.02 and consider the same increase in h_{mt} from 5% to 10%. The direct housing appreciation effect is still a 1.3 percentage point decrease in the two-year mobility rate. The indirect property-tax-increase effect, however, will in-

in states, I use the 2000 Census data as a baseline and construct the state-year specific ETR_{st}.

[25] I multiplied the ETR_{ist} and ETR_{st} by 100 to obtain easy-to-read estimated coefficients.

crease mobility by 2.4 ($= 0.235 \times 0.05 \times 0.02 \times 100$) percentage points. Therefore, the net effect is a 1.1 ($= -1.3 + 2.4$) percentage point increase in the two-year mobility rate.

The IV probit estimation results suggest that homeowners living in places with both high effective property tax rates and rapid housing value appreciation are the most affected by rising property taxes. Exploiting variations in housing value appreciation rates rather than in state-provided property tax relief programs, the results shown in this section complement and reinforce the previous finding that property taxes play an important role in elderly homeowners' moving decisions. Furthermore, finding a large and statistically significant effect of $ETR_{ist} \times h_{mt}$ on elderly mobility suggests that increasing demand for local public services cannot fully explain the sharp increases in property taxes during the recent years, at least not for elderly homeowners. If property tax increases are entirely driven by elderly homeowners' demand for more and better local public services, then we would not expect elderly mobility to rise in areas with high ETR and fast housing price appreciation.

2.4.3 Do Liquidity Constraints Matter?

There are two potential explanations for why elderly homeowners move when property taxes increase. The first focuses on liquidity constraints. Under the assumption that elderly homeowners have substantial emotional and psychological attachment to their homes, they do not wish to move unless they become liquidity-constrained and have to trade down. This explanation implies that elderly homeowners do not perceive their housing wealth in the same way they perceive their financial wealth. It also suggests that providing property tax relief programs may be welfare-improving for elderly homeowners with difficulties accessing the credit market and cashing out their housing wealth.

The second explanation is a demand adjustment story. When homeowners were in their prime-age living with school-aged children, they were willing to pay high property taxes and receive local services including public schools. As homeowners grow older, they no longer have school-age children and consequently, they do not value school services as much. When property taxes increase, elderly homeowners may decide to relocate to places with lower property taxes and fewer, or different public services. This explanation suggests that property tax relief programs may have distorted elderly homeowners' mobility decisions and locked in people who optimally should have moved.

The liquidity constraint explanation and demand adjustment explanation have distinct policy implications. Pioneered by Zeldes (1989), a sizable literature shows both theoretically and empirically that liquidity constraints may have a significant impact on household consumption and saving trajectories. If there exist impediments in the credit market that prevents elderly homeowners from borrowing against their housing wealth, providing property tax relief to them may help to smooth consumption and to enhance welfare. On the other hand, if moving from a place with high property taxes to a place with low property taxes gives elderly homeowners higher utility, then property tax relief may artificially lock elderly homeowners into their long-time home. To explore whether liquidity constraints explain the effect of property taxes on elderly mobility, I estimate

$$\begin{aligned} \text{Prob}(Move_{ist}) &= \Phi(\eta_1 Tax_{ist} \times LowIncome_{ist} + \eta_2 Tax_{ist} \times HighIncome_{ist} \\ &+ \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t) \end{aligned} \tag{2.4}$$

where $LowIncome_{ist}$ is an indicator variable that equals one if household i is in the bottom income quintile at time t, and $HighIncome_{ist}$ indicates whether household i is in the top four income quintiles at time t. If liquidity constraints are the reason why higher property taxes induce elderly homeowners to move, then we would expect that low-income individuals are more affected by property taxes. Thus, we would expect $\eta_1 > \eta_2$. Using eligible benefits for property tax relief programs as instruments for $Tax_{ist} \times LowIncome_{ist}$ and $Tax_{ist} \times HighIncome_{ist}$, I find that for households in the bottom income quintile, a \$100 increase in property taxes induces the two-year mobility rate to increase by 1.25 percentage points. In contrast, a \$100 increase in property taxes induces the two-year mobility rate to increase by only 0.61 percentage points. Unfortunately, standard errors of the estimated marginal effects are large and we cannot reject the null hypothesis that $\eta_1 \leq \eta_2$. Nevertheless, the estimation results provide suggestive evidence that liquidity constraints may have played a role in property taxes' impact on elderly homeowners' moving decisions.

2.5 Program Evaluations

In this section, I investigate policy aspects of property tax relief programs. More specifically, I examine how program types, program generosity, and implementation strategies affect the impact of property tax relief programs on elderly mobility. Recall that there are three categories of property tax relief programs: homestead exemptions, homestead credits, and circuit-breakers; assessment value freeze programs; and property tax freeze programs. To investigate whether different types of programs have different impact on elderly mobility, I estimate

$$\begin{aligned}\text{Prob}(Move_{ist} = 1) = \Phi(&\alpha_1 Benefit_{ist} + \alpha_2 ValueFreeze_{ist} + \alpha_3 TaxFreeze_{ist} \\ &+\mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t) \qquad (2.5)\end{aligned}$$

where $Benefit_{ist}$, $ValueFreeze_{ist}$, $TaxFreeze_{ist}$ are measures of eligibility for the three categories of property tax relief programs. If property tax relief programs reduce elderly mobility, We expect α_1, α_2, and α_3 to be all negative. Larger magnitudes of these coefficients suggest stronger impact of the corresponding programs.

Since $Benefit_{ist}$ contains variations in a range of individual characteristics and some of these characteristics may be correlated with unobserved tendencies to move, I use simulated benefits $\widetilde{Benefit}_{ist}$ to instrument for it. The estimation results are presented in panel A of Table 2.9. The estimated marginal effects of all program types are negative, suggesting that eligibility for property tax relief programs reduces mobility. The IV probit point estimates suggest that a \$100 increase in eligible benefits for homestead exemptions, homestead credits, and circuit-breakers decreases the two-year mobility rate by almost 1 percentage point. The eligibility for assessment value freeze programs reduces the two-year mobility rate by 0.7 percentage points, and the eligibility for property tax freeze programs reduces the two-year mobility rate by 2 percentage points. Panel A of Table 2.9 also shows that across specifications, the marginal effect of eligibility for property tax freeze programs is statistically significant at 0.01 level. Overall, property tax freeze programs appear to have the largest impact on elderly homeowners' moving decisions.

Policy-makers may ask whether, given a policy objective of reducing mobility among elderly homeowners in the face of rising property taxes, should they give everyone at least some benefits or should they give a great deal of benefits to a well-targeted group? Knowing whether eligibility or benefit level is more effective in decreasing mobility can help us design more cost-effective property tax relief programs. To capture the difference between eligibility and benefit level, I focus on two variables: an indicator variable that equals one if the individual is eligible for a positive amount of benefits, and a continuous variable measuring the dollar amount of eligible benefits conditional on eligibility. More specifically, I estimate

$$\text{Prob}(Move_{ist} = 1) = \Phi\big(\rho_1 \mathbf{1}(Benefit_{ist} > 0) + \rho_2 \mathbf{1}(Benefit_{ist} > 0) \times Benefit_{ist}$$
$$+\rho_3 ValueFreeze_{ist} + \rho_4 TaxFreeze_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t\big) \tag{2.6}$$

The estimated marginal effects are shown in panel B of Table 2.9. Unfortunately, the data do not have enough statistical power to identify the effect of eligibility and benefit level precisely. The estimated marginal effect of eligibility appears to be positive and small in magnitude; the estimated marginal effect of conditional benefits appears to be negative and large in magnitude, although neither is statistically significant. The data seem to suggest that the amount of benefits has to reach a critical level in order to have any noticeable effects on mobility.

Property tax relief programs in the U.S. are implemented in three ways: directly incorporated in property tax bills, a rebate check sent to the homeowners after the bill is paid, or income tax credits on state personal income tax returns. Anecdotal evidence seems to suggest that homeowners may not perceive a relief program that is implemented by income tax credits as *property tax* reliefs. One possible explanation for such perception is that income tax returns are filed at a time distant from the time when property taxes are paid. In contrast, programs implemented by direct incorporation in property tax bills provide immediate relief. Homeowners can typically file rebate applications and receive their rebate checks right after they pay tax bills. Another potential explanation is that homeowners may perceive such income tax credits as *income tax* reliefs rather than *property tax* reliefs.

If relief granted in the form of income tax credits is not perceived by beneficiaries as property tax relief, then the negative correlation between simulated benefits and reported property taxes may disappear. Therefore, simulated benefits cannot be used as valid instruments in regression analysis due to the lack of a first-stage relationship.

To avoid such a problem, I have excluded states that use income tax credits to provide property tax relief in my estimations so far. These states are District of Columbia, Massachusetts, Michigan, Missouri, Montana, New Jersey, New Mexico, New York, Oklahoma, Rhode Island, Vermont and Wisconsin.

To test whether implementation strategies affect property tax relief programs' effect on elderly mobility, I estimate

$$\begin{aligned}\text{Prob}(Move_{ist} = 1) = \Phi\big(&\lambda_1 ITC_s \times Benefit_{ist} + \lambda_2(1 - ITC_s) \times Benefit_{ist} \\ &+\lambda_3 ValueFreeze_{ist} + \lambda_4 TaxFreeze_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t\big)\end{aligned} \tag{2.7}$$

where ITC_s is an indicator variable that equals one if state s has property tax relief programs implemented by income tax credits. If implementation strategy does not matter, we would expect that both λ_1 and λ_2 to be negative and of roughly the same magnitude.

The estimation results are shown in panel C of Table 2.9. Instrumenting $ITC_s \times Benefit_{ist}$ and $(1 - ITC_s) \times Benefit_{ist}$ using their simulated counterparts, I find that higher eligible benefits implemented by income tax credits are associated with higher mobility rates, and higher eligible benefits implemented in ways other than income tax credits are associated with lower mobility rates. Although the estimated marginal effects are not statistically significant at conventional levels, these results suggest that implementation methods of relief programs have significant influence on people's moving behaviors. This finding echoes the recent literature on tax salience such as Chetty, Looney and Kroft (2007) and Finkelstein (2007). If reducing mobility among elderly homeowners at times of rising property taxes is the policy objective, then using state personal income tax credits to provide property tax reliefs does not appear to be very effective.

2.6 Conclusion

Property taxes are the most important tax revenue source for local governments. The recent housing market boom led to significant increases in homeowners' property tax liabilities. Both policy-makers and the general public are concerned by the prospect that house-rich but income-poor elderly homeowners are overburdened by rising property taxes. The goal of this paper is to provide empirical evidence on whether property taxes play an important role in elderly homeowners' moving decisions.

Using instrumental variable approaches, this paper finds that property taxes are important in elderly homeowners' moving decisions. The central point estimates suggest that a \$100 increase in annual property taxes leads to a 0.76 percentage point increase on average in two-year mobility rates. The median annual property tax payment in my sample is \$1200, and the average two-year mobility rate of elderly homeowners is 9 percent. My point estimates suggest that the impact of property taxes on elderly mobility is economically significant. In addition, the effect of property taxes is most pronounced for homeowners living in areas that rely heavily on property taxes and that experience remarkable housing value appreciation. A variety of robustness checks are performed to test the stability of the found effects. Moreover, eligibility for property tax relief programs reduces the probability of moving. In designing property tax relief programs, program types, program generosity, and program implementation all need to be considered to achieve policy objectives. For instance, targeting a small population of homeowners in great need of assistance seems more effective than broadening eligibility. Property tax freezes, although costly, reduce mobility the most. Benefits granted through state personal income tax credits are not perceived by homeowners as property tax relief and do not appear to reduce mobility among the eligible homeowners.

According to the 2004 data, property tax relief programs cost about \$10 billion a year in the United States.[26] In some states, these relief programs are provided at a great expense of lost revenues. For example, circuit-breakers in Vermont cost about 10% of total property tax revenues every year. Is the money well spent? Does the benefit of having these programs justify their cost? If the effect of property taxes on mobility is driven by demand adjustment, property tax relief programs would keep elderly homeowners from relocating to places where the marginal price of local services matches the marginal benefit. Thus, we essentially spend valuable resources locking homeowners in their houses and preventing them from following an optimal housing consumption path. In contrast, if the effect is due to liquidity constraints, providing generous property tax relief programs would alleviate such constraints and allow people who value the house the most to stay in it. This paper offers suggestive evidence that liquidity constraints may have been a driving force behind the effect of increasing property taxes on elderly mobility, but we need to conduct more empirical studies before assessing the welfare implication of property tax relief programs.

Many intriguing and important questions remain unexplored with regard to the mobility decision of the elderly. At present, little empirical analysis has been conducted to address the question why effective tax rates did not decline in proportion to the increases in housing prices. Dye and Reschovsky (2007) suggests that cuts in state school aid caused by state fiscal crisis may be partially responsible for rising property taxes in recent years. Furthermore, virtually no evidence has been presented to rationalize the prevalence of property tax relief programs and the political popularity associated with expanding these programs. Do elderly homeowners enjoy more political power by voting more often than non-elderly homeowners? Do state and local governments use property tax relief programs to entice retiree migrants? Do population aging and baby-boomers' entering retirement age imply diminishing support for public-school spending? Poterba (1998) discusses issues related to demo-

[26] Author's estimation using 2004 circuit-breaker cost data reported in Lyons et al (2007).

graphic change and the political economy of public education. More research is called for to address such questions.

The second set of research questions includes investigating the best way of providing property tax relief. Economists believe that reverse mortgages are an efficient mechanism for elderly homeowners to tap into their housing wealth and to achieve consumption smoothing toward the end of their life-cycle. The fact that very few eligible elderly homeowners take up property tax deferral programs is consistent with the observation that the reverse mortgage market in the U.S. is tiny. Studying why property tax deferral programs are unpopular among elderly homeowners could help us better understand how elderly perceive their housing wealth and whether the absence of a thriving reverse mortgage market is due to a lack of demand. Given the similarities between reverse mortgages and annuity products, such research endeavors will also contribute to our knowledge on the demand for annuity products among the elderly.

The third set of questions extends beyond mobility. If rising property taxes strain the elderly, there may be other interesting behavioral responses. I am studying the impact of property taxes on elderly homeowners' labor supply and retirement decisions to explore the mechanism through which rising property taxes induce elderly homeowners mobility and asset decummulation decisions.

Bibliography

[1] "Property Tax Circuit-Breakers: Current Status and Policy Issues." U.S. Advisory Commission on Intergovernmental Relations Information Report, February 1975.

[2] "Significant Features of Fiscal Federalism." U.S. Advisory Commission on Intergovernmental Relations. various years.

[3] Baer, David. "The State Economic, Demographic and Fiscal Handbook." AARP Public Policy Institute, 1996, 1998, 2000, 2003 and 2006.

[4] Baer, David. "Awareness and Popularity of Property tax Relief Programs." AARP Public Policy Institute, Feburary 1998.

[5] Bertrand, Marianne, Esther Duflo and Sendhil Mullainathan. "How Much Should We Trust Difference in Differeces Estimates?" *Quarterly Journal of Economics* 119 (1) (2004) 249-275.

[6] Bradley, David. "Property Taxes in Perspective." Center on Budget and Policy Priorities, March 2005.

[7] Cebula, Richard. "Interstate Migration and the Tiebout Hypothesis: An Analysis According to Race, Sex and Age." *Journal of American Statistical Association* 69 (1974) 876-879.

[8] Chetty, Raj, Adam Looney and Kory Kroft. "Salience and Taxation: Theory and Evidence." Working Paper 2007.

[9] Clark, David and William Hunter. "The Impact of Economic Opportunity, Amenities and Fiscal Factors on Age-Specific Migration Rates." *Journal of Regional Science* 32 (1992) 349-365.

[10] Cameron, Colin, Jonah Gelbach and Douglas Miller. "Robust Inference with Multi-Way Clustering." Working Paper, 2006.

[11] Conway, Karen and Andrew Houtenville. "Elderly Migration and State Fiscal Policy: Evidence from the 1990 Census Migration Flows." *National Tax Journal* 54 (2001) 103-123.

[12] Currie, Janet and Jonathan Gruber. "Health Insurance Eligibility, Utilization of Medical Care, and Child Health." *Quarterly Journal of Economics*, vol 111 (2) (1996) 431-466.

[13] Currie, Janet and Jonathan Gruber. "Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women." *Journal of Political Economy* 104 (6) (1996) 1263-1296.

[14] Cutler, David and Jonathan Gruber. "Does Public Insurance Crowd Out Private Insurance?" *Quarterly Journal of Economics* 111 (2) (1996) 391-430.

[15] Davidoff, Thomas and Gerd Welke. "Selection and Moral Hazard in the Reverse Mortgage Market." Working Paper, 2007.

[16] Dresher, Katherine. "Local Public Finance and the Residential Location Decisions of the Elderly." Univeristy of Wisconsin dissertation, 1994.

[17] Duncombe, William, Mark Robbins and Douglas Wolf. "Chasing the Elderly: Can State and Local Governments Attract Recent Retirees?" Aging Studies Program Paper, vol. 22. Center for Policy Research, Maxwell School of Citizenship and Public Affairs, Syracuse University, 2003.

[18] Dye, Richard and Andrew Reschovsky. "Property Tax Responses to State Aid Cuts in the Recent Fiscal Crisis." Working Paper, 2007.

[19] Engelhardt, Gary and Anil Kumar. "Pensions and Household Wealth Accumulation." Working Paper, 2007.

[20] Farnham, Martin and Purvi Sevak. "State Fiscal Institutions and Empty-Nest Migration: Are Tiebout Voters Hobbled?" *Journal of Public Economics* 90 (2006) 407-427.

[21] Feinstein, Jonathan and Daniel McFadden. "The Dynamics of Housing Demand by the Elderly." *Economics of Aging* edited by David Wise. Chicago, IL: University of Chicago Press.

[22] Ferreira, Fernando Vendramel. "You Can Take It With You: Proposition 13 Tax Benefits, Residential Mobility, and Willingness to Pay for Housing Amenities." Working Paper, 2005.

[23] Finkelstein, Amy. "E-Z Tax: Tax Salience and Tax Rates." Working Paper 2007.

[24] Hausman, Jerry. "Specification Tests in Econometrics." *Econometrica*, Vol. 46, No. 6, 1273-1291, 1978.

[25] Hausman, Jerry. "The Effect of Taxes on Labor Supply." In "How Taxes Affect Economic Behavior," edited by Henry Aaron and Joseph Pechman. Brookings Institution, 1981.

[26] Hausman, Jerry and David Wise. "The Evaluation of Results from Truncated Samples: The New Jersey Income Maintenance Experiment." *Annals of Economic and Social Measurement* 5 1976, 421-446.

[27] Hansen, Christian, Jerry Hausman and Whitney Newey. "Estimation with Many Instrumental Variables." Working Paper, 2006.

[28] Horowitz, Joel. "The Bootstrap." Handbook of Econometrics, Vol 5., edited by J.J. Heckman and E. Leamer, Elsevier Science B.V., 2001, Ch 52, 3159-3228.

[29] Hoxby Caroline and Ilyana Kuziemko. "Robin Hood and His Not-So-Merry Plan." NBER Working Paper No. 10722, 2004.

[30] Imbens, Guido and Joshua Angrist. "Identification and Estimation of Local Average Treatment Effects." *Econometrica*, Vol. 62, No. 2 (March, 1994), 467-475.

[31] Longino, Charles and William Crown. "The Migration of Old Money." *American Demographics* vol 11 iss 10 (1989) 28-31.

[32] Lyons, Karen, Sarah Farkas and Nicholas Johnson. "The Property Tax Circuit Breaker: An Introduction and Survey of Current Programs." Center on Budget and Policy Priorities, March 2007.

[33] Mackey, Scott and Karen Carter. "State Tax Policy and Senior Citizens." Denver, CO: National Conference of State Legislatures, 2nd edition.

[34] "A Guide to Property Taxes: Series." National Conference of State Legislatures, 2005.

[35] Poterba, James. "Taxation and Housing: Old Questions, New Answers." *American Economic Review* Vol. 82, No. 2 (May, 1992)

[36] Poterba, James. "Demographic Change, Intergenerational Linkages, and Public Education." *Americon Economic Review* Vol. 88, No. 2 (May, 1998)

[37] Rivers, Douglas and Quang Vuong. "Limited Information Estimators and Exogeneity Tests for Simultaneous Probit Models." *Journal of Econometrics* 39 (1988) 347-366.

[38] Rosen, Harvey. "Taxes in a Labor Supply Model with Joint Wage-Hours Determination." *Econometrica*, Vol. 44, No. 3 (May, 1976) 485-507.

[39] Seslen, Tracey. "Property Tax Abatement Programs: Does Relief Really Keep Grandma in Her Home?" Ph.D. Dissertation, MIT Economics Department, 2005.

[40] Silverstein, Merril and Joseph Angelelli. "Older Parents' Expectations of Moving Closer to Their Children." *The Journals of Gerontology* vol. 53 iss. 3 (1998) S153-S163.

[41] Stock, James, Jonathan Wright and Motohiro Yogo. "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments." *Journal of Business and Economics Statistics* 20 (4) (2002) 518-529.

[42] Venti, Steven and David Wise. "Aging, Moving, and Housing Wealth." *Economics of Aging* edited by David Wise. Chicago, IL: University of Chicago Press.

[43] Venti, Steven and David Wise. "But They Don't Want to Reduce Housing Equity." *Issues in Economics of Aging* edited by David Wise. Chicago, IL: University of Chicago Press.

[44] Venti, Steven and David Wise. "Aging and Housing Equity: Another Look." NBER Working Paper 8608, 2001.

[45] Wasi, Nada and Michelle White. "Property Tax Limitations and Mobility: The Lock-In Effect of California's Proposition 13." NBER Working Paper 11108, 2005.

[46] Woo, Seokjin. "Tiebout Migration and Retirement of Older Workers." University of Wisconsin dissertation, 2005.

[47] Zeldes, Stephen. "Consumption and Liquidity Constraints: An Empirical Investigation." *Journal of Political Economy* Vol. 97, No. 2 (April, 1989)

Table 2.1: Mobility and Homeownership of HRS Households

A. Mobility Rate Among Homeowners			
	Mean	SE	N
1992-1994	0.063	0.004	3641
1994-1996	0.082	0.005	2764
1996-1998	0.075	0.005	3264
1998-2000	0.092	0.005	3084
2000-2002	0.133	0.006	2851
2002-2004	0.119	0.006	2643

B. Tenure Distribution				
	Homeowner	Renter	Other	N
1992	0.771	0.194	0.035	6726
1994	0.788	0.180	0.031	5999
1996	0.800	0.177	0.024	5712
1998	0.804	0.164	0.032	5432
2000	0.809	0.157	0.034	5071
2002	0.817	0.148	0.035	4826
2004	0.807	0.147	0.046	4645

C. Tenure Transition Matrix				
	To Own(%)	To Rent(%)	To Other(%)	N
From Own	80.66	14.37	4.97	1909
From Rent	21.98	70.68	7.34	1266
From Other	22.87	44.58	32.54	206

Notes: This table refers to HRS cohort (born between 1931 and 1941) households only. Household weights are used.

Table 2.2: Summary Statistics of Key Variables

	Mean	Median	SD
Moved between Waves	0.09		0.29
Fraction of Cross-State Moves	0.17		0.38
Property Tax	1,756	1,200	2,693
House Value	149,811	110,849	179,146
Income	63,776	41,900	102,144
Financial Wealth	126,526	26,000	528,838
Having Mortgage or Home Loan	0.46		0.50
Age	64.5		9.8
Male	0.52		0.50
White	0.91		0.29
Household Size	2.25		1.15
Number of Children	3.06		1.93
Less than High School	0.22		0.41
High School Graduates	0.32		0.47
Some College	0.22		0.42
College Graduates	0.24		0.43
Currently Working	0.43		0.49
Currently Retired	0.48		0.50
Currently Disabled	0.02		0.13
Newly Retired	0.08		0.28
Married	0.66		0.47
Separated or Divorced	0.12		0.32
Widowed	0.20		0.40
Newly Widowed	0.02		0.14
Recently Hospitalized	0.30		0.46

Notes: $N = 29,213$. The sample is restricted to households who were homeowners in the current wave and who have valid data for all variables. Property tax, income, house value, and financial wealth are in 2000 dollars. Household weights are used.

Table 2.3: Property Tax Relief Benefit Formula Examples

State	**Formula Used For a Hypothetical Homeowner** (year=2000, age=65, married, Income=\$20,000, SSB=\$10,000, HV=\$100,000)	**Benefit**
FL	$0.5 \times ETR_{FL} \times \min(HV, 25000)$	\$146
CA	$0.01 \times \min(34000, HV) \times 0.96 \times \frac{34877-\max(Income,8719)}{34877-8719}$	\$186
MI	$\min(1200, \max(0, ETR_{MI} \times HV - 0.035 \times Income))$	\$536
TX	$0.7 \times ETR_{TX} \times \min(HV - \min(HV, 15000), 10000)$	\$119
NY	$0.5 \times ETR_{NY} \times \min(HV - \min(HV, 10000), 40000)$	\$367
IL	$ETR_{IL} \times \min(HV, 6000)$ $+\min\left(700 - 630 \times \frac{\min(Income,14000)}{14000}, \max(0, ETR_{IL} \times HV - 0.035 \times Income)\right)$	\$166
PA	$\min\left(ETR_{PA} \times HV, 500 \times \left(1 - \frac{\max(Income-0.5\times SSB,5500)-5500}{15000-5500}\right)\right)$	\$0
NJ	$250 \times (Income - SSB \leq 10000)$ $+\max(150, \min(750, ETR_{NJ} \times HV - 0.05 \times Income))$	\$1000
GA	$0.5 \times ETR_{GA} \times \min(HV, 10000) + 0.5 \times ETR_{GA} \times \min(HV, 25000)$	\$150
MN	$\min\left(510, 0.5 \times \max\left(0, ETR_{MN} \times HV - \left(0.01 + 0.03 \times \frac{Income}{71700}\right) \times Income\right)\right)$	\$510

Notes: ETR is the state-year specific average effective property tax rate. HV is house value. SSB is Social Security benefits. Benefits shown here refer to eligible benefits from state-provided homestead exemptions, homestead credits, and circuit-breakers.

Table 2.4: Property Tax Relief Program Benefits

	Eligible Benefits		Simulated Benefits	
	Percentage Eligible (1)	Conditional Avg Benefit (2)	Percentage Eligible (3)	Conditional Avg Benefit (4)
		A. By Age Groups		
age<55	5%	363	6%	248
55-59	6%	382	7%	262
60-64	13%	332	14%	274
65-74	45%	313	45%	305
≥75	53%	350	53%	319
		B. By Income Quintiles		
Lowest	55%	359	34%	275
2nd Quintile	33%	338	31%	294
3rd Quintile	18%	330	26%	287
4th Quintile	11%	300	20%	283
Highest	5%	158	16%	281
		C. By House Value Quintiles		
Lowest	34%	187	29%	253
2nd Quintile	25%	272	25%	272
3rd Quintile	23%	380	25%	230
4th Quintile	22%	427	24%	302
Highest	15%	458	23%	285

Notes: All dollar amounts are in 2000 dollars. Eligible benefits are calculated based on individual homeowners' characteristics. Simulated benefits are calculated by simulation for each state-age-year cell. Percentage Eligible is the percentage of homeowners who are eligible for homestead exemptions, homestead credits, or circuit-breakers. Conditional Avg Benefit is the average benefit from homestead exemptions, homestead credits, and circuit-breakers conditional on being eligible for these programs. Household weights are used.

Table 2.5: Effect of Property Taxes on Mobility - Probit Model

	Probit	**IV Probit**
	(1)	(2)
Property Taxes (in 10,000)	0.0065	0.7624***
	(0.0115)	(0.2692)
Male	0.0021	-0.0010
	(0.0060)	(0.0067)
White	0.0458***	0.0385***
	(0.0086)	(0.0101)
Household Size	-0.0105***	-0.0104***
	(0.0026)	(0.0028)
Number of Children	0.0070***	0.0070***
	(0.0013)	(0.0014)
Married	-0.0363	-0.0413
	(0.0370)	(0.0394)
Newly Widowed	0.0391***	0.0453***
	(0.0131)	(0.0140)
Currently Working	-0.0186***	-0.0264***
	(0.0064)	(0.0076)
Newly Retired	0.0236***	0.0129
	(0.0076)	(0.0114)
Spouse Currently Working	-0.0196**	-0.0188*
	(0.0077)	(0.0101)
Spouse Newly Retired	-0.0013	0.0052
	(0.0098)	(0.0109)
Recently Hospitalized	0.0108**	0.0096
	(0.0051)	(0.0060)
Hausman Test		-4.1179***
(Coef on first-stage residual $\hat{v}$)		(1.3602)
First Stage F-Stat		43
Concentration Parameter		126

Notes: The regression model is Prob($Move_{ist} = 1$) = $\Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $N = 22,250$. Other than the variables shown in the table, $\mathbf{X}_{ist}$ also includes income quintile dummies, house value quintile dummies, financial wealth quintile dummies, age dummies, and spouse age dummies. ζ_s is states fixed effects. δ_t is year fixed effects. The simulated benefit measure $\widetilde{Benefit}_{ist}$ and binary variables $ValueFreeze_{ist}$ and $TaxFreeze_{ist}$ are used as instruments in the IV specification. Marginal effects shown are weighted averages across the population. Standard errors are bootstrapped by 500 random draws with replacement clustered at state level. Household weights are used in estimation. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 2.6: Effect of Property Taxes on Mobility - Probit, Random Effect Probit, and LPM

	Probit		RE Probit		LPM	
		IV		IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)
Property Taxes	0.0065	0.7624***	0.0046	0.7260**	0.0068	0.6058**
(in 10,000)	(0.0115)	(0.2692)	(0.0054)	(0.2901)	(0.0095)	(0.2398)
Hausman Test		-4.0079***		-4.9617***		
(Coef on first-stage residual $\hat{v}$)		(1.3602)		(1.7912)		

Notes: The regression model is Prob($Move_{ist} = 1$) $= \Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $N = 22,250$. $\mathbf{X}_{ist}$ includes income quintile dummies, house value quintile dummies, financial wealth quintile dummies, male, white, household size, number of children, whether married, whether newly widowed, education categories, whether currently working, whether newly retired, whether spouse is currently working, whether spouse is newly retired, whether hospitalized, age dummies and spouse age dummies. The simulated benefit measure $\widetilde{Benefit}_{ist}$ and binary variables $ValueFreeze_{ist}$ and $TaxFreeze_{ist}$ are used as instruments in the IV specification. Marginal effects shown are weighted averages across the population. Standard errors in column (1)-(4) are bootstrapped with 500 random draws with replacement clustered at state level. Standard errors in column (5) and (6) are clustered at state level. Household weights are used. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 2.7: Effect of Property Taxes on Mobility - Robustness Checks

	A. Various Sub-Samples							
	Orig. Sample		Drop AHEAD		Drop CA		Drop Freq. Movers	
		IV		IV		IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Property Taxes	0.0065	0.7624***	0.0039	0.7565**	0.0010	0.7697***	0.0084	0.7676***
(in 10,000)	(0.0115)	(0.2692)	(0.0124)	(0.3172)	(0.0114)	(0.2590)	(0.0098)	(0.2670)
N	22,520	22,520	17,082	17,082	19,963	19,963	21,691	21,691
	B. Add Two-Way Interaction Fixed Effects							
	Orig. Specification		Add State*Year		Add Year*Age		Add State*Age	
		IV		IV		IV		IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Property Taxes	0.0065	0.7624***	0.0054	0.5242**	0.0057	0.7726***	0.0091	0.8716*
(in 10,000)	(0.0115)	(0.2692)	(0.0117)	(0.2649)	(0.0120)	(0.2555)	(0.0206)	(0.4570)
N	22,520	22,520	22,520	22,520	22,520	22,520	22,520	22,520

Notes: The regression model is Prob($Move_{ist} = 1$) = $\Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $\mathbf{X}_{ist}$ includes income quintile dummies, house value quintile dummies, financial wealth quintile dummies, male, white, household size, number of children, whether married, whether newly widowed, education categories, whether currently working, whether newly retired, whether spouse is currently working, whether spouse is newly retired, whether hospitalized, age dummies and spouse age dummies. The simulated benefit measure $\widetilde{Benefit}_{ist}$ and binary variables $ValueFreeze_{ist}$ and $TaxFreeze_{ist}$ are used as instruments in the IV specification. Marginal effects shown are weighted averages across the population. Standard errors are bootstrapped by 200 random draws with replacement clustered at state level. Household weights are used in estimation. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 2.8: Interaction between ETR and House Price Appreciation Rates

	Probit (1)	IV Probit (2)	OLS (3)	2SLS (4)
$ETR_{ist} \times h_{mt}$	0.031	0.235**	0.026	0.234**
	0.053	(0.107)	(0.082)	(0.093)
ETR_{ist}	0.181	2.81	0.169	2.93
	(0.303)	(2.70)	(0.276)	(2.90)
h_{mt}	-0.042	-0.262*	-0.036	-0.260**
	(0.087)	(0.137)	(0.142)	(0.102)
Coef on first-stage residual $\hat{v}_1$		-1.67***		
		(0.52)		
Coef on first-stage residual $\hat{v}_2$		-15.5		
		(19.0)		
N	29,102	29,102	29,102	29,102

Notes: The regression model is Prob($Move_{ist}$ = 1) = $\Phi(\gamma_1 ETR_{ist} \times h_{mt} + \gamma_2 ETR_{ist} + \gamma_3 h_{mt} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $\mathbf{X}_{ist}$ includes income quintile dummies, house value quintile dummies, financial wealth quintiles, male, white, household size, number of children, whether married, whether newly widowed, education categories, whether currently working, whether newly retired, whether spouse is currently working, whether spouse is newly retired, whether hospitalized, age dummies and spouse age dummies. h_{mt} is MSA level housing value appreciation rates. State effective tax rate ETR_{st} and the interaction between ETR_{st} and h_{mt} are used as instruments for ETR_{ist} and $ETR_{ist} \times h_{mt}$ in the IV specification of both regression models. Marginal effects shown are weighted averages across the population. Standard errors in column (1) and (2) are bootstrapped by 500 random draws with replacement clustered at state level. Standard errors in column (3) and (4) are clustered at state level. Household weights are used in estimation. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 2.9: Program Evaluation: Effect of Property Tax Relief Programs on Mobility

	Probit (1)	IV Probit (2)	OLS (3)	2SLS (4)
A. Program Types (N=22,520)				
Eligible Benefits (in 10,000)	-0.0671	-1.3048**	-0.0746	-0.9957*
	(0.1753)	(0.5169)	(0.2139)	(0.5761)
Value Freeze Dummy	-0.0132	-0.0101	-0.0085	-0.0059
	(0.0103)	(0.0104)	(0.0099)	(0.0098)
Tax Freeze Dummy	-0.0323***	-0.0290***	-0.0248***	-0.0215***
	(0.0052)	(0.0055)	(0.0071)	(0.0071)
B. Program Generosity (N=22,520)				
Eligibility Dummy	0.0045	0.0145	0.0022	0.0143
	(0.0093)	(0.0474)	(0.0116)	(0.0750)
Benefits Conditional on	-0.1369	-1.6205	-0.1087	-1.4453
Eligibility (in 10,000)	(0.2493)	(2.4595)	(0.3136)	(3.2640)
C. Program Implementation (N=29,210)				
ITC*Eligible Benefits	0.2089	0.3746	0.2193	0.4362*
(in 10,000)	(0.1377)	(0.3460)	(0.1387)	(0.2231)
(1-ITC)*Eligible Benefits	-0.0472	-0.7882	-0.0677	-0.7529
(in 10,000)	(0.1853)	(0.5162)	(0.2164)	(0.6064)

Notes: The regression model in Panel A is $\text{Prob}(Move_{ist} = 1) = \Phi(\alpha_1 Benefit_{ist} + \alpha_2 ValueFreeze_{ist} + \alpha_3 TaxFreeze_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. The regression model in Panel B is $\text{Prob}(Move_{ist} = 1) = \Phi(\rho_1 \mathbf{1}(Benefit_{ist} > 0) + \rho_2 \mathbf{1}(Benefit_{ist} > 0) \times Benefit_{ist} + \rho_3 ValueFreeze_{ist} + \rho_4 TaxFreeze_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. The regression model in Panel C is $\text{Prob}(Move_{ist} = 1) = \Phi(\lambda_1 ITC_s \times Benefit_{ist} + \lambda_2(1 - ITC_s) \times Benefit_{ist} + \lambda_3 ValueFreeze_{ist} + \lambda_4 TaxFreeze_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $\mathbf{X}_{ist}$ includes income quintile dummies, house value quintile dummies, financial wealth quintile dummies, male, white, household size, number of children, whether married, whether newly widowed, education categories, whether currently working, whether newly retired, whether spouse is currently working, whether spouse is newly retired, whether hospitalized, age dummies and spouse age dummies. Simulated measures are used to instrument for eligible benefits in IV specifications. Marginal effects shown in column (1) and (2) are weighted averages across the population. Standard errors in column (1) and (2) are bootstrapped with 500 random draws with replacement clustered at state level. Standard errors in column (3) and (4) are clustered at state level. Household weights are used. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Chapter 3

Property Taxes and Elderly Labor Supply

3.1 Introduction

During the late 1990s and early 2000s, the U.S. housing market experienced a remarkable boom, which led to sharp increases in residential property taxes. The census data indicate that from 2000 to 2005, median housing values went up by 50% and median property taxes rose by 30% in real terms. Anecdotal evidence suggests that such unexpected rises in property taxes may induce elderly homeowners, especially those housing-rich but income-poor elderly homeowners, to increase their labor supply by delaying retirement. Unfortunately, there have been no systematic studies investigating the link between property taxes and elderly labor supply. This paper serves as the first attempt to study this link.

Property taxes may potentially influence elderly labor supply through two channels: wealth effects and liquidity constraints. Economists have long recognized that unexpected changes in wealth may induce individuals to adjust their labor supply. Previous research that employs various data and identification strategies to estimate such wealth effects in general supports the theoretical prediction. Because increases in property taxes are equivalent to declines in wealth, they may lead elderly homeowners to consume less leisure and supply more labor. Property taxes may also cause elderly homeowners to increase their labor supply because of liquidity constraints. For example, according to the 1992-2004 Health and Retirement Study (HRS) sample, , 10% of the homeowners between age 50 and 75 reported paying 9% or more of their income for property taxes. For 25% of these homeowners, annual property tax payments represented at least 40% of household financial assets. The lack of liquid assets among many elderly homeowners make them vulnerable to increases in property taxes. Therefore, they may resort to delaying retirement, reentering the labor force, and/or working longer hours in order to stay in their homes.

On the other hand, property taxes may not have a significant impact on elderly labor supply for two reasons. First, the reduction in wealth caused by rising property taxes may be small relative to elderly homeowners' total wealth. For example, consider a 60 year old who expects to live in his house for another 20 years. Even if annual property taxes increase permanently by $500, it only translates into a $6,731 wealth decline in present discounted value (PDV) assuming a 5% discount rate. Previous studies on wealth effects find that on average, a $100,000 increase in wealth causes retirement rates to decline by 10%. A reduction of several thousand dollars in wealth simply may not be enough to trigger noticeable changes in retirement and labor force reentry behavior. Second, elderly homeowners may respond to rising property taxes by relocating to low-tax areas or by downsizing to smaller houses rather than by increasing labor supply. In fact, Shan (2008) finds evidence suggesting that property taxes raise the mobility rate among elderly homeowners. If elderly homeowners have already lowered their property tax burdens by moving to low-tax areas or smaller houses, it may no longer be necessary for them to increase their labor supply at the same time.

In this paper, I empirically test the relationship between property taxes and elderly labor supply. Specifically, I use panel data from the 1992-2004 Health and Retirement Study (HRS) and newly-collected data on state-provided property tax relief programs to estimate the property tax effect on the labor supply of homeowners aged 50-75. In particular, I focus on three labor supply outcome variables in my regression analysis: retirement, reentry to the labor market, and working hours. Because property tax payments may be endogenous to individuals' labor supply decisions, I exploit the variation in state-provided property tax relief programs and construct simulated relief benefits as instruments for property taxes. Such simulated relief benefits measure the generosity of property tax relief programs and thus, are negatively correlated with property tax payments. The simulation procedure makes sure that these instruments contain only the variation in program rules and depend exclusively on state, age, and year. To the extent that state, age, and year are exogenous, these simulated instruments satisfy the exclusion restriction. The central IV estimation results cannot reject that property taxes have no significant impact on elderly homeowners' decisions to retire, to reenter the labor force, or to increase working hours. Such findings imply that incidences reported in news articles where elderly homeowners have been delaying retirement to keep up with rising property taxes are unlikely to be widespread. Elderly homeowners may have chosen to move rather than to increase labor supply in their effort to reduce property tax burdens.

This paper contributes to the property tax literature and the wealth effect literature in several ways. First, to my knowledge, it is the first study to look at how property taxes affect labor supply. Property taxes are the most important tax revenue source for local governments, and property tax relief programs cost about $10 billion annually in the United States.[1] Studying the behavioral impact of property taxes on elderly homeowners is indispensible for any normative analysis of property taxes and

[1] Author's estimation using 2004 data reported in Lyons, Farkas and Johnson (2007).

property tax relief programs. Second, previous research studying the wealth effect on retirement behavior has exploited variations in Social Security and pension benefits, stock market booms and busts, housing market movements, inheritances, and lottery winnings. This paper complements the existing literature by using property taxes and property tax relief programs as a novel source of variation. Third, while most existing studies focus only on retirement behavior, this paper examines both the extensive margin - whether rising property taxes induce elderly homeowners to delay retirement or reenter the labor force, and the intensive margin - whether elderly homeowners work longer hours when property taxes increase. By looking beyond retirement decisions, this paper provides more comprehensive evidence on how wealth shocks affect elderly labor supply.

The rest of this paper proceeds as follows. Section 2 overviews the existing literature on the wealth effect and introduces the background on property taxes. In section 3, I describe the HRS data used in this paper. I explain my empirical strategy, discuss the estimation results, and present robustness checks and extensions in section 4. The last section concludes and points out some caveats of this paper.

3.2 Background

A sizable literature exists on the wealth effect and retirement behavior. As mentioned above, previous studies have relied on variations in Social Security and pension benefits, stock market movements, housing market movements, inheritances, and lotteries for identification. Earlier works on retirement incentives of Social Security benefits, including Diamond and Hausman (1984), Burtless (1986), Krueger and Pischke (1992), and Blau (1994), generally find that even though the effect of Social Security is statistically significant, it is small relative to the trend toward early labor force exit among older men. More recent works adopt the "option value" approach developed by Stock and Wise (1990) and estimate the dynamic effect of Social Security and pensions on retirement decisions. Samwick (1998), Chan and Stevens (2004), and Coile and Gruber (2007) implement such dynamic models and show that forward-looking incentive measures for Social Security and private pensions are significant determinants of retirement.

The stock market boom and bust as well as the remarkable housing value run-up in recent years have provided researchers arguably exogenous sources of variation for studying the wealth effect on retirement behavior. Using the HRS data, Coronado and Perozek (2003) find that individuals who held corporate equity immediately before the bull market of the 1990s on average retired earlier than those who did not. Sevak (2005) compares individuals with defined contribution pension plans and individuals with defined benefit pension plans. She finds that unexpected gains in wealth during the 1990s bull market induced earlier retirement. Using the HRS, Current Population Survey (CPS), and Survey of Consumer Finances (SCF), Coile and Levine (2006) exploited both the stock market boom in the late 1990s and the stock market bust in the early 2000s to study the impact of wealth shocks on retirement decisions. They find that the stock market has very little influence on aggregate labor market behavior. Farnham and Sevak (2007) and Goodstein (2008) use cross-MSA variation in housing price movements to identify the wealth effect on retirement timing. They find that increases in housing wealth raise the probability of retirement significantly.

In search for exogenous sources of variation to measure the wealth effect, researchers have also estimated the effect of inheritance receipt and lottery winning on labor supply. Brown, Coile and Weisbenner (2006) show that inheritance receipt is associated with a significant increase in the probability of retirement, and the effect is stronger when the inheritance is unexpected. Imbens, Rubin and Sacerdote (2001) use an original survey of people playing the lottery in Massachusetts in the mid-1980s and find that wealth shocks reduce labor supply.

In summary, the existing literature generally supports the theoretical prediction that labor supply responds to wealth shocks. Nevertheless, the evidence shown in the literature is far from conclusive. Studies that employ difference-in-differences frameworks rely heavily on the assumption that in absence of the wealth effect, treatment groups and control groups would have the same propensity to retire conditional on covariates. Such an assumption may be too strong in many cases. Even for studies that have reasonably tight identification strategies, the magnitudes of estimated wealth effects vary considerably from one study to another. As tens of millions of baby-boomers approach retirement age in coming years, the field calls for more research to provide new evidence on this important subject. This paper uses variations in property taxes and property tax relief programs to estimate the wealth effect on elderly labor supply.

Property taxes are responsible for approximately 72% of all local tax revenues, representing the most important tax revenue source for local governments. In 2004, property tax collections in the U.S. exceeded $300 billion.[2] The housing market boom of the late 1990s and early 2000s led to significant increases in residential property taxes. Such steep rises in property taxes may be more burdensome to elderly homeowners than to non-elderly homeowners for two reasons. First, the current U.S. tax system allows taxpayers who choose to itemize their deductions on federal income tax returns to deduct property tax payments. Because mortgage interest payments are usually the main reason for choosing itemized deductions over standard deductions, and because elderly homeowners are likely to have paid off their mortgages and take standard deductions, the marginal cost of paying an extra dollar in property tax is usually higher for elderly homeowners than for non-elderly homeowners. As a result, elderly homeowners may have to increase their labor supply in order to stay in their homes.

[2]See Bradley (2005) and NCSL (2005).

Second, many elderly homeowners do not have substantial liquid assets to cover rising property taxes. Table 3.1 displays the present discounted values (PDV) of hypothetical property tax increases for homeowners at different age and discount rate. For simplicity, I assume away longevity risks and impose that everyone lives to age 80 exactly. The PDV as a percentage of median household financial wealth is shown in parenthesis. The top panel illustrates the case where annual property taxes experience a permanent increase of $300. For example, if the homeowner paid $2,000 for property taxes last year, he would be paying $2,300 for property taxes every year from now on. Even though the PDV of such an increase in property taxes is only a few thousand dollars, it represents 16-47% of the median household financial wealth. The bottom panel of Table 3.1 illustrates the case where property taxes increase by $300 annually. Using the previous example, the homeowner who paid $2,000 for property taxes last year would pay $2,300 this year, $2,600 next year, and so on. The PDV of such an increase in property taxes overwhelms the median household financial wealth. Thus, for elderly homeowners who have little financial wealth and are liquidity constrained, increases in property taxes may induce labor supply responses.

However, there are also reasons to be skeptical that increasing property taxes have generated a large impact on elderly homeowners' labor supply. First, the wealth effect alone may not induce noticeable changes in labor supply behavior. The top panel of Table 3.1 shows that even for a homeowner of age 50 with a low discount rate of 0.02, a hypothetical property tax increase of $300 only amounts to a $7,019 reduction in wealth. Given that previous studies on wealth effects suggest that a $100,000 increase in wealth raises retirement rates by roughly 10%, a one-time permanent increase in property taxes is unlikely to generate much wealth effect on elderly homeowners. Elderly homeowners may respond to rising property taxes by increasing labor supply only if they expect property taxes continue to rise in coming years as illustrated in the bottom panel of Table 3.1.

The second reason why the relationship between property taxes and elderly labor supply may not be empirically detectable is that elderly homeowners have an alternative strategy to reduce their property tax payments, namely, by moving to low-tax area or by downsizing. As noted above, Shan (2008) finds evidence suggesting that rising property taxes induce higher mobility among elderly homeowners. If the disutility from delaying retirement, reentering labor force, or working longer hours outweighs the transaction cost associated with moving and downsizing, we may not find a significant impact of property taxes on elderly labor supply. For these reasons, the theoretical prediction of the degree to which property taxes may affect elderly labor supply is ambiguous, and we have to rely on empirical studies to determine the relationship between property taxes and elderly labor supply.

3.3 Data Description

The data used in this paper has two components: the Health and Retirement Study (HRS) and the newly-collected data on property tax relief programs. HRS is a biannual panel of a nationally representative sample of elderly and near-elderly individuals in the United States. At present, seven waves of the survey (1992-2004) have been released to researchers. HRS includes households from four different cohorts: the HRS cohort (born between 1931 and 1941), the AHEAD cohort (born before 1924), the "Children of the Depression" (CODA) cohort (born between 1924 and 1930), and the "War Baby" (WB) cohort (born between 1942 and 1947).[3] The HRS cohort appear in all seven waves. The AHEAD cohort was first interviewed in 1993 and then in

[3] In 2004, a fifth cohort, Early Boomers (born between 1948 and 1953), was added to HRS. Because households in this cohort have only been interviewed once and I need at least two adjacent surveys to study whether this period's property taxes affect labor supply between this period and the next period, I exclude them from my analysis.

1995. Since 1998, the AHEAD cohort has been interviewed concurrently with the HRS cohort biannually. The CODA and WB cohorts appear only in the last four waves (1998-2004). The raw dataset has 26,867 individuals and 126,104 person-wave observations.

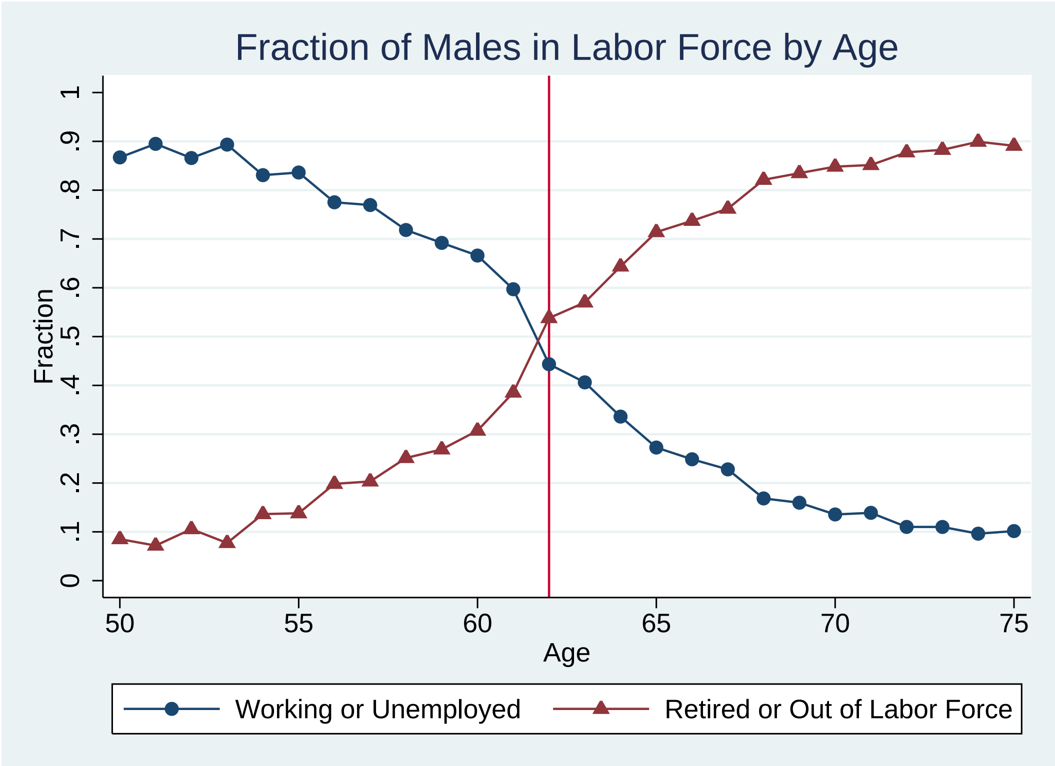
The HRS data have detailed information on demographics, health, labor supply, and finances. Whenever possible, I use the RAND HRS Data File, a user-friendly version that contains a subset of HRS variables.[4] Because this paper examines the wealth effect on elderly labor supply, I limit the sample to individuals of age between 50 and 75. Figures 3-1 and 3-2 display the relationship between age and labor force status for male and female HRS respondents, respectively. At age 50, around 85% of males were in the labor force and only 10% of males were out of the labor force.[5] In contrast, almost 30% of females were out of the labor force at age 50. As people grew older, the fraction of respondents remaining in the labor force declined. Females appeared to exit the labor force earlier than males. For both males and females, the biggest jump in retirement occurred at age 62. This is probably because 62 is the early retirement age at which beneficiaries can claim Social Security benefits. At age 75, only 5% of females remained in the labor force, whereas over 10% of males were still in the labor force. Because of these apparent differences in male and female labor supply behavior, I perform regression analysis for older men and older women separately. Figure 3-3 plots the empirical retirement hazard rate for homeowners between age 50 and 75. Conditional on being in the labor force, the probability that one retires within the next two years goes up with age. For both males and females, the hazard rate increases sharply around age 60 and again around age 70.

To measure changes in labor supply, I use three outcome variables: retirement, reentry to the labor force, and working hours. I define retirement as a transition from

[4] See St.Clair et al (2006) for more information on the RAND HRS Data File.

[5] The other 5% of male respondents were disabled or had missing labor force status.

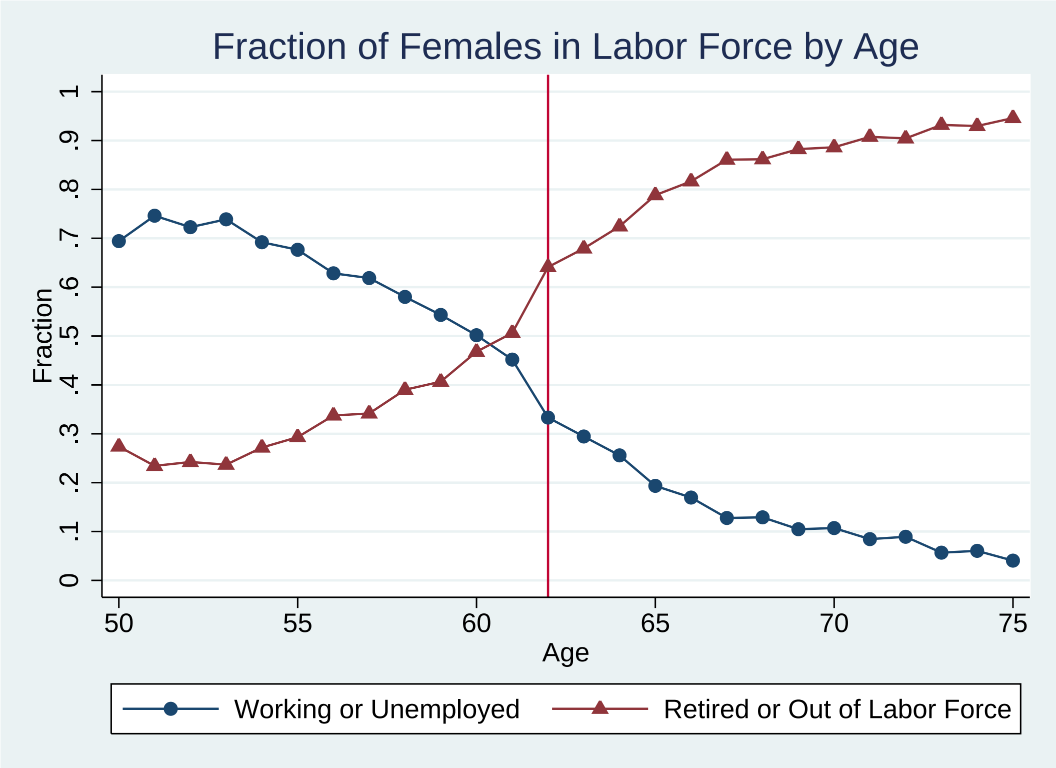
Figure 3-1: Labor Force Participation of Male Homeowners



working or being unemployed to being retired or out of the labor force. Similarly, a transition from being retired or out of the labor force to working or being unemployed is defined as reentry to the labor force. Working hours refer to the self-reported total number of hours worked during the past year. Table 3.2 shows that on average, the two-year retirement rate is 18.3% for males and 20.4% for females in the sample. The average two-year reentry rate is much lower: 5.8% for males and 5.2% for females. Conditional on being in the labor force, male respondents report an average of 2,283 annual working hours, and female respondents report an average of 1,880 annual working hours.

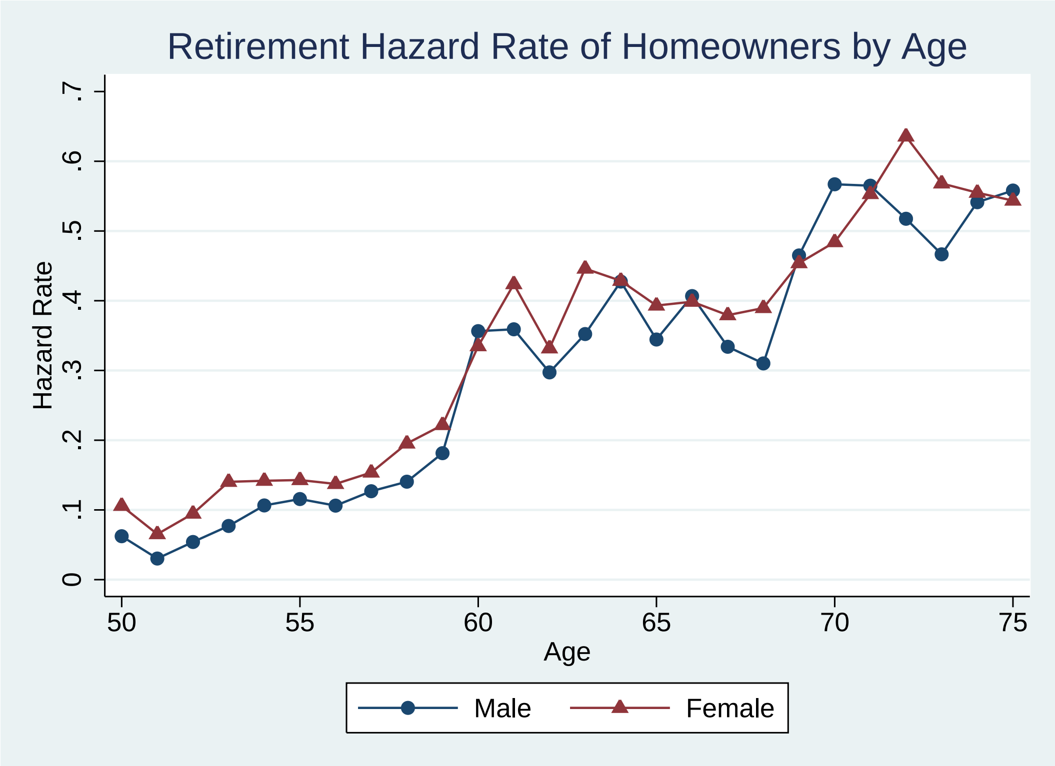
The key independent variable in this paper is property taxes. In all seven waves, respondents were asked to report the amount of property taxes paid on their primary residence during the past year. I assume these self-reported property tax payments are

Figure 3-2: Labor Force Participation of Female Homeowners



the actual payments *after* all relevant property tax exemptions, rebates or refunds have been applied. Such an assumption is crucial for the first-stage regression in my IV strategy. For programs where participation is automatic and property tax bills are mailed to homeowners after benefits have been netted out, this assumption seems justified. For programs where homeowners receive rebate checks soon after paying property taxes, it is unclear whether respondents report their before-relief property tax payments or after-relief property tax payments. For programs that are implemented by state personal income tax credits, respondents are likely to report their before-relief benefits for two reasons. First, relief benefits are usually received long after homeowners have paid their property taxes. Second, property tax relief benefits may appear less salient on state personal income tax returns. For example, filers may view property tax credits that they claim against income tax liabilities

Figure 3-3: Empirical Retirement Hazard Rate of Homeowners



as *income tax* relief benefits rather than *property tax* relief benefits. Recent studies including Chetty, Looney and Kroft (2007) and Finkelstein (2007) suggest that tax salience could have a significant impact on behavior. Therefore, I exclude in my regression analysis states where relief benefits are granted by tax credits on state personal income tax returns.[6] The dropped observations represent about 25% of the sample. I also drop individuals living in mobile homes and individuals living on farms or ranches because these properties may be treated differently from other residential properties for tax purposes.

Table 3.2 displays summary statistics of key demographic and socio-economic

[6]These states are District of Columbia, Massachusetts, Michigan, Missouri, Montana, New Jersey, New Mexico, New York, Oklahoma, Rhode Island, Vermont, and Wisconsin. I do not exclude states that use rebate checks to implement relief programs because the sample size would drop significantly and asymptotic theory no longer applies when there are only a few states left in the sample and standard errors are clustered at the state level.

variables for the retirement, reentry, and working-hour samples. Unsurprisingly, individuals in the retirement and working-hour samples are younger, healthier, better-educated, and have significantly higher household income than individuals in the reentry sample. Individuals in the retirement and working-hour samples also live in more expensive houses and pay higher property taxes than individuals in the reentry sample. On the other hand, they have lower financial wealth than their counterparts in the reentry sample. Such a pattern in housing wealth and financial wealth may suggest that homeowners transform their housing wealth into financial wealth by downsizing as they age and exit the labor force.

In addition to the publicly available HRS data, I obtained restricted access to household level geographic identifiers in each survey year, including state, county, census tract, and zip code. The state identifier is crucial in my analysis because it links households with the state-provided property tax relief programs for which they are eligible. The county identifier allows me to control for county-year specific unemployment rate published by the Census Bureau in my regression analysis.

The second component of the data used in this paper is the data on property tax relief programs. As of present, all 50 states and District of Columbia have some form of property tax relief programs for homeowners, especially for low-income and elderly homeowners. Many of these programs were first established well before my sample period started. Broadly speaking, there are four categories of relief programs: Homestead Exemptions and Credits, Circuit-Breakers, Deferral Programs, and Limitations. Shan (2008) has detailed descriptions on how these programs work, how the data were collected, and how these programs are codified. At the end of the process, a computer program is written to produce three output variables: the amount of benefits from homestead exemption, homestead credit, and circuit-breaker programs that a homeowner is eligible for, whether eligible for an “assessment value freeze” program, and whether eligible for an “property tax freeze” program. Such output variables can be generated for any homeowner in the U.S. in any year between 1990 and 2004 provided that input parameters, including state of residence, year, age, income, house value, Social Security income, marital status, household size and wealth, are non-missing.

3.4 Empirical Strategy and Estimation Results

In this section, I present the empirical model and estimation results in studying the effect of property taxes on elderly homeowners' decisions to retire, to reenter the labor force, and to increase working hours. Estimations are performed for men and women separately. Robustness checks and extensions are carried out and discussed at the end of this section.

3.4.1 Property Taxes and Retirement Decisions

To investigate whether property taxes have an impact on retirement behavior, I start with a simple probit model[7]

$$\text{Prob}(Retire_{ist} = 1) = \Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t) \tag{3.1}$$

where $Retire_{ist}$ indicates whether household i in state s retired between time t and $t+1$, ζ_s denotes state fixed effects, δ_t denotes year fixed effects, and the covariate vector $\mathbf{X}_{ist}$ includes a constant, income quintile indicators, house value quintile indicators, financial wealth quintile indicators, race/ethnicity (i.e. White, black, and Hispanic), whether married, education categories (i.e. less than high school, high school graduates, some college, and college graduates), whether hospitalized between

[7] I use a probit model in this paper because the mean of dependent variables is not near 0.5. A linear probability model may be biased when the dependent variable is close to zero or one, and will produce predictions beyond the range of zero to one.

the last interview and the current interview, whether have pension coverage, whether have retiree health insurance coverage, county unemployment rate, industry dummies, occupation dummies, and age dummies.[8] The key variable of interest in equation (3.1) is Tax_{ist}, property tax payments by household i in state s at time t. If higher property taxes cause elderly homeowners to delay retirement, then we expect $\beta_1 < 0$.

Columns (1) and (3) in Table 3.3 present estimation results of equation (3.1) for males and females, respectively. To make the results interpretable, I show marginal effects of independent variables by calculating the predicted marginal effect for each observation and then averaging them across all observations. To be consistent with results presented later in this section, standard errors shown in parentheses are bootstrapped by 500 random draws with replacement. I implement a block-bootstrap scheme to make certain that observations are clustered at state level in estimating standard errors. The estimated effects of property taxes are negative as expected, but statistically insignificant. The magnitudes of the marginal effects are small, suggesting that a $100 increase in annual property taxes is associated with a 0.03 percentage point decrease in two-year retirement rate for men and 0.09 percentage point decrease for women.

There are three reasons why such estimates of β_1 may be inconsistent. First, property taxes are products of tax rates and house values. At a given tax rate, higher house values lead to higher property taxes. If house values affect elderly homeowners' labor supply decisions through channels other than property taxes (e.g. housing wealth effect), then the probit estimate of β_1 will be biased to the extent that house values are not fully controlled for. Second, property taxes are used to provide local public services. Higher property taxes often correlate with better local public services. If local public services such as parks and senior centers are complements to the con-

[8]For the first wave in 1992, HRS asked whether the individual was hospitalized in the past year. From the second wave on, HRS asked whether the individual was hospitalized since the last interview.

sumption of leisure, we will not be able to estimate β_1 consistently without controlling for local public services which are unobservable to econometricians. Lastly, property tax payments are self-reported in the HRS. To the extent that elderly homeowners do not know and/or report property taxes accurately, measurement errors will cause attenuation bias in estimating β_1. To deal with these three problems, I use measures of property tax relief program generosity to instrument for property taxes.

More specifically, I use the set of instruments - $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ - that are described in detail in Shan (2008). Because property tax relief benefits reduce property tax payments, these measures of program generosity should be negatively correlated with property tax payments. Such a negative correlation serves as the first stage in this paper. On the other hand, these instruments essentially capture variations in property tax relief program rules and are rid of variations stemming from individual characteristics. Thus, they are orthogonal to the individual level error term ϵ_{ist} and satisfy the exclusion restriction. Table 3.2 illustrates the summary statistics of $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$. In the retirement sample, 5.0% of males and 9.3% of females are eligible for relief benefits from homestead exemptions, homestead credits, and circuit-breakers. Conditional on eligibility, the average benefits from these programs are \$144 for males and \$202 for females. In addition, 7.5% of both males and females are eligible for assessment value freeze programs. 13.4% of males and 14.7% of females are eligible for property tax freeze programs.

To implement the simulated IV strategy in a probit framework, I use the two-step estimator suggested by Rivers and Vuong (1988).[9] Beside computational ease, the Rivers-Vuong two-step IV approach has another appealing feature. The usual probit t-test on $\hat{v}$, which is a consistent estimate of the first-stage error term, is a valid test of

[9]The Rivers-Vuong two-step approach is a limited information procedure. Thus, it is less efficient than the conditional maximum likelihood estimation (MLE). In practice, I find MLE computationally difficult, and iterations do not converge.

the null hypothesis that Tax_{ist} is exogenous. Such a test is equivalent to the Hausman specification test suggested by Hausman (1978). Because I use a two-step procedure to estimate the IV-probit model, standard errors need to be adjusted accordingly. I choose to obtain consistent estimates of standard errors by bootstrapping in lieu of the delta-method for two reasons. First, bootstrapping is computationally easier to implement. Second, bootstrapping provides higher-order refinements while the delta-method is only a first-order approximation (Horowitz (2001)).

Columns (2) and (4) of Table 3.3 show the IV-probit estimation results using $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ as instruments. The estimated marginal effects of property taxes remain negative and statistically insignificant. The magnitudes of these marginal effects become much larger than the probit results. They suggest that a \$100 increase in annual property tax payments reduces the two-year retirement rate by 0.71 percentage points for men and 1.35 percentage points for women. Given the average two-year retirement rate of 18.3% for men and 20.4% for women, these represent a 3.9 percent decline in retirement rate for men and 6.6 percent decline for women. Although the point estimates imply a sizable property tax effect on retirement behavior, the standard errors are large and we cannot reject the null hypothesis that property taxes do not affect retirement. Note that the first-stage F-statistic is only 2.10 for the male sample and 14.28 for the female sample. The reason why the instruments do not strongly correlate with property tax payments for males is probably that male respondents in the retirement sample tend to have significantly higher household income than females. In addition, males tend to be older than their spouses. Since the age requirement in property tax relief programs often refers to the oldest person in the household, male homeowners are less likely to qualify for relief benefits than female homeowners of the same age. High incomes and not having older spouses may have prevented male homeowners from taking advantages of property tax relief programs which often have income and age as qualification criteria. Stock, Wright and Yogo (2002) suggest that the rule of thumb for detecting weak instruments is to check whether the first-stage F-stat exceeds 10. By this standard, the male sample may have a weak instrument problem and the IV-probit estimates may be biased in the direction of the probit estimates. Moreover, the Hausman test rejects the null hypothesis that property tax payments are exogenous in the female sample but not in the male sample.

The estimated marginal effects of other covariates are mostly consistent with our expectation and previous literature's findings. For example, health shocks, approximated by the indicator variable "whether the respondent was recently hospitalized," raise the two-year retirement rate by 5 percentage points for both men and women, or a 25 percent increase from the baseline level. Financial wealth is correlated with higher probability of retirement. However, such a correlation should not be interpreted as causal since individuals who have strong desires to retire early may have saved more aggressively over their life-cycle. In addition, male respondents who have retiree health insurance coverage are more likely to retire than those who do not, but the effect is insignificant for females. Female respondents who have pension coverage are less likely to retire than those who do not, but the effect is insignificant for males. Black and Hispanic women are more likely to retire than white women, although race/ethnicity does not appear to matter among male respondents. Such differences between males and females highlight the importance of analyzing male and female individuals separately in studying labor supply behavior.

3.4.2 Property Taxes and Reentry Decisions

In the previous section, I estimate a retirement regression model and the results cannot reject the null hypothesis that property taxes do not have a significant effect on elderly homeowners' retirement decisions. In this section, I explore the impact of property taxes on labor force reentry behavior in a similar regression analysis by estimating the following probit model:

$$\text{Prob}(Reentry_{ist} = 1) = \Phi(\beta_2 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t) \tag{3.2}$$

where $Reentry_{ist}$ indicates whether individual i who is out of the labor force at time t reenters the labor force between time t and $t+1$. If higher property taxes cause retired elderly homeowners to reenter the labor force, then we expect $\beta_2 > 0$.

Because property tax payments, Tax_{ist} may be endogenous to elderly homeowners' labor supply decisions, I use $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ as instruments for property taxes to obtain consistent estimates of β_2. As shown in Table 3.2, individuals in the reentry sample are relatively older and have lower household income because they have to be out the labor force to be in this sample. As a result, they are more likely to be eligible for property tax relief programs that target low-income and elderly homeowners. On average, 22.5% of males and 27.2% of females in the reentry sample are eligible for homestead exemptions, homestead credits, or circuit-breakers. 10.1% of males and 10.8% of females are eligible for assessment value freeze programs, and 17.6% of males and 18.1% of females are eligible for property tax freeze programs. The average two-year reentry rate among homeowners age 50-75 is low, 5.8% for males and 5.2% for females.

Table 3.4 presents estimation results of both probit and IV-probit specifications for males and females separately. For the male sample, the estimated marginal effect of property taxes is positive but statistically insignificant in the probit specification. The marginal effect doubles in the IV-probit specification, but remains statistically indistinguishable from zero. For the female sample, both the probit and IV-probit specifications produce negative estimates of β_2, and the marginal effects of property taxes on reentry behavior are also statistically insignificant. The first-stage relationship between property taxes and the instruments are strong, with a F-statistic of 138.47 for the male sample and 11.09 for the female sample. Nevertheless, the evidence is inconsistent with the claim that homeowners who face higher property taxes are more likely to reenter the labor force.

Estimation results displayed in Table 3.4 also suggest that both male and female Hispanic homeowners are more likely to reenter the labor force than white and black elderly homeowners. When county unemployment rate is high, older men and women are less likely to reenter the labor force. Higher income is correlated with higher probability of reentry behavior, especially among male homeowners. Among female homeowners, individuals who live in more expensive houses are more likely to reenter the labor force. Among male homeowners, individuals with more financial wealth are less likely to reenter the labor force. Moreover, negative health shocks appear to prevent older men from reentering the labor force. Married women are less likely to reenter the labor force than their unmarried counterparts.

3.4.3 Property Taxes and Working Hours

The previous two sections have examined the property tax effect on the extensive margin of elderly labor supply, namely, whether to exit or reenter the labor market. In this section, I investigate the intensive margin of labor supply by estimating the effect of property taxes on whether elderly homeowners' working hours. I employ a regression model in the following form:

$$Hours_{ist} = \beta_3 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t + \epsilon_{ist} \tag{3.3}$$

where $Hours_{ist}$ is the total number of hours individual i reports working at time t conditional on being in the labor force. If higher property taxes indeed induce elderly homeowners to work longer hours, we expect $\beta_3 > 0$.

As explained before, Tax_{ist} may be endogenous to individuals' labor supply decisions and cause bias in estimating β_3. To deal with such concerns, I use measures of property tax relief program generosity, $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$, to instrument for Tax_{ist}. Because elderly homeowners have to be in the labor force at time t to be considered in this analysis, individuals in the working-hour sample are relatively young and have high household income. Such characteristics imply that they also tend to be ineligible for property tax relief programs that are designed to help low-income and older homeowners. Table 3.2 shows that on average only 4.7% of males and 9.0% of females in the working-hour sample are eligible for homestead exemptions, homestead credits, and circuit-breakers. 7.3% of male and 7.3% of females are eligible for assessment value freeze programs, and 13.3% of males and 14.6% of females are eligible for property tax freeze programs. On average, the male respondents report to work 2,283 hours annually and the female respondents report to work 1,880 hours annually.

Table 3.5 presents the estimation results of the OLS and 2SLS specifications for males and females separately. In the male samples, the OLS estimate suggests that property taxes have a positive, small, and statistically insignificant effect on working hours. In the female sample, however, the OLS estimate suggests that property taxes have a negative and statistically significant effect on working hours. Such a counterintuitive result may reflect that property taxes are endogenous to labor supply decisions. Once property taxes are instrumented using relief program generosity measures, the effect of property taxes on working hours appears to be negative and statistically insignificant for both the male and female sample. The estimated coefficients are large, but the standard errors are also large and I cannot reject the null hypothesis that property taxes have no impact on elderly homeowners' working hours. Similar to the retirement analysis, the first-stage relationship between property taxes and the instruments is weak for males in the working-hour sample, probably because they have high incomes and they tend to have younger spouses. On the other hand, the first stage F-statistic is 18.31 for the females, suggesting that I do not have a weak-instrument problem in the female sample. Nevertheless, the 2SLS estimate of the coefficient on property taxes is inconsistent with the hypothesis that higher property taxes induce elderly homeowners to work longer hours.

Results shown in Table 3.5 also suggest that income is highly correlated with working hours. In the male sample, black homeowners work fewer hours than white and Hispanic homeowners. In the female sample, homeowners with higher financial wealth appear to work fewer hours. Married women work fewer hours than women with other marital status. Women with college degrees work more hours than women with less education. Female homeowners living in counties with high unemployment rates work slightly fewer hours compared with those in counties with low unemployment rates.

3.4.4 Robustness Checks and Extensions

In previous sections, I have used a simulated IV strategy to identify the potential effect of property taxes on elderly homeowners' labor supply decisions both on the extensive margin and the intensive margin. The estimation results suggest that property taxes may have no significant impact on elderly homeowners' decision to retire, to reenter the labor force, or to increase working hours. In this section, I first carry out robustness checks by using various sub-samples. Then I extend the regression models and allow property taxes to differentially affect the labor supply decisions of homeowners at different ages. Because the weak-instrument problem may exist in the male retirement sample and the male working-hour sample, I focus on females when analyzing retirement and working-hour responses, and I look at both males and females when studying reentry behavior.

As shown in Figure 3-1 and Figure 3-2, most elderly homeowners exit the labor market between age 55 and 70. In the first robustness check, I limit the sample to homeowners of age 55-70 and investigate whether the estimates change once homeowners younger than 55 or older than 70 are dropped. In the second robustness check, I exclude elderly homeowners who live in California because Proposition 13 may have created a very unusual institutional setting. Proposition 13 was adopted in California in 1978. It limits property tax rates at 1% and requires assessment values to grow no more than 2% per year unless the house is sold and re-assessment is carried out. In the third robustness check, I drop individuals who claim to be self-employed because self-employed individuals may face higher or lower costs than others when adjusting their labor supply. Lastly, I exclude elderly homeowners who report to have moved between time t and $t+1$ and focus on individuals who stay in the same house in both periods.

Table 3.6, 3.7 and 3.8 present the estimation results using these sub-samples in the retirement, reentry, and working-hour regressions, respectively. In the retirement analysis, the estimated marginal effect of property taxes is negative across sub-samples for female respondents, which is consistent with the hypothesis that rising property taxes induce elderly homeowners to delay retirement. However, none of the estimates is statistically different from zero at conventional confidence level, and thus, I cannot reject the null hypothesis that property taxes have no significant impact on retirement behavior. In the reentry analysis, the estimated coefficient on property taxes is positive in some cases and negative in others. In addition, they are all statistically indistinguishable from zero. Therefore, there appears to be little evidence that elderly homeowners who are out of the labor force actually reenter the labor force in order to boost their incomes and pay for rising property taxes. In the working-hour analysis, most estimates of the property tax effect are negative, which is inconsistent with the notion that higher property taxes may have caused elderly homeowners to work longer hours. Additionally, none of the estimates are statistically significant.

Next, I allow for heterogeneity in the property tax effect for individuals of different ages. If the reason why I do not find evidence of significant property tax effect on elderly labor supply is that I have restricted the coefficients on property taxes to be the same for individuals of different ages, then this extension should be able to identify the age groups at which property taxes may have a noticeable impact on labor supply decisions. Because Figure 3-3 shows that retirement hazard rate increases sharply at age 60, and because age 62 and 65 are the Social Security early retirement age and normal retirement age respectively, I allow the coefficients on property taxes to differ across five age groups: 50-59, 60, 61-62, 63-65, and 66-75. Specifically, I estimate the following probit models:

$$\begin{aligned}
\text{Prob}(Retire_{ist} = 1) &= \Phi(\alpha_1 Tax_{ist} \cdot \mathbf{1}(Age < 60) + \alpha_2 Tax_{ist} \cdot \mathbf{1}(Age = 60) \\
&+ \alpha_3 Tax_{ist} \cdot \mathbf{1}(Age = 61-62) + \alpha_4 Tax_{ist} \cdot \mathbf{1}(Age = 63-65) \\
&+ \alpha_5 Tax_{ist} \cdot \mathbf{1}(Age > 65) + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)
\end{aligned} \tag{3.4}$$

$$\begin{aligned}
\text{Prob}(Reentry_{ist} = 1) &= \Phi(\gamma_1 Tax_{ist} \cdot \mathbf{1}(Age < 60) + \gamma_2 Tax_{ist} \cdot \mathbf{1}(Age = 60) \\
&+ \gamma_3 Tax_{ist} \cdot \mathbf{1}(Age = 61-62) + \gamma_4 Tax_{ist} \cdot \mathbf{1}(Age = 63-65) \\
&+ \gamma_5 Tax_{ist} \cdot \mathbf{1}(Age > 65) + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)
\end{aligned} \tag{3.5}$$

$$\begin{aligned}
Hours_{ist} &= \lambda_1 Tax_{ist} \cdot \mathbf{1}(Age < 60) + \lambda_2 Tax_{ist} \cdot \mathbf{1}(Age = 60) \\
&+ \lambda_3 Tax_{ist} \cdot \mathbf{1}(Age = 61-62) + \lambda_4 Tax_{ist} \cdot \mathbf{1}(Age = 63-65) \\
&+ \lambda_5 Tax_{ist} \cdot \mathbf{1}(Age > 65) + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t + \epsilon_{ist}
\end{aligned} \tag{3.6}$$

where $\mathbf{1}(\cdot)$ returns one if the expression in parenthesis holds true and zero otherwise. Since Tax_{ist} is endogenous to individual i's labor supply decisions, I use the interactions between the five age group dummies and the three program generosity measures, $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$, to instrument for the interactions between the five age group dummies and Tax_{ist}. Thus, I have 5 endogenous explanatory variables and 15 instruments in each equation.

Table 3.9 shows the estimation results of IV-probit and 2SLS specifications. Many of the estimated coefficients on property taxes have signs inconsistent with the hypothesis that rising property taxes induce elderly homeowners to increase their labor supply. Among the ones that have the expected signs, none of the IV estimates is statistically different from zero at conventional significance level. Overall, the above extension, where heterogenous property tax effects are allowed, does not detect a systematic relationship between property taxes and elderly labor supply.

In summary, despite efforts to identify the link between property taxes and elderly labor supply using various sub-samples and allowing for heterogeneous effects across age groups, there appears to be little evidence suggesting that property taxes play a significant role in elderly homeowners' labor supply decisions. Note that the instruments used in this paper to identify the causal effect of property taxes - simulated relief benefits from homestead exemptions, homestead credits, and circuit-breakers, eligibility for assessment value freeze programs, and eligibility for property tax freeze programs - affect property taxes of only homeowners who are eligible for property tax relief programs and actually take up these programs. To the extent that these people are more sensitive and responsive to property taxes, the estimates presented here may provide the upper bound of the property tax impact on elderly labor supply due to the local average treatment effect (LATE) formulated by Imbens and Angrist (1994). Therefore, finding little evidence supporting the claim that elderly homeowners respond to rising property taxes by increasing labor supply in this paper implies that property taxes probably play an insignificant role in labor supply decisions of the general public.

3.5 Conclusion

Property taxes are the most important tax revenue source of local governments in the United States. The recent housing market boom led to drastic increases in property taxes which in turn has caught the attention of both policy makers and the general public. Despite news articles reporting anecdotes where elderly homeowners have been delaying retirement in the face of rising property taxes, there has been no empirical study on the relationship between property taxes and elderly labor supply. Exploiting the arguably exogenous variation in state-provided property tax relief programs, this paper is the first study examining the role property taxes play in elderly homeowners' labor supply decisions. I examine both the extensive and intensive margins of labor supply behavior. Various sub-samples are analyzed and the property tax effect is allowed to differ across age groups. Overall, I find little evidence supporting the claim that elderly homeowners have been delaying retirement, reentering the labor force, or working longer hours to deal with increasing property taxes. In addition to being the first study to investigate the behavioral impact of property taxes on elderly labor supply, this paper also contributes to the existing literature on the wealth effect by using property taxes and property tax relief programs as a novel source of variation.

There are two caveats worth mentioning. First, I have to focus on people who are in the labor force in order to study their retirement and working-hour behavior in this paper. This limits the power of my instruments significantly in the retirement and working-hour regressions among male respondents because these people are often too young and their incomes tend to be too high for them to be eligible for property tax relief programs. Since weak instruments may bias the IV estimates, not finding retirement and working-hour responses to property taxes in the male sample in the

IV-probit specification cannot completely rule out the possibility that property taxes play an important role in older men's retirement and working-hour decisions. On the other hand, the first-stage is quite strong in the reentry analysis. Nevertheless, there appears to be little evidence of labor force reentry response to property taxes.

Second, even though this paper find evidence suggesting that the wealth shock generated by unexpected increases in property taxes may not have a significant effect on elderly labor supply, it does not necessarily mean that the wealth effects on elderly labor supply are in general insignificant. Perhaps increases in property taxes do not translate into a large enough reduction in wealth and therefore, do not induce a detectable effect on labor supply. Furthermore, even if homeowners truly do not respond to the negative wealth shock produced by rising property taxes, they may still be very responsive to other forms of wealth shocks such as stock market and housing market booms and busts. The field calls for more research on whether and to what degree different types of wealth affect elderly labor supply differently.

Taken together with Shan (2008), the findings of this paper have important policy implications. Shan (2008) shows evidence suggesting that higher property taxes induce higher mobility rate among elderly homeowners. Property taxes may affect elderly mobility through various channels: the wealth effect, the liquidity constraint effect, and the substitution effect. The wealth effect exists because increases in property taxes are equivalent to declines in total wealth. The liquidity constraint effect means that elderly homeowners would have preferred staying in their homes if they were able to afford rising property taxes. The only reason that they move in response to higher property taxes is that they have no incomes or liquid assets to pay for increases in property taxes. The substitution effect refers to the fact that elderly homeowners, who typically do not have school-age children living in the house, often find the marginal cost of paying high property taxes exceeds the marginal benefit of consuming local public services such as schools. Thus, increases in property taxes may trigger an adjustment in their choice of housing consumption bundles, and such an adjustment is usually accomplished by moving. These different mechanisms have different welfare implications. Although Shan (2008) shows the relationship between property taxes and elderly mobility, she does not identify whether this relationship is driven by the wealth effect, the liquidity constraint effect, or the substitution effect. On the other hand, property taxes affect elderly labor supply only through the wealth effect and the liquidity constraint effect. Finding little evidence supporting that property taxes play a significant role in elderly homeowners' labor supply decisions, this paper points in the direction that property taxes may have influenced elderly mobility through the substitution effect. If this is true, property tax relief programs may have kept elderly homeowners who optimally should have moved to areas with lower property taxes and fewer public services in their homes.

Bibliography

[1] Blau, David. “Labor Force Dynamics of Older Men.” *Econometrica* 62(1) 1994, 117-156.

[2] Brown, Jeffrey, Courtney Coile, and Scott Weisbenner. “The Effect of Inheritance Receipt on Retirement.” NBER Working Paper #12386, 2006.

[3] Bradley, David. “Property Taxes in Perspective.” Center on Budget and Policy Priorities, March 2005.

[4] Burtless, Gary. “Social Security, Unanticipated Benefit Increases, and the Timing of Retirement.” *Review of Economic Studies* 53(176) 1986, 781-805.

[5] Chan, Sewin and Ann Huff Stevens. “Do Changes in Pension Incentives Affect Retirement? A Longitudinal Study of Subjective Retirement Expectations.” *Journal of Public Economics* 88(7-8) 2004, 1307-1333.

[6] Chetty, Raj, Adam Looney and Kory Kroft. “Salience and Taxation: Theory and Evidence.” Working Paper 2007.

[7] Coile, Courtney and Jonathan Gruber. “Future Social Security Entitlements and the Retirement Decision.” *Review of Economics and Statistics* 89(2) 2007, 234-246.

[8] Coile, Courtney and Phillip Levine. “Bulls, Bears, and Retirement Behavior.” *Industrial and Labor Relations Review* 59(3) 2006, 408-429.

[9] Coronado, Julia and Maria Perozek. “Wealth Effects and the Consumption of Leisure: Retirement Decision During the Stock Market Boom of the 1990s.” Federal Reserve Board Finance and Economics Discussion Series, 2003-20.

[10] Diamond, Peter and Jerry Hausman. “Retirement and Unemployment Behavior of Older Men.” *Retirement and Economic Behavior* edited by Henry Aaron and Gary Burtless. Washington D.C.: Brookings Institution, 1984.

[11] Farnham, Martin and Purvi Sevak. “Housing Wealth and Retirement Timing.” Michigan Retirement Research Center Working Paper 2007-172.

[12] Finkelstein, Amy. “E-Z Tax: Tax Salience and Tax Rates.” Working Paper 2007.

[13] Goodstein, Ryan. "The Effect of Wealth on Labor Force Participation of Older Men." University of North Carolina - Chapel Hill Dissertation Chapter, 2008.

[14] Hausman, Jerry. "Specification Tests in Econometrics." *Econometrica* 46(6) 1978, 1273-1291.

[15] Horowitz, Joel. "The Bootstrap." Handbook of Econometrics, Vol 5., edited by J.J. Heckman and E. Leamer, Elsevier Science B.V., 2001, Ch 52, 3159-3228.

[16] Imbens, Guido and Joshua Angrist. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* 62(2) 1994, 467-475.

[17] Imbens, Guido, Donald Rubin, and Bruce Sacerdote. "Estimating the Effect of Unearned Income on Labor Earnings, Savings, and Consumption: Evidence from a Survey of Lottery Players." *American Economic Review* 91(4) 2001, 778-794.

[18] Krueger, Alan and Jorn-Steffan Pischke. "The Effect of Social Security on Labor Supply: A Cohort Analysis of the Nortch Generation." *Journal of Labor Economics* 10(4) 1992, 412-437.

[19] Lyons, Karen, Sarah Farkas, and Nicholas Johnson. "The Property Tax Circuit Breaker: An Introduction and Survey of Current Programs." Center on Budget and Policy Priorities, March 2007.

[20] "A Guide to Property Taxes: Series." National Conference of State Legislatures, 2005.

[21] Rivers, Douglas and Quang Vuong. "Limited Information Estimators and Exogeneity Tests for Simultaneous Probit Models." *Journal of Econometrics* 39(3) 1988, 347-366.

[22] Samwick, Andrew. "New Evidence on Pensions, Social Security, and the Timing of Retirement." *Journal of Public Economics* 70(2) 1998, 207-236.

[23] Sevak, Purvi. "Wealth Shocks and Retirement Timing: Evidence from the Nineties." Michigan Retirement Research Center Working Paper 2002-027, Revised 2005.

[24] Shan, Hui. "Property Taxes and Elderly Mobility." Massachusetts Institute of Technology Dissertation Chapter, 2008.

[25] St. Clair, Patricia, et al. "RAND HRS Data Documentation, Version F." Labor & Population Program, RAND Center for the Study of Aging, May 2006.

[26] Stock, James and David Wise. "Pensions, the Option Value of Work, and Retirement." *Econometrica* 58(5) 1990, 1151-1180.

[27] Stock, James, Jonathan Wright and Motohiro Yogo. "A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments." *Journal of Business and Economics Statistics* 20(4) 2002, 518-529.

Table 3.1: PDV of Hypothetical Property Tax Increases

	A Level Increase of $300		
Discount Rate	Age=50	Age=60	Age=70
0.02	7,019	4,912	3,677
	(47%)	(33%)	(25%)
0.05	5,205	4,039	3,245
	(35%)	(27%)	(22%)
0.08	2,995	2,617	2,313
	(20%)	(18%)	(16%)
	Annual Increase of $300		
Discount Rate	Age=50	Age=60	Age=70
0.02	101,253	60,110	38,091
	(685%)	(406%)	(258%)
0.05	53,491	37,324	26,918
	(362%)	(252%)	(182%)
0.08	17,376	14,429	12,119
	(117%)	(98%)	(82%)

Notes: I Assume the individual lives to age 80. Numbers in parenthesis represent the PDV as a percentage of median household financial wealth among homeowners of that age. Based on the 1992-2004 HRS data, the median household financial wealth is $14,790 for homeowners of age 50, $27,472 for homeowners of age 60, and $29,171 for homeowners of age 70.

Table 3.2: Summary Statistics of Analysis Samples

	Male (N=6,389)			Female (N=5,661)		
Retirement Sample	Mean	Median	SD	Mean	Median	SD
Retire	0.183		0.387	0.204		0.403
Simulated Benefits						
Fraction Eligible	0.050		0.148	0.093		0.163
Conditional Benefits	144	99.5	190	202	154.4	172
Value Freeze	0.075		0.263	0.075		0.264
Tax Freeze	0.134		0.340	0.147		0.355
Property Tax	1,839	1,307	2,149	1,621	1,220	1,727
Household Income	103,582	72,436	179,602	76,915	59,159	104,146
House Value	175,393	132,055	222,679	155,066	121,564	122,653
Financial Wealth	125,771	23,320	689,661	85,662	19,144	223,768
Age	57.5	57	4.5	57.2	57	4.4
Black	0.057		0.231	0.068		0.252
Hispanic	0.055		0.228	0.051		0.219
Married	0.876		0.330	0.697		0.459
Recently Hospitalized	0.132		0.339	0.119		0.323
Less than High School	0.162		0.369	0.136		0.343
High School Graduates	0.279		0.448	0.337		0.473
Some College	0.215		0.411	0.286		0.452
College Graduates	0.344		0.475	0.241		0.428
Pension Coverage	0.665		0.472	0.601		0.490
Retiree Health Insurance	0.460		0.498	0.334		0.472

Continued on next page

Table 3.2: Summary Statistics of Analysis Samples (*Continued*)

	Male (N=6,389)			Female (N=5,661)		
Reentry Sample	Mean	Median	SD	Mean	Median	SD
Reentry	0.058		0.235	0.052		0.221
Simulated Benefits						
Fraction Eligible	0.225		0.312	0.272		0.304
Conditional Benefits	179	138	160	194	147	157
Value Freeze	0.101		0.302	0.108		0.310
Tax Freeze	0.176		0.381	0.181		0.385
Property Tax	1,455	1,056	1,520	1,436	1,000	3,381
Household Income	58,901	40,171	75,595	57,313	35,243	123,611
House Value	154,180	119,650	132,410	150,789	110,000	190,515
Financial Wealth	164,095	38,134	415,897	155,324	34,334	433,659
Age	66.0	66	6.2	64.6	65	6.8
Black	0.062		0.241	0.059		0.236
Hispanic	0.044		0.204	0.058		0.234
Married	0.842		0.364	0.729		0.444
Recently Hospitalized	0.247		0.431	0.188		0.391
Less than High School	0.255		0.436	0.223		0.417
High School Graduates	0.275		0.447	0.382		0.486
Some College	0.201		0.401	0.229		0.420
College Graduates	0.269		0.443	0.166		0.372

Continued on next page

Table 3.2: Summary Statistics of Analysis Samples (*Continued*)

	Male (N=6,389)			Female (N=5,661)		
Working-Hour Sample	Mean	Median	SD	Mean	Median	SD
Working Hours	2,283	2,100	639	1,880	2,080	665
Simulated Benefits						
Fraction Eligible	0.047		0.141	0.090		0.160
Conditional Benefits	142	98	189	200	154	170
Value Freeze	0.073		0.261	0.073		0.261
Tax Freeze	0.133		0.339	0.146		0.353
Property Tax	1,910	1,317	4,149	1,619	1,207	1,688
Household Income	101,912	71,912	168,523	77,651	58,210	113,154
House Value	173,267	132,055	169,279	155,439	121,491	125,985
Financial Wealth	121,845	23,048	653,379	92,258	19,144	421,381
Age	57.5	57	4.4	57.2	57	4.4
Black	0.058		0.233	0.067		0.251
Hispanic	0.060		0.237	0.052		0.223
Married	0.876		0.329	0.702		0.457
Recently Hospitalized	0.137		0.344	0.120		0.325
Less than High School	0.167		0.373	0.141		0.348
High School Graduates	0.277		0.447	0.343		0.475
Some College	0.219		0.413	0.285		0.451
College Graduates	0.338		0.473	0.231		0.422

Note: One has to be working or unemployed at time t to be included in the retirement sample and the working hours sample. One has to be retired or out of labor force at time t to be included in the reentry sample. Property tax, household income, house value, and financial wealth are in 2000 dollars. Individual weights are applied.

Table 3.3: Retirement Estimation Results

	Male		Female	
	(1)	(2)	(3)	(4)
	Probit	IV-Probit	Probit	IV-Probit
Property Taxes (in 10,000)	-0.0264	-0.7057	-0.0905	-1.3460
	(0.0342)	(0.9739)	(0.0580)	(1.1622)
Income Quintile 2	-0.0407*	-0.0319	-0.0505**	-0.0499**
	(0.0230)	(0.0274)	(0.0237)	(0.0236)
Income Quintile 3	-0.0375*	-0.0332	-0.0166	-0.0080
	(0.0215)	(0.0231)	(0.0249)	(0.0263)
Income Quintile 4	-0.0191	-0.0206	-0.0031	0.0037
	(0.0230)	(0.0230)	(0.0275)	(0.0277)
Income Quintile 5	-0.0444*	-0.0205	-0.0081	0.0288
	(0.0237)	(0.0401)	(0.0301)	(0.0478)
House Value Quintile 2	0.0141	0.0285	0.0054	0.0414
	(0.0165)	(0.0233)	(0.0211)	(0.0385)
House Value Quintile 3	-0.0174	0.0151	-0.0020	0.0738
	(0.0183)	(0.485)	(0.0217)	(0.0714)
House Value Quintile 4	-0.0053	0.0535	-0.0031	0.1123
	(0.0204)	(0.0841)	(0.0244)	(0.1082)
House Value Quintile 5	-0.0253	0.1270	-0.0179	0.2221
	(0.0230)	(0.2216)	(0.0277)	(0.2222)
Financial Wealth Quintile 2	0.0198	0.0157	0.0418**	0.0296
	(0.0157)	(0.0177)	(0.0175)	(0.0211)
Financial Wealth Quintile 3	0.0289*	0.0289	0.0748***	0.0652***
	(0.0166)	(0.0179)	(0.0202)	(0.0228)
Financial Wealth Quintile 4	0.0535***	0.0510**	0.0914***	0.0805***
	(0.0174)	(0.0201)	(0.0199)	(0.0239)
Financial Wealth Quintile 5	0.0930***	0.1268**	0.1249***	0.1494***
	(0.0194)	(0.0548)	(0.0229)	(0.0417)
Black	0.0169	0.0112	0.0619***	0.0562**
	(0.0199)	(0.0220)	(0.0202)	(0.0225)
Hispanic	-0.0562**	-0.0505	0.0378	0.0471*
	(0.0248)	(0.0313)	(0.0270)	(0.0273)
Married	-0.0309*	-0.0315	0.0194	0.0057
	(0.0184)	(0.0267)	(0.0161)	(0.0231)

Continued on next page

Table 3.3: Retirement Estimation Results (*Continued*)

	Male		Female	
	(1)	(2)	(3)	(4)
	Probit	IV-Probit	Probit	IV-Probit
High School Graduate	-0.0220	-0.0200	-0.0240	-0.0151
	(0.0157)	(0.0181)	(0.0163)	(0.0181)
Some College	-0.0177	-0.0165	-0.0300	-0.0146
	(0.0179)	(0.0199)	(0.0187)	(0.0235)
College Graduate	-0.0237	-0.0038	-0.0307	0.0051
	(0.0203)	(0.0392)	(0.0225)	(0.0389)
Recently Hospitalized	0.0479***	0.0507***	0.0527***	0.0522**
	(0.0138)	(0.0165)	(0.0170)	(0.0203)
Pension Coverage	-0.0147	-0.0036	-0.0606***	-0.0721***
	(0.0118)	(0.0191)	(0.0140)	(0.0240)
Retiree Health Insurance	0.0487***	0.0502***	0.0085	0.0036
	(0.0103)	(0.0140)	(0.0139)	(0.0149)
County Unemployment Rate	0.0015	0.0013	0.0000	-0.0025
	(0.0025)	(0.0026)	(0.0029)	(0.0039)
First Stage F-stat		2.10		14.28
Hausman Test		3.0650		5.4105***
(coeff on first-stage residuals)		(5.8850)		(1.5272)
N	6,388	6,388	5,657	5,657
Pseudo R2	0.1489	.	0.1093	.

Note: The regression model is Prob($Retire_{ist} = 1$) $= \Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. Other than the variables shown in the table, $\mathbf{X}_{ist}$ also includes a constant, age dummies, industry dummies, and occupation dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist} in the IV-probit specifications. The numbers shown in the table are marginal effects averaged across observations. Standard errors in parentheses are bootstrapped by 500 random draws with replacement clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.4: Reentry Estimation Results

	Male		Female	
	(1)	(2)	(3)	(4)
	Probit	IV-Probit	Probit	IV-Probit
Property Taxes (in 10,000)	0.0263	0.0581	-0.0030	-0.7037
	(0.0188)	(0.4371)	(0.0155)	(0.4619)
Income Quintile 2	0.0253**	0.0255**	0.0156*	0.0156
	(0.0103)	(0.0112)	(0.0081)	(0.0134)
Income Quintile 3	0.0422***	0.0427***	0.0238***	0.0240
	(0.0113)	(0.0125)	(0.0086)	(0.0166)
Income Quintile 4	0.0334***	0.0338**	0.0222**	0.0213
	(0.0120)	(0.0134)	(0.0090)	(0.0173)
Income Quintile 5	0.0870***	0.0859***	0.0284***	0.0519**
	(0.0130)	(0.0250)	(0.0102)	(0.0224)
House Value Quintile 2	0.0032	0.0026	0.0077	0.0288*
	(0.0109)	(0.0151)	(0.0089)	(0.0152)
House Value Quintile 3	0.0025	0.0007	0.0151	0.0578**
	(0.0115)	(0.0254)	(0.0093)	(0.0248)
House Value Quintile 4	-0.0201	-0.0231	0.0073	0.0775*
	(0.0125)	(0.0393)	(0.0102)	(0.0469)
House Value Quintile 5	0.0021	-0.0045	0.0110	0.1512*
	(0.0135)	(0.0747)	(0.0111)	(0.0860)
Financial Wealth Quintile 2	0.0048	0.0049	0.0082	0.0111
	(0.0106)	(0.0115)	(0.0086)	(0.0113)
Financial Wealth Quintile 3	-0.0213*	-0.0216*	0.0068	0.0149
	(0.0112)	(0.0120)	(0.0088)	(0.0123)
Financial Wealth Quintile 4	-0.0393***	-0.0395***	-0.0017	0.0063
	(0.0119)	(0.0129)	(0.0093)	(0.0166)
Financial Wealth Quintile 5	-0.0465***	-0.0479***	-0.0243**	-0.0126
	(0.0127)	(0.0172)	(0.0101)	(0.0183)
Black	-0.0107	-0.0103	0.0111	0.0115
	(0.0127)	(0.0145)	(0.0091)	(0.0125)
Hispanic	0.0323**	0.0322**	0.0188*	0.0338**
	(0.0131)	(0.0147)	(0.0109)	(0.0153)
Married	0.0106	0.0104	-0.0227***	-0.0305***
	(0.0097)	(0.0115)	(0.0071)	(0.0103)

Continued on next page

Table 3.4: Reentry Estimation Results (*Continued*)

	Male		Female	
	(1) Probit	(2) IV-Probit	(3) Probit	(4) IV-Probit
High School Graduate	-0.0102	-0.0104	-0.0093	-0.0106
	(0.0092)	(0.0098)	(0.0070)	(0.0097)
Some College	0.0022	0.0020	0.0015	0.0079
	(0.0097)	(0.0115)	(0.0074)	(0.0116)
College Graduate	-0.0122	-0.0133	0.0004	0.0222
	(0.0104)	(0.0195)	(0.0092)	(0.0206)
Recently Hospitalized	-0.0285***	-0.0283***	-0.0023	0.0015
	(0.0087)	(0.0097)	(0.0062)	(0.0107)
County Unemployment Rate	-0.0031*	-0.0032*	-0.0023**	-0.0039**
	(0.0017)	(0.0019)	(0.0011)	(0.0017)
First Stage F-stat		138.47		11.09
Hausman Test		-0.3136		5.2854
(coeff on first-stage residuals)		(2.4302)		(3.6295)
N	6,475	6,475	9,406	9,406
Pseudo R2	0.1538	.	0.1391	.

Note: The regression model is Prob($Reentry_{ist} = 1$) = $\Phi(\beta_2 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. Other than the variables shown in the table, $\mathbf{X}_{ist}$ also includes a constant and age dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist} in the IV-probit specifications. The numbers shown in the table are marginal effects averaged across observations. Standard errors in parentheses are bootstrapped by 500 random draws with replacement clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.5: Working Hour Estimation Results

	Male		Female	
	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
Property Taxes (in 10,000)	36.4	-776.7	-164.9**	-3069.7
	(23.9)	(2044.0)	(80.8)	(2967.2)
Income Quintile 2	116.0***	98.2	142.7***	146.3**
	(24.9)	(62.6)	(44.2)	(53.7)
Income Quintile 3	168.9***	139.6*	178.3***	195.6***
	(29.6)	(81.2)	(46.0)	(63.0)
Income Quintile 4	198.6***	161.3	230.3***	241.2***
	(28.7)	(109.9)	(51.2)	(63.9)
Income Quintile 5	262.7***	262.3**	271.6***	348.6***
	(34.9)	(65.1)	(51.9)	(112.4)
House Value Quintile 2	-20.6	-2.5	-47.6	29.1
	(31.7)	(69.8)	(42.5)	(100.6)
House Value Quintile 3	-43.5*	16.1	-9.7	155.5
	(21.9)	(160.0)	(42.3)	(197.5)
House Value Quintile 4	-23.6	74.7	-45.8	212.9
	(31.1)	(270.3)	(37.4)	(289.5)
House Value Quintile 5	30.0	227.8	-78.3*	462.0
	(43.9)	(529.2)	(40.5)	(557.7)
Financial Wealth Quintile 2	- 35.2	25.9	-8.4	-28.3
	(22.9)	(37.5)	(31.4)	(35.8)
Financial Wealth Quintile 3	- 36.1	43.1	-25.6	-28.8
	(28.5)	(32.5)	(30.7)	(36.0)
Financial Wealth Quintile 4	26.5	32.0	-78.1**	-84.3**
	(34.0)	(37.3)	(31.7)	(39.0)
Financial Wealth Quintile 5	-0.5	45.4	-123.0**	-32.3
	(34.8)	(110.0)	(45.2)	(107.1)
Black	-86.8**	-94.7**	-30.6	-10.7
	(34.5)	(44.2)	(28.9)	(44.0)
Hispanic	-58.9	-62.9	-35.8	6.3
	(44.5)	(50.2)	(41.5)	(65.3)
Married	-22.8	-17.5	-201.7***	-219.1***
	(32.4)	(34.9)	(18.4)	(27.8)

Continued on next page

Table 3.5: Working Hour Estimation Results (*Continued*)

	Male		Female	
	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
High School Graduate	-17.0	-2.7	-15.0	3.9
	(24.3)	(33.2)	(37.1)	(37.0)
Some College	-6.4	-2.0	-13.0	17.8
	(31.1)	(27.1)	(47.0)	(55.6)
College Graduate	-35.2	-6.8	114.5**	194.4**
	(37.6)	(64.7)	(52.5)	(87.0)
Recently Hospitalized	-36.1	-46.6	5.2	4.1
	(25.2)	(43.6)	(29.9)	(38.5)
County Unemployment Rate	4.6	6.6	-8.9**	-15.3*
	(4.2)	(9.3)	(3.8)	(7.6)
First Stage F-stat		0.90		18.31
Hausman Test		813		2906
(coeff on first-stage residuals)		(1851)		(2713)
N	7,442	7,442	6,552	6,552
Pseudo R2	0.3289	.	0.3009	.

Notes: The regression model is $Hours_{ist} = \beta_3 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t + \epsilon_{ist}$. Other than the variables shown in the table, $\mathbf{X}_{ist}$ also includes a constant and age dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist} in the IV-probit specifications. The numbers shown in the table are marginal effects averaged across observations. Standard errors in parentheses are bootstrapped by 500 random draws with replacement clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.6: Robustness Checks of the Retirement Regression Analysis - Female Sample

	Original Sample (1)	Age 55-70 (2)	Drop CA (3)	Drop Self-Emp (4)	Drop Movers (5)
Property Tax (in 10,000s)	-1.3460	-2.1484*	-1.3017	-1.1614	-1.5251
	(1.1622)	(1.2251)	(1.0253)	(0.8116)	(1.0902)
First Stage F-stat	14.28	52.40	30.98	53.20	13.15
Hausman Test	5.4105***	9.1413***	4.9379***	4.4769***	6.6443***
	(1.5272)	(1.8524)	(1.4293)	(1.6544)	(2.4953)
N	5,657	4,173	5,016	4,895	5,159

Note: The regression model is Prob($Retire_{ist} = 1$) $= \Phi(\beta_1 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $\mathbf{X}_{ist}$ includes a constant, income quintile indicators, house value quintile indicators, financial wealth quintile indicators, race/ethnicity dummies, whether married, education categories, whether recently hospitalized, whether have pension coverage, whether have retiree health insurance coverage, county unemployment rate, industry dummies, occupation dummies, and age dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist}. The numbers shown in the table are marginal effects averaged across observations. Standard errors in parentheses are bootstrapped by 500 random draws with replacement clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.7: Robustness Check of the Reentry Regression Analysis - Male and Female Samples

	Original Sample	Age 55-70	Drop CA	Drop Self-Emp	Drop Movers
Male Sample	(1)	(2)	(3)	(4)	(5)
Property Tax (in 10,000s)	0.0581	-0.5875	0.1829	-0.0426	0.0666
	(0.4371)	(0.7952)	(0.4226)	(0.4412)	(0.5505)
First Stage F-stat	138.47	59.36	157.38	137.71	74.29
Hausman Test	-0.3136	4.1826	-1.3394	0.6739	-0.4483
	(2.4302)	(4.9770)	(2.2783)	(2.8774)	(3.1330)
N	6,475	4,396	5,684	5,879	5,848
Female Sample	(1)	(2)	(3)	(4)	(5)
Property Tax (in 10,000s)	-0.7037	-0.2826	-0.6094	-0.5013	-0.4970
	(0.4619)	(0.3991)	(0.4040)	(0.4391)	(0.4574)
First Stage F-stat	11.09	21.87	24.93	10.18	9.07
Hausman Test	5.2854	2.3178	4.4741	4.1369	3.8471
	(3.6295)	(4.6416)	(3.1770)	(4.4222)	(4.1965)
N	9,406	6,485	8,282	9,095	8,550

Note: The regression model is Prob($Reentry_{ist} = 1$) = $\Phi(\beta_2 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t)$. $\mathbf{X}_{ist}$ includes a constant, income quintile indicators, house value quintile indicators, financial wealth quintile indicators, race/ethnicity dummies, whether married, education categories, whether recently hospitalized, county unemployment rate, and age dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist}. The numbers shown in the table are marginal effects averaged across observations. Standard errors in parentheses are bootstrapped by 500 random draws with replacement clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.8: Robustness Check of the Working Hour Regression Analysis - Female Sample

	Original Sample (1)	Age 55-70 (2)	Drop CA (3)	Drop Self-Emp (4)	Drop Movers (5)
Property Tax (in 10,000s)	-3069.7	-3377.7	-3233.4	1075.9	-592.9
	(2967.2)	(2406.0)	(3045.4)	(927.3)	(3455.7)
First Stage F-stat	18.31	49.68	18.55	75.22	11.70
Hausman Test	2906	3193	3076	-1168	442
	(2713)	(2208)	(2696)	(949)	(3487)
N	6,552	4,810	5,812	5,647	5,997

Note: The regression model is $Hours_{ist} = \beta_3 Tax_{ist} + \mathbf{X}_{ist}\mathbf{\Pi} + \zeta_s + \delta_t + \epsilon_{ist}$. $\mathbf{X}_{ist}$ includes a constant, income quintile indicators, house value quintile indicators, financial wealth quintile indicators, race/ethnicity dummies, whether married, education categories, whether recently hospitalized, county unemployment rate, industry dummies, occupation dummies, and age dummies. ζ_s is state fixed effects. δ_t is year fixed effects. $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for Tax_{ist}. Standard errors in parentheses are clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.

Table 3.9: Property Tax Effect on Homeowners of Different Age Groups

	Retirement Female (1)	Reentry Male (2)	Reentry Female (3)	Hours Female (4)
	IV-Probit	IV-Probit	IV-Probit	2SLS
PropTax*(Age<60)	0.0422	0.1188	0.9829	-219.2
	(0 .9843)	(0.6499)	(0.7588)	(2093.0)
PropTax*(Age=60)	2.1577	-0.2512	-1.2467	-2989.1
	(1.5298)	(1.3140)	(4.0628)	(3275.2)
PropTax*(Age=61-62)	-0.6278	0.4428	-2.8188**	-3230.7
	(1.0570)	(1.4484)	(1.3672)	(3567.4)
PropTax*(Age=63-65)	1.2753	0.6301	0.1007	1648.4
	(1.7913)	(1.6568)	(1.3454)	(2679.0)
PropTax*(Age>65)	-0.5854	0.1884	-0.4449	254.6
	(0.7493)	(0.7787)	(0.9967)	(1054.7)
N	5,657	6,475	9,406	6,552

Note: Other controls include a constant, income quintile indicators, house value quintile indicators, financial wealth quintile indicators, race/ethnicity dummies, whether married, education categories, whether recently hospitalized, whether have pension coverage, whether have retiree health insurance coverage, county unemployment rate, industry dummies, occupation dummies, and age dummies, state fixed effects, and year fixed effects. The interactions between the five age groups and $\widetilde{Benefits}_{ist}$, $ValueFreeze_{ist}$, and $TaxFreeze_{ist}$ are used as instruments for the interactions between the five age groups and property taxes. Standard errors in parentheses are clustered at state level. Individual weights from HRS are applied. * significant at 0.10 level, ** significant at 0.05 level, *** significant at 0.01 level.