2016

Essays On Retirement, Labor Supply, And Living Arrangements

Anudeep Gill
University of South Carolina

ESSAYS ON RETIREMENT, LABOR SUPPLY, AND LIVING ARRANGEMENTS

by

Anudeep Gill

Bachelor of Arts
Whitman College, 2006

Master of Arts
Binghamton University, 2010

Submitted in Partial Fulfillment of the Requirements

For the Degree of Doctor of Philosophy in

Economics

Darla Moore School of Business

University of South Carolina

2016

Accepted by:

McKinley L. Blackburn, Major Professor

Orgul D. Ozturk, Committee Member

Daniel B. Jones, Committee Member

Gregory R. Niehaus, Committee Member

Cheryl L. Addy, Vice Provost and Dean of the Graduate School

ABSTRACT

Three changes in the U.S. Social Security program affected recent cohorts of older individuals: repeal of the earnings test, increases in the normal retirement age, and increases in the delayed retirement credit. All three policy changes were expected to affect work decisions for those eligible for social security benefits. The first two chapters of my dissertation assess the impact of these policy changes. In the first chapter, using data from the Survey of Income Program and Participation (SIPP) for the years 1996-2013, I study the influence of the three policy changes on the retirement and benefit-claiming decisions of older men and women. There is minimal evidence in the recent literature investigating the sensitivity of estimated responses to the definition of retirement. In particular, I explore how responses to the policy changes differ when assessed using an objective relative to a subjective definition of retirement. The evidence from the empirical analysis indicates that the response to these policy changes is partly sensitive to the retirement definition. I find no effect of recent policy changes on retirement based on a labor force definition, while changes in the delayed retirement credit reduce self-reported retirement among men and women above the normal retirement age. There is stronger evidence of an effect of recent policy changes on the claiming behavior of older individuals. The earnings test repeal raised claiming among men above the normal retirement age, and changes in the delayed retirement credit reduced claiming among men and women who are directly affected.

The second chapter examines the influence of the earnings test on the labor supply decisions of older workers. In analyzing the labor supply response to the earnings test, previous researchers have used a static labor supply framework and ignored the possible influence of the delayed retirement credit adjustments in mitigating the earnings test penalty. In contrast to the

past research, I assess the labor supply response to the earnings test within a life-cycle model that accounts for the delayed retirement credit adjustments. Using data from the SIPP covering years 1996-2013, I consider the effect on both labor force participation and hours of work. Two potential responses to the policy changes have been relatively less studied: the intertemporal response by individuals and the differential response by sub-groups of individuals who possibly face liquidity constraints or misunderstand the rules of the earnings test. In my work, I provide evidence on both these responses. I find evidence in support of the view that both men and women perceive the earnings test as a tax, as older men and women above the normal retirement age and below age 70 respond to the earnings test by reducing their labor force participation. For working women, I also observe a reduction in the hours of work in response to the earning test, which indicates the somewhat greater flexibility in the choice of hours of work that may be available to women relative to men.

The third chapter draws on reports of a rise in the share of young people living with their parents during and following the period after the 2007-2009 recession in the U.S. Previous research has analyzed the relationship between changes in economic conditions and living arrangements of young people by focusing on data based on infrequent (annual/biennial) interviews. In my empirical analysis I use high frequency SIPP data for the years 1996-2013. I examine the change in a young adult's likelihood of living with a parent in response to changes in local labor-market conditions, and further identify the breakdown of this change into young adults returning home versus their changing their tendency to leave the parental home. Unlike, previous researchers I find no robust evidence that poor labor markets conditions affect the living arrangements of young adults.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

THE EFFECT OF RECENT CHANGES IN SOCIAL SECURITY ON RETIREMENT AND CLAIMING DECISIONS OF OLDER WORKERS

1.1. INTRODUCTION

Confronted with the persistent decline in the labor force participation rate of older men and challenges facing the long term solvency of the social security program, the Social Security Reform Act of 1983 introduced important changes in the program to address these problems. Two of these changes – increases in the normal retirement age (NRA) and the delayed retirement credit (DRC) – have affected recent cohorts of older individuals. An increase in the normal retirement age implies a reduction in the social security benefits, while an increase in the delayed retirement credit raises the reward for delaying the receipt of benefits for individuals above their NRA. In an unanticipated announcement in 2000, President Clinton repealed the earnings test, an unpopular tax on the earnings of older individuals who claimed benefits while earning above a certain threshold amount. All three policy changes were adopted to encourage work. Changes in the normal retirement age and the delayed retirement credit encouraged a delay in the receipt of benefits as well. The effectiveness of these policy changes in modifying the retirement and claiming behavior of older individuals, depends on how individuals respond to the different incentives provided by the social security system.

Earlier research examining the effect of social security incentives on the retirement behavior of older men was spurred by the negative relationship between the rapidly rising level of social security benefits (and coverage) and the persistent decline in the labor force participation of older men observed in the 1950-1980 period. In a review essay, Mitchell and Fields (1982) note the inconclusive evidence provided by this early research. Even within studies that find evidence of a significant impact of the rise in social security benefits in raising the probability of retirement, there is large disagreement on the magnitude of this impact. Krueger and Pischke (1992) critique the early literature, noting the lack of exogenous variation that marred the cross-section studies, and the inability to distinguish the effect of social security benefits from other variables that trended over time and may have affected older individuals' labor force participation. They make use of an exogenous reduction in the social security benefits resulting from a 1977 reform to identify the effect of social security. They find no evidence that reducing social security benefits slows down the trend towards earlier retirement. Based on these findings they conclude that the observed negative relationship between benefit levels and labor force participation is due to factors other than increasing social security wealth.

By the mid-1980s the labor force participation rate of older men had levelled off, and since the late 1990s it has been rising. Several changes took place over this period that may have contributed to the recent trends. The 1986 Amendment to the Age Discrimination in Employment Act eliminated mandatory retirement at all ages. The 1980s were also a period when many companies adopted defined contributions plans which do not provide the strong incentives to retire at a particular age that were a feature of defined benefits plans. Moreover, the share of companies offering retiree health insurance declined over this period. Beginning in 1990, the social security earnings test tax rate was lowered for those above the normal retirement age and the delayed retirement credit started rising.

Recent research attempts to assess the impact of the latest changes in social security program: the repeal in the earnings test, the rising normal retirement age and the delayed retirement credit on the rising labor force participation among older men and women. Using aggregate CPS data, Gruber and Orszag (2003) examine the impact of the loosening of the earnings test on work and claiming decisions. They find no evidence of a change in the earnings test parameters on the work decision, but a significant increase in the claiming behavior of both older men and women. Blau and Goodstein (2010), create an aggregate synthetic panel data using CPS, SIPP and SSA files find that the increases in the NRA and the DRC explain 25-50 percent of the recent rise in the labor force participation of older men. Using CPS data, to focus on individuals below their normal retirement age (ages 61-65) Mastrobuoni (2009) finds a strong response to the increases in the normal retirement age.

There is minimal evidence in the recent literature assessing the sensitivity of the response to the social security policy changes to the retirement definition used. In light of past research on the importance of divergent pathways into retirement chosen by older individuals, and the inability of objective definitions to account for discouraged workers, self-reported retirement status of individuals may provide useful insight into older workers response to the policy changes. As noted by Gruber and Orszag, there is sparse recent (or past) research on the response of the work decision of older workers to the earnings test repeal in the presence of labor market rigidities. Moreover, relatively few recent studies examine the response of older women to the social security policy changes, even as the labor force participation of older women continues to rise. Blau and Goodstein's study assessed only an impact of the increase in the NRA functioning through financial incentives, so they are unable to account for any norm related effects associated with the NRA changes. Mastrobuoni assesses the total effect, both the financial and any norm related effects of the NRA changes, but is unable to disentangle the two. With the availability of finer data that provides information on the birth year of the affected individuals, it is possible to separately identify the financial and norm related effects arising from the NRA changes.

I assess the impact of the latest changes in the social security policies on the retirement and claiming behavior of older men and women using individual level data from the Survey of Income Program and Participation (SIPP). I find no evidence of an impact of the policy changes on the retirement behavior when retirement is assessed using an objectively defined criterion of retirement from the labor force. The changes in the delayed retirement credit, exert a statistically significant large impact on reducing the probability of self-reported retirement among men and women above their normal retirement age. The earnings test repeal raises the probability of claiming benefits among men. The increase in the normal retirement age raises claiming among men through norm effects only; women do not respond to these norm effects.

1.2. BACKGROUND AND THEORETICAL PREDICTIONS

Two of the recent policy changes – increases in the normal retirement age and increases in the delayed retirement credit – were introduced as part of the 1983 Reform, while the most recent changes in the earnings test were unanticipated. The extent of these changes and their potential impact on retirement behavior is illuminated when viewed in the context of the major historical changes in these policies since the inception of the Social Security program. I first briefly describe the changes in the three policies over the course of time, and then discuss the predictions for the impact of these changes on the retirement age.

1.2.1. BACKGROUND AND RECENT CHANGES IN SOCIAL SECURITY

Normal Retirement Age (NRA): The size of the monthly Social Security benefit an insured worker receives depends on the age the benefit is first claimed. A worker is considered insured if forty or more quarters of his earnings are covered under Social Security.[1] The Social Security Act of 1935 set the normal retirement age for both men and women at 65; it is the minimum age at which an insured individual can claim full or unreduced benefits based on his earnings history, equivalent to hundred percent of his Primary Insurance Amount (PIA).[2] Later amendments introduced the option of claiming

[1] Throughout the paper I focus on workers who claim benefits as primary insurer. A primary insurer receives benefits based on his own past earnings record. Dependent or survivor beneficiaries collect benefits as secondary insurers based on the earnings record of a primary insurer.

[2] The monthly benefit for a worker is calculated in three steps. The first step is to index the annual taxable earnings of a worker to the national average wage index; the indexation is done to reflect the real value of the past earnings relative to the year in which the worker turns age 60 (two years prior to age of first eligibility, which under current law is age 62). From these indexed earnings, the highest 35 years of earnings between ages 21 and 62 are chosen to compute the Average Indexed Monthly Earnings (AIME). The second step is to compute the workers' Primary Insurance Amount (PIA) from the AIME; the PIA is the full benefit an individual is entitled to if he claims at the normal retirement age. The progressive nature of the formula used to compute the PIA from the AIME results in a decline in the ratio at which the monthly benefit (PIA) replaces the workers' past average earnings (AIME) as the past earnings history (AIME) of the worker rises. The formula uses two bend points which are determined based on the year in which the worker turns 62. The third step is to compute the final benefit amount received by the worker; this amount could be higher or lower than the PIA depending on the age at which the benefit is first claimed.

reduced benefits below the normal retirement age at the early retirement age of 62; this option was provided to women in 1956 and men in 1961.

Reduced benefits can be claimed at any age between the early retirement age of 62 and the normal retirement age (NRA). For each year that an individual claims benefits below his NRA of 65, benefits are reduced by 6.67 percent of the full benefit amount available at the normal retirement age. [3] Early claiming can affect lifetime benefits of an individual in two ways: first, it results in a lower monthly benefit amount received by the individual relative to those available at the NRA, and second, the benefits are received for a longer duration than if claimed later.[4] Above the NRA, under current law an individual can choose to delay the receipt of benefits until age 70 and receive a Delayed Retirement Credit (DRC). This delayed credit is awarded to increase the benefit level for postponement beyond the normal retirement age. I describe the DRC in detail below.

The Social Security Amendments of 1983 raised the normal retirement age for individuals' born in 1938 and later from 65 to 67, while leaving the early retirement age unchanged at 62. The rise in the NRA is slated to be phased in over a twenty-two-year period. For birth cohorts 1938-43 the NRA was raised by 2-month increments, until it reached 66 for individuals' born in 1943; the normal retirement age will remain at 66 for

[3] Alternatively, for each year after age 62 and before 65 (NRA) that benefit claiming is delayed, the age 62 benefit amount is increased by 6.67 percent. This adjustment of the benefit amount for postponement of benefits beyond the early retirement age and below the NRA is called actuarial adjustment.

[4] The incentive to claim early is impacted by individuals' life expectancy and discount rate: a shorter life expectancy creates a stronger incentive to claim early because benefits are received for a shorter duration, while a higher discount rate also creates a stronger incentive to claim early because benefits received today are valued more highly. Other factors affecting the decision to claim early include: interest rates, risk aversion, and borrowing constraints. I describe these in more detail later.

birth cohorts 1943-54 and rise again at 2-month intervals for those born after 1954 until it reaches 67. The normal retirement age for birth years 1938-54 is listed in Table 1.1. The first cohort to be impacted by an increase in the NRA attained age 62 in 2000.

An increase in the normal retirement age implies a reduction in current benefits at all ages above 62. When the normal retirement age rises above 65, there is a greater reduction in the current benefits if claimed below the normal retirement age. Specifically, current benefits are reduced by 6.67 percent per year when claimed up to 3 years before the NRA, and 5 percent per year when claimed in excess of 3 years prior to the NRA. Above the normal retirement age, change in the benefit levels is determined by the delayed retirement credit.

Delayed Retirement Credit (DRC): The original Social Security Act of 1935 did not include a provision for increasing the benefits for individuals' postponing benefit receipt beyond the normal retirement age. Benefits not claimed following the attainment of normal retirement age were essentially lost. To compensate individuals' forfeiting benefits under the original system, the 1972 Amendments provided a delayed retirement credit which increased all subsequent benefits by 1 percent for each year in which benefits are postponed beyond the normal retirement age up to age 72 (which was later lowered to age 70). The credit applied to delayed benefits was raised to a yearly rate of 3 percent in 1977.

At an annual rate of 1 or 3 percent the delayed retirement credit adjustments were considered actuarially unfair, because the adjustments did not fully compensate for the loss in present value of future streams of benefits (Social Security Wealth) arising from the delayed receipt.[5]Advancing the system towards actuarial fairness, the 1983 Amendments included a gradual increase in the delayed retirement credit beginning in 1990 and continuing until 2009 when the yearly rate of delayed retirement credit adjustments reaches 8 percent. The DRC increases were assigned by birth year and implemented every other year for individuals' born in odd years between 1925-43. The delayed retirement credit per year ranges between 3-8 percent for birth years 1921-43 and later, is listed in Table 1.2. All cohorts born after 1943 have a DRC of 8 percent. At an annual rate of 8 percent per year the DRC is considered to be more than actuarially fair for the average individual.[6]

Retirement Earnings Test (RET): The retirement earnings test has been a controversial feature of the Social Security program since its inception. The 1935 Act included a very restrictive criterion under which an individual at or above his NRA could qualify for benefits: no earnings in regular employment. Any positive earnings for all individuals above age 65 resulted in a complete loss of current benefits, with no compensations made at a later date. The earnings test has been modified numerous times. These modifications have been along four dimensions: the age range over which the earnings test is binding, the earnings amount under covered employment that is exempt

[5] Burkhauser and Turner (1981) and Diamond and Gruber (1999).

[6] An actuarially fair rate for the average individual may still be considered unattractive for some. This will depend on an individual's: life expectancy, discount rate, level of risk aversion, borrowing constraints, interest rates, and marital status. Coile, et al. (2002) note that for a given level of delayed retirement credit, incentives to delay are stronger among those with: longer life expectancy, higher level of risk aversion when the real annuity value of social security benefits is taken into account, and among married individuals when the value of spousal and survivor benefits is taken into account. Shoven and Slavov (2014) stress the importance of interest rates in determining the actuarial fairness of adjustments for the average individual: lower interest rates raise the present value of gains from delaying benefits. In their empirical work, however, they do not find evidence that individuals' actual claiming behavior is influenced by lower interest rates.

from the earnings test (earnings test threshold); the rate at which benefits above the exempt amount are reduced (tax rate); and the actuarial adjustments made to benefits that are lost or postponed under the earnings test. Table 1.3 summarizes the recent changes in the annual earnings test threshold amount and tax rate which are described in detail below.

The original retirement earnings test applied to all individuals above age 65, but since then the age range has altered four times narrowing the scope of the earnings test. The Amendments of 1951 and 1954 exempted all individuals above age 75, and 72, respectively, from the earnings test; the exemption implies that individuals above age 75 (or later 72) could claim their full benefits while remaining employed. With the introduction of the early retirement age for men in 1961, the earnings test was binding at ages 62-71 until the 1983 reform removed the test for those at and above age 70. The most recent change to the age bracket over which the RET applies became effective in 2000, when the earnings test was eliminated for ages at and above the NRA (ages NRA-69). After 2000, the earnings test applies to all individuals between ages 62 and less than their NRA; there is a looser earnings test that applies in the year an individual attains his NRA.[7]

An eligibility condition that requires complete withdrawal from employment with no later compensations for foregone benefits deters work at older ages. The 1939

[7] In the year an individual attains his NRA, the earnings test exempt amount is higher and the tax rate is lower than for those age 62 and less than their NRA. For instance, someone born in June 1944 has a NRA of 66, and turns his NRA on June 2010. If the individual is working between June 2006 and December 2009, he faces the stricter earnings test that applies to those between ages 62 and less than their NRA. Beginning in January 2010 (the year he attains his NRA), he faces the looser earnings test until the month he is at his NRA.

Amendment introduced an exempt amount (Earnings Test Threshold), which is an amount that serves as an upper limit up to which a worker can earn under Social Security covered employment and receive full benefits.[8] The initial exempt amount was set at $15 per month (25 percent of the monthly minimum wage in 1939). Over the years the earnings test threshold was raised several times on an ad hoc basis until 1972, when it was indexed to increases in the national average wage index. After the introduction of the early retirement age, the same exempt amount applied to all age ranges under the earnings test. The 1977 Amendment, however, separated the threshold level for those below and above the normal retirement age; the threshold level for ages 62 to less than normal retirement age was set at a lower amount than for ages at and above normal retirement age. The most recent changes in the earnings test threshold level were made in 1996; these changes introduced ad hoc increases in the exempt amount of individuals (in real terms, above the increases due to indexation to average wage level) at and above the NRA. The higher threshold levels relax the earnings test for those working at and above the NRA, so more workers will have earnings that fall below the higher threshold and can claim full benefits while working.

Workers with earnings above the threshold level receive reduced current benefits if they claim while working; in other words their earnings were taxed. The1960 Amendment lowered the rate at which benefits were reduced for earnings in excess of the threshold level by setting two tax rates; if the earnings were above the threshold amount but below another higher upper limit on earnings then benefits were reduced by 50

[8] Under the earnings test, the earnings are defined as wages under covered employment and self-employment. Payments from pensions, interest, dividend, and other unearned income are not included in earnings subject to the earnings test.

percent of earnings ($1 for every $2 of earnings) but for earnings above the higher upper limit benefits were reduced by 100 percent of earnings. A later Amendment in 1973 removed the 100 percent tax rate on earnings extending the 50 percent tax rate for all earnings above the threshold. These changes lowered the penalty in terms of current benefits lost by workers for earning above the threshold. Until 1990, all individuals covered by the earnings test (below, at, and above NRA) faced the same tax rate on earnings. The 1983 Amendment, which became effective in 1990, lowered the tax rate for individuals at and above the NRA to approximately 33 percent of earnings while leaving the tax rate unchanged at 50 percent for those below the NRA. After the 2000 repeal of the earnings test individuals below the normal retirement age continue to have their earnings taxed at 50 percent, while in the year an individual attains his normal retirement age the tax rate is reduced to 33 percent.

Current benefits lost to the earnings test either due to postponement of receipt while working or lower current benefits received while continuing to work are compensated at a later date through an increase in future benefits. Benefits lost to earnings test below the NRA are compensated at an annual rate of 6.67 percent, and benefits lost to earnings test above the NRA are adjusted by applying the DRC.

1.2.2. LIFE-CYCLE PREDICTIONS OF RECENT CHANGES IN THE LAW

To understand how changes in the normal retirement age, the delayed retirement credit and the earnings test should affect the retirement decision, it is useful to think of the decision problem facing an individual in a life-cycle context. In this section I present a stylized model of intertemporal utility maximization that serves two purposes. First it describes the decision problem faced by an older worker and second it makes predictions about the response of the retirement age to changes in Social Security rules.[9]

A far sighted individual who begins planning his retirement with the aim of maximizing his lifetime utility by choosing a retirement age z, is faced with the choice between working another year with benefits postponed and retiring now along with benefit receipt (this year or age 62). At every age in the foreseeable future, the individual chooses whether to retire or not by evaluating his utility in the two states: work and retirement. The state which provides the highest utility is chosen. Retirement is an absorbing state. In this simple model, with no uncertainty the individual is assumed to face no borrowing constraints and to have perfect foresight regarding the length of his life. For simplicity it is assumed that there are only two sources of income: earnings and social security benefits. The utility function is additively separable in consumption and leisure.

Given the above setup, an older worker's decision problem can be summarized as follows:

$$\max_{z\ ,\, C_t} V(z) = \int_0^z e^{-\delta t}\, U_W(C_t)dt + \int_z^D e^{-\delta t}\ U_R(C_t)dt \tag{1}$$

s.t.

[9] The model presented here uses the same approach to solving the intertemporal utility problem as followed by Colombino (2003) and Mastrobuoni (2006). Mastrobuoni derives the predictions arising from an increase in the normal retirement age only. I extend these results to changes in the delayed retirement credit.

$$\int_0^D e^{-rt}\, C_t\, dt = \int_0^z e^{-rt}\, W_t\, dt + \int_z^D e^{-rt}\, B_t(z, NRA, g, W) dt \qquad (2)$$

where U_W is the instantaneous utility of a working individual at time t, U_R is the instantaneous utility of a retired person at time t given that he retired in year z, C_t is the consumption at time t, δ is the discount rate, D is the (known) date of death, r is the interest rate, and B_t is the level of social security benefits an individual receives upon retirement ($t \geq z$). The discount rate is set equal to the interest rate, δ=r and real earnings W_t are assumed to be constant over the working life W_t=W, as these assumptions simplify the analysis. I also introduce a term ε that captures the disutility from work $U_W = U_R - \varepsilon$. To obtain interior solutions, the utility function is assumed to be logarithmic. The individuals' objective is to maximize lifetime utility (1) with respect to z and C_t subject to the budget constraint (2).

The size of the social security benefits, B_t, a worker receives upon retirement is a function of the workers' past earnings R (W), the age at which the benefits are claimed z, and the social security policy variables g and NRA.[10]

$$B_t(z, NRA, g, W) = R(W)\,[1 + g(z - NRA)] \qquad (3)$$

The term (z-NRA) captures the change in the benefit level based on whether the benefits are claimed at, before, or above the NRA. The size of benefits can change with each year of delayed retirement through adjustments made to benefits when they are postponed.[11]

[10] I do not account for COLA (cost of living adjustments) adjustments made to benefits. The size of social security benefits an individual receives at any age t, such that t>z, is the same as the size of social security benefits received at age of retirement z, when benefits are first claimed.

[11] Benefits can be adjusted for two reasons: through benefit recomputation as higher earnings in years after an individual turns age 62 replace years of previous lower earnings used to compute

The variable g, captures the actuarial adjustments applied to benefits. To simplify the analysis, g the size of the actuarial adjustments is the same for individuals below and above the NRA.[12]

Under the above assumptions, Colombino (2003) shows that the necessary condition for utility maximization can be expressed as a static comparison between the instantaneous utility from work at age z and the instantaneous utility from retirement minus the opportunity cost of retiring at age z.

$$U_{Wz}(C_z) = U_{Rz}(C_z) - Y(z)$$

where $Y(z) = \mu \left[W - B_z(z) + \int_z^D e^{r(z-t)} \frac{\partial B_t(z)}{\partial z} dt \right]$ and μ is the Lagrange multiplier.

The term Y (z) captures the loss in utility when the individual chooses not to postpone retirement by one more year. This loss is given by the sum of the costs and benefits associated with retiring at age z which includes: the earnings, (W), foregone upon retirement, the social security benefits received if the individual retires at age z, B_z (z), and the foregone future gain in social security benefits that would have resulted from postponing benefits by one additional year. The above mathematical formalization of the problem yields two predictions (upon the satisfaction of certain conditions) that are derived in the appendix and summarized below.

First, an increase in the normal retirement age increases the optimal retirement age.

---

the AIME and through adjustments made to benefits for postponement of receipt. In the analysis that follows, I do not account for benefits adjustment through automatic benefit recomputation.

[12] To derive predictions for the impact of changes in the DRC, I let g vary by focusing on the case only when z>NRA.

$$\frac{dz}{dNRA} = \frac{gR[-\varepsilon(e^{-rz} - e^{-rD}) - 1 + e^{-rD}]}{\Delta}$$

where $\Delta = gR\left[\left(1 + e^{-r(-z+D)}\right)(-1 + e^{-rD}) + \varepsilon e^{-rz}\left(r(z - NRA) + e^{r(z-D)} - 1\right)\right]$

$$- \varepsilon r e^{-rz}(W - R)$$

The numerator of $\frac{dz}{dNRA}$ is always negative, so the sign of the derivative depends on the sign of Δ.

For individuals retiring before their normal retirement age, an increase in the NRA always results in an increase in the optimal retirement age. But, among individuals who are above their NRA, an increase in the normal retirement age increases the optimal age of retirement only if the date of death, D, is not close to the age of retirement and the interest rate is between (0,10) percent.

If $\Delta < 0$, then $\frac{dz}{dNRA} > 0$

Second, an increase in the delayed retirement credit has an ambiguous effect on the optimal retirement age of those above the NRA.

$$\frac{dz}{dg} = \frac{R[(\varepsilon(e^{-rz} - e^{-rD}) + 1 - e^{-rD})(z - NRA) - \frac{1}{r}\left(1 - e^{r(z-D)}\right)(1 - e^{-rD})]}{\Delta}$$

If $\Delta < 0$, then the sign of $\frac{dz}{dg} \gtrless 0$ is ambiguous among individuals retiring above their NRA.

In the mathematical model, I adopted many simplifying assumptions some of which I relax in the discussion that follows. Following Mastrobuoni (2006), I assume in equation (3) a very straightforward relationship between the size of benefits and the actuarial adjustments. This formalization of the actuarial adjustments makes no distinction regarding the differential effect that an actuarially fair relative to an actuarially unfair adjustment has on the retirement decision. This specification of the actuarial adjustments also assumes that the adjustments are the same for all individuals above age 62. Moreover, many older workers face borrowing constraints. In the presence of these borrowing constraints and actuarially unfair adjustments, the presence of the earnings test impacts the retirement decision if the older worker faces a discontinuous static budget constraint. Without formally introducing these intricacies in the mathematical model, I will instead discuss intuitively how the complex incentives provided by the social security benefit calculation formula impact the retirement decision at different ages.

I begin with a graphical description of the consumption–retirement age tradeoff faced by an individual (Fig. 1.1) that highlights three things: the impact of a change in the normal retirement age; changes in the delayed retirement credit; and how the tradeoff changes based on the actuarial fairness of the credits. The slope expresses the tradeoff between annual consumption per year in postretirement years and the length of time spent in retirement for each possible age of retirement between ages 60 and 70.[13] For each year that an individual postpones retirement beyond age 62 his benefits are automatically

[13] Whether the slope of the tradeoff between ages 62-NRA is steeper or flatter relative to slope between ages 60-62 depends on the actuarial fairness of the adjustments: for the average individual the slope is steeper between ages 62-NRA, but if actuarial adjustments are unfair then this slope is flatter relative to the one before age 62.

adjusted. The tradeoff between annual consumption and age of retirement is impacted by the actuarial fairness of the adjustments applied to forgone benefits. Fig. 1.1 is drawn under the assumption of zero interest and discount rates, perfect capital markets, and with no uncertainty regarding age of death.

The three budget constraints shown in Fig. 1.1 highlight how the consumption – retirement age tradeoff changes with the normal retirement age and the delayed retirement credit applied to postponed benefits. Birth cohorts 1917-24 faced a normal retirement age of 65 and an annual delayed retirement credit of 3 percent. These DRC adjustments are considered actuarially unfair. An actuarially unfair delayed retirement credit affects the tradeoff by introducing a convex kink at the NRA; at every age beyond the NRA there is a decline in the rate of growth of lifetime benefits. In other words, the flatter slope of the constraint to the right of the kink reflects that upward adjustments to future benefits do not completely make up for the loss of benefits after the normal retirement age. At an actuarially neutral rate of DRC, there is no kink in the budget constraint, as reflected by the tradeoff for birth cohort 1937 (NRA is 65, and DRC is 6.5 percent). Delayed retirement credit adjustments at an annual rate of 8 percent are considered more than actuarially fair for the average individual. A more than actuarially fair adjustment introduces a non-convex kink at the normal retirement age as seen in the budget constraint facing an average individual in the birth cohort 1943-54 (NRA is 66, and DRC is 8 percent).

To intuitively derive the theoretical predictions for the impact on retirement age of the recent policy changes I evaluate each policy change in isolation (holding constant all other policies). The theoretical predictions of the life-cycle model arising from the recent changes in social security law are summarized in Tables 1.4 A and B.

Changes only in Normal retirement age: Changes in the normal retirement age affect the retirement decision through their impact on anticipated lifetime wealth. In the life-cycle context, the present value of the stream of benefits an individual expects to receive by retiring at any given age (Social Security Wealth), can be thought of as providing increments to the anticipated lifetime wealth. Fig. 1.1 shows the impact of a change only in the NRA and a change in the DRC. Between ages 60-65, we see an impact of a change in the normal retirement age (only, no change in the DRC) for birth cohorts 1943-54 relative to that of cohorts 1917-24 and 37. While between ages 65-70, we see the impact of changing both the DRC and the NRA for the affected cohorts. An increase in only the normal retirement age causes a parallel downward shift in the constraint, encouraging later retirement at all ages due to an income effect if leisure is assumed to be a normal good.[14]

Changes only in Delayed Retirement Credit: Recent increases in the delayed retirement credit affect the retirement decision of those working beyond their NRA in two ways: by changing their tradeoff; and increasing their social security wealth. First, a higher delayed retirement credit raises the tradeoff between retiring today and continuing work for another year while postponing benefits; it increases the return to postponement

[14] Between ages 62-63, birth cohorts with a normal retirement age of 66 have an actuarial adjustment of 5 percent instead of the 6.67 percent. Individuals between these ages, thus, experience two opposing effects on their retirement age as a result of an increase in the NRA: a decline in their SSW (negative income effect) and a decline in the return to an additional year of work (negative substitution effect). The net effect on the retirement age of those between ages 62-63 is, thus, ambiguous.

of benefits by making retirement leisure more expensive (substitution effect), thus encouraging workers to postpone retirement. Second, the value of anticipated SSW depends on the assumptions regarding the DRC an individual expects to receive; an increase in the DRC increases the SSW of a worker, encouraging earlier retirement through an income effect.[15] Changes in the delayed retirement credit, thus, have an ambiguous effect on retirement age for those between ages 65-70.

In the life-cycle model in the presence of the earnings test, increases in the delayed retirement credit also change the net wage workers expected to earn above their normal retirement age. Earnings above the earning test threshold amount are taxed and returned later by increasing benefits through actuarial adjustments. The actuarial adjustment, thus, impacts the net wage an individual expects to earn over different periods of his life. As the delayed retirement credit rises to 8 percent per year, it raises the relative wage workers expect to earn. Realizing these changes, individuals might wish to increase their work effort more at ages above the normal retirement age while reducing work below the normal retirement age due to increases in the reward for working at older ages. The direct effect of an increase in the delayed retirement credit on ages below the normal retirement age will be to lower the retirement age. These individuals may leave work earlier, with the expectation to return at a later age when they can earn a higher return on their work. If, however, there are large labor force entry and exit costs (transition costs) then I expect spillover effects to arise among those below the normal

[15] As the delayed retirement credit rises, individuals planning to retire at the normal retirement age experience only an increase in the tradeoff they face (substitution effect).

retirement age. These spillover effects may lead younger workers to stay at work, so they can avail the higher net wage at older ages.[16]

Changes only in the Retirement Earnings Test: Whether the earnings test impacts the retirement decision depends on two factors: if the earnings test is a tax on earnings in the life-cycle perspective; and if there are discontinuities in the static labor – leisure budget constraint facing an individual.[17] The earnings test is a tax altering the net wage in the life-cycle perspective only if the actuarial adjustments are unfair and/or if workers are unware of the actuarial adjustments applied to withheld benefits. Researchers in the past have debated the actuarial fairness of these adjustments. Blinder, Gordon, and Wise (1980) showed that the actuarial adjustments applied between age 62 and 65 (normal retirement age) are actuarially fair while at an annual rate of 1 percent the DRC adjustments applied above NRA were actuarially unfair.[18] They contend that the earnings test is irrelevant for individuals below the normal retirement age because an actuarially fair adjustment does not cause social security wealth to fall.[19] Regardless of the actuarial fairness of these adjustments the earnings test is a tax in the life-cycle context if many older workers are unware of the adjustments made to benefits lost to the earnings test. Friedberg (1998) and Gruber and Orszag (2003) ignore the DRC adjustments applied to benefits foregone to the earnings test under the assumption that most older workers are unware of these adjustments and view the earnings test as a pure tax.

[16] Workers might lose their skill or their skills might be outdated; there may be large search costs.
[17] Reimers and Honig (1993, 1996).
[18] They find that the actuarial adjustments are fair even for individuals above age 65 when automatic benefit recomputation is taken into account. This recomputation increases benefits by replacing lowest earnings in an individuals' earnings history with higher earnings in later years.
[19] Burkhauser and Turner (1981) show that the actuarial fairness of adjustments for workers between ages 62-65 is very sensitive to the choice of interest rates (real versus nominal).

The standard life-cycle model implicitly assumes the presence of perfect capital markets, risk neutrality, and certainty of death. If the actuarial adjustments are fair; in the presence of perfect capital markets the earnings test is not a tax because individuals can simply transfer purchasing power over time by borrowing and lending. This assumption of free access to borrowing and lending is, however, untenable if a majority of older individuals face borrowing constraints.[20] In the presence of liquidity constraints, risk aversion, high interest rates or lower than average life expectancy, individuals above 62 will perceive the earnings test as a tax.

The presence of the earnings test with actuarially unfair adjustments impacts the retirement decision only if there are rigidities in the labor market preventing the worker from participating. If the budget constraint facing an individual choosing his labor supply is continuous, then the presence of an earnings test will not impact his decision to work. An individual who would have chosen to work in the absence of the earnings test, will choose to work even in its presence if the budget constraint is continuous by limiting his hours of work below the threshold level so as to avoid the tax on his earnings. The budget constraint, however, is discontinuous if there are constraints on minimum hours of work, few available part time jobs, and large fixed costs of working.[21] In Fig. 1.2A the presence of minimum hours constraints or scarcity of acceptable part time jobs gives rise to gaps in the budget constraints if some hours and wage combinations are unavailable to the worker.[22] If an individual who would have chosen to work at point H2 in the absence

[20] Diamond and Hausman (1984b) and Kahn (1988).
[21] Hurd (1996) notes the presence of minimum hours constraints in jobs for older workers due to productivity gain from team production and high fixed costs borne by the employers.
[22] Figure 1.2 A and B are taken from Reimers and Honig (1993)

of the earnings test is unable to find an hour wage combination that will allow him to earn below the threshold, in the presence of the earnings test he may choose to retire.

Fixed costs of work create a discontinuity at zero hours in the budget constraint as shown in Fig. 1.2B. High fixed costs can impact the retirement decision by shifting the budget constraint down and imposing a restriction on the minimum number of hours an individual must work to cover his fixed costs. If the fixed costs are very large and if in the absence of an earning test the individual would have chosen to work above the earnings test threshold; in the presence of the earnings test, these large fixed costs can enhance the possibility of earlier retirement.

An increase in the threshold amount only raises the optimal retirement age of the affected individuals, while an increase in the tax rate lowers it. A higher threshold level increases the likelihood of finding jobs below the exempt amount (that can also cover the high fixed costs), thereby allowing workers who face a discontinuous budget constraint to avoid the tax on their earnings while continuing to work. The tax rate for individuals below the NRA is 50 percent while for those above (before the 2000 repeal) was approximately 33 percent. After the 2000 repeal all individuals above age 65 and below their new NRA saw an increase in their tax rate from 33 percent to 50 percent while the tax rate remained at 33 percent for individuals in the year they attain their NRA. As the tax rate rises from 33 percent to 50 percent lowering the return to work, an individual who would have earlier chosen to work might choose to retire if he is either unable to find a job with earnings below the threshold level. A complete removal of the earnings test raises the retirement age of the affected individuals.

The life-cycle model predicts that removal of the earnings test above the normal retirement age and below age 70 encourages work between these ages by raising the net wage, while lowering work below the NRA (ages 62-NRA). If there are spillover effects, however, then the repeal of the earnings test might increase work at ages below the normal retirement age and at ages above 70 as well. These spillover effects may arise because individuals below the NRA perceive a higher return to their work at ages above normal retirement age, but if it is difficult to find a new job after a break from work at older ages, so to avail this higher return they might continue working below NRA. It is also possible that before the repeal of the earnings test individuals who retired from work to avoid the tax on earnings could not return to work at a later age of 70 and above where the earnings test ceased to apply. This possibility is also a consequence of transitions costs that may keep workers from entering and exiting work as they desire. The repeal of the earnings test could potentially raise retirement age among those above 70, if more individuals who work between normal retirement age and age 70 continue to work past age 70.

1.3. REVIEW OF PREVIOUS RESEARCH

The changes in normal retirement age and delayed retirement credit impact the retirement decision only in the life-cycle model through a change in the SSW. But, the earnings test can affect the retirement decision in the life-cycle context only under conditions that make the actuarial adjustments less than fair and if labor market rigidities restrict the choices available to an individual. The approach chosen by previous researchers studying the response of work decision among older individuals to earnings test changes depends on whether they model the earnings test as a tax in the life-cycle or static context.[23] I attempt to assess the impact of the earnings test on the retirement decision of individuals in a life-cycle context.

Researchers in the 1980s studied the impact of increases in SSW on the labor force participation of older men using time series variation in benefits. These increases in SSW arose either due to ad hoc changes implemented in the late 1960s and early 1970s or due to overindexation of benefits in the 1970s. The findings from these studies support the view that higher social security benefits lower labor force participation among older men.[24] Krueger and Pischke (1992) note the difficulty in separately identifying the impact of social security benefit increases in time-series analysis from other variables that have trended over time. They instead identify the effect of social security benefits by relying on an exogenous unanticipated downward movement in benefits as a result of the 1977 Amendment which permanently lowered the benefit level for a cohort of individuals termed the “notch babies”. They use aggregated CPS data from 1976-88. In their analysis they control for actuarial adjustments applied to foregone benefits by including a measure for accrual of benefits. They assess the impact of social security benefit reductions on the labor supply of men by examining the response along three dimensions: labor force participation, self-reported retirement, and the number of weeks worked last calendar year. They find no robust significant influence of social security benefit reductions on the retirement decision.[25]

[23] As described in the previous section, given the recent increases in the DRC, at an actuarially fair level of DRC, the earnings test is not a tax in the life-cycle model. But, if individuals are unaware of these credits, face borrowing constraints, or have lower than average life expectancy then the earnings test may still be a tax in the life-cycle context.
[24] Krueger and Pischke (1992) discuss the methodology used in these studies.
[25] Moulton and Stevens (2015) emphasize that the Notch legislation changed both retirement wealth and incentives for delaying retirement in offsetting ways. They note that the Notch

The finding of Krueger and Pischke's study analyzing the effect of benefit reductions cannot be directly applied in evaluating the response in retirement age due to the recent benefit reductions. The recent benefit reductions are instituted through a change in the normal retirement age instead of a permanent benefit cut with an unchanging normal retirement age. In light of past evidence of large spikes in the retirement hazard at the social security early and normal retirement age, and recent evidence of a shift in these spikes it may be important to control for any noneconomic norm related effects that may impact the retirement decision in addition to the financial incentives. This norm related effect may arise if older individuals view the normal retirement age as a focal point (guidance) for retirement.

Gruber and Orszag (2003) use aggregate CPS data from 1973-98 to examine the effect of earnings test policy changes on the work decision, hours worked, and claiming behavior of older men and women.[26]They also evaluate the impact of the 1983 earnings test repeal for individuals between ages 70-72. In their study they identify the ad hoc increases implemented from 1996 onwards in the earnings test threshold amount as an important source of variation in earnings test parameters among individuals for whom the earnings test was still in place. But, they find no robust evidence of a change in work decision of older men and women in response to a loosening of the threshold amount.

legislation reduced incentives for delaying retirement as it changed the benefit computation formula to not include earnings beyond age 61; it is, thus, inaccurate to interpret Kruger and Pischke's findings as evidence of insensitivity of retirement to wealth levels.

[26] Some other studies that have analyzed the impact of the earnings test on work decision are as follows: Tran (2002), Disney and Smith (2002), and Friedberg and Webb (2009).

They, however, find robust evidence that the threshold amount increases lead to higher benefit receipt among both men and women.

In their analysis Gruber and Orszag note that their finding of no impact of earnings test parameters on the work decision of older workers is best understood when applied to individuals facing the earnings test. Due to the relatively fewer observations available for individuals between ages 70-72, they are unable to precisely measure the full impact of the earnings test repeal. They also ignore the effect of the delayed retirement credit under the assumption that older workers either are misinformed, or do not understand delayed retirement credit adjustments, and hence the earnings test acts as a tax for all individuals. This view may be correct for the time period covered in their study, but cohorts reaching normal retirement age in 2002 and later have a delayed retirement credit of 6.5 percent per year or higher (which is approximately actuarially fair for the average individual). The birth cohorts affected by the recent earnings test repeal of 2000 experienced increases in their normal retirement age, and/or delayed retirement credit. Keeping in mind the recent increases in the delayed retirement credit and normal retirement age (which did not affect the cohorts studied in Gruber and Orszag's analysis), it seems important to try and assess how changes in earnings test parameters affect individuals' retirement decision in a life-cycle context controlling for the influence of DRC and NRA changes.

Three recent studies have examined the effect of both changes in normal retirement age and changes in the delayed retirement credit on the labor force participation or employment decision of older men; two of these studies also control for the effect of the 2000 earnings test repeal. Blau and Goodstein (2010) create a synthetic panel data using CPS and SIPP which is aggregated by birth cohort, year of age and four education groups covering for the time period 1962-2005. They use Social Security Administrative (SSA) data to match the average lifetime earnings for each cohort which are then used to derive the average social security benefits for each cohort. Blau and Goodstein's specification only accounts for financial incentive effects of NRA changes, but it does not allow them to capture the norm effects. They ignore the earnings test noting that there is no straightforward way for measuring its effect in their framework. They find the changes in normal retirement age and delayed retirement credit explain about one fourth to one half of the recent increase in the labor force participation of older men.

Mastrobuoni (2009) focuses on the effect of changes in normal retirement age. He uses CPS data from 1989-2007 to identify the impact of NRA increases on the retirement age of both men and women by using a regression discontinuity design, and he restricts his analysis to individuals between ages 61-65. He identifies the impact of the normal retirement age by estimating the difference between the yearly trend in average retirement age of cohorts born before 1938 (not affected by the NRA increases) relative to cohorts born after 1937, while controlling for the influence of the DRC changes and earnings test repeal. He finds that a 2-month increase in the normal retirement age increases the average retirement age of the affected cohorts by 1-month.

Pingle (2006) focuses on the effect of DRC changes. Using data from SIPP panels covering a period of 1983-2003, he finds that a delayed retirement credit increase of 1 percent raises the employment rate of 65-70-year-old men by 1.5 percent. Pingle's analysis extends only until 2003 when individuals in the first cohort affected by the NRA increases attain their normal retirement age, so he is unable to assess the impact of normal retirement age changes among those above their NRA. Both Mastrobuoni and Pingle's study control for effect of the earnings test repeal on labor force participation. Their specifications allow them to capture the norm related effects of normal retirement age changes because they capture these changes by including a variable indicating the change in the NRA assigned by birth year instead of a variable for changes in social security benefits. But they are unable to separately identify the norm and financial effects associated with NRA changes.

The three studies that analyze the recent policy changes can be improved upon in at least four ways. First, none of these studies incorporate the sizeable variation in the earnings test threshold amount (noted by Gruber and Orszag) that were introduced through the ad hoc changes implemented after 1996. Second, even though older women are a sizeable part of the labor force there is only one study (Mastrobuoni, 2009) analyzing the impact of recent changes in the NRA and DRC on the retirement decision among older women between ages 61-65, and none examining the retirement decision changes of women above their normal retirement age. Third, there is little work in the recent research disentangling the financial and norm related effects arising from the NRA changes. It is possible to separately identify these effects through use of a dataset that includes accurate birth year information of individuals. Finally, previous researchers have only focused on changes in the labor force participation decision of older workers in response to the recent changes in Social security policy. Given the large evidence of the variety of options explored by older individuals in their transition to retirement, it seems important to analyze how older workers response differs when evaluated using alternative definitions of retirement. I attempt to include these suggested changes to the previous work in my empirical analysis.

1.4. DATA AND SPECIFICATION OF THE RETIREMENT MODEL

My aim in this study is to analyze the change in older workers' retirement decision in response to recent changes in Social Security policies, and to pursue this aim I employ a reduced form strategy. Because of the diverse pathways into retirement actually chosen by older individuals leaving work, previous literature has emphasized the inadequacy in relying on a single definition of retirement to capture retirement behavior.[27] I, therefore, define retirement age in two ways: the age at which individual withdraws from labor force; or the age at which the individual assesses himself to be retired. This approach allows me to explore the sensitivity of the estimation results to the different definitions of retirement.

I focus on the first observed retirement among individuals who are working and have not previously retired. Both the retirement definitions include only individuals at work (or looking for work) at the time of sampling wave. I condition on work for two reasons. First, even though many older workers reenter the labor force after retirement, there are many others for whom the transition costs to work following retirement may be very high either due to loss of skills, or high search costs. If I do not condition on work and include all individuals regardless of their current labor force status, then the response of older workers to policy changes may be difficult to discern. Second, to analyze the

[27] Diamond and Hausman (1984), Quinn, Burkhauser, and Myers (1990), Ruhm (1990), Blau (1994), Gustman and Steinmeier (2000).

change in the retirement behavior of older women in response to these policy changes, it is useful to focus on women retiring conditional on working at a prior date. This is beneficial because even though the work attachment of older women has increased substantially over time, there are still many older women with no work history. Other researchers have also noted the importance of reentry behavior among older men, the idea that individuals return to work at a later date after retiring.[28] Following the first observed retirement from work, I, however, ignore any subsequent return to the labor market by the individual.[29]

1.4.1. DATA

Survey of Income and Program Participation (SIPP): To empirically assess the validity of the theoretical predictions, the data used are from the four most recent panels 1996-2008 of the Survey of Income and Program Participation (SIPP), which include data from years 1996-2013. The SIPP has a rotating panel design, and individuals within a panel are followed for a period of 3 to 4 years. Table 1.5 summarizes the length of each panel used in the study along with the reference period for which the data are available. Beginning in 1996, the SIPP was redesigned to include a larger initial sample than earlier panels. Panel members are randomly assigned to four different rotation groups, and each month members of one of the rotation groups are interviewed. These interviews are scheduled at intervals of four months; at each interview individuals are asked questions about their activity in the preceding four months. The SIPP oversamples households from

---

[28] Diamond and Hausman (1984), Quinn, Burkhauser, and Myers (1990), and Maestas (2010).

[29] I do this for two reasons. First, as Diamond and Hausman (1984) note, allowing for reentry behavior will require a more elaborate formal model. Second, the SIPP dataset that I use for the empirical analysis follows individuals for a relatively short duration of 3 to 5 years, which does not provide enough time to observe un-retirement behavior following retirement from work.

areas with high poverty concentration.[30] Due to Census budget cuts the sample for the 2004 panel was cut in half at the end of the eighth wave (reference period June 2006-December 2007).[31]

In comparison to the two other recently used datasets (the Health and Retirement Study and the Current Population Survey) the SIPP has both strengths and weaknesses. The main strengths of the SIPP relative to the Health and Retirement Study (HRS) are its larger sample size and the shorter duration between interviews.[32] Compared to the Current Population Survey (CPS) the SIPP has more accurate birth year information.[33] This is useful because both the normal retirement age and delayed retirement credit policy changes are assigned by birth year. Moreover, unlike the CPS, the SIPP follows individuals when they relocate. The main weaknesses in using the SIPP relative to the HRS are its shorter panel length and the unavailability of detailed pension plan incentives (defined benefit or defined contribution) information, and information on retiree health insurance. The SIPP contains information on both these variables in its topical wave modules which are administered once in a panel.[34] I do not include these variables because of the infrequency of the topical modules and the associated decrease in sample

[30] Survey of Income and Program participation User Guide Chapter 2, 2009.
[31] After the eighth wave fifty percent of the sample was dropped and not interviewed in subsequent months. The data for wave 1 through wave 8 was collected for the full sample.
[32] In the Health and Retirement Study individuals are interviewed every two years while in the SIPP individuals are interviewed every four months.
[33] The birth year information had some inconsistencies for individuals in panel 2004 and 2008. In such cases, for the affected individuals I kept only those observations where the individual responds to the interview personally and his birth year and month are not imputed.
[34] The data on both these variables are available in the following topical modules of the SIPP: Retirement Expectations and Pension Plan Coverage, and Employer Provided Health Benefits.

size as fewer individuals are present in survey at the time of data collection on these variables.

1.4.2. RETIREMENT MODEL SPECIFICATION

I analyze the retirement decision using a hazard model framework. An individual is at risk of retiring between ages 60 to 75. The discrete-time retirement hazard at any age is the probability of retiring at that age conditional on not having already retired. The retirement hazard at a particular age depends on various explanatory variables that make retirement more or less appealing than staying at work, I discuss these variables in detail below. The discrete-time retirement hazard h_{it} for an individual i at any age t (in the interval (t-1, t])

$$h_{it} = \Pr(Retire_{it} = 1 \mid Retire_{it-1} = 0 \;\; x_{it}, y_i)$$
$$= \Pr(T_i \in (t-1, t] \mid T_i > t-1, x_{it}, y_i)$$

where T is the random variable denoting the age at retirement (beginning at age 60), and x_{it}, y_i are a set of time varying and time constant explanatory variables respectively that affect the retirement hazard rate. Following Allison (1982), I use a logit model to specify the dependence between the retirement hazard and the explanatory variables.

$$\log\left(\frac{h_{it}}{(1-h_{it})}\right) = \alpha_t + \beta' x_{it} + \gamma' y_i$$

α_t is a set of constants (age dummies) denoting the non-parametric baseline hazard; this specification allows the retirement hazard to vary by age while holding other explanatory variables constant.

The explanatory variables included in x_{it} and y_i that affect an individuals' retirement hazard include both Social Security policy variables and socio-economic variables. I capture the effect of changes in the Social Security policies by including a variable for the normal retirement age which indicates (in months) the NRA assigned to individuals by their birth cohort, and another variable for the delayed retirement credit which indicates (in percent) the DRC assigned to each birth cohort. The impact of these two policy changes can be separately identified because the DRC changes started affecting cohorts born after 1924 and were implemented every other year for individuals born in odd years. The full impact of the earnings tests being in place among individuals above the NRA relative to not having the earnings test is identified through the inclusion of two variables: an earnings test in place dummy (equals 1 if individual is within the age range covered by the earnings test) and the difference between the earnings test threshold amount (in thousands of real 2013 dollars) and the amount at which the threshold was before the 2000 repeal.

Recent evidence by Mastrobuoni (2009) and Blau and Behaghel (2012) notes a shift in the hazard for retirement and claiming social security benefits at the new NRA. The life-cycle model predicts the NRA as the optimal retirement age only in the presence of a convex kink in the lifetime budget constraint, but with an actuarially fair or more than fair delayed retirement credit there is no such kink and hence, no reason to observe the spike at the new NRA. The recent evidence, thus, points in the direction of a norm effect or reference dependence effect of the normal retirement age. Under norm effects, workers view the NRA as a focal point at which to retire. I examine the strength of the influence of these norm effects on the retirement probability by including dummy variables for being below, at, or above the NRA.

Aside from these policy variables, there are two other variables that could potentially cause retirement probabilities to differ among individuals by changing the value of their SSW: marital status and education. The retirement probability of married workers can differ from single individuals because for married men the benefit from a change in the SSW from an additional year of work is not restricted only to them but also extends to their spouse through spousal (if she claims spousal benefits) and/or survivor benefits.[35] The probability of retirement among older individuals may also differ by educational attainment for two reasons: difference in opportunities available for continued work and education may be correlated with mortality. To assess differences in retirement probabilities arising due to marital status and education, I include dummy variables for being married and for educational attainment.

Health insurance and the availability of pension are two other factor affecting older workers retirement probability. Rust and Phelan (1997) find that men with employer provided health insurance and no retiree insurance are less like to leave labor force before age 65 than those with retiree health insurance. They also note that even after age 65, these men have a lower retirement hazard suggesting that employer provided health insurance may be more generous.[36] To control for differences in the retirement

[35] A workers' wife is entitled to 50 percent of his PIA, and a surviving spouse is entitled to 100 percent of the workers' PIA. If the wife has an earnings record, she can choose to claim the higher of the benefits either based on her own record or as a dependent spouse.
[36] Madrian and Beaulieu (1998) suggest one reason for this generosity could be the availability of coverage of dependents that is provided by employer health insurance but not by Medicare. Another reason could be that Medicare coverage is not comprehensive. Older individuals covered

hazard arising from the availability of employer provided health insurance I include two dummy variables. The first variable is a lagged dummy variable indicating whether an individual is covered by employer provided health insurance, intended to capture the difference in the retirement probability among individuals with and without employer provided health insurance at all ages 60 to 75. The second variable captures the differential effect of employer provided health insurance on those below age 65. The availability of a pension may also impact the retirement decision of older workers. Without knowledge regarding the detailed pension plan incentives I am unable to account for the precise incentives faced by a worker.[37] I try to capture the effect of availability of a pension plan by the inclusion of a union dummy variable which indicates whether an individual is covered by a union.

The race of an individual may also affect the retirement hazard. Past researchers have suggested that differences in retirement probabilities by race may arise due to difference in preference for leisure, but Gustman and Steinmeier (2004) attribute these differences in retirement to difference in time preference rates. I include race dummies to capture these differences. Other factors affecting the probability of retirement may include difference in opportunities for work available at older ages; to allow the hazard to vary by these differences I include region dummies and the state specific unemployment rate. Personal responsibilities and financial needs may also cause otherwise similar individuals to have varying retirement propensities. I capture the impact of these factors

by retiree health insurance, however, will not be affected by health insurance availability and eligibility for Medicare in making their retirement decisions.

[37] Freeman (1985) shows that the presence of unions increases pension coverage.

by including three variables: a dummy variable indicating whether an individual has children under 18, a dummy variable for ownership of home, and the number of members in the household.

1.5. RETIREMENT DEFINITIONS AND DESCRIPTIVE FINDINGS

1.5.1. DEFINITIONS OF RETIREMENT

There is no universal definition of retirement. With the availability of different pathways to retirement for older individuals, it has become increasingly difficult to understand retirement behavior by relying on a single definition. Many researchers have documented the varied options explored by older individuals. For example, the transition from full time work can happen abruptly with complete retirement from work, gradually through part time work at the current job or at a new and final job, or by a temporary withdrawal from work followed by reentry into the labor force at a later date.[38] These empirically observed diverse choices stress the need to understand that the idea of retirement means different things to different individuals, and any attempt to study these choices using a single definition may prove inadequate.

In the past, researchers have explored the retirement behavior of older individuals using both objective and subjective definitions. I use two definitions of retirement summarized in Table 1.6.[39] The retirement from labor force definition relies only on

[38] Gustman and Steinmeier (1984 a), Quinn, Burkhauser, and Myers (1990), Ruhm (1990), and Blau (1994).

[39] In the above retirement criterion, retirement is defined as an absorbing state. Once an individual satisfies the criterion he cannot un-retire; I drop all observations on the individual following first incidence of retirement. For individuals who leave the sample and then return later, I keep only the first uninterrupted spell of observations. The sample used in the analysis also does not include individuals who were currently serving in the armed forces (this question is asked only until age 60). Individuals living in the following states were also excluded from all panels, because the

objective measures in classifying an individual as retired, while the second definition relies only on a subjective self-reported measure of retirement. In both the definitions described below only individuals ages 60 to 75 are considered.

Retirement due to Withdrawal from Labor Force: This definition is the broader of the two employed in the study, and is also the one most frequently used by researchers studying retirement behavior. The initial sample consists of all individuals who have a paid job/business or have looked for work sometime in the four months preceding their first interview.[40] An individual is viewed as retiring at some point in a four month interval prior to the first interview at which he first reports not working at any job/business and not looking for work. Since I do not observe the actual month of retirement, I calculate the retirement age of an individual by choosing the second month in the four month interval as the month of retirement.[41]

---

states could not be separately identified to assign state specific unemployment rate in the 1996 and 2001 panels: Maine, Vermont, North Dakota, South Dakota, and Wyoming. Individuals are asked if they worked at least one job/business in the four months preceding the month of interview or in the month of interview, but all subsequent questions refer to the four-month period prior to the month of interview. Among the individuals who do not have a job during the entire four-month period, the question looking for work is asked only of those who did not stop working because of retirement or disability. First interview refers to the first time an individual was interviewed when he falls between the ages 60 and 75 (which may be different from the first time the individual was interviewed when he entered the survey). I determine the retirement age for all definitions by assuming that the individual retired in the second month of the four-month window.

[40] The question looking for work is only asked of those individuals who when asked the reason for not having a paid job do not state themselves either as retired or unable to work because of a chronic condition.

[41] I am unable to make full use of the birth year information available in the SIPP because of the inability to accurately observe the timing of retirement. To calculate the age of retirement in the four-month window in which an individual is observed to retire, I recode the birth month/year information provided by the individual. I code the birth month for each individual by rotation groups to fall on the second month of the four-month interval and then calculate the retirement age using the recoded birth month. For some individuals I also changed the birth year assigned to

An advantage of selecting the initial sample at the sampling wave on the basis of labor force status in the previous four months is that it allows for a larger sample size. The number of men and women included under each definition of retirement are summarized in Tables 1.7 and 1.8. The larger sample size for both men and women under this definition arises because of its broad scope: any individual with a job/business or looking for work. Regardless of whether it is full time work, or part time work at reduced pay and possibly reduced work load, any worker is considered a part of the initial sample and treated as if not retired. The total number of men and women participating in the labor force at older ages do not differ by a large amount from men confirming the increasing attachment of older women to the labor force. Under this definition an individual is labeled retired based on a researchers' retirement criterion of being out of the labor force. A limitation of this definition is that it misclassifies discouraged workers (who might reenter the labor force at a later date) as permanently retired.

Self-Reported Retirement (Ever Retired): Objective measures of retirement allow a researcher to avoid the ambiguity attached with self-assessed measures of retirement, but they miss vital information conveyed by individuals' own view of his retirement status which may be different due to difficulty in objectively measuring retirement. The sample under this definition includes individuals who are working and state themselves as having never-retired from any paid work. An individual is considered retired when he first declares himself retired.

---

the individuals. For instance, in the 1996 panel for someone in rotation group 1 who was born in December 1938, I recode the birth month to January and the birth year to 1939.

Self-reported or subjectively assessed retirement status may vary by individuals. If all older individuals viewed retirement as a complete disassociation from any subsequent involvement with paid work, then the self-assessed retirement status will be unambiguously determined. All older individuals, however, do not view retirement in the same way, and different individuals may attach their own meaning to the idea of being retired. For instance, two individuals employed full time at the same bridge job may view their retirement status differently.[42] Such nuances cannot be completely captured by objective measures.

Under this definition the observed number of women in the sample is higher than the number of men at all ages (Tables 1.7 and 1.8) suggesting that among those employed at a job more women do not view themselves retired than men. There could be two possible explanations for this observation. One, it is possible that some women continuing part-time employment at their career job may not view themselves retired. Two, women's intermittent labor supply over their lifetime, along with the higher number of women employed at part-time jobs may lead some to view any attachment with work as a state of being not retired. The difference between the number of women employed in the labor force and those declaring themselves not retired, however, indicates that the above explanations while true for some women are not true in general; many women, like men, continue to work after declaring that they have retired from work at least once.

There are two additional advantages in using this definition. First, it does not misclassify individuals leaving paid work due to termination or layoff or any other

[42] Ruhm (1990) describes bridge jobs as jobs held after departure from a long time held career job but before complete withdrawal from labor force.

reason, who do not view themselves as retired even after they have stopped looking for a job in the last four months (discouraged workers). Second, it also does not misclassify those older workers who do not view themselves retired when they leave the labor force intending to reenter at a later date. The disadvantage in relying on a subjective measure of retirement is its inconsistency. Instead of relying on a universal retirement criterion chosen by a researcher, a subjective definition relies on each individuals' personal retirement criterion.

To better understand the differences in behavior under the two retirement definitions I compare the number of individuals at the sampling wave and the number of individuals actually observed retiring in the sample under each definition. The higher number of men and women under the labor force definition relative to the self-reported definition (in Tables 1.7 and 1.8) suggests that many older workers continue to work while viewing themselves as retired. In Table 1.9, I further explore the relationship between the two retirement definitions by focusing on the actual retirement behavior of individuals in the sample to understand how the timing of withdrawal from labor force may differ from self-reported retirement. The first row indicates the percentage of men for whom the timing of self-reported retirement is greater than timing of withdrawal from the labor force. The first column focuses on those individuals whose retirement status is based on withdrawal from the labor force, while the second column focuses on individuals who report themselves as retired at some point in the survey. I find that in the SIPP data, only about 5 percent of men who are categorized as retired based on their withdrawal from labor force have not yet described themselves as such. Among individuals who report themselves retired only 11 percent withdraw from the labor force before they claim to be retired. This suggests that most individuals declare themselves retired before they leave the labor force. The second row indicates the amount of overlap in the two retirement definitions. I find some support for the view that many older workers today continue to work after retiring from a (career) job, in the higher percentage of individuals reported in the third row for whom the timing of withdrawal from labor force is greater than the timing of self-reported retirement.

1.5.2. HAZARD RATES

The plots of the hazard rate into retirement at various ages for the different SIPP panels provide a convenient snapshot that aides in carrying out a preliminary visual analysis of the change in retirement behavior over time (without accounting for covariates). I model retirement age in discrete-time to account for the discrete nature of the available data, and in doing so I also assume that the true nature of the underlying retirement age is discrete.[43] The discrete Kaplan-Meier empirical hazard at each possible retirement age is the fraction of individuals who retire at that age conditional on not having retired at any age prior to that.[44] Figures 1.3-1.4 show the hazard rate plots into retirement for the retirement definitions discussed above.

[43] Allison (1982) notes that regardless of the assumption about the true underlying nature of time (discrete or continuous), the results from estimating models that take into account the discrete nature of the available data are very similar.

[44] Specifically, the hazard rate at any age t, H_t = Number of failures_t / Number of Individual at risk_t is the ratio of the number of individuals who retire (fail) between the age interval (t-1, t] and the number of individuals at risk of retiring at the beginning of the interval (at age t-1). The number of individuals at risk of retiring at age t includes all individuals who have not retired in any previous age interval and who were not censored at the beginning of the age interval (t-1, t]. Number of individuals censored at age t includes those who were observed in the sample at the beginning of the interval (t-1, t] at age t -1 but not at age t, we do not observe the retirement age for such individuals. Individuals can be censored for three reasons: they leave the survey (attrition), they are no longer in the age range 60-75, or they have not retired at the time of the last survey interview (last wave).

The hazard rate plots for the labor force retirement definitions are shown in Fig 1.3. Among older men observed in the 1996 panel, there is a higher initial peak at age 62 relative to age 65.[45] This highlights the well documented preference among men (due to liquidity constraints) to leave the labor force at age 62 when social security benefits first become available. The hazard declines after age 62 then increases at age 65, but after age 65 the hazard into retirement is higher at all ages as the increasingly fewer individuals who remain in the labor force at these advanced ages withdraw. Women have a higher hazard than men at most ages (except age 62) which indicates that at every possible age of retirement the probability that an older woman will retire at that age conditional on not having retired earlier is higher than that of an older man.

The retirement hazard peaks observed in the 1996 panel at ages 62 and 65 among older men diminish in the later panels, while a new peak emerges in the 2008 panel at age 66 (new NRA). For older women, the age 65 spike continues to remain prominent even in the 2008 panel suggesting that women persist in their earlier retirement behavior either because they continue to view age 65 as a focal point for retirement or due to the eligibility for Medicare. The hazard for women below age 69 declines in later panels but this decline is less than the one experienced by men, and is offset by slight increases in the 2008 panel. The peak in the retirement hazard at age 66 observed among older men is surprising, this behavior among older men points to other reasons such as social norms

[45] Due to the fewer number of individuals at ages above 70 who are observed both at work and in the sample, while computing the hazard rate of retirement at these advanced ages I do not observe any individual retiring for certain age intervals. To avoid the sharp declines in hazard rate to zero within such empty intervals I assign the hazard rate observed in the previous age interval to the empty age interval. This explains the flat hazard rates observed at older ages in some of the definitions.

which may lead individuals to associate the Social Security normal retirement age with a focal point for retirement.[46]

Unlike the hazard rates discussed above, the hazard rate for self-reported retirement (Figures 1.4 A and B) among men in the 1996 panel has a much higher peak at age 65 than at age 62. Among women in the 1996 panel the peaks at ages 62 and 65 are not as prominent as among men. The hazard into self-reported retirement in the 1996 panel does not vary much between men and women below age 70. The higher hazards observed for men at both ages 62 and 65 decline in the later panels as can be seen in Figures 1.3 A and B.[47] For men a new peak also emerges at age 66 in the 2008 panel but it is relatively smaller than the one that existed at age 65, and not much higher than the hazards of retiring at neighboring ages. A visual inspection of the hazard for self-reported retirement among men and women in the four panels suggests a decline at all ages in the 2001 and 2004 panel, but in the 2008 panel the hazard increases for those above age 65. For women the age 65 spike in the hazard does not diminish with time, and there is no evidence of a higher retirement hazard into retirement at age 66 in the 2008 panel.

To summarize, a common feature of the hazard plots discussed above is the existence of pronounced spikes in the retirement hazard at ages 62 and 65 for both men and women in the 1996 panel. With the course of time these spikes decline for men and a new modest spike in the hazard appears at age 66. The age 65 spike diminishes but does

[46] Lumsdaine, Stock, and Wise (1996).

[47] The number of observations listed next to each hazard plot indicates the number of individuals who were in the labor force at the beginning of the sample, and who subsequently retired or left the sample at an age greater than 60. Individuals who are 60 years of age and who retire or leave the sample after being observed for only one wave are not included in the hazard rate calculations. This explains why the number of men and women listed next to the hazard plots differ from those present in the initial sample selected from each panel.

not completely disappear.[48] Interestingly, unlike men among women the observed spike in the hazard at age 65 does not diminish even in later panels, while the decline in the spike at age 62 is less than that observed among men, and the spike at age 66 is missing.

1.5.3. DESCRIPTIVE STATISTICS

The summary statistics for men included in the sample under the labor force retirement definition are provided in Tables 1.10 A and B. The average birth year of the individuals included is 1940 or higher (range 1921-53) which indicates the prevalence of relatively younger age individuals in the sample. This concentration of younger individuals at the sampling wave was also previewed in Tables 1.7 and 1.8, where almost half the number of individuals observed under each definition at the time of first interview were those below age 62. Because of the higher presence of younger birth cohorts the average delayed retirement credit is also approximately 7 percent (range 3-8 percent) which is considered actuarially fair for the average person with no borrowing constraints. About half of the men and women observed under the labor force definition face the earnings test at some point in the sample. Among individuals facing the earnings test there is not enough variation in the tax rate for clearly identifying the differential impact of the earnings test among those with a tax rate of 50 percent relative to those

[48] Previous studies have documented a spike in the retirement hazard at the normal retirement age of 65 and ascribed the following reasons for the spike: actuarially unfair DRC which creates a convex kink at NRA, the NRA acts as a focal point, the eligibility for Medicare at age 65, and the incentives available in defined benefits pension plans. The significance of some of these reasons have reduced over time: the delayed retirement credit has increased and is actuarially fair for the average person with no borrowing constraints; and as companies shift away from defined benefits plans more workers are covered under defined contributions plans which do not provide early retirement incentives. It is difficult to test the Medicare eligibility hypothesis because the eligibility age has not changed since the beginning of the program in 1965.

with a tax rate of 33 percent. Approximately 80 percent of men and 55 percent of women are married.[49]

1.6. METHOD OF ESTIMATION

I estimate the discrete-time hazard model using maximum likelihood. The particular initial specification is:

$$\log\left(\frac{h_{it}}{(1-h_{it})}\right) = \alpha_t + \theta_1\, NRA_i + \theta_2\, DRC_i + \theta_3\, ET\ dummy_{it}$$
$$+ \theta_4\, ET_{it} * (Threshold_{it} - \$21.67) + \theta_5\, At\ NRA\ dummy_{it}$$
$$+ \theta_6\, Above\ NRA\ dummy_{it}$$
$$+ \theta_7\, DRC_i * Outside\ Affected\ Age\ \ Range_{it} + \ \theta_8\, socio\ economic\ controls$$

To estimate the model, following Jenkins (1995) I arrange the data into person wave observations representing the time at risk for each individual. In doing so I take into account the sample selection process, because not all individuals are observed entering into the initial unretired state at age 60 (the beginning of an individuals' time at risk of retirement). I assume a flexible non-parametric specification for the baseline hazard, and treat the time varying variables as constant within each discrete-time interval.[50]

[49] I define all those individuals as single who are currently not married. Single individuals include all those who are: divorced, widowed, and never married. In defining individuals as single in this way, I am unable to account for the different incentives in the social security law for divorced and widowed individuals. These incentives may especially be important for divorced and/or widowed women who claim benefits on their husband's record.
[50] In estimating the model parameters, I do not account for any unobserved source of variation in the retirement hazard among individuals.

Suppressing the conditioning on covariates, the log likelihood function as derived by Jenkins for a stock sample of n individuals is as follows:

$$\log L = \sum_{i=1}^{n} \sum_{t=\tau}^{\tau+s_i} y_{it} \, . \log\left(\frac{h_{it}}{(1-h_{it})}\right) + \sum_{i=1}^{n} \sum_{t=\tau}^{\tau+s_i} \log(1-h_{it})$$

where τ represents the age at which an individual i was selected into the sample of each retirement definition and first became at risk of retiring. τ may differ by individuals for two reasons. First, only individuals who are 60 to 75 years of age are included in the estimation sample, so we will observe a different τ for younger individuals as they attain age 60 at different points and enter the sample. Second, individuals falling in this age range may enter the actual SIPP survey sample at different waves. s_i represents number of waves in the sample for each individual. For individuals who are observed retiring in the survey (uncensored), $\tau + s_i$ represents the SIPP wave at the time of retirement. Those individuals who have not retired at the time of the last interview wave of the SIPP survey, or who have not retired at the time they leave the survey are viewed as censored, because their retirement age is not known. For censored individuals, $\tau + s_i$ represents their last wave of observation in the survey. y_{it} represents the retirement status of the individual; the last observation of an individual who retires in the sample (uncensored) is set to y_{it} = 1 (if $t = \tau + s_i$). All other observations (other than the last observation) of uncensored individuals have $y_{it} = 0$, and it is also set to zero for all observations of censored individuals.

1.7. FINDINGS FROM ESTIMATION OF RETIREMENT MODELS

In this section I present the estimation findings from the labor force and the self-reported definitions of retirement separately for men and then women. In presenting these findings I first discuss the basic estimation results from the retirement models, and then the robustness checks. The dependent variable in the estimation of both retirement models is a dichotomous dummy variable indicating whether an individual has retired since the last wave, where retirement status is determined based on the retirement criterions described earlier.[51]

1.7.1. ESTIMATION FINDINGS FOR MEN

Retirement due to Withdrawal from Labor Force: The results from the estimation of the retirement model for men based on withdrawal from the labor force definition are presented in Table 1.11. To assess the sensitivity of the estimation results to the inclusion of different variables, I estimate the retirement models with seven different specifications. All seven specifications include controls for age dummies (age in years), quarter/year dummies to control for variation over time, and panel dummies to control for overlapping panels.[52] The first specification shown in column 1 captures the effect of the

[51] Diamond and Hausman (1984) note that a selection rule which conditions on working in the previous four months introduces a "dynamic self-selection" bias in the estimation findings. They raise the concern that individuals who are observed at risk of retiring in a given time frame may not be representative of the population at risk for given past values of policy variables. For instance, if the delayed retirement credit increases cause individuals above the normal retirement age to retire earlier, then the individuals who continue to work after many years of being affected by the policy change are the one who are less likely to retire. With the exception of Gruber and Madrian (1993), most researchers do not account for this dynamic selection problem. In my empirical analysis, I also do not account for the dynamic selection.

[52] The last wave of SIPP panel 2001 and the first wave of SIPP panel 2004 both cover the months of October, November and December 2003.

main Social Security policy variables on the retirement hazard without controlling for the influence of other covariates. Both the delayed retirement credit and the normal retirement age variables assess the impact of a change in these policy variables on individuals of all ages. Each individual regardless of his current age is assigned a time constant value for the delayed retirement credit (percent) and normal retirement age (months) based on their birth year.

The specification in column 1 also includes a dummy variable set to one if the individual currently faces the earnings test. I also include an earnings test threshold variable that is implicitly interacted with the earnings test dummy. Among individuals facing the earning test the earnings test threshold variable is set to the amount based on whether the individual is between ages 62-NRA or ages NRA-70, while it is set to zero for individuals not facing the earnings test. Before the earnings test was repealed in 2000, the threshold amount for the affected individuals who were above their NRA (and below age 70) was approximately at $21, 671. In each of the seven specifications I include the difference between the threshold amount applicable for each individual facing the earnings test and $21,671. The benefit of specifying the threshold amount variable as the difference from $21,671 is that the coefficient on the earnings test in place dummy can now be interpreted as measuring the impact of having the earnings test in place at $21,671 relative to not having the earning test among individuals above their normal retirement age. In other words, I can, thus, assess the impact of repealing the earnings test at the higher threshold amount which was in place before 2000.

The coefficient for the earnings test in place dummy has an unexpected negative sign suggesting the presence of the earnings test at a threshold amount of $21,671 reduces the probability of retirement from labor force, but it is not significant. The coefficient for the normal retirement age has the expected negative sign, but it is also not significant. In this specification the normal retirement age variable captures both the financial and norm related effects related to the normal retirement age increases. The threshold level coefficient has the expected sign and is statistically significant, indicating a significant impact of each thousand-dollar increase in the threshold amount above $21,671 in reducing the probability of retiring from labor force among men.

In the second specification, I allow the retirement hazard to vary by individual specific characteristics, financial circumstances, and local labor market conditions through the inclusion of controls for race, education, children, ownership of home, number of members in household, region dummies and the state specific unemployment rate. Controlling for these variables does not alter the estimated impact of the policy variables. As found in previous studies, the probability of leaving the labor force is higher among black men relative to whites. Married men have a lower probability of retiring from labor force relative to single men, but the impact is not significant. Older men with less than a high school degree are more likely to retire from the labor force relative to those with only a high school degree, while men with at least a college degree are less likely to retire from the labor force. In the third specification I control for any influence on the retirement hazard that may function through norm related effects of the NRA by including two dummy variables for being at, and above the normal retirement age.[53] The inclusion of these variables does not alter the previous findings. The coefficient on the

[53] Since, I do not accurately observe each individuals' age at the time of retirement, I code the dummy variable for being at the normal retirement age to equal one at the first observed age after the individual has attained his normal retirement age.

dummy for being at the normal retirement age has a positive sign indicating the higher probability of retirement among individuals who are at their NRA relative to those below the NRA, but the impact is not significant.

The theoretical prediction for the impact of an increase in the DRC on the retirement probability differs depending on whether the affected individual is above or below the normal retirement age. There is a clear theoretical prediction of an increase in the probability to retire for those below their NRA, but an ambiguous prediction for those above their NRA (and below age 70).[54] To examine these predictions I include an interaction term in the fourth specification that interacts the DRC with a dummy for being outside the age range affected by the DRC changes. The interaction term captures the differential response to delayed retirement credit changes among those above the NRA and below age 70 relative to those below the NRA and those above age 70. The coefficient for the DRC interaction term, indicates that a higher DRC lowers the retirement probability among those outside the affected age range relative to those above the NRA and below age 70, and the effect is statistically significant. This unexpected finding suggests the stronger influence of the delayed retirement credit functioning through the spillover effects on individuals below their normal retirement age. In the presence of the DRC interaction term, the coefficient on the delayed retirement credit variable now captures the effect of the DRC increases among individuals above the normal retirement age who are directly impacted by the policy change.

[54] In the presence of labor force entry and exit costs, increases in the DRC may reduce probability to retire among individuals below the NRA.

With the inclusion of the DRC interaction, the coefficient on the earnings test in place dummy has the expected sign, but the impact remains insignificant. Interestingly, the coefficient for being at the NRA dummy variable now has a negative sign suggesting a lower probability of retirement among men at their NRA and the effect is statistically significant at the 10 percent level. I also observe that the probability of retirement is lower among those above the normal retirement age relative to those below, and the effect is statistically significant. The coefficient for the NRA variable also has an expected negative sign, and the effect is statistically significant suggesting that as the normal retirement age rises it reduces the probability of retirement among men at all ages. In this specification the NRA variable captures only the impact arising through the financial incentives of changing the normal retirement age.

The retirement hazard may also vary by the availability of health insurance with current employer, and this impact may differ among those below the age of eligibility for Medicare (age 65). I control for the influence of health insurance availability by including two dummy variables for lagged health insurance with current employer in the fifth specification. The coefficient on the interaction of health insurance among those below age 65 is negative and statistically significant indicating the lower probability of retirement from labor force among men below age 65 who are at a job which provides their health insurance coverage relative to similar men above age 65. Accounting for this reduction in retirement probability for ages below 65, reduces the strength of the relative impact of the delayed retirement credit among individuals not directly affected by the changes. Both the dummy variable capturing the norm effects and the NRA variable capturing the financial incentives of the NRA changes now have a statistically insignificant impact on the probability of retirement.

In the sixth specification I try to capture the effect that the availability of a pension may have on the retirement hazard by including a dummy variable indicating whether the individual is covered by union. I find that individuals covered by a union have a higher probability of retirement relative to individuals not covered, and the effect is statistically significant. This finding suggests that the availability of a pension raises the probability of retirement from the labor force among men. In the final specification, to capture the differential impact of having the earnings test among those below the normal retirement age relative to those above, I include another dummy variable indicating the presence of the earnings test at a tax rate of 50 percent. The impact of the earnings test at 33 percent, captured by the earnings test in place dummy remains insignificant. There is a statistically significant negative impact of the earnings test with a tax rate of 50 percent, suggesting that the presence of the earnings test among men below the normal retirement age reduces their probability of retirement. The negative sign on the earnings test with tax rate of 50 percent dummy is in contrast to the theoretical predictions of the life-cycle model, however, this sign is not surprising if spillover effects are taken into account. If individuals below the normal retirement age respond to the repeal of the earning test (for those above the normal retirement age), by planning to work longer at older ages, then in the presence of transition costs we will observe less retirement among them. The coefficient on the DRC interaction term now becomes insignificant.

In Table 1.12, I present the partial effects of these estimated coefficients.[55] Following Greene (2010), I do not report statistical tests for either partial effects or cross partial effects (interaction terms) because of the difficulty in interpretation of the hypothesis tests. A one percentage point increase in the DRC reduces the probability of retirement among men outside the affected age range by approximately 0.5 percentage points relative to those directly affected (in specifications 4-6), which is approximately 30 percent of the average retirement hazard. The probability of retirement among older men below their NRA who face the earnings test with tax rate at 50 percent reduces by 4 percentage points. This reduction is large, as the presence of the earnings test below the NRA reduces the retirement hazard by more than two times the average hazard among men.

Self-Reported Retirement: The findings from the retirement behavior of older men based on a subjectively assessed retirement criterion summarized in Tables 1.13-1.14, are different from the objectively assessed definition of retirement from labor force in several ways. In specifications 1-3, the delayed retirement credit changes have a significant impact on reducing the probability of self-reported retirement among older men at all ages. With the inclusion of the DRC interaction term, the main DRC variable remains statistically significant indicating the reduction in the self-reported retirement probability among men above their NRA and below age 70. A one percentage point increase in the

[55] The partial effects for continuous variable are estimated using Stata's margins dydx option. The margins dydx(drc nra) command gives the average effect of the DRC and the NRA variable increases while holding all other variables constant. The cross partial effects are estimated using r.margins (contrast) option. The command: margins, over(r.outside age range affected by DRC) dydx(drc), gives the partial effect of the DRC interaction term by contrasting the effect of DRC increases on those outside the affected age range relative to those between the affected age range.

DRC reduces the probability of retirement among men directly affected by the delayed retirement credit increases by approximately one percentage point. This effect is large: a one percentage point increase in the DRC reduces the retirement hazard of individuals directly affected by approximately forty percent of the average retirement hazard. But, the delayed retirement credit does not significantly alter the relative probability of retirement among those directly versus those not affected by the DRC changes.

The coefficient on the dummy variable for being at the normal retirement age has a positive sign in specifications 3-4, and is statistically significant. This finding suggests that being at the normal retirement age increases the probability of self-reported retirement among men through norm effects. This finding, however is fragile, and not robust to the inclusion of health insurance dummies. The impact of the earnings test being in place at the threshold amount before the repeal remains insignificant in all the specifications. Similar to the labor force definition, I find that the earnings test at a 50 percent tax rate significantly reduces the probability of self-reported retirement among men below their NRA. The presence of the earnings test among men below their NRA has a large statistically significant effect on reducing their probability to self-report themselves as retired. It lowers their retirement hazard by approximately hundred and twenty-five percent of the average retirement hazard. Men with a college degree are less likely to report themselves retired relative to those with a high school degree only.

1.7.1.1. ROBUSTNESS CHECKS

Gruber and Orszag (2003) documented strong trends over time in labor force participation by age groups among older individuals, and note that models which control for these underlying age specific trends are more reliable. Following Gruber and Orszag, I also check the sensitivity of the estimation findings to the inclusion of age specific trends in retirement and age specific sensitivity to business cycles. To perform robustness checks, I estimate four specifications for both the labor-force and the self-reported retirement definitions. The findings are reported in Table 1.15. The first specification adds linear age specific trends to specification 7 presented in the Tables 1.11 and 1.13. In the second specification I include interaction terms of the age dummies with the state specific unemployment rate. Gruber and Madrian (2004) review the sizeable literature investigating the effect of health insurance availability (with current employer that may or may not continue providing retiree health insurance) on the retirement decision. They note that reduced form studies examining the relationship between retiree health insurance availability and retirement may suffer from a selection problem if there is a correlation between retiree health insurance availability and an individual's taste for leisure. In my study the estimates for health insurance availability and union coverage may be biased if the individuals working at jobs that offer health insurance and/or pension are selecting themselves into such jobs (maybe individuals with stronger taste for work seek and find employment at such jobs). In the third and the fourth specifications, I examine the robustness of the findings to the exclusion of lagged health insurance and union dummies.

Similar to Gruber and Orszag, I find no robust influence of the full impact of the earnings test repeal on the probability of retirement from labor force for men. Unlike them, the common finding from all four specifications for the labor force and self-reported retirement is the influence of the earnings test threshold increases in reducing the probability of retirement from labor force among older men who face the earnings test. This supports the earlier discussion about the presence of substantial fixed costs, minimum hours constraints or lack of part time jobs available to men which may be keeping them from participating in the labor force in the presence of the earnings test. The looser threshold level enhances the work options available to men. My findings for retirement from labor force definition are also similar to those of Kruger and Pischke; I do not find any impact of the benefit reductions on the probability of retirement among older men. My findings are not directly comparable to those of Mastrobuoni's because in his specification he captures the total effect of the increase in the normal retirement age, which includes both the financial and any norm related effect. Unlike Blau and Goodstein, I do not find any influence of the NRA or the DRC changes on the probability of retirement from labor force among older men.

A robust finding from the estimation of models based on subjectively assessed retirement behavior among older men is the significant impact of delayed retirement credit increases in reducing the probability of self-reported retirement among individuals directly affected by these policy changes. The influence of the delayed retirement credit on the self-reported retirement but not the retirement based on withdrawal from the labor force suggests that discouraged workers who are not observed under the labor force definition but are observed under the self-reported retirement definition might be responding to the delayed retirement credit while assessing their retirement status.[56]

[56] As noted earlier, the SIPP oversamples poor individuals. The above estimation findings are unweighted. To assess the validity of these findings for the entire population, I estimate both the retirement and claiming models (described in the next section) using person weights provided in

1.7.2. ESTIMATION FINDINGS FOR WOMEN

For women, I report only the findings that are robust to the inclusion of linear age trends and age specific effects of business cycles in Table 1.16. Similar to men, I do not find a response in the retirement from labor force among women to the main policy changes. I find a robust influence only from the threshold level in reducing retirement probability among women conditional on the presence of the earnings test. This finding does not concur with Reimers and Honig's (1996) prediction that loosening the earnings test threshold may affect the labor force participation of older men, but not women. This suggests that even though a higher number of older women are concentrated in part time jobs, and may thus not face the same minimum hours constraints as men, the loosening of the earnings test annual amount still prompts women to extend the duration of their work life. The presence of the earnings test seems to reduce the probability of retirement among women above their NRA, which is an unexpected finding, but the impact is not statistically significant. This suggests that even though loosening the earnings test threshold reduces the probability of retirement among women who face the earnings test, a complete repeal of the earnings test does not alter the behavior of women above their NRA in a significant manner relative to women who do not face the earnings test. I am also unable to find support for Reimers and Honig's prediction that since women are far-sighted in their behavior they are more likely to respond to increases in the delayed retirement credit than men. Increases in the delayed retirement credit reduces the probability of retirement from labor force among women above their NRA, but the

the SIPP. I do not report those results here, but they are very similar to the unweighted findings discussed above.

impact is not statistically significant indicating that like men, women are not sensitive to the DRC changes in the timing of their departure from the labor force.

The response in self-reported retirement behavior among women above their normal retirement age and below age 70 to the DRC changes is similar to men. The main DRC variable captures a statistically significant reduction in the self-reported retirement behavior among women directly affected by the policy. Unlike men, I find that in specifications which include the union coverage dummy the DRC interaction variable has a marginally statistically significant effect in reducing the self-reported retirement probability among women outside the affected age range relative to those directly affected. This finding provides some evidence for the impact of the DRC operating through spillover effects among women not directly affected by the changes. The main normal retirement age variable capturing the financial incentive effects of the NRA changes is statistically significant in all the specifications but the sign is unexpected. A one month increase in the NRA raises the probability of self-reported retirement among women at all ages.[57] Thus, for both men and women I do not find an impact of the recent changes in the Social Security policy variables on the probability of retirement when retirement is assessed using an objective measure, but find a statistically significant response to the DRC changes among individuals directly affected by the policy changes when retirement is assessed based on a subjective measure.

[57] To assess if the unexpected sign on the normal retirement age variable was due to the interaction between married women and their husband's retirement decision, I estimated separate models for married and single women. I find that the normal retirement age variable has a statistically significant positive impact on the self-reported retirement behavior of both married and single women.

1.8. CLAIMING BENEFITS AS PRIMARY BENEFICIARY

Increases in the normal retirement age or delayed retirement credit impact individuals by changing their social security wealth; these changes in social security wealth are tied only to the timing of initial receipt of social security benefits and not to earnings (except through automatic benefit recomputations (ABR)). It is possible that individuals below and above the NRA may retire but continue delaying the receipt of benefits to avail the actuarial adjustments applied to postponed benefits.[58] Coile, et al. (2002) emphasize that retirement is a necessary but not sufficient condition for claiming social security benefits by noting a variety of circumstances under which delayed claiming is optimal for an individual. If the timing of retirement is distinct from the timing of benefit receipt, then increases in the normal retirement age or delayed retirement credit will not impact retirement age, instead we will observe only a change in the benefit claiming behavior.

The earnings test, however, ties earnings of workers within the affected age range to their social security wealth.[59] Older workers earning above the threshold amount see their social security wealth decline. By providing a direct link between work and social security wealth, the earnings test affects the timing of both retirement and benefit receipt among older workers. Complete removal of the earnings test will impact older workers in two ways: delay retirement and hasten benefit receipt. Gruber and Orszag (1999, 2003) oppose the removal of earnings test for individuals below the normal retirement age with

[58] The argument seems plausible only for those individuals for whom the adjustments for delaying benefits are actuarially unfair.
[59] In the life-cycle model, the earnings test ties earnings of workers to their social security wealth only in cases where it acts as a tax.

the argument that it will increase benefit receipt among individuals while continuing to work.

To understand the change in timing of initial receipt of benefits in response to the changes in the Social Security policies, I analyze the benefit claiming decision separately from the retirement decision. The sample selection criterion is summarized in Table 1.15, and the actual number of men and women observed at the sampling wave are summarized in Tables 1.16-1.17. The sample consists of all individuals who work at a job/business and have not claimed social security benefits as a primary beneficiary at the time of their first interview. Individuals claiming disability benefits at any point in the survey are excluded.[60] An individual is viewed as claiming benefits when he first claims benefits as a primary beneficiary (regardless of his work status following the first interview).[61]

A comparison of the number of individuals present at the sampling wave under the labor force retirement definition and claiming definition in Tables 1.7 and 1.18 highlights the difference between those continuing work without receiving benefits and

[60] In total all observations for 392 men and 367 women claiming disability benefits (at any point in the survey) were dropped. An individual claiming disability benefits below the normal retirement age receives a benefit amount equivalent to his full PIA that he would have received at his normal retirement age. When individuals receiving disability benefits reach the normal retirement age their disability benefits are automatically converted to social security benefits. The work incentives under the social security disability program differ from the work incentives under social security. Individuals receiving disability benefits can also receive Medicare coverage at an age before 65 (the eligibility age for Medicare).

[61] I ignore all observations on the individual after he is observed (within the sample) to claim social security for the first time, in effect I ignore individuals who may change their mind regarding receipt of benefits following initial claim. Individuals above the normal retirement age and below age 70 can change their mind, and suspend receiving their benefits at any point. Individuals below the normal retirement age can withdraw their application at any point within the first twelve months of becoming entitled. I also exclude individuals who are above age 70 and have not yet claimed benefits because under social security rules all eligible beneficiaries automatically receive benefits at that age. It is also possible that based on their earnings history these individuals may not be eligible for social security benefits, these individuals may then not respond to social security incentives, so I do not include them in the analysis.

those continuing work while receiving benefits. In making this comparison I focus only on individuals at and above age 62 because this is the earliest age at which benefits can be claimed. Only one third of the older men employed at a job/business have not claimed benefits. Among working older women, approximately one fourth have not claimed benefits, suggesting that more women claim benefits while continuing to work than men.[62] More than sixty percent of the men who are working and view themselves as not retired have also not claimed their social security benefits. Upon further calculations not seen in the table, among those who are employed and are not claiming benefits, only one fifth report themselves as having retired from a previous job.[63] The above comparison reveals that for many older men and women the decision to claim benefits is separate from the decision to continue work. It appears that most men and women decide to claim benefits before they leave the labor force. For older men, there is also a suggestion that those working and not claiming social security benefits do not assess themselves as retired.

The difference between the timing of retirement and the claiming of social security benefits is also apparent from a comparison of the claiming hazard plot with the retirement hazard plots described earlier. One clear difference between the hazard rate plots for both men and women claiming benefits in the 1996 panel (Figures 1.5 A and B) and the retirement hazard rate plots described earlier, is the presence of very conspicuous

[62] The larger difference between the number of women who are employed at ages below 62 and those not claiming benefits while employed at these ages could arise because some women may indicate claiming benefits as a widow. Reduced widow benefits can be received as early as age 60. If the surviving spouse is disabled, then she can begin receiving benefits as early as age 50.
[63] Among men 62 years and older who are at a job and not claiming social security benefits at the time of first interview, 421 individuals report themselves as having retired from a job. Among women 62 years and older, only 5 declare themselves as having retired from a previous job.

claiming spikes at ages 62, 65, and 70 relative to the retirement spikes at these ages. A second difference is the relatively flat hazard rates of claiming benefits at all other ages below 70. The flat hazard rates imply that most individuals who are at a job and have not claimed social security benefits tend to claim benefits at ages 62, 65, and 70, while the retirement hazard rates vary at other ages below 70 as individuals continue to retire at other ages as well. With the passage of time for both men and women a much higher spike emerges at the new higher normal retirement age than at any other age. The peaks at ages 62 and 65 remain stable over time. There is also a change in the behavior of men and women who are between the normal retirement age and age 70, as their hazard rate is no longer flat but rather it increases in the later panels indicating an increase in the benefit claiming activity among these older individuals.[64]

1.8.1. ESTIMATION FINDINGS AND ROBUSTNESS CHECKS FOR MEN

I first present the findings for estimation of claiming hazard for men without controls for age specific linear time trends and age specific business cycle effects in columns 1 through 4 of Table 1.19. The partial effects are summarized in Table 1.20. Similar to the retirement definitions, I do not find any effect of the earnings test repeal on the probability of claiming benefits when evaluated at the threshold level in place before the earnings test repeal of 2000. The changes in the delayed retirement credit, reduce the probability of claiming benefits among older men above the normal retirement age, and the effect is statistically significant. I also find that men with at least a college education have a lower likelihood for claiming benefits relative to men with a high school degree only. Men below the age of 65 who are covered by health insurance through their current employer also have a statistically significant lower claiming hazard relative to men above age 65 whose current employer provides their health insurance coverage.

When I include controls for age specific linear time trends in columns 5 through 8 of Table 1.19, the evidence for an impact of the delayed retirement credit on the claiming hazard of men above their NRA dissipates. The effect is no longer significant once I include controls for health insurance. Similar to the finding of Gruber and Orszag (2003), I also observe a robust statistically significant effect of the presence of the earnings test in reducing the claiming hazard among older men. The claiming hazard among these men is reduced by 8-11 percentage points relative to men who did not face the earnings test. The size of this impact is almost two times the average claiming hazard at all ages. Another robust finding is of a norm related impact of the normal retirement age in accelerating claiming among men. I find a positive, statistically significant impact of being at the normal retirement age. The magnitude of this effect is fairly large ranging from a 20-40 percentage points increase in the probability of claiming at the NRA relative to below.[65] I also find weak evidence that changes in the delayed retirement credit, reduce the probability of claiming social security benefits among older men who are within the affected age range relative to those outside the affected range, but this

[65] The unusually large partial effects for the earnings test repeal and for being at the normal retirement age warrant further investigation due to two reasons. First, the earnings test repeal and the norm effect at the normal retirement age exert a statistically significant influence only when I control for linear age trends. To understand the large partial effects of these two variables, I need to explore how the linear age trends are affecting the claiming behavior. I am working on this. Second, the dummy for being at the normal retirement age may be capturing the effect of Medicare. I attempt to control for the effect of Medicare by including an age 65 dummy but it is possible that an individual whose normal retirement age is 65 years and 2 months may claim his/her benefits at this age either due to norm effects or Medicare or by choosing to coordinate the two. I am working on isolating the impact on claiming behavior that arises through norm effects.

effect is only marginally significant in the presence of controls for employer provided health insurance and the effect vanishes when control for union coverage is added.

1.8.2. FINDINGS AND ROBUSTNESS CHECKS FOR WOMEN

Women's claiming behavior summarized in Table 1.21 differs from that of men in at least three ways. First, the earnings test in place variable exerts a significant influence on the claiming hazard of women only in specifications which do not control for linear age trends, and then too in the opposite direction from men. In the first four columns of Table 1.21, I find that the earnings test repeal reduces claiming among women, but this effect disappears when I add linear age trends to the model. Thus, I am unable to provide evidence in support of the finding by Gruber and Orszag (2003) that loosening of the earnings test raises claiming among both men and women.[66] One explanation for the non-response in claiming behavior of women to the earnings test repeal suggested by Reimers and Honig (1996) could be that women have more opportunities for part time work relative to men which provides them the option to be employed in part time jobs and receive full social security benefits. A repeal of the earnings test may prompt these women to work more hours but will not influence their benefit claiming decision. Second, being at the normal retirement age reduces claiming among women, but this effect is not robust to the exclusion of health insurance dummies in the specification where I control for linear age trends.

[66] One possible reason for the difference in my findings could be that I am evaluating the impact of the full earnings test repeal, while Gruber and Orszag (2003) focus on the impact of a loosening of the earnings test threshold.

Third, I find weak evidence that an increase in the delayed retirement credit reduces claiming more among women below the normal retirement age relative to those above; the finding is sensitive to the exclusion of controls for employer provided health insurance. If benefit delay is tied to work for women, particularly single women, then men and women may vary in their response to the changes in the delayed retirement credit possibly due to differences in their work attachment. Younger women who continue to work may be able to afford the postponing their benefits more easily than older single women above the normal retirement age. I also find that married women have a higher claiming hazard relative to single women.

1.8.3. CLAIMING BEHAVIOR BY MARITAL STATUS

A feature of the Social Security system is that upon a married worker's death his/her spouse is eligible for survivor's benefits based on the worker's earnings records. Because of this feature, when a married worker delays receiving benefits for one year, the actuarial adjustment applied to his benefits exerts two effects: it increases his future benefits and raises the benefits that his surviving spouse may receive.[67] Coile et. al. (2002) highlight the different incentives faced by a married relative to a single worker by simulating the change in the expected present discounted value of net future benefits from delayed claiming.[68] They find that gains from delayed claiming are greater for

---

[67] A non-disabled surviving spouse can receive reduced benefits as early as age 60, and is eligible to receive full survivor's benefits at his/her normal retirement age. If the surviving spouse is also eligible for social security benefits based on his/her own earnings record, the higher of the two benefits will be awarded.

[68]The simulations are performed for a married worker with a non-working wife (she has no earnings history), assuming the worker retires at age 62, his wage history is calculated from the economy-wide median earnings profile for his cohort of workers between ages 20 to 50, a household discount rate of 3 %, and mortality risks are taken from the Social Security Administration's sex- and cohort-specific survival tables.

married relative to single men, so married men have a stronger financial incentive to delay.

Coile et. al. (2002) empirically test this prediction for men by using Social Security Administration's New Beneficiary Data System (NBDS). Contrary to their prediction, they find that single men are more likely to delay claiming benefits relative to those married. They reconcile this unexpected finding, by noting that even though married workers face a greater incentive to delay, they may not do so because the presence of a spouse may provide self-insurance against the uncertainty of death. Figinski (2012) examined differences in the claiming behavior of women, but he assessed the difference by separating women based on the type of benefit that they are eligible for: spousal relative to primary. He finds that the earnings test repeal raised claiming among men and women; for women, he observes higher claiming by both primary and spousal beneficiaries. Based on his findings, it appears that women who are primary beneficiaries respond to the repeal in a manner similar to men, while spousal beneficiaries may be influenced by their husband's claiming decision.[69] It is possible, however, that the claiming behavior is impacted by marital status of the woman. Married women who are primary beneficiaries may also be influenced by their husband's incentives, or coordinating their claiming decision with their husband.[70] Similar to Coile et. al.'s

[69] A woman eligible for Social Security spousal benefits can file to receive her benefits, only after her husband (the primary beneficiary) has claimed his benefits. Figinski (2012) is unable to examine the link between the claiming behavior of spousal beneficiaries and that of their husband, as in his administrative data he unable to match spouses.

[70] The Senior Citizens Freedom to Work Act of 2000, also introduced the claim and suspend policy. A feature of the claim and suspend option is that it permits one individuals from a married couple who has attained normal retirement age to claim his/her primary benefits, thus, allowing their spouse to file for the spousal benefits. The primary beneficiary who initially claimed the benefits, can then choose to suspend his/her primary benefits so as to earn delayed retirement credit actuarial adjustments on them. With this option, married women with high earnings may

finding for men, we may observe delayed claiming by single women because they may lack the self-insurance provided by a spouse. I extend the previous studies in two ways. First, I investigate the existence of any difference in claiming behavior of married and single men in response to the recent changes in Social Security policies. Second, I also explore whether the benefit claiming response varies between married and single women.

In Tables 1.22 and 1.23, I present the findings from the estimation of separate claiming models for married and single men and women. I report the estimation findings from four different specifications in columns 1 through 4, each specification includes controls for age-group specific trends in benefit receipt. Specifications 1 and 2 assess the sensitivity of the findings to the inclusion of controls for employer provided health insurance and union coverage status of the individual. Under the earnings test, the earnings of individuals below the normal retirement age are taxed at a higher rate of 50 percent relative to the 33 percent tax rate faced by individuals above the normal retirement age. In specification 3, I account for the presence of a higher tax on the earnings of individuals below the normal retirement age by including an earnings test tax at 50 percent dummy which takes a value of one for all individuals between ages 62-NRA; specification 4 examines the robustness of the findings in specification 3 to inclusion of controls for health insurance and union coverage by employer. In order to assess whether married couples account for their spouse's incentives in making their

claim spousal benefits initially, while postponing their primary benefits and accruing a delayed retirement credit on them. If spousal benefits are used strategically by married couples then it is not clear how one should interpret the empirical findings related to spousal beneficiaries.

claiming decision, the four specifications for married individuals include the incentives faced by the spouse as well.

Findings for Married and Single Men: The first set of results in Table 1.22 pertains to single men. I find some weak support that the earnings test repeal raised claiming among single men; this finding surfaces only in the absence of controls for health insurance and union coverage. Apart from this weak evidence of the influence exerted by the earnings test repeal, I find no other response in the claiming behavior of single men to changes in the Social Security policies.[71] The second set of results for married men highlights the stronger responsiveness of their claiming behavior relative to single men in response to the policy changes. I find that the repeal of the earnings test lead married men above the normal retirement age to hasten their claiming, this finding is robust to controls for health insurance and union coverage, as well as the control for the higher tax at 50 percent. I also find that married men are more likely to claim when they reach their normal retirement age.[72]

Changes in the delayed retirement credit reduce claiming among men above the normal retirement age as well those below, but this reduction is larger among men above the normal retirement age. This implies that as the actuarial adjustments applied to future benefits rise, older married men directly affected by the policy change respond more strongly than younger men by modifying their claiming behavior. The claiming response

[71] In specification 3 there is evidence of a larger reduction in the claiming hazard of men above the normal retirement age relative to those below in response to changes in the delayed retirement credit, but even in specifications that exclude controls for health insurance and union coverage, this finding is not robust to the exclusion of tax at 50 percent dummy.
[72] As I noted earlier, this finding warrants further investigation as the dummy for being at the normal retirement age may be picking up the effect of Medicare eligibility that begins at age 65.

of married men to the earnings test repeal and changes in the delayed retirement credit provides evidence in support of the prediction made by Coile et. al. (2002) that married men have a stronger financial incentive to delay than single men.

I find weak evidence that suggests husband's account for the incentives faced by their wife while making their claiming decision. The repeal of the earnings test raises claiming among men whose wife's are above the normal retirement age and no longer subject to the earnings test, this finding is not robust to controlling for the differential effect of the earnings test on individuals below the normal retirement age who face a tax of 50 percent. An increase in the delayed retirement credit of a wife who is above the normal retirement age also influences the claiming decision of her husband by leading him to delay his claiming. These responses may arise if married couples coordinate their claiming decisions; they may file for benefits together.[73] The response of the husband's claiming decision to changes in the wife's delayed retirement credit, in particular, suggests coordination of benefit claiming, because for wives above the normal retirement age an increase in the delayed retirement credit only affects their own primary benefits.[74] As older women respond to the delayed retirement credit by postponing benefit receipt, their husband's may match their wives' filing decision. To summarize, I find that the response in the claiming decision of men to the policy changes is driven by married men.

Findings for Married and Single Women: I begin by discussing the findings from the estimation of the claiming models for single women presented in the first four

[73] In the claiming sample the average age difference between husband and wife is three years (standard deviation is 5 years). It is possible some of this response could arise due to the correlation in the policy changes affecting husbands and wives of similar ages.
[74] The changes in the delayed retirement credit do not affect spousal benefits.

columns of Table 1.23. I do not find any robust response to the policy changes by single women.[75] The second set of findings refers to married women, for whom I do not find robust evidence of an effect of the earnings test repeal. I find weak support that the repeal increased claiming among married women, only in specification which do not control for employer provided health insurance and union coverage. The earnings test repeal, thus, exerts opposite influences on the claiming behavior of married relative to single women, which explains the finding of no response in the claiming behavior of women to the earnings test repeal when estimated on the full sample. Changes in the delayed retirement credit reduce claiming among married women above the normal retirement age, but the effect is not statistically significant. Similar to married men, I find weak support that the earnings test repeal for husband raises benefit claiming by wife possibly either due to wife's ability to now claim spousal benefits (because husband claims) or co-ordination in claiming decisions of spouses, this finding is not robust to controlling for the higher tax rate face by individuals below the normal retirement age.

1.9. SUMMARY

Three changes in the U.S. Social Security program affected recent cohorts of older individuals: repeal of the earnings test, increases in the normal retirement age, and increases in the delayed retirement credit. All three policy changes were intended to encourage work for those eligible for social security benefits. Three recent studies, Blau and Goodstein (2010), Mastrobuoni (2009), and Pingle (2006) have examined the effect of these policy changes on the labor force participation or retirement behavior of older

[75] For the analysis of single women, I exclude widowed women.

men and women. I improve upon the work of these researchers in four ways. First, there is minimal evidence in the recent literature investigating the sensitivity of estimated responses to the definition of retirement for a broad age-range of older individuals. Given the variety of options explored by older individuals in their transition to retirement, it seems important to analyze how older workers' response to the policy changes differs when evaluated using alternative definitions of retirement. I examine how responses to the policy changes differ when assessed using an objective relative to a subjective definition of retirement. Second, none of the recent studies evaluating the influence of the earnings test on labor force participation or retirement from labor force incorporate the sizeable variation in the earnings test threshold amount (noted by Gruber and Orszag) that was introduced through the ad hoc changes implemented after 1996. Third, there is little work in the recent literature that attempts to disentangle the financial and norm related effects arising from the NRA changes. In my work I am able to separately identify these effects because the SIPP data includes accurate birth year information of individuals. Finally, although older women are a sizeable part of the labor force none of the recent studies have examined the changes in the retirement behavior of older women (particularly those above the normal retirement age) in response to changes in the Social Security policies. I provide evidence for the response in retirement behavior for both men and women between ages 60 – 74.

My findings indicate that the response to the recent policy changes is sensitive to the retirement definition under consideration. I find no effect of the recent policy changes on retirement based on labor force activity. The lack of response in retirement from labor force to the reduction in benefits associated with the rising normal retirement age supports the finding of Krueger and Pischke (1992), while the non-response in retirement from labor force to the earnings test repeal supports the findings of Gruber and Orszag (2003). In contrast, to other researchers I find that increases in the delayed retirement credit reduce the probability of self-reported retirement among men and women above the normal retirement age. There is stronger evidence of an effect of the recent policy changes on claiming behavior of older individuals. I observe that increases in the delayed retirement credit reduced claiming among men and women who are directly affected by these changes. Similar to Gruber and Orszag (2003), I find that the earnings test repeal raised claiming among men, and the effect is large. Unlike Gruber and Orszag, I find no impact of the earnings test repeal on the benefit claiming decision of older women.

I do find evidence that changes in the delayed retirement credit had a significant impact in modifying the self-reported retirement and claiming behavior of older men and women above the normal retirement age. In Table 1.9, I show that the self-reported retirement behavior differs from the retirement from labor force behavior; many older workers continue to participate in the labor force while stating themselves as retired. Older workers may consider themselves retired when they end their full time employment at a long held career job but continue work at a new part-time job, switch to part-time work or reduced work load at their career job, or move to a job where their earnings are substantially reduced, self-reported retirement behavior might capture that response. The findings from my paper suggest that self-reported retirement may also be linked to the decision to claim Social Security benefits. It is possible that older workers who have not yet claimed their Social Security benefits respond to increases in the delayed retirement credit by postponing the receipt of their benefits, this delay in claiming benefits may in turn induce affected individuals to be less likely to view themselves as retired.

Table 1.1 Normal Retirement Age by Birth

Year of birth	Normal Retirement Age (NRA)
1937 and prior	65
1938	65 and 2 months
1939	65 and 4 months
1940	65 and 6 months
1941	65 and 8 months
1942	65 and 10 months
1943-54	66

Note: Source -- U.S. Social Security Admin.
The normal retirement age is scheduled to rise again in increments of 2 months for birth cohorts born after 1954 until it reaches age 67.

Table 1.2 Delayed Retirement Credit by Birth Year

Year of birth	Delayed Retirement Credit (DRC) per year
1921-24	3.00%
1925-26	3.50%
1927-28	4.00%
1929-30	4.50%
1931-32	5.00%
1933-34	5.50%
1935-36	6.00%
1937-38	6.50%
1939-40	7.00%
1941-42	7.50%
1943 and later	8.00%

Note: Source -- U.S. Social Security Administration.

Table 1.3 Earnings Test Annual Threshold Amount and Tax Rate

	Below normal retirement age[1]		Year individual attains normal retirement age[2]		At and Above normal retirement age - age 70	
Year	Threshold Amount ($)	Tax Rate (%)	Threshold Amount ($)	Tax Rate (%)	Threshold Amount ($)	Tax Rate (%)
1996	8,280	50	8,280	50	12,500	33.33
1997	8,640	50	8,640	50	13,500	33.33
1998	9,120	50	9,120	50	14,500	33.33
1999	9,600	50	9,600	50	15,500	33.33
2000	10,080	50	17,000	33.33	Earnings test eliminated in 2000 for those at and above normal retirement age.	
2001	10,680	50	25,000	33.33		
2002	11,280	50	30,000	33.33		
2003	11,520	50	30,720	33.33		
2004	11,640	50	31,080	33.33		
2005	12,000	50	31,800	33.33		
2006	12,480	50	33,240	33.33		
2007	12,960	50	34,440	33.33		
2008	13,560	50	36,120	33.33		
2009	14,160	50	37,680	33.33		
2010	14,160	50	37,680	33.33		
2011	14,160	50	37,680	33.33		
2012	14,640	50	38,880	33.33		
2013	15,120	50	40,080	33.33		

Note: Source -- U.S. Social Security Administration.

The threshold amount is in nominal dollars. As the normal retirement age for a specific birth cohort changes, the above rules extend to the new higher normal retirement age. In any giver year members of various birth cohorts fall either below or above the normal retirement age. Here, the normal retirement age refers to the normal retirement age specific to each birth cohort.

[1] Below normal retirement age refers to all ages 62 and older that are below the normal retirement age (specific to each birth cohort).

[2] Year an individual reaches normal retirement age, refers only to months before an individual reaches his normal retirement age in the year that he attains his normal retirement age.

Table 1.4A Theoretical Predictions for impact on Probability of Retirement of recent changes in Social Security Law

Age Group		Changes in Social Security law				
		Normal retirement age rises	Delayed retirement credit rises	Earnings Test		
				Threshold rises	Tax rate falls	Complete repeal
60 to 62	Substitution effect	No effect	↑	No effect	No effect	No effect
	Income effect	↓	No effect	No effect	No effect	No effect
	Overall	↓	↑	No effect	No effect	No effect
62 to 63	Substitution effect	↑	↑	↑	↑	↑
	Income effect	↓	No effect	No effect	No effect	No effect
	Overall	Ambiguous	↑	↑	↑	↑
63 to 65	Substitution effect	No effect	↑	↑	↑	↑
	Income effect	↓	No effect	No effect	No effect	No effect
	Overall	↓	↑	↑	↑	↑

Note: The policy changes and their impact on retirement age summarized here are described for each policy change in isolation (holding all other policy variables constant). The analysis for the delayed retirement credit and earnings test changes are summarized assuming the normal retirement age stays at age 65, while the earnings test predictions of the recent changes are made for those above their normal retirement age of 65.
** The substitution effect from an increase in the normal retirement age applies between age 65 and the new higher NRA. For instance if the new higher NRA is 65 years and 6 months then the susbtition effect applies between age 65 and 65 years and 6 months only. As the normal retirement age increases to 66, the substitution effect extends over the entire age range 65-66.

Table 1.4B Theoretical Predictions for impact on Probability of Retirement of recent changes in Social Security Law

Age Group		Changes in Social Security law				
		Normal retirement age rises	Delayed retirement credit rises	Earnings Test		
				Threshold rises	Tax rate falls	Complete repeal
65 to 66	Substitution effect	↓**	↓	↓	↓	↓
	Income effect	↓	↑	No effect	No effect	No effect
	Overall	↓	Ambiguous	↓	↓	↓
66 to 70	Substitution effect	No effect	↓	↓	↓	↓
	Income effect	↓	↑	No effect	No effect	No effect
	Overall	↓	Ambiguous	↓	↓	↓
70 to 75	Substitution effect	No effect	No effect	No effect	No effect	No effect
	Income effect	↓	No effect	No effect	No effect	No effect
	Overall	↓	No effect	No effect	No effect	No effect

Table 1.5 Survey of Income Program and Participation (SIPP) Panel Details

Panel	Date of First Interview	Date of Last Interview	Data Available for Reference Period	Number of Waves	Length of Panel (years)
1996	Apr. 1996	Mar. 2000	Dec. 1995 - Feb. 2000	12	4
2001	Feb. 2001	Jan. 2004	Oct. 2000 - Dec. 2003	9	3
2004	Feb. 2004	Jan. 2008	Oct. 2003 - Dec. 2007	12	4
2008	Sep. 2008	Aug. 2013*	May 2008 - Jul. 2013*	15*	5*

Note: Date of first interview refers to the date of interview for rotation group 1 which was the first group to be interviewed, while the date of last interview refers to the date of interview for rotation group 4 which was the last group to be interviewed. The data is available for four months preceding the date of each interview. Length of the panel refers to the number of years that members of a specific rotation group were interviewed (alternatively, the maximum number of years for which an individual can be observed in a given panel).

*The full set of data files for 2008 panel was not available at the time of compiling the dataset used in the analysis here. The date of the last interview for panel 2008 listed above refers to the last interview for the data used in the paper. The actual date of the last interview for panel 2008 is December 2013. The panel has 16 waves in total, but I use only the first 15 waves because the data for the 16th wave was unavailable.

Table 1.6 Retirement Definitions and Selection at Sampling Wave

Retirement Definition	Selection of Sample	Retirement Criterion
Withdrawal from Labor Force	All individuals between ages 60 and 75 who are i) with a job/business[1] or ii) looking for work[2] sometime in the four months preceding the first interview[3].	An individual is considered retired if he states that he i) has not worked at any paid job/business and ii) not looked for work in any of the four months preceding the month of interview. I consider the individual to have retired sometime in the four month period prior to the one in which he reports complete withdrawal from the labor force[4].
Self-Reported Retirement	All individuals between ages 60 and 75 who are i) with a job/business and ii) report themselves as having never retired from a job/business in any of the four months preceding the first interview.	An individual is considered retired when he i) first reports that he has retired from a job/business sometime in the four months precdeing the month of interview. I consider the individual to have retired sometime in the four month period when he first reports himself as retired.

Table 1.7 Total Number of Men under each Retirement Definition

Retirement Definition	Panel	Number of Men (all ages 60-75)	Number of Men At and Above age 62	Number of Men Below age 62
Withdrawal from Labor Force	1996	2434	1265	1169
	2001	2246	1233	1013
	2004	2852	1579	1273
	2008	4058	1975	2083
	Total	11,590	6,052	5,538
Self-Reported Retirement	1996	1475	631	844
	2001	1418	656	762
	2004	1801	835	966
	2008	2757	1151	1606
	Total	7,451	3,273	4,178

Note: I split the total number of men into those above and below age 62 because 62 is the earliest age of eligibility for social security benefits. The number of men below and above age 62 indicates only those who are below and above 62 at the time of their first interview. As the survey progresses some men below age 62 may provide observations above age 62.
The number of individuals at the sampling wave for each retirement definition are restricted to those initially working.

Table 1.8 Total Number of Women under each Retirement Definition

Retirement Definition	Panel	Number of Women (all ages 60-75)	Number of Women At and Above age 62	Number of Women Below age 62
Withdrawal from Labor Force	1996	2153	1111	1042
	2001	1852	979	873
	2004	2722	1423	1299
	2008	3801	1666	2135
	Total	10,528	5,179	5,349
Self-Reported Retirement	1996	1589	744	845
	2001	1363	638	725
	2004	2033	953	1080
	2008	2915	1097	1818
	Total	7,900	3,432	4,468

Refer to Table 1.7 notes.

Table 1.9 Relationship between Retirement Definitions for Men

Timing of Retirement	Retirement Definition	
	Withdrawal from Labor Force (WLF) observed (%)	Self-Reported Retirement (SR) observed (%)
WLF < SR	4.74	11.27
Same	8	21.13
WLF > SR	87.27	67.60

Table 1.10A Descriptive Statistics for Men: Retirement from Labor Force

		Mean (Standard deviation)	Minimum	Maximum
Time Constant Variables	Normal retirement age (years)	65.51 (0.44)	65	66
	Delayed retirement credit (percent)	6.8 (1.32)	3	8
	Birth year	1939.91 (6.75)	1921	1953
	Black	0.09	0	1
	Asian	0.03	0	1
Time Varying Variables	Age (years)	64.4 (4.11)	60	75
	Earnings test in place dummy	0.34	0	1
	At normal retirement age	0.02	0	1
	Below normal retirement age	0.67	0	1
	Above normal retirement age	0.31	0	1
	Retirement rate[a]	0.26	0	1
	Age at retirement (years)[a]	65.2 (4.27)	60	75
	Unemployment rate	6.12 (1.93)	2.22	13.82
	Number of members in household	2.34 (1.07)	1	14
	Married	0.8	0	1
	Union coverage	0.12	0	1
	Employer health insurance	0.48	0	1
	Less than high school	0.14	0	1
	Some college	0.27	0	1
	College	0.33	0	1
	Children under 18	0.23	0	1
	Ownership of home	0.87	0	1
	Number of individuals	11,590		

Note: The mean and standard deviation for time varying variables are calculated by first averaging over all the observations available for each individual (person wave information).

[a] The retirement rate and average age at retirement variables are not averaged over the person wave information, instead the numbers reported here are the means among individuals at the time of their last observed wave in the sample.

Table 1.10B Descriptive Statistics for Men: Retirement from Labor Force

	Mean (Standard deviation)	Minimum	Maximum
Individuals facing earnings test			
Threshold amount - $21.67 (thousands of dollars)	-5.83 (3.65)	-9.38	19.23
Taxrate of 50 percent dummy	0.85	0	1
Number of individuals	5731		

Table 1.11 Estimates of Retirement Hazard for Men: Withdrawal from Labor Force

	Specification						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delayed retirement credit (percent)	0.005	0.005	-0.044	-0.024	-0.003	-0.007	-0.082
	(0.045)	(0.045)	(0.057)	(0.059)	(0.067)	(0.067)	(0.073)
Normal retirement age (months)	-0.012	-0.010	-0.014	-0.019**	-0.013	-0.012	-0.006
	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)
Earnings test in place dummy	-0.087	-0.097	-0.105	0.172	0.251	0.246	0.068
	(0.095)	(0.095)	(0.106)	(0.166)	(0.188)	(0.188)	(0.199)
Earnings test dummy * (Earnings threshold - $21.67)	-0.115***	-0.116***	-0.114***	-0.119***	-0.124***	-0.123***	-0.182***
(thousands of dollars)	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)	(0.020)
Earnings test with tax rate at 50 percent dummy							-0.940***
							(0.312)
At normal retirement age dummy			0.065	-0.507*	-0.502	-0.479	-0.531
			(0.164)	(0.308)	(0.357)	(0.356)	(0.357)
Above normal retirement age dummy			-0.244	-0.742***	-0.712**	-0.682**	-0.735**
			(0.161)	(0.273)	(0.318)	(0.317)	(0.319)
Delayed retirement credit * Outside age range affected by				-0.123**	-0.121*	-0.117*	-0.038
delayed retirement credit dummy				(0.055)	(0.063)	(0.062)	(0.070)
Black		0.247***	0.245***	0.243***	0.246***	0.200***	0.199***
		(0.066)	(0.066)	(0.066)	(0.075)	(0.076)	(0.076)
College		-0.390***	-0.390***	-0.389***	-0.400***	-0.373***	-0.375***
		(0.052)	(0.052)	(0.052)	(0.059)	(0.059)	(0.059)
Lagged union coverage dummy						0.426***	0.426***
						(0.065)	(0.065)
Lagged employer health insurance dummy					-0.061	-0.125*	-0.116*
					(0.067)	(0.068)	(0.068)
Lagged employer health insurance * Below age 65 dummy					-0.263***	-0.263***	-0.280***
					(0.090)	(0.090)	(0.090)
Number of observations	64,927	64,927	64,927	64,927	53,244	53,244	53,244
Number of individuals	11438	11438	11438	11438	9473	9473	9473

Note: The above findings are coefficient estimates from the estimation of a dicrete hazard logit model. All seven specifications include controls for age dummies (age in years) to allow the retirement hazard to vary by age (non-parametric baseline hazard), quarter dummies, and panel dummies. Specifications 2 through 7 also include controls for region, number of household members, children under age 18, and ownership of home. Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals. *** p<0.01, ** p<0.05, * p<0.1

Table 1.12 Partial Effects of Retirement Hazard for Men: Withdrawal from Labor Force

	Specification						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delayed retirement credit (percent)	0.0002	0.0002	-0.0019	-0.0051	-0.0040	-0.0040	-0.0046
Normal retirement age (months)	-0.0005	-0.0005	-0.0006	-0.0008	-0.0006	-0.0005	-0.0003
Earnings test in place dummy	-0.0038	-0.0042	-0.0046	0.0076	0.0107	0.0104	0.0028
Earnings test dummy * (Earnings threshold - $21.67) (thousands of dollars)	-0.0051	-0.0051	-0.0050	-0.0052	-0.0052	-0.0052	-0.0076
Earnings test with tax rate at 50 percent dummy							-0.0402
At normal retirement age dummy			0.0029	-0.0180	-0.0172	-0.0165	-0.0180
Above normal retirement age dummy			-0.0104	-0.0317	-0.0294	-0.0281	-0.0304
Delayed retirement credit * Outside age range affected by delayed retirement credit dummy				-0.0050	-0.0049	-0.0047	-0.0010

Note: Partial effects are computed using stata's margins and over command.

Table 1.13 Estimates of Retirement Hazard for Men: Self-Reported Retirement

	Specification						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delayed retirement credit (percent)	-0.347***	-0.347***	-0.299***	-0.306***	-0.231***	-0.242***	-0.290***
	(0.067)	(0.068)	(0.076)	(0.076)	(0.075)	(0.075)	(0.080)
Normal retirement age (months)	0.010	0.010	0.009	0.012	0.015	0.017	0.020
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Earnings test in place dummy	0.008	0.012	0.114	-0.062	0.047	0.031	-0.079
	(0.114)	(0.115)	(0.131)	(0.192)	(0.195)	(0.195)	(0.203)
Earnings test dummy * (Earnings threshold - $21.67)	-0.103***	-0.103***	-0.115***	-0.111***	-0.105***	-0.104***	-0.143***
(thousands of dollars)	(0.011)	(0.011)	(0.012)	(0.011)	(0.010)	(0.010)	(0.017)
Earnings test with tax rate at 50 percent dummy							-0.650**
							(0.308)
At normal retirement age dummy			0.384**	0.798**	0.467	0.484	0.461
			(0.184)	(0.382)	(0.380)	(0.378)	(0.378)
Above normal retirement age dummy			0.211	0.584*	0.229	0.252	0.224
			(0.183)	(0.345)	(0.342)	(0.341)	(0.340)
Delayed retirement credit * Outside age range affected by				0.084	0.026	0.031	0.088
delayed retirement credit dummy				(0.066)	(0.066)	(0.066)	(0.073)
Lagged union coverage dummy						0.498***	0.498***
						(0.067)	(0.067)
Lagged employer health insurance dummy					0.078	-0.005	0.000
					(0.072)	(0.073)	(0.073)
Lagged employer health insurance * Below age 65 dummy					-0.335***	-0.334***	-0.346***
					(0.097)	(0.097)	(0.098)
Number of observations	41,687	41,687	41,687	41,687	37,071	37,071	37,071
Number of individuals	7184	7184	7184	7184	6579	6579	6579

Note: Refer to Table 1.11.

Table 1.14 Partial Effects of Retirement Hazard for Men: Self-Reported Retirement

	Specification						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delayed retirement credit (percent)	-0.0160	-0.0160	-0.0138	-0.0112	-0.0109	-0.0112	-0.0115
Normal retirement age (months)	0.0004	0.0005	0.0004	0.0006	0.0008	0.0009	0.0010
Earnings test in place dummy	0.0004	0.0006	0.0052	-0.0029	0.0024	0.0016	-0.0041
Earnings test dummy * (Earnings threshold - $21.67) (thousands of dollars)	-0.0047	-0.0048	-0.0053	-0.0051	-0.0054	-0.0054	-0.0074
Earnings test with tax rate at 50 percent dummy							-0.0340
At normal retirement age dummy			0.0206	0.0508	0.0290	0.0301	0.0284
Above normal retirement age dummy			0.0101	0.0304	0.0123	0.0135	0.0119
Delayed retirement credit * Outside age range affected by delayed retirement credit dummy				0.0114	0.0068	0.0073	0.0111

Note: Partial effects are computed using stata's margins and over command.

Table 1.15 Robustness Check: Coefficient Estimates of Retirement Hazard for Men

	Retirement from Labor Force				Self-Reported Retirement			
	Specification				Specification			
Variables	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Delayed retirement credit (percent)	-0.063	-0.109	-0.135	-0.146	-0.247**	-0.236**	-0.262**	-0.288**
	(0.098)	(0.110)	(0.110)	(0.097)	(0.103)	(0.118)	(0.118)	(0.116)
Normal retirement age (months)	-0.016	-0.012	-0.011	-0.020	-0.007	0.001	0.001	-0.001
	(0.019)	(0.018)	(0.018)	(0.016)	(0.021)	(0.020)	(0.020)	(0.020)
Earnings test in place dummy	0.043	0.073	0.038	0.100	0.078	0.008	-0.029	0.072
	(0.219)	(0.222)	(0.222)	(0.195)	(0.229)	(0.228)	(0.227)	(0.225)
Earnings test dummy * (Earnings threshold - $21.67)	-0.183***	-0.181***	-0.181***	-0.172***	-0.196***	-0.148***	-0.148***	-0.149***
(thousands of dollars)	(0.028)	(0.018)	(0.018)	(0.015)	(0.026)	(0.012)	(0.012)	(0.012)
Earnings test with tax rate at 50 percent dummy	-0.091				-0.835			
	(0.520)				(0.523)			
At normal retirement age dummy	-0.294	-0.220	-0.084	-0.135	0.867	0.514	0.639	0.529
	(0.624)	(0.663)	(0.663)	(0.583)	(0.656)	(0.678)	(0.676)	(0.670)
Above normal retirement age dummy	-0.308	-0.228	-0.091	-0.174	0.794	0.495	0.621	0.519
	(0.606)	(0.649)	(0.648)	(0.570)	(0.636)	(0.660)	(0.658)	(0.652)
Delayed retirement credit * Outside age range affected by	-0.030	-0.031	-0.008	-0.027	0.176	0.081	0.103	0.081
delayed retirement credit dummy	(0.114)	(0.107)	(0.107)	(0.095)	(0.120)	(0.109)	(0.109)	(0.108)
Lagged union coverage dummy	0.424***	0.423***	0.338***		0.499***	0.504***	0.448***	
	(0.065)	(0.065)	(0.063)		(0.067)	(0.067)	(0.065)	
Lagged employer health insurance dummy	-0.116*	-0.114*			-0.010	-0.012		
	(0.068)	(0.068)			(0.073)	(0.073)		
Lagged employer health insurance * Below age 65 dummy	-0.281***	-0.285***			-0.327***	-0.330***		
	(0.091)	(0.091)			(0.098)	(0.098)		
Interaction of unemployment rate with age dummies	no	yes	yes	yes	no	yes	yes	yes
Number of observations	53,244	53,244	53,244	64,927	37,071	37,071	37,071	41,687
Number of individuals	9473	9473	9473	11438	6579	6579	6579	7184

Note: All four specifications include controls for age dummies (age in years), quarter dummies, panel dummies, marital status, education dummies, state specific unemployment rate, race dummies, region dummies, number of household members, children under age 18, ownership of home, and linear age trends. Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals. *** p<0.01, ** p<0.05, * p<0.1

Table 1.16 Robustness Check: Coefficient Estimates of Retirement Hazard for Women

	Retirement from Labor Force				Self-Reported Retirement			
	Specification				Specification			
Variables	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Delayed retirement credit (percent)	-0.140	-0.046	-0.075	-0.061	-0.496***	-0.533***	-0.551***	-0.553***
	(0.110)	(0.120)	(0.120)	(0.102)	(0.102)	(0.117)	(0.117)	(0.118)
Normal retirement age (months)	0.019	0.021	0.021	0.008	0.059***	0.070***	0.069***	0.064***
	(0.018)	(0.018)	(0.018)	(0.015)	(0.019)	(0.019)	(0.018)	(0.018)
Earnings test in place dummy	-0.221	-0.178	-0.194	-0.199	-0.108	-0.087	-0.097	-0.063
	(0.217)	(0.221)	(0.220)	(0.198)	(0.222)	(0.225)	(0.225)	(0.224)
Earnings test dummy * (Earnings threshold - $21.67)	-0.163***	-0.177***	-0.178***	-0.170***	-0.176***	-0.187***	-0.188***	-0.189***
(thousands of dollars)	(0.026)	(0.018)	(0.018)	(0.015)	(0.030)	(0.022)	(0.022)	(0.022)
Earnings test with tax rate at 50 percent dummy	0.192				0.093			
	(0.542)				(0.521)			
Delayed retirement credit * Outside age range affected by	-0.050	-0.094	-0.081	-0.064	-0.220*	-0.207*	-0.199*	-0.178
delayed retirement credit dummy	(0.120)	(0.111)	(0.111)	(0.100)	(0.118)	(0.117)	(0.117)	(0.117)
Lagged union coverage dummy	0.405***	0.407***	0.314***		0.500***	0.506***	0.420***	
	(0.066)	(0.067)	(0.064)		(0.068)	(0.068)	(0.065)	
Lagged employer health insurance dummy	-0.119*	-0.120*			-0.202***	-0.205***		
	(0.070)	(0.070)			(0.073)	(0.073)		
Lagged employer health insurance * Below age 65	-0.324***	-0.323***			-0.140	-0.140		
dummy	(0.091)	(0.091)			(0.095)	(0.095)		
Interaction of unemployment rate with age dummies	no	yes	yes	yes	no	yes	yes	yes
Number of observations	47,610	47,610	47,610	58,183	40,523	40,523	40,523	45,517
Number of individuals	8572	8572	8572	10369	7035	7035	7035	7643

Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.17 Claiming Definition and Selection into Initial Sample

Initial Selection of Sample	Claiming Criterion
All individuals between ages 60 and 75 who are i) with a job/business in the four months preceding the first interview and ii) have not claimed social security benefits as a primary beneficiary in the four months preceding the first interview and iii) do not report receiving disability benefits throughout their duration in the sample.	An individual is considered to claim social security benefits as a primary beneficiary when he first reports i) receiving social security benefits as a retired person or ii) receiving social security benefits for some reason other than as retired, disabled, widower, or a dependant spouse sometime in the last four months. I consider the social security benefits to be first claimed in the four month period when the individual first meets the above criterion[1].

Note: While choosing the initial sample, I assumed that all 60 to 75 year olds are covered by social security (that is they meet the social security requirement of having 40 quarters of covered earnings). In the above claiming criterion, claiming is defined as an absorbing state. Once an individual satisfies the criterion he cannot change his claiming decision, I drop all observations on the individual following first incidence of claiming benefits. Refer to Table 6 notes for other restrictions on sample.

[1] Since, the actual date of claiming social security benefits is not reported by the indivdual, I consider him to have claimed the benefits during the second month of the four month window during which benefits were first claimed.

Table 1.18 Total Number of Men and Women at the time of first inclusion in the sample: Claiming Definition

	Panel	Number of Men (all ages 60-75)	Number of Men At and Above age 62	Number of Men Below age 62
Men	1996	1247	254	993
	2001	1377	422	955
	2004	1719	517	1202
	2008	2665	793	1872
	Total	7,008	1,986	5,022

	Panel	Number of Women (all ages 60-75)	Number of Women At and Above age 62	Number of Women Below age 62
Women	1996	1016	198	818
	2001	1060	278	782
	2004	1561	398	1163
	2008	2410	557	1853
	Total	6,047	1,431	4,616

Note: These individuals include only those who are working and have not claimed social security benefits at the time of their first interview. Individuals claiming disability benefits at any point in the survey are not included. Refer to Table 1.7 notes.

Table 1.19 Estimates of Claiming Hazard for Men

	Specification							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delayed retirement credit (percent)	-0.432**	-0.543***	-0.543***	-0.389*	-0.578**	-0.276	-0.271	-0.283
	(0.202)	(0.207)	(0.207)	(0.216)	(0.265)	(0.278)	(0.278)	(0.278)
Normal retirement age (months)	-0.038	-0.036	-0.036	-0.075**	0.014	-0.001	-0.001	-0.006
	(0.031)	(0.032)	(0.032)	(0.034)	(0.034)	(0.034)	(0.034)	(0.036)
Earnings test in place dummy	-0.519	-0.400	-0.397	-0.101	-1.696***	-1.293***	-1.292***	-1.234***
	(0.340)	(0.352)	(0.350)	(0.361)	(0.343)	(0.355)	(0.354)	(0.376)
Earnings test dummy * (Earnings threshold - $21.67)	-0.146***	-0.144***	-0.145***	-0.352***	-0.280***	-0.252***	-0.253***	-0.298***
(thousands of dollars)	(0.012)	(0.012)	(0.012)	(0.042)	(0.037)	(0.030)	(0.031)	(0.056)
Earnings test with tax rate at 50 percent dummy				-2.686***				-0.464
				(0.531)				(0.688)
At normal retirement age dummy	1.331	1.090	1.107	1.143	4.018***	2.213**	2.197**	2.301**
	(0.810)	(0.824)	(0.824)	(0.817)	(0.978)	(0.977)	(0.977)	(0.995)
Above normal retirement age dummy	-0.734	-0.991	-0.975	-0.989	2.566***	0.707	0.692	0.790
	(0.759)	(0.774)	(0.774)	(0.767)	(0.965)	(0.974)	(0.973)	(0.991)
Delayed retirement credit * Outside age range affected by	0.077	0.033	0.035	0.167	0.545***	0.263*	0.260	0.286*
delayed retirement credit dummy	(0.131)	(0.135)	(0.135)	(0.140)	(0.160)	(0.160)	(0.160)	(0.169)
Lagged union coverage dummy			0.148*	0.145*			0.151*	0.150*
			(0.080)	(0.080)			(0.081)	(0.081)
Lagged employer health insurance dummy		-0.127	-0.145	-0.138		-0.143	-0.162*	-0.161*
		(0.091)	(0.091)	(0.091)		(0.093)	(0.094)	(0.094)
Lagged employer health insurance * Below age 65 dummy		-1.028***	-1.026***	-1.036***		-0.997***	-0.994***	-0.996***
		(0.112)	(0.112)	(0.112)		(0.114)	(0.114)	(0.114)
Linear age Trends	no	no	no	no	yes	yes	yes	yes
Number of observations	34,502	30,199	30,199	30,199	34,517	30,199	30,199	30,199
Number of individuals	6485	5876	5876	5876	6487	5876	5876	5876

Note: All specifications include controls for age dummies (age in years), quarter dummies, panel dummies, region dummies, number of household members, children under age 18, and ownership of home. Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.20 Partial Effects of Claiming Hazard for Men

Variables	Specification							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delayed retirement credit (percent)	-0.0155	-0.0243	-0.0243	-0.0117	-0.0049	-0.0025	-0.0024	-0.0019
Normal retirement age (months)	-0.0016	-0.0017	-0.0017	-0.0035	0.0006	-0.0001	-0.0001	-0.0003
Earnings test in place dummy	-0.0256	-0.0211	-0.0209	-0.0049	-0.1132	-0.0845	-0.0844	-0.0795
Earnings test dummy * (Earnings threshold - $21.67) (thousands of dollars)	-0.0062	-0.0068	-0.0068	-0.0166	-0.0117	-0.0117	-0.0118	-0.0139
Earnings test with tax rate at 50 percent dummy				-0.2258				-0.0242
At normal retirement age dummy	0.0863	0.0720	0.0734	0.0764	0.3989	0.1891	0.1870	0.1999
Above normal retirement age dummy	-0.0252	-0.0358	-0.0354	-0.0357	0.2102	0.0400	0.0390	0.0457
Delayed retirement credit * Outside age range affected by delayed retirement credit dummy	0.0484	0.0582	0.0583	0.0477	0.0819	0.0402	0.0395	0.0419
Linear age Trends	no	no	no	no	yes	yes	yes	yes

Table 1.21 Estimates of Claiming Hazard for Women

	Specification							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delayed retirement credit (percent)	0.028	0.067	0.069	0.197	-0.213	0.186	0.189	0.187
	(0.241)	(0.249)	(0.249)	(0.258)	(0.314)	(0.311)	(0.311)	(0.312)
Normal retirement age (months)	-0.028	-0.033	-0.034	-0.062*	0.032	0.015	0.014	0.014
	(0.034)	(0.034)	(0.034)	(0.036)	(0.035)	(0.036)	(0.036)	(0.038)
Earnings test in place dummy	1.091**	1.271***	1.281***	1.500***	-0.133	0.321	0.331	0.339
	(0.443)	(0.454)	(0.453)	(0.465)	(0.430)	(0.445)	(0.445)	(0.466)
Earnings test dummy * (Earnings threshold - $21.67)	-0.155***	-0.153***	-0.153***	-0.304***	-0.296***	-0.272***	-0.272***	-0.279***
(thousands of dollars)	(0.014)	(0.014)	(0.014)	(0.040)	(0.045)	(0.040)	(0.040)	(0.067)
Earnings test with tax rate at 50 percent dummy				-2.060***				-0.070
				(0.567)				(0.810)
At normal retirement age dummy	-2.679***	-2.807***	-2.811***	-2.929***	-0.725	-2.396**	-2.402**	-2.388**
	(1.005)	(1.025)	(1.024)	(1.019)	(1.242)	(1.194)	(1.194)	(1.206)
Above normal retirement age dummy	-4.455***	-4.590***	-4.596***	-4.736***	-2.108*	-3.829***	-3.834***	-3.821***
	(0.931)	(0.951)	(0.951)	(0.945)	(1.226)	(1.176)	(1.176)	(1.188)
Delayed retirement credit * Outside age range affected by	-0.623***	-0.662***	-0.664***	-0.572***	-0.264	-0.535***	-0.537***	-0.534***
delayed retirement credit dummy	(0.166)	(0.171)	(0.171)	(0.174)	(0.203)	(0.197)	(0.197)	(0.205)
Lagged union coverage dummy			-0.102	-0.100			-0.093	-0.093
			(0.098)	(0.098)			(0.099)	(0.099)
Lagged employer health insurance dummy		-0.291***	-0.275**	-0.264**		-0.272**	-0.258**	-0.257**
		(0.108)	(0.109)	(0.109)		(0.112)	(0.113)	(0.113)
Lagged employer health insurance * Below age 65		-0.977***	-0.983***	-0.997***		-0.991***	-0.996***	-0.997***
dummy		(0.129)	(0.129)	(0.129)		(0.133)	(0.133)	(0.133)
Linear age Trends	no	no	no	no	yes	yes	yes	yes
Number of observations	30,441	26,540	26,540	26,540	30,445	26,540	26,540	26,540
Number of individuals	5625	5108	5108	5108	5626	5108	5108	5108

Note: All specifications include controls for age dummies (age in years), quarter dummies, panel dummies, region dummies, number of household members, children under age 18, and ownership of home. Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.22 Estimates of Claiming Hazard for Men by Marital Status

	Single Men				Married Men			
Variables	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Earnings Test in Place dummy	-1.717**	-1.499	-1.971**	-1.419	-1.704***	-1.265***	-1.788***	-1.396***
	(0.842)	(0.921)	(0.864)	-1.004	(0.403)	(0.409)	(0.433)	(0.440)
Earnings Test Tax at 50 percent dummy			-1.371	-0.460			-1.423	-0.327
			(1.742)	(1.540)			(1.918)	(0.839)
Normal Retirement Age (months)	0.075	0.065	0.054	0.057	-0.010	-0.023	-0.023	-0.032
	(0.087)	(0.093)	(0.092)	(0.104)	(0.038)	(0.038)	(0.039)	(0.040)
At Normal Retirement Age dummy	0.079	-0.377	2.871**	-0.261	4.611***	2.715**	5.049***	2.954***
	(2.333)	(2.404)	(1.152)	(2.469)	(1.103)	(1.114)	(1.173)	(1.141)
Delayed Retirement Credit (percent)	-0.262	-0.264	-0.606	-0.270	-0.650**	-0.352	-0.696**	-0.373
	(0.677)	(0.729)	(0.629)	(0.728)	(0.297)	(0.313)	(0.300)	(0.314)
Delayed Retirement Credit * Outside Age Range	0.015	-0.051	0.508***	-0.020	0.615***	0.315*	0.715***	0.370*
Affected by Delayed Retirement Credit dummy	(0.369)	(0.382)	(0.153)	(0.411)	(0.182)	(0.184)	(0.202)	(0.195)
Wife's Earnings Test in Place dummy					-0.266**	-0.305**	0.464	0.323
					(0.131)	(0.137)	(0.292)	(0.306)
Wife's Earnings Test Tax at 50 percent dummy							-1.726***	-1.518***
							(0.467)	(0.489)
Wife's Normal Retirement Age (months)					0.039**	0.044**	0.036**	0.042**
					(0.017)	(0.017)	(0.017)	(0.018)
Wife At Normal Retirement Age dummy					-0.080	-0.058	-0.229	-0.188
					(0.317)	(0.323)	(0.322)	(0.329)
Wife's Delayed Retirement Credit (percent)					-0.162*	-0.207**	-0.109	-0.160*
					(0.093)	(0.090)	(0.096)	(0.093)
Wife's Delayed Retirement Credit * Wife Outside					-0.011	-0.015	0.007	0.001
Age Range Affected by Delayed Retirement Credit					(0.043)	(0.044)	(0.044)	(0.045)
Lagged Union and Employer Health Insurance dummies (for Both Husband and Wife)	no	yes	no	yes	no	yes	no	yes
Number of Observations	4,884	4,634	4,884	4,634	27,006	23,644	27,006	23,644
Number of Individuals	1057	1013	1057	1013	5091	4626	5091	4626

All regressions control for earnings test threshold amount for both husband and wife, and linear age trends for men. Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.23 Estimates of Claiming Hazard for Women by Marital Status

	Single Women				Married Women			
Variables	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Earnings Test in Place dummy	0.657	0.372	0.343	0.494	-1.216*	-0.613	-1.256*	-0.672
	(0.739)	(0.799)	(0.806)	(0.849)	(0.627)	(0.648)	(0.641)	(0.663)
Earnings Test Tax at 50 percent dummy			-0.444	-0.993			0.875	1.198
			(1.764)	(2.105)			(1.481)	(1.053)
Normal Retirement Age (months)	0.040	0.038	0.032	0.030	0.099*	0.086*	0.098*	0.088*
	(0.066)	(0.069)	(0.070)	(0.073)	(0.051)	(0.051)	(0.052)	(0.053)
At Normal Retirement Age dummy	-1.513	-2.053	-1.830	-1.815	0.198	-2.062	-0.048	-2.435
	(2.126)	(2.096)	(2.164)	(2.174)	(1.686)	(1.625)	(1.733)	(1.637)
Delayed Retirement Credit (percent)	-0.571	-0.164	-0.185	-0.185	-0.623	-0.205	-0.600	-0.178
	(0.548)	(0.597)	(0.579)	(0.600)	(0.419)	(0.417)	(0.426)	(0.419)
Delayed Retirement Credit * Outside Age Range	-0.481	-0.512	-0.469	-0.459	-0.033	-0.398	-0.088	-0.484*
Affected by Delayed Retirement Credit dummy	(0.356)	(0.354)	(0.376)	(0.379)	(0.279)	(0.270)	(0.295)	(0.279)
Husband's Earnings Test in Place dummy					-0.236*	-0.280**	0.113	0.164
					(0.135)	(0.140)	(0.223)	(0.231)
Huband's Earnings Test Tax at 50 percent dummy							-1.297***	-1.626***
							(0.476)	(0.507)
Husband's Normal Retirement Age (months)					-0.013	0.002	-0.013	0.003
					(0.018)	(0.018)	(0.018)	(0.019)
Husband At Normal Retirement Age dummy					0.631***	0.542**	0.449*	0.318
					(0.232)	(0.235)	(0.252)	(0.252)
Husband's Delayed Retirement Credit (percent)					0.050	0.038	0.073	0.061
					(0.090)	(0.088)	(0.093)	(0.090)
Huband's Delayed Retirement Credit * Husband Outside Age Range Affected by Delayed Retirement Credit dummy					0.034	0.037	0.035	0.038
					(0.026)	(0.026)	(0.026)	(0.026)
Lagged Union and Employer Health Insurance dummies (for Both Husband and Wife)	no	yes	no	yes	no	yes	no	yes
Number of Observations	8,956	7,770	8,956	7,770	17,843	15,538	17,843	15,538
Number of Individuals	1683	1499	1683	1499	3333	3055	3333	3055

Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals. *** p<0.01, ** p<0.05, * p<0.1

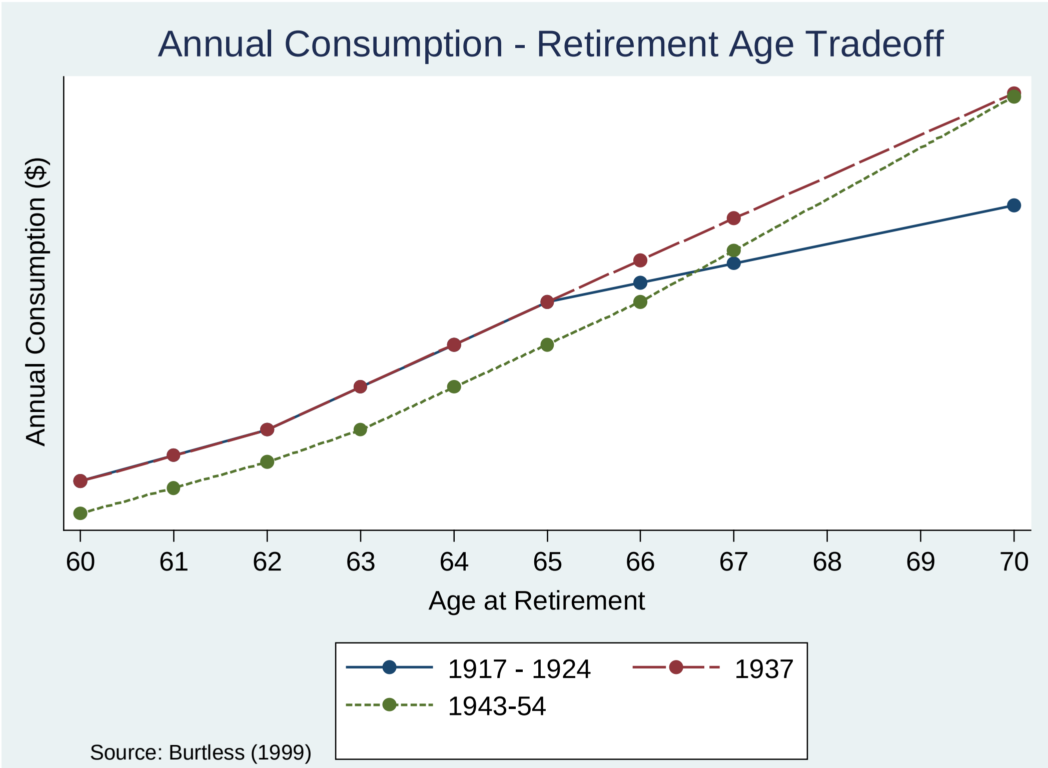


Figure 1.1 Annual Consumption-Retirement Tradeoff

Note: Birth cohorts 1917-24 have an NRA of 65 and annual rate of DRC of 3 percent (the before 1983 Reform, actuarially unfair rate). Birth cohort 1937 has an NRA of 65 as well and annual rate of DRC of 6.5 percent (approx. actuarially fair). Birth cohorts 1943-54 have an NRA of 66 and annual rate of DRC of 8 percent (more than actuarially fair). The actuarial adjustment for postponement of benefits between age 62 and NRA is 6.67 percent for birth cohorts 1927-1937. For birth cohorts 1943-54, the actuarial adjustment for postponing benefits between ages 62-63 is 5 percent and between ages 63 and NRA is 6.67 percent. The discount rate is assumed to be zero.

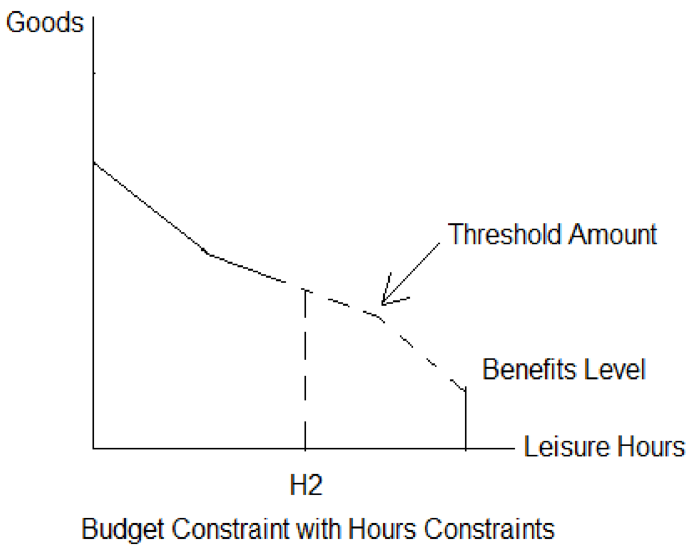


Figure 1.2A Budget Constraint with Hours Constraint

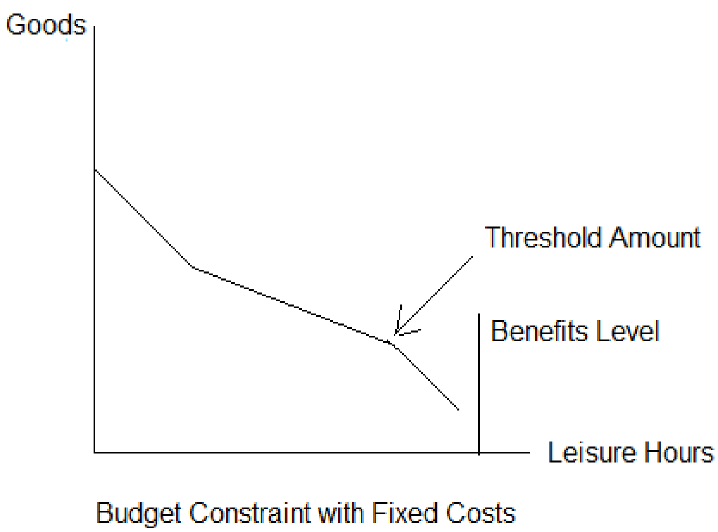
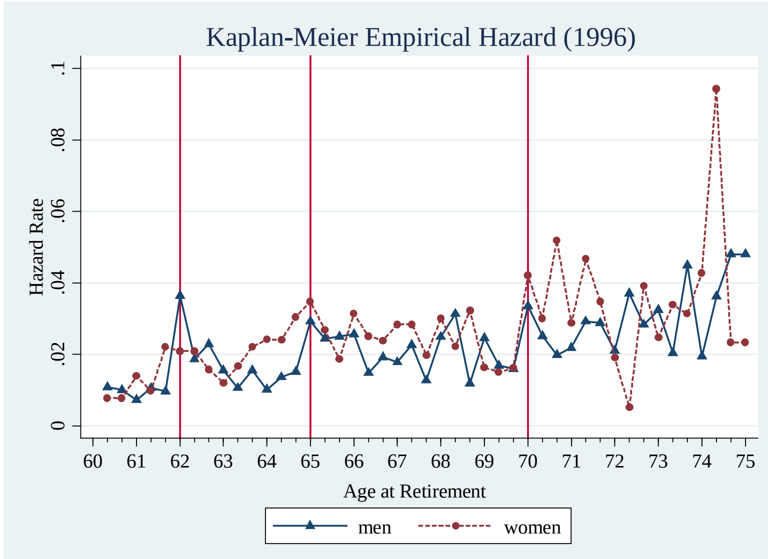
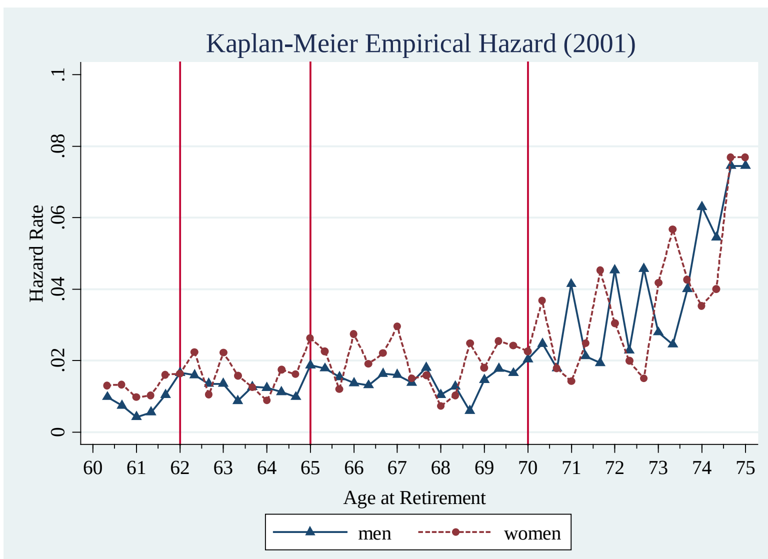


Figure 1.2B Budget Constraint with Fixed Costs



Men: 2,310

Women: 2,058

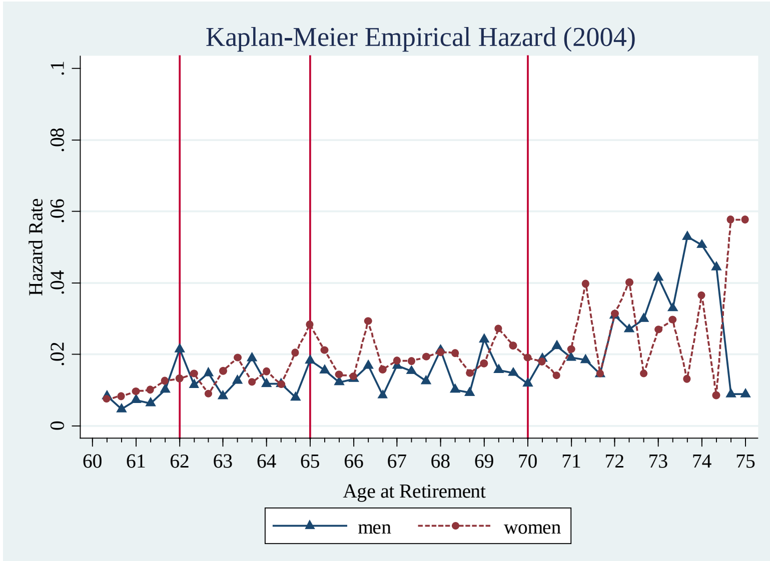


Men: 2,150

Women: 1,771

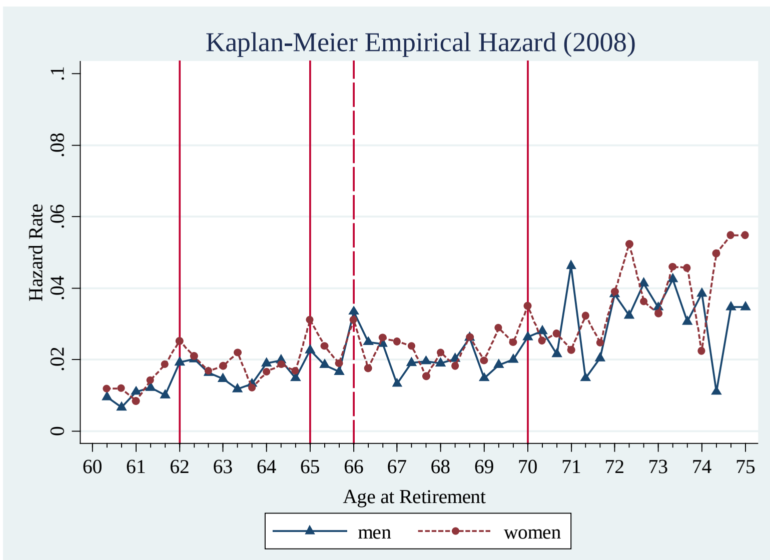
Figure 1.3A Hazard Rate for Retirement due to Nonparticipation in Labor Force

Note: The vertical line at age 62 indicates the earliest age at which social security benefits can be claimed (which is the same for all panels), while the vertical line at age 65 indicates the normal retirement age that was in effect before the recent changes in NRA that began in 2000. The dashed vertical line at age 66 indicates the new higher normal retirement age for the younger birth cohorts in the 2008 panel. The vertical line at age 70 indicates the age at which the earnings test ended before it was repealed in 2000 and the age at which delayed retirement credits cease to apply.



Men: 2,782

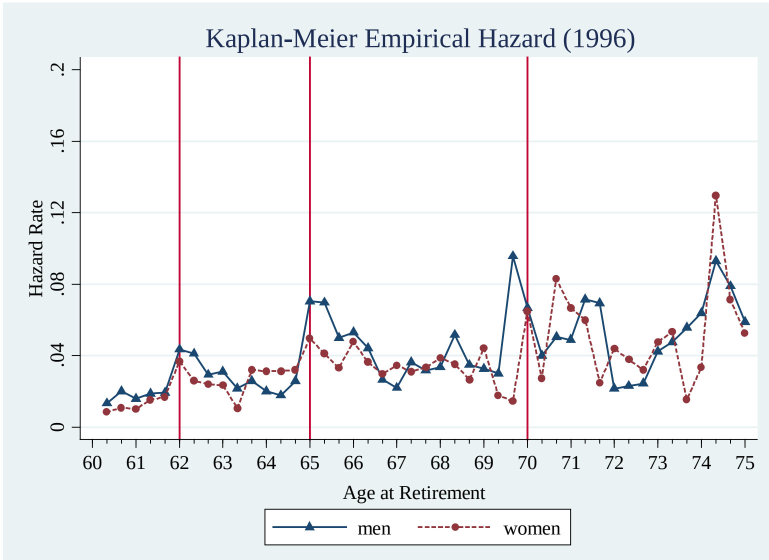
Women: 2,660



Men: 4,015

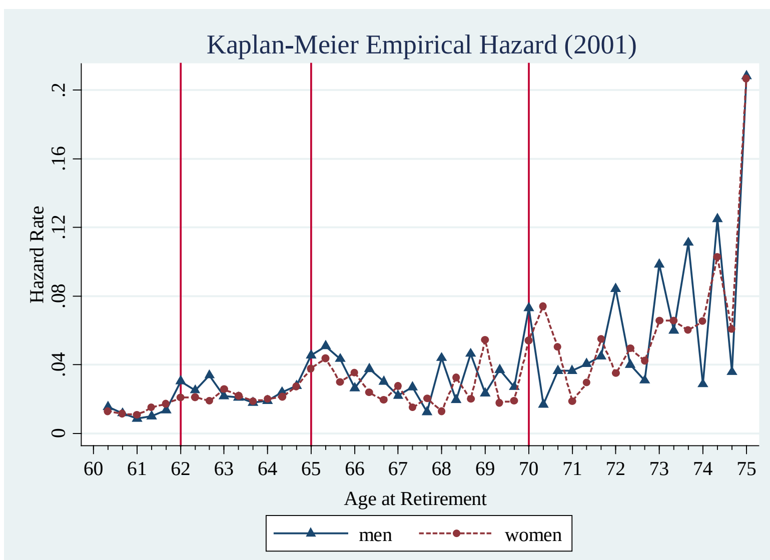
Women: 3,727

Figure 1.3B Hazard Rate for Retirement due to Nonparticipation in Labor Force



Men: 1,428

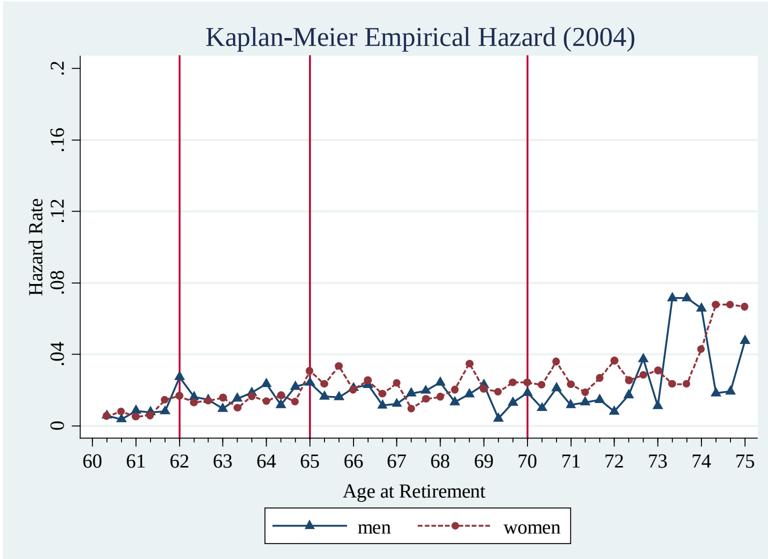
Women: 1,540



Men: 1,363

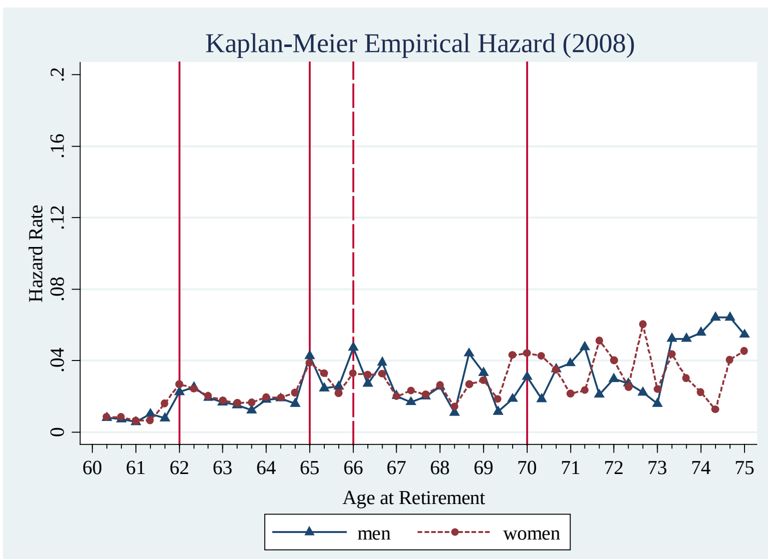
Women: 1,314

Figure 1.4A Hazard Rate for Self-Reported Retirement



Men: 1,760

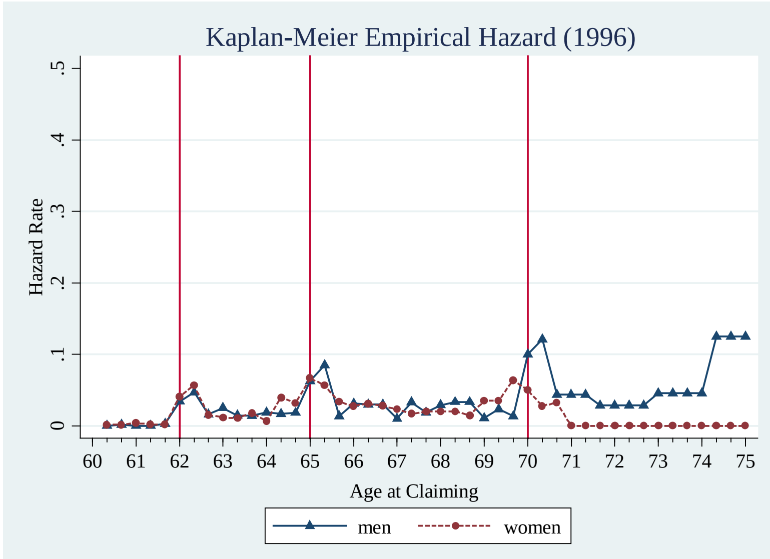
Women: 2,010



Men: 2,767

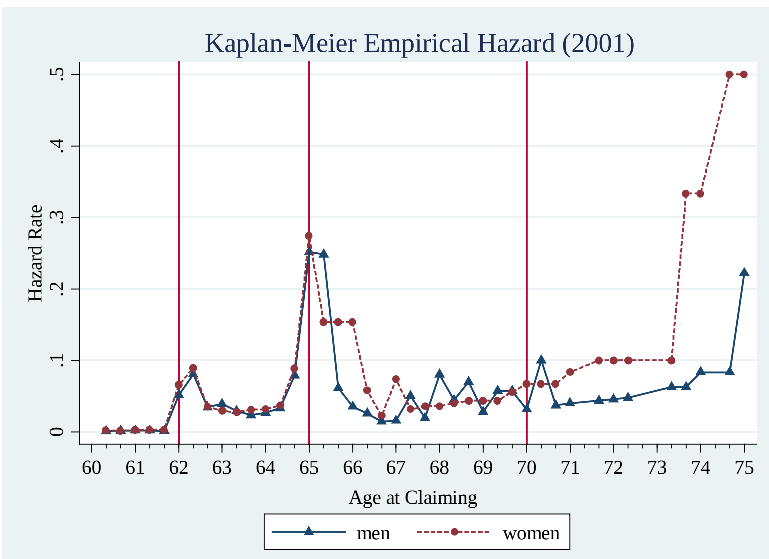
Women: 2,883

Figure 1.4B Hazard Rate for Self-Reported Retirement



Men: 1,488

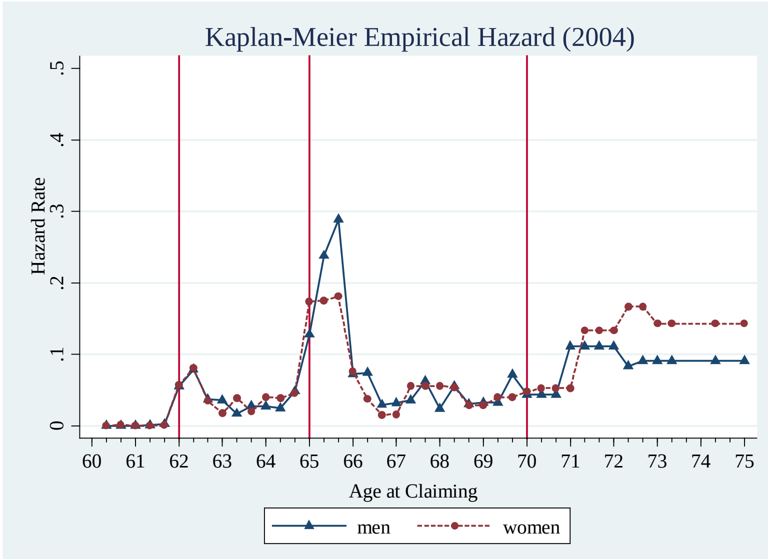
Women: 1,234



Men: 1,322

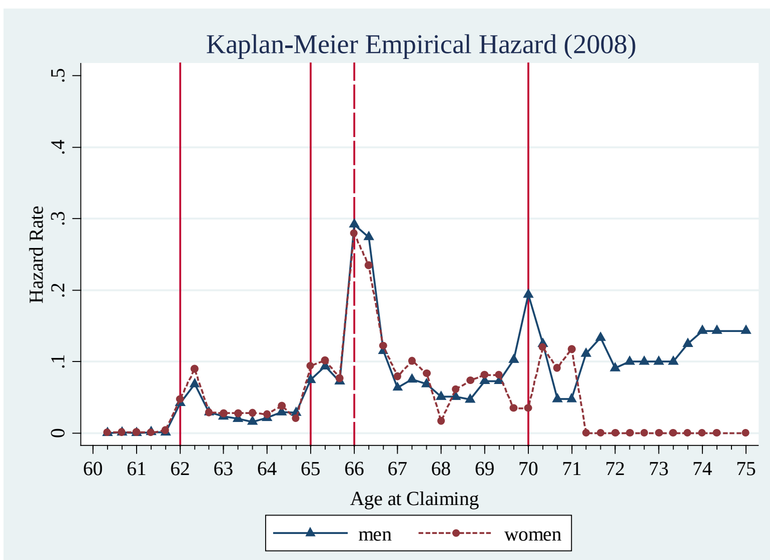
Women: 1,014

Figure 1.5A Hazard Rate for Claiming Social Security as Primary Beneficiary



Men: 1,675

Women: 1,540



Men: 2,692

Women: 2,410

Figure 1.5B Hazard Rate for Claiming Social Security as Primary Beneficiary

CHAPTER 2

LIFE-CYCLE LABOR SUPPLY RESPONSE TO THE EARNINGS TEST

2.1. INTRODUCTION AND REVIEW OF PREVIOUS RESEARCH

In March 2000, U.S. Congress passed the Senior Citizens Freedom to Work Act that repealed the earnings test for older workers above the normal retirement age; the aim of the repeal was to "avoid penalizing seniors who chose to work in retirement."[1] This view stems from the assumption that prior to the repeal the earnings test was perceived as a tax that reduced current benefits of Social Security beneficiaries above the normal retirement age (NRA) whose earnings exceeded the annual exempt amount.[2] A related feature of the earnings test complicates the analysis, however, as current benefits withheld under the earnings test are later returned to individuals through increases in their future benefits. The size of these future increases depends on the actuarial adjustment; for individuals above the NRA future benefits are increased by the delayed retirement credit (which is assigned by birth year).[3] According to this alternate view, if people do not face any borrowing constraints and are aware of these future actuarial adjustments, the earnings test is not a tax; depending on the size of the actuarial adjustments, they either weaken or completely remove any work related penalty imposed by the earnings test. Thus, the earnings test repeal may have a negligible or even zero effect on labor supply.

---

[1] A Brief History. Social Security Administration. (http://www.ssa.gov/pubs/EN-21-059.pdf)
[2] The earnings test is still in place for individuals between ages 62 and NRA.
[3] For individuals below the NRA (ages 62-NRA), future benefits are increased at a rate of 6.67 percent.

Despite the commonly held view that the earnings test reduces labor supply, the early empirical research investigating the influence of the earnings test as a tax has arrived at a consensus opinion of a relatively small negative effect on labor supply behavior. This conclusion was more mixed in later studies. Leonesio (1990) summarizes a large number of studies that analyzed the reforms prior to the 2000 repeal, and arrived at the conclusion that the earnings test had a small impact on the labor supply of older workers.[4] Burtless and Moffitt (1985) model the kinked budget constraint that arises under the earnings test and find that the earnings test exerts little effect on hours of work of older individuals. Their analysis, however, is based on a period that witnessed no major changes in the earnings-test characteristics, which raises the concern that their estimates rely on between individual variation in the wage rate and unearned income.[5] Friedberg (2000) adds to these earlier structural studies, by estimating the labor supply response (along different parts of the non-linear budget constraint) to the 1983 Social Security Reform that repealed the earnings test for individuals above age 70. She creates a repeated cross-section to examine the labor supply response before and after the policy change, and unlike previous studies, finds a much larger response in hours of work of affected individuals to changes in the earnings test rules. Gruber and Orszag (2003), adopt a reduced form approach to examine the effect on both hours of work and the labor force participation decisions while accounting for trends in the labor supply of older men and women. They do not find any robust evidence of an influence from changes in the

[4] Leonesio (1990) notes that the earliest studies attempted to assess the effect of the earnings test by either examining various changes in the earnings test using bunching analysis or relied on a structural approach to model the kinked budget constraint created by the earnings test.
[5] Such estimates may be biased if unobserved tastes for work of an individual are correlated with his current wage rate or unearned income.

earnings test characteristics (prior to the 2000 repeal) on either hours of work or labor force participation.[6] Based on these recent studies, Krueger and Meyer (2002) modify the consensus opinion of a small effect summarized by Leonesio (1990) to "mixed".

Recent studies evaluating the earnings test repeal of 2000 in the U.S., and the repeal of the earnings test in Canada and United Kingdom, concur in finding a negligible effect on labor force participation, a small rise in hours of work, and evidence supporting the existence of strong labor market rigidities that impede the ability of older workers to adjust their labor supply. [7] Tran (2002), Haider and Loughran (2008), and Engelhardt and Kumar (2009) use difference-in-differences estimation to examine the effect of the earnings test repeal of 2000 in the U.S. These studies find that older workers ages 65-69 respond to the repeal by increasing hours worked, but little evidence of an effect on labor force participation. Tran (2002) finds weak support for an increase in labor force participation among older white men between ages 65 to 69. Baker and Benjamin (1999) evaluate the sequential elimination of the earnings test in the Canadian Pension Plan (in 1975) and the Quebec Pension Plan (in 1977) by using the geographic variation offered by these reforms as their main source of identification. Similar to the findings for the U.S., they report no effect of the repeal on labor force participation, but an increase in weeks worked conditional on working. The United Kingdom repealed the earnings test for older workers in 1989; Disney and Smith (2002) assess the response to this repeal by

[6] Haider and Loughran (2008) note that one reason Gruber and Orszag (2003) did not find any response to the earnings test repeal of 1983 is that it affected relatively older individuals between the ages 70-71 who could not respond as much as the relatively younger individuals affected by the 2000 repeal. Gruber and Orszag (2003) also caution that their findings are best interpreted as effects of changes in the earnings test parameters (threshold amount, tax rate) in the presence of the earnings test.

[7] Engelhardt and Kumar (2014).

comparing the changes in labor supply of individuals directly affected by the policy before and after the repeal to changes in the labor supply of two other groups of older individuals ages 60-64 and 70-74 who were not directly affected. They find no effect of the earnings test repeal on labor force participation, but an increase in weekly hours of work.[8] In contrast to early research, these recent studies (relying on both survey and administrative data) highlight the existence of strong labor market rigidities that may obscure the response to the earnings test.

There are two main concerns with the difference-in-differences approach adopted by recent studies analyzing the short-run effect of the earnings test repeal. First, most recent studies analyze the effect of the earnings test within a static labor supply model using slightly younger individuals between ages 62-64 and slightly older individuals between ages 70-74 as control groups in their analysis, and ignoring the delayed retirement credit (DRC) actuarial adjustments applied to future benefits.[9] For instance, Friedberg and Webb (2009) ignore the DRC adjustments in their analysis under the assumption that most people either misunderstand or are misinformed about this feature of the Social Security program. The DRC has increased substantially from 3 to 8 percent for recent cohorts of older individuals. In the life-cycle model of labor supply an increase in the net wage at ages 65-69, may influence short-run labor supply decisions at other ages through intertemporal substitution. Younger age groups may not serve as adequate control groups, if there is intertemporal substitution of labor supply in response to the

[8] Disney and Smith (2002) find a strong statistically significant response in hours of work only when the control group includes both 60-64 and 70-74 year olds.

[9] Appendix Table B.1 provides a list of acronyms used in the paper and the associated expansions.

earnings test repeal.[10] Second, most previous studies assume that in the absence of the earnings test repeal the labor supply of older individuals in different age groups would have followed a common trend; the data used in these studies is often restricted to a few years after the earnings test repeal which makes it difficult to analyze the common trends assumption. Gruber and Orszag (2003) documented differential trends by age in the labor supply behavior of older individuals and emphasized that models which include controls for age specific trends in labor supply are more reliable.

Two studies have attempted to address some of these concerns. Tran (2000) examines the impact of the earnings test in a life-cycle model by first showing that the earnings test is a tax in a life-cycle context, (even after accounting for the DRC adjustments) if individuals have shorter than average life expectancy. He uses Outgoing Rotation Group CPS monthly data from 1995-2002 to study the immediate effect of the earnings test repeal of 2000 on older white men between ages 65-69; he also studies the intertemporal response to the earnings test repeal among younger individuals between ages 55-61 and those between ages 62-64. Tran employs a difference-in-differences method as well, and assumes that individuals above age 70 who do not face the earnings test are unaffected before and after the earnings test repeal. He, however, does not explicitly control for the effect of changes in the DRC. He finds that the repeal increases the employment rate of men between ages 65-69 and this increase is due to men continuing to work longer rather than through reentry of older people who had left the labor force. He observes intertemporal substitution of labor supply among members of

[10] Friedberg and Webb (2009) also note that it is possible that in the long run individuals at ages other than 65-69 may increase their labor supply through spillover effects because of the presence of constraints that limit transitions into and out of work at older ages.

the youngest group. Men between ages 55-61 reduce their labor supply in response to the repeal, but, he does not find any intertemporal effect on the labor supply of men between ages 62-64. Tran further provides evidence that the response to the earnings test repeal is concentrated among subgroups of individuals with less education.

Michaud and Van Soest (2008) use data from the Health and Retirement Study (HRS) covering the years 1992-2004 to examine the immediate effect of the earnings test repeal on work expectations of younger individuals between ages 51 to 61. They study the impact of the repeal on younger individual’s self-assessed probability of working full-time after age 62 and after age 65. In their analysis, they use Social Security Administrative data to link each individual’s earnings history with their record in the HRS and determine the percentage of social security benefits predicted to be lost for each individual if he does not change hours of work at the NRA. They divide individuals into four groups based on their predicted losses and then use difference-in-differences method. In the empirical analysis they estimate the labor supply response using panel data models with fixed and random effects that allow them to implicitly control for birth cohort effects and the common effect of changes in the DRC at all ages. They find evidence indicating that the earnings test is perceived as a tax at ages above the NRA; the repeal raises the self-assessed probability of younger workers to work full-time after age 65. They do not find any effect of the repeal on self-assessed probability of working full-time after age 62. Interestingly, they do not find any effect of the repeal on the subjective probability of working full-time past age 62 and 65 for women between ages 51 to 61. A third study by Friedberg and Webb (2009) explores the effect of the earnings test repeal on younger individuals as well, but, unlike the previous two studies (and the focus of my work) Friedberg and Webb study the long run effect of the earnings test repeal by including a set of variables that reflect currently applicable and future thresholds for cohorts at their current age and when the cohort is age 62 (or 65). Given the emphasis of their study on long run effects they believe the response among younger workers is due to constraints on labor supply transitions and not due to intertemporal substitution.

I add to the work of Tran and Micahud and Van Soest in several ways. In the present study, I use the latest four panels from the Survey of Income Program and Participation (SIPP) data to examine the immediate effect of the earnings test repeal on older men and women between ages 62 to 74. First, in my work I implicitly control for the effect of the DRC at all ages and explicitly control for the differential effect of the DRC changes along the age range directly affected by the DRC relative to those that are not directly affected. Thus, I am able to assess whether the earnings test is perceived as a tax even after accounting for the effect of changes in the reward for postponing benefits. Second, I use panel data models with fixed-effects which allow me to implicitly control for birth cohort effects, the effect of actuarial adjustments that raise the reward for future benefits, and any fixed individual tastes for work that may be correlated with the explanatory variables. Third, in contrast to previous studies I assess the response in hours of work decisions using a modeling framework that reflects the discrete and constrained nature of the labor supply adjustments made by older workers. Moreover, I use the SIPP data covering years 1996 to 2013, which allows me to observe and control for trends in labor supply of different age groups. Finally, I explore whether the response to the earnings test repeal or changes in the future reward to work is driven by the particular subgroups of individuals that possibly facing liquidity constraints or those who are more likely to be unaware of the earnings test rules. I find that the earnings test is perceived as a tax by both men and women as both reduce their labor force participation in response to the earnings test. The labor supply response for men is driven by the changes in their labor force participation behavior, while for women I observe a response along both participation and hours of work (among those who continue working).

2.2. MODIFICATIONS TO THE EARNINGS TEST

The Social Security Act of 1935 laid the foundation for the old age pension program in the U.S.; a key feature of the Act was the earnings test, a test that all eligible individuals above age sixty-five must pass to receive their benefits.[11] In its earliest form the earnings test withheld complete monthly benefits of an eligible individual who had positive earnings in that month. Older workers who lost their benefits to the earnings test were not compensated at a later date upon cessation of work. [12] The 1939 Amendment set an "exempt" amount on earnings is an upper limit which an employed older worker could earn and still qualify to receive full benefits. Workers who claimed benefits while earning above the exempt amount had their full benefits withheld, in other words their benefits were taxed at a rate of hundred percent. The earnings test in its early form, thus, actually provided and was accurately perceived by workers as providing a strong disincentive to work. In an effort to reduce the work impeding effects inherent in the initial version of the earnings test, it was modified on several occasions. The major modifications to the

[11] The Social Security Act of 1935 also set the NRA (NRA) for both men and women to 65. The NRA is the earliest age at which an individual can receive his full social security benefits based on his earnings record.

[12] In its original version, the earnings test reduced incentives to work for individuals above age sixty-five in a manner similar to a cash grant welfare program.

earnings test can be succinctly summarized by considering how they affected the age range covered by the earnings test and the earnings test parameters that apply within each age range.

Ages 62-NRA: Later amendments to the Social Security Act introduced the early retirement age of sixty-two to provide individuals below the NRA (NRA) early access to their benefits. This option was extended to women in 1956, and men in 1961; earlier access to benefits, however, imposed a penalty by reducing the size of the benefits available at age sixty-two.[13] The eligibility for early retirement benefits also extended the earnings rules to ages below the NRA. Until the late 1970s individuals below the NRA faced the same earnings test parameters as individuals above the NRA. Both age ranges shared a common exempt amount, and earnings in excess of the exempt amount were also taxed at the common rate of 50 percent.[14] The Social Security Amendment of 1977 introduced the first major change in the earnings test parameters that applied differently for those below relative to those above the NRA. Beginning in 1978, social security beneficiaries below the NRA faced a lower exempt amount than those above. [15] The

[13] An insured individual can claim unreduced benefits based on his earnings history and equivalent to hundred percent of his Primary Insurance Amount (PIA) at his NRA. The monthly benefit for a worker is calculated in three steps. The first step is to index the annual taxable earnings of a worker to the national average wage index. From these indexed earnings, the highest 35 years of earnings between ages 21 and 62 are chosen to compute the Average Indexed Monthly Earnings (AIME). The second step is to compute the workers' PIA from the AIME; the PIA is the full benefit an individual is entitled to if he claims at the NRA. The third step is to compute the final benefit amount received by the worker; this amount could be higher or lower than the PIA depending on the age at which the benefit is first claimed.

[14] In 1961, the earnings test was modified to include two exempt amounts, earnings above the lower exempt amount were taxed at a rate of 50 percent while earnings in excess of the upper exempt amount were taxed at a rate of 100 percent. The Amendment of 1972 removed the100 percent tax rate, all individuals between ages 62-69 who earned above the exempt amount faced a tax rate of 50 percent until benefits were fully exhausted.

[15] Over the years the earnings test threshold was raised several times on an ad hoc basis until 1972, when it was indexed to increases in the national average wage index.

Greenspan Commission led to the Social Security Reform Act of 1983 that laid down further provisions separating the earnings test parameters that applied to those below relative to above the NRA. The 1983 Reform, which became effective in 1990, stipulated that working beneficiaries above the NRA whose earnings exceeded the exempt amount would face a lower tax penalty of 33 percent while individuals below the NRA would continue to face a penalty of 50 percent. Since the late 1970s, when the labor force participation rate of older men declined precipitously, the general direction of the modifications to the earnings test rules have been towards loosening them for individuals above the NRA relative to those below. Table 2.1 summarizes the earnings test rules faced by individuals ages 62 and above during the time period 1996-2013.

Another important difference in the set of rules that apply to individuals below the NRA relative to those above is the treatment of delayed or postponed receipt of benefits. Since the creation of the early retirement age, individuals below the NRA receive a compensation for delayed receipt of benefits. If an individual below the NRA delays receiving benefits for one year, his future benefits are raised to compensate for the loss of current benefits by applying an adjustment called the actuarial adjustment factor. The size of the actuarial adjustment factor has remained constant at an annual rate of approximately 6.67 percent.[16] These actuarial adjustments to future benefits also apply to benefits that are withheld under the earnings test. In other words, when older beneficiaries between the ages 62-NRA earn more than the exempt amount and lose some

[16] Between ages 62 – NRA, each year of delayed receipt of benefits raises future benefits by 6.67 percent. In recent years the NRA has also risen from 65 to 66. For older individuals with a NRA of 66, the actuarial reduction factor between ages 62 – 63 is lowered to an annual rate of 5 percent.

or all of their benefits to the earnings test, their future benefits are raised at an annual rate of 6.67 percent to compensate for the loss of current benefits. The actuarial adjustments for benefits withheld under the earnings test, however, apply only after the individual attains the NRA. To summarize, despite the modifications to the earnings test parameters over time, in recent years the two main parameters that apply to individuals below the NRA have been fairly constant. During the time period 1996-2013 working beneficiaries below the NRA face an unchanging tax rate of 50 percent for earnings above the exempt amount, and the loss of current benefits withheld due to the earnings test is later compensated by applying an unchanging annual actuarial adjustment of 6.67 percent to future benefits.[17]

NRA-age 69: Although individuals below and above the NRA faced a common exempt amount until 1978 and a common tax rate of 50 percent until 1990, they differed in the rate at which delayed/postponed current benefits were compensated. Before the 1972 Amendment, individuals above the NRA were not compensated for current benefits that were lost either due to delayed claiming or benefits that were withheld under the earnings test. To compensate individuals above the NRA for delayed/postponed claiming, a DRC provision was first introduced in 1973, it raised future benefits at an annual rate of 1 percent. At an annual rate of 1 percent, however, the DRC actuarial adjustment was much lower than the actuarial adjustment of 6.67 percent that was applied to future benefits of individuals below the NRA. To encourage work among older individuals, the

[17] Since the earnings test repeal of 2000, a looser earnings test applies in the year an individual attains his NRA. An individual below the NRA faces a higher exempt amount and a lower tax rate of 33 percent in the year he reaches his NRA, the looser earnings test applies only in the months prior to the month is which the individuals is at his NRA.

1983 Reform to Social Security raised the NRA and the DRC for future cohorts of older workers. Both these policy changes affected individuals based on their birth cohort. Table 2.2 summarizes the birth cohorts affected by each policy change. The NRA increased from 65 to 66 for birth cohorts 1938-43 in increments of two months, while the DRC increased every other year at a rate of half a percentage point from 3-8 percent for birth cohorts 1925-43. Viewed in light of the historical context of the Social Security program, these recent increases in the DRC from 3 to 8 percent are quite generous, and substantially raise the reward for delayed benefit receipt for older individuals above the NRA.

The most prominent change in the Social Security system that has directly affected older workers between NRA-age 69 is the unanticipated repeal of the earnings test in April 2000. As described above, prior to the 2000 elimination older workers eligible for social security benefits were subject to the earnings test between ages 62-69, and workers between NRA-age 69 faced a higher exempt amount and a lower tax rate of 33 percent relative to older workers below the NRA. The earnings test repeal of 2000 did not affect the earnings test parameters for individuals below the NRA.

The repeal of the earnings test in the year 2000 also affected the manner in which older workers can benefit from the DRC. Before 2000, older individuals above the NRA could raise their future social security benefits through the DRC in two ways, by either postponing the receipt of benefits if the individual has not already claimed, or by working above the exempt amount if already collecting benefits. Beneficiaries who worked above the exempt amount and lost some or all of their benefits to the earnings test could increase future benefits through the DRC adjustments. After the earnings test repeal of

2000, however, beneficiaries above NRA could take advantage of the DRC through the "claim and suspend" policy. In the absence of the earning test, the claim and suspend policy allows beneficiaries who have claimed their benefits to temporarily suspend them and receive DRC adjustments on the foregone (suspended) benefits. In summary, during the 1990s, individuals above the NRA faced looser earnings test parameters in the form of a higher exempt amount and a lower tax rate than those below the NRA, and in April 2000 the earnings test was completely eliminated for those above the NRA. In the time period after the 1990s, older individuals above the NRA also experienced a substantial increase in the DRC adjustments that compensate them for loss of current benefits by raising future benefits.

Ages 70-74: Under the Social Security Act of 1935 the earnings test applied to all older workers above age 65. This restriction on the age limit was first relaxed in 1951, when older workers above age 75 were exempted from the earnings test. Four years later another amendment repealed the earnings test for all workers above age 72. The 1983 reform further repealed the earnings test for individuals between ages 70-72. Since, the year 1983 all workers above age 70 have not faced the earnings test. Age 70 is also the maximum age until which social security benefits can be postponed; after age seventy benefits are automatically paid to all eligible beneficiaries. The upper limit on the age until which social security benefits can be postponed is important because it signifies that the DRC adjustments also cease to apply after age 70. In recent years, thus, older workers above age 70 were not subject to either the earnings test or any DRC adjustments.

2.3. PERCEPTIONS OF EARNINGS TEST AND THEORETICAL PREDICTIONS

The theoretical predictions from the life-cycle model regarding the impact of the earnings test repeal of 2000 depend on how individuals between NRA-age69 perceive the earnings test. As described above, prior to 2000 the earnings test was in place for ages 62-69; social security beneficiaries within this age range who earned above the exempt amount experienced a reduction in the size of their current benefits. The size of the reduction in current benefits depended on the tax rate (which differs for those below or above the NRA), but the earnings test is not a tax as benefits lost to the earnings test were returned later by raising future benefits by the size of the actuarial adjustments. Below the NRA these adjustments have remained almost unchanged at an annual rate of 6.67 percent, while above the NRA these adjustments have risen from 3 to 8 percent in recent years. In effect, these features of the Social Security program induce older beneficiaries to participate in a forced savings plan that withholds current benefits and returns them later by applying an adjustment factor to future benefits.

Previous literature on the earnings test in the U.S. has debated whether individuals are aware of the actuarial adjustments and if so, whether these adjustments fully compensate the loss of current benefits. The DRC adjustments applied to foregone benefits are considered actuarially fair if they fully compensate older beneficiaries above the NRA (with an average life expectancy) for foregone current benefits; in other words, actuarially fair adjustments leave the present value of social security wealth unchanged. If older individuals are unaware of the actuarial adjustments or if the adjustments are actuarially unfair then the earnings test is a tax even in the life-cycle context and affects life-cycle labor supply by introducing a kink in the budget constraint. Reimers and Honig

(1993) note that in this case Social Security beneficiaries will behave myopically taking into account their current benefits while making their labor supply decisions. But, if instead, older workers fully comprehend the "forced saving" features of the Social Security system and the adjustments are actuarially fair then the earnings test does not create a kink in the budget constraint. In such a scenario, the earnings test repeal should have no effect on labor supply. In their work Reimers and Honig (1993) emphasize that the response of older beneficiaries to any modification of the earnings test parameters and/or the size of the DRC adjustments relies crucially on older workers' perception regarding the link between current earnings and future Social Security benefits.

2.3.1. PERCEPTIONS OF THE EARNINGS TEST

How do older workers between ages 62-69 perceive the budget constraint in the presence of the earnings test? The answer depends on whether older individuals respond to the actuarial adjustments applied to foregone benefits. The responsiveness to the actuarial adjustments in turn depends on two factors, an older individual's awareness of the complicated features of the Social Security program and the actuarial fairness of these adjustments.

Awareness of Actuarial adjustments: There is limited evidence on older worker's awareness of the interaction between the earnings test rules and the actuarial adjustments applied to foregone benefits. Most researchers in the past ignored the actuarial adjustments and treated the earnings test as a tax. For instance, Gruber and Orszag (2003) and Friedberg and Webb (2009) quote *J.K. Lasser's Your Income Tax 1998* guide, and articles in *Money* and *Los Angeles Times* that describe the earnings test but do not mention the DRC actuarial adjustments, as evidence that most sources of information regarding Social Security benefits do not advise older individuals about future adjustments to their benefits. As a result, the DRC adjustments are likely to be widely misunderstood or overlooked by older beneficiaries. Benitez-Silva and Heiland (2007) argue that the full incentives provided by the earnings test and the actuarial adjustments for both individuals below and above the NRA are widely misunderstood by beneficiaries because of the complexity of the rules and the lack of information provided by Social Security officials. They note the interaction between the earnings test rules and the actuarial adjustments is neither well documented in Social Security publications, nor is there any reference to these adjustments in the online benefit calculator provided by the Social Security officials. They highlight the discrepancy in Social Security Administration's (SSA) dissemination of information by noting the ease with which older individuals can learn about the earnings test exempt amount, and contrasting it to the difficulty in learning about adjustments to future benefits if current benefits are lost to the earnings test. According to them, it is because of the lack of clear and accessible information provided by the SSA that many beneficiaries may view the earnings test as a tax and make their labor supply decisions accordingly.

Liebman and Luttmer (2012) administered an online survey to a nationally representative sample of individuals ages 50 -70 to learn about the extent to which older individuals in the U.S. understand the different incentives provided by the Social Security system. In support of the "bunching" literature that provides visual evidence of beneficiaries raising their earnings as the earnings test exempt amount moves, Liebman and Luttmer report that older workers are well informed of the exempt amount. They also find evidence that older workers understand that delayed claiming between ages 62-69 raises future benefits through actuarial adjustments. Most beneficiaries, however, are unaware of the distinction in the size of the actuarial adjustments before and after the NRA. Interestingly, although older individuals are aware that delayed claiming raises future benefits, survey responses to further questions that enquire about whether future benefits would increase if current benefits were reduced as a result of the earnings test indicate that most older individuals are unware of the delayed receipt of benefits withheld due to the earnings test. The general consensus that emerges from the previous studies is that older workers view the earnings test as a tax because they are either unaware or misunderstand the future adjustments associated with benefits that are lost to the earnings test.

Actuarial Fairness of Adjustments: Whether older workers perceive the earnings test as a tax depends not only on their awareness of the future adjustments to benefits but also on the extent to which these adjustments adequately compensate individuals for the loss of current benefits. Older beneficiaries who are aware of and understand the complicated rules governing the earnings test may still perceive it as a tax if the adjustments are individually viewed as actuarially unfair. The degree to which increments to future benefits compensate for the loss of current benefits depends on the size of the actuarial adjustments (or DRC). Previous researchers have debated the actuarial fairness of the adjustments; the leading arguments are put forth in three papers that are summarized below.

Blinder et al. (1980) cast doubt on the prevalent view that the earnings test provides a disincentive to work by taxing the earnings of working beneficiaries above the exempt amount. In their work they showed that the annual actuarial adjustment of 6.67 percent applied to future benefits of workers below the NRA fully compensates them for the loss of current benefits. They further go on to say that at low enough real interest rates the Social Security system may in fact be providing individuals below the NRA a subsidy to work. Their analysis, however, rests on the assumption that many older individuals are fully aware and understand the provisions of the Social Security program. For older individuals above the NRA with a DRC of 1-3 percent, they find that the actuarial adjustments to future benefits do not fully compensate for the loss of current benefits, and thus, conclude that the earnings test is a tax on earnings of working beneficiaries in this age range. They further note that even for workers above the NRA the earnings test may not be a tax if they account for the automatic benefit recomputation feature of the Social Security system, which replaces lower years of earnings with higher earnings in the computation of Social Security benefits. Blinder et al. stress that their conclusions are valid only if the intricate rules of the Social Security program are understood by older people, but as noted above there is sparse evidence to shed light on the degree to which older workers are aware of the rules.

Two subsequent papers by Burkhauser and Turner (1981) and Kahn (1989) challenged Blinder et al.'s claims. Burkhauser and Turner note that Blinder et al.'s computations show the earnings test is not a tax on individuals above the NRA because they fail to frame the individual's decision problem from a life-cycle perspective; in a life-cycle context older individuals can substitute labor supply across periods by comparing the Social Security tax rate at different ages. They stress that because Blinder et al. consider the decision problem from a single period perspective, they find that working beyond age 65 yields a higher marginal returns to work as higher earnings at those ages may replace lower earnings from earlier years in the social security benefits calculation formula, thus potentially negating the tax penalty imposed by the earnings test. Burkhauser and Turner argue that from a life-cycle perspective, however, the dollar increase in earnings from extra work yields a much higher return at younger ages, specifically at ages which are not subject to the earnings test, creating an incentive for individuals to work more at younger ages. When viewed from the life-cycle perspective the earnings test is, thus, still a tax for individuals above the NRA. They also show that Blinder et al. do not use the appropriate interest rate for computing the present value of lifetime social security wealth, they use the real interest rate instead of the nominal interest rate. They find that when they recalculate the present value of social security wealth using nominal interest rate for individuals between ages 62-NRA, the social security wealth declines as the future adjustments to benefits do not fully compensate for the lost benefits. Based on these findings they conclude that adjustments are actuarially unfair and the earnings test is a tax for younger individuals below the NRA as well.

Contributing to the debate, Kahn (1989) casts further doubt on Blinder et al.'s claim that if older workers are fully aware of the provisions of the Social Security program the earnings test is not a tax. He identifies two problems with the Blinder et al.'s approach: first, in their analysis they assume perfect capital markets that permit individuals to freely borrow and lend at the market determined interest rate, and second their calculations only include a subsample of older individuals who continue to work. In his work, Kahn focuses on individuals between ages 62-NRA and shows that for these individuals the Social Security system does not provide a subsidy to continued work.

Earlier work by Diamond and Hausman (1984) found a sizeable fraction of older individuals possessed low levels of financial wealth that may constrain them from behaving as lifetime utility maximizers. Kahn emphasizes that for older individuals Social Security benefits are non-marketable assets, and liquidity constrained individuals will, therefore, not discount future social security benefits at the same rate as a market-determined interest rate. Due to the presence of imperfections in the credit markets that limit borrowing against future Social Security benefits, liquidity constrained older individuals will have a discount rate that is much higher than the one used by Blinder et al. Kahn shows that on average for low wealth individuals, the actuarial adjustments do not fully compensate for foregone benefits, and the system provides a clear disincentive to continued work at ages below the NRA.

To summarize, past evidence indicates that the earnings test is a tax for individuals below the NRA under three possible scenarios: first, if the nominal interest rate is high; second, if older workers are unaware of the Social Security rules, and third, if older workers are liquidity constrained. For individuals above the NRA, the earnings test is a tax even if individuals are aware of the Social Security rules and are not liquidity constrained if the DRC adjustments do not fully compensate for the loss of current benefits.

2.3.2. THEORETICAL PREDICTIONS

If older individuals above the NRA face no borrowing constraints and are fully aware of the actuarially fair DRC adjustments, the earnings test is not a tax in the life-cycle model and the 2000 repeal will leave the far-sighted older worker's lifetime budget constraint unchanged. In this case, the earnings test repeal will not affect lifetime labor supply decision of older individuals above or below the NRA. If, however, older workers are myopic in their behavior and perceive the earnings test as a tax either due to liquidity constraints, or unawareness/misunderstanding of future adjustments that are applied to foregone benefits, then the repeal will affect their lifetime labor supply decisions. Prior to the repeal myopic individuals will reduce their labor supply at ages where the earnings test applies and raise labor supply at ages where the net reward for work is higher.

In the life-cycle model for individuals who are myopic in their outlook or are constrained to behave myopically, the earnings test repeal of 2000 increases the net wage workers expect to earn above their NRA. This increase in the net wage changes the relative net wage an individual expects to earn during different periods of his life. Realizing these changes, individuals below the NRA may wish to increase their work effort more at ages above the NRA while reducing work below the NRA due to increases in the reward for working at older ages. The direct effect of the earnings test repeal on individuals below the NRA will be an intertemporal substitution effect that tends to reduce labor supply. Younger individuals who planned to work above the NRA also experience an income effect. Both the substitution and income effect work in the same direction to reduce labor supply at younger ages. These individuals may reduce current labor supply, with the expectation to work more at older ages above the NRA when they can earn a higher return on their work.[18] These predictions are summarized in Table 2.3.

[18] If, however, there are large labor force entry and exit costs then I expect spillover effects to arise among those below the NRA. Friedberg and Webb (2009) view these spillovers costs as constraints on labor supply transitions that can arise because workers might lose their skill over time, or their skills might become outdated; there may be large search costs. These spillover

For older individuals above the NRA and below age 70, the repeal of the earnings test generates two predictions. Older beneficiaries who work and earn above the exempt amount experience both a substitution effect and an income effect when the earnings test is repealed. The substitution effect arises because the repeal increases the cost of leisure and encourages the affected workers to increase their hours of work. But the increase in the net wage also exerts an income effect as workers now earn more in real terms than before the repeal, so the higher real income encourages the affected workers to reduce their hours of work. The net effect of the earnings test repeal on hours of work of working beneficiaries above NRA and below age 70 is therefore ambiguous. There is, however, a clear prediction regarding the effect of the earnings test repeal on the labor force participation of this age group. The earnings test repeal raises the net wage and in the presence of minimum hours constraints or fixed costs of work (that may inhibit flexible adjustment of hours to below the exempt amount) the substitution effect may lead affected older nonparticipants to respond by reentering the labor force and increasing their labor force participation rate.[19]

A third group that may potentially respond to the earnings test repeal is older individuals between ages 70-74. Unlike people below the NRA, older people ages 70-74 are exempt from the earnings test, so they do not experience a change in relative net wage when the earnings test is repealed. It is possible, however, that workers between the

effects may lead younger workers to stay at work, so they can avail themselves of the higher net wage at older ages.

[19] Past research by Cogan (1981), Altonji and Paxson (1988), Gustman and Steinmeier (1985), Hurd (1996) and Haider and Loughran (2008) have emphasized the presence of fixed costs and minimum hours constraints which restrict the wage offer curve of employers to a limited number of hours wage bundles that restrict the available opportunity set, and provided evidence for the existence of such constraints in limiting the labor supply behavior of older individuals.

NRA-age 69 who had planned to continue working in their seventies may after the repeal experience an increase in their social security wealth. These individuals will respond to the income effect at ages 70-74 by reducing their labor supply.

In the presence of the earnings test, increases in the DRC also change the net wage workers expect to earn above their NRA. Earnings above the earning test threshold amount are taxed and returned later by increasing future benefits through the DRC adjustments. Increases in the DRC adjustments, thus, raise the net wage an individual expects to earn over different periods of his life. If older beneficiaries are aware and understand the DRC adjustments, they will respond to the DRC increases in a manner similar to the earnings test repeal. After the earnings test repeal further changes in the DRC are not directly tied to earnings. These increases in the DRC only exert an income effect on workers at all ages, thus reducing labor supply at all ages.

2.4. MODEL SPECIFICATION AND METHOD OF ESTIMATION

In my work, I aim to use high frequency data from the Survey of Income Program and Participation (SIPP) to empirically assess the short-run intertemporal labor supply response to the recent rise in the reward for work among individuals above the NRA. I examine both the response in labor force participation and hours of work decisions.

Labor Force Participation: Leonesio (1990) predicts that if older workers face considerable minimum hours or fixed cost constrains then in the short run they will respond to the earnings test repeal by changing their labor force participation. He notes that the change in the labor force participation rate of older workers above the NRA will occur through the re-entry of workers who had already left the labor force. In their work

Reimers and Honig (1993) studied the reentry behavior of men below the NRA and found that men respond to the loosening of the earnings test exempt amount by reentering the labor force. Based on their findings, they conclude that men are myopic in their behavior, ignore actuarial adjustments to future benefits, and face substantial constraints that limit them from flexibly adjusting their labor supply below the exempt amount.

One previous study by Tran (2002) has attempted to identify whether an observed change in labor force participation rate of older men above the NRA in response to the 2000 repeal of the earnings test arises due to re-entry or workers continuing to stay longer in the labor force. Tran creates a panel of older white men using the CPS which allows him to observe re-entry behavior over a one-year gap. He does not find evidence of a rise in labor force participation due to reentry. He does find, however, some suggestive evidence of a rise in labor force participation arising from workers continuing to stay in the labor force longer. Relying on Canadian data, Baker and Benjamin (1999) do not find any evidence of the effect of the earnings test reforms on employment when assessed using a measure of weeks worked for the full sample, but find an increase in weeks worked conditional on work that is stimulated by the flow of workers from part year full time work to full year full time work. They attempt to decompose this finding by narrowing the range of the piecewise-linear budget constraint along which individuals exhibit this response. To their surprise, they find the rise in full year full time work is observed among individuals who work part year full time and who would have left the labor force had the earnings test not been repealed. Baker and Benjamin’s findings lend support to the idea that in the presence of the earnings test, limited opportunities for part time work lead many older workers to leave the labor force. Their findings imply that the response to the earnings test repeal will be concentrated at the extensive margin.

Hours Changes: A common finding in previous studies is that of strong suggestive evidence of the presence of minimum hours constraints or limited opportunities for part time work that may constrain the adjustment of hours among older workers to discrete changes. Haider and Loughran (2008) use administrative data to analyze the bunching behavior of individuals, and observe the presence of labor market rigidities that keep workers from locating precisely at the exempt amount. Both Engelhardt and Kumar (2009) and Disney and Smith (2002) also explore the source behind their finding of an increase in hours of work, and observe that it is driven by an increase in full time work, suggesting that older workers adjust their labor supply in discrete jumps and not continuous increments as implied by the traditional labor supply model.

Previous studies, however, have modeled the hours response to earnings test repeal as a binary variable defined on a sample that includes both workers and non-workers. This form of specifying the dependent variable imposes a restriction on behavior: part time workers are assumed to respond or be affected by explanatory variables in a manner similar to workers who are unemployed or not in the labor force, a restriction that may be too strong.[20] Ham (1982) notes that if older workers are truly constrained in their labor supply choices then the above mentioned modeling approaches

[20] In his work Ham (1982) draws a distinction between the labor supply decision process of unemployed and underemployed workers, noting that he finds evidence at least for prime age workers that the factors affecting the probability of unemployment are different from factors affecting the probability of underemployment.

are inappropriate; he shows that in such a scenario least squares estimates will be biased because they provide the combined effect of a variable (say, earnings test repeal) on the desired hours of work, and on the probability of being constrained.

Keeping in mind the substantial evidence provided by previous studies regarding labor market constraints that hinder flexible adjustment of hours among older workers, I choose a modeling framework for hours of work that accommodates the discrete nature of the labor supply adjustment process. In my empirical analysis, I model the dependent variable as: full time; part time; and not in the labor force.[21] Zabalza et al. (1980) use a structural model to study the impact of the earnings test in United Kingdom. Citing the strong restrictions older workers face in adjusting their hours of work, they also model the hours decision as a discrete choice between full-time, part-time, and not in the labor force.

Specification: I estimate panel data fixed-effects models to assess the short-run labor supply response of older individuals to the earnings test. I use the following linear model specification which allows me to implicitly control for the increases in future benefits lost to the earnings test:

[21] The multinomial logit framework has been frequently employed in past studies related to the labor supply decision of married women, who also were observed to adjust their labor supply in discrete jumps; see Lehrer (1992) for an example. Due to the small number of observations of unemployed workers, I remove them from the sample instead of constraining the response of unemployed workers to be the same as those not in the labor force.

$$
\begin{aligned}
Y_{it} = \; & \alpha_i + \theta_1 \, Earnings \; in \; place \; dummy_{it} \\
& + \; \theta_2 \, DRC_i * \; Within \; age \; range \; directly \; affected \; by \; DRC_{it} \\
& + \theta_3 \; Age \; 62 - NRA \; dummy_{it} + \theta_4 \; At \; NRA \; dummy_{it} \\
& + \; \theta_6 \; socio \; economic \; controls + \eta_a \; + \; \theta_t + u_{it}
\end{aligned}
$$

where *i* indexes individual and *t* indexes time; *Y* is a dichotomous measure of labor supply (labor force participation or discrete measure of hours of work) α_i represents individual effects, θ_t is a set of time dummies (quarters), and η_a is a set of age dummies.

The main variable of interest is the earnings test dummy, which takes a value of one when an individual is within an age range covered by the earnings test and zero otherwise. The DRC variable is defined in percentage form based on the birth cohort of the individual, while the dummy variable within age range directly affected by the DRC is one for individuals between NRA and age 69 who are directly affected by the DRC and zero for older individuals between age 62-NRA and ages 70-74. To account for observable differences in labor supply of otherwise similar individuals I include controls for marital status, region, home ownership, number of household members, whether the individual is a guardian of children under age 18, and the state unemployment rate.

Since my aim is to estimate the labor supply response to the earnings test while controlling for the possible influence of the DRC, I rely on fixed-effects models. Within a fixed-effects framework, the variation in the earnings test dummy arises as an individual ages. In the empirical analysis I consider individuals between ages 62-74, which can be subdivided into three groups: 62-NRA, NRA-69, 70-74. For years prior to 2000, the earnings test in place variable is set to one for all individuals ages 62-69 and zero for individuals ages 70-74, while for the year 2000 and later the earnings test in place dummy is set to one for individuals ages 62-64 only and zero for all ages above the NRA. The earnings test dummy variable, thus, captures the change in labor supply when an individual transitions from an age range covered by the earnings test to an age range for which the earnings test is not in place. To account for the differences in labor supply that may arise due to the effect of being below the NRA, I also add a dummy variable for age 62-NRA. This variable controls for differences in labor supply for those below the NRA that do not change over time. These differences could exist due to rules that apply for those below the NRA and that have not changed over time, such as the 50 percent tax at age 62-NRA and the actuarial adjustment of 6.67 percent applied to future benefits.

I described earlier that in the life-cycle model with no constraints on borrowing, the earnings test is a tax on labor supply if people either ignore the actuarial adjustments or consider them actuarially unfair. In the empirical analysis I account for these adjustments to future benefits. The main effect of the DRC controls for a common effect of the DRC at all ages. Since, it is a time constant variable it is implicitly controlled in the fixed-effect model. The DRC interaction term captures the differential effect of changes in the DRC on individuals when they are in an age range directly affected by the DRC changes relative to an age range in which the DRC is not directly applicable.

I chose to employ panel data fixed-effects estimation method because it provides me with three advantages. First, it allows me to estimate the earnings test effect by exploiting variation in the earnings test that arises due to the natural aging of people while accounting for the effect of the DRC. Second, I am able to control for cohort effects that may confound the analysis, as the DRC changes are assigned by birth cohort.

Pingle (2006) and Blau and Goodstein (2010) find that their estimates of the effect of the DRC adjustments on labor supply of older individuals are sensitive to the manner in which they specify the effect of birth cohort. The fixed-effects model controls for birth cohort effects in a flexible way. Third, it allows me to control for a time invariant unobserved individual specific taste for work that may be correlated with other explanatory variables. In the fixed effects models, I am unable to report estimates for the main effect of the DRC and the NRA variables (or any other time constant variables like education) because it is not possible to distinguish the effect of time constant variables from time constant unobserved tastes for work.

To check the sensitivity of the findings to the functional form specifying the relationship between the dependent and the explanatory variables, I estimate a linear probability as well as a binomial logit model, using linear fixed-effects and Chamberlain's conditional logit models respectively to handle individual effects. An advantage of the linear fixed-effects model relative to the conditional logit is that it provides estimates of the average partial effects, as Wooldridge (2010) notes that we cannot estimate the average partial effects in the conditional logit model without specifying a distribution for the unobserved tastes.[22] The response probabilities associated with multinomial logit model for the discrete choice hours of work are:

$$P(y = j \mid x) = \frac{exp(x\beta_j)}{[\,1 + \sum_{h=1}^{3} exp(x\beta_h)]}\ , \qquad j = 1,2,3$$

[22] I use robust standard errors for the linear fixed effects and the conditional logit model. Wooldridge (2010) observes that for the linear fixed effects model we need to make inference robust to heteroscedasticity and serial correlation.

where *j*=1, 2, 3 represents the three states full-time, part-time work, and non-participation. Wooldridge (2010) notes that a useful fact about the multinomial logit model is that since

$$P(y = 1 \; or \; y = 2 \mid x) = p_1(x, \beta) + p_2(x, \beta) \;,$$

it can be expressed as $P(y = 1 \mid y = 1 \; or \; y = 2, x) = \frac{p_1(x,\beta)}{p_1(x,\beta)+p_2(x,\beta)} = \Lambda[x(\beta_1 - \beta_2)]$

where $\Lambda(.)$ is the logistic function. That is, conditional on the choice being either full-time or part-time work, the probability that the outcome is full-time work follows a standard logit model with parameter vector $\beta_1 - \beta_2$. The multinomial logit model can thus be estimated as a series of bivariate logit models. This property is useful because I assess the hours-worked response using a multinomial logit model with fixed effects, which I estimate using the Chamberlain conditional logit estimator (1980).[23] I also report the estimates of partial effects from a linear fixed-effects model.

2.5. DATA AND DESCRIPTIVE FINDINGS

To perform the empirical analysis, I use data from the Survey of Income Program and Participation (SIPP). The data are from the four most recent panels 1996-2008 which cover years 1996 to 2013. Each panel interviews a set of new households who are followed for a period of 3 to 5 years. The longitudinal nature of the SIPP panels allows me to observe the same individuals over multiple years.[24] The SIPP has a rotating panel

[23] Rosenzweig and Wolpin (1993) use a similar approach.
[24] I can follow an individual as long as he stays within the age range of the sample and does not leave the sample due to attrition.

design in which panel members are randomly assigned to one of four rotation groups, where each month members of one of the rotating groups are interviewed. The survey oversamples households from areas with a high concentration of poverty.

An attractive feature of the SIPP is that individuals are interviewed every four months and are asked detailed questions regarding their labor force activity in each of the previous four months. A possible alternative is the Health and Retirement Study (HRS). The HRS is a longitudinal survey that is specifically designed to study older individuals, and it follows them for a longer duration than the SIPP, but the interviews are held every two years. The greater frequency of the interviews in the SIPP is valuable in accurately observing reentry behavior and transitions from full-time to part-time work; the SIPP also has a larger sample size of older individuals relative to the HRS.[25] Most previous researchers have relied on the Current Population Survey (CPS) to analyze the impact of the earnings test repeal, as the CPS provides a very large sample size. Tran (2002) creates one year panels using the monthly CPS data to study transitions into and out of employment. Friedberg and Webb (2009) note that these one year panels from the CPS data may not provide reliable estimates of the response in labor supply transitions to the earnings test repeal due to attrition bias. Unlike the SIPP, which follows individuals when they move, the CPS does not follow individuals, and may understate labor supply transitions if employment transitions are correlated with moving.

[25] In the main empirical analysis, I use data from the fourth reference month (the month directly preceding the interview month) as it is likely to have the least recall bias. I also check the sensitivity of my findings by performing the same analysis using monthly data with the additional inclusion of an interview month dummy.

2.5.1. GRAPHICAL EVIDENCE

For the empirical analysis I focus on older men and women aged 62-74. The descriptive findings for general trends in the labor force participation and hours of work for older men are summarized in Table 2.4. The rows highlighted in bold indicate the average labor force participation rate or rate of full-time work for each of the three age groups during the years 1996 to 2013, while subsequent rows indicate the average labor force participation rate and full-time work percentage observed in each of the SIPP panels. The general pattern is that of an increase over time in labor force participation and full-time work (relative to nonparticipation) for each age-group, while individuals below the NRA experienced a rise in their likelihood of working part-time relative to full-time in later panels.

In Figures 2.1-2.3, I provide graphical analysis of the pattern in labor force participation and hours of work for men, and in Figures 2.4-2.6 for women. In the discussion below, I focus on the trends in labor supply observed for men. Figure 2.1 show the evolution of labor force participation rate of older men during the time period 1996-2013. There are three things to note in Fig. 2.1. First, there is an upward trend in the labor force participation rate of older men in each of the three age ranges. Second, unlike the common trends assumption adopted in studies that employ the difference-in-differences approach, the upward trend in labor force participation rate does not appear to be parallel for the different age ranges. Third, in the year immediately following the repeal, for older men below the NRA there appears to be a clear decline in the labor force participation rate, while the labor force participation rate for those above the NRA-age 69 shows a steep rise, and the behavior of individuals aged 70-74 remains roughly the same. This pattern seems to suggest that the spike in the labor force participation rate of men between NRA-age 69 is driven by reentrants. In her graphical analysis of the survival rates in retirement using the HRS, Maestas (2010) finds that individuals between ages 62-64 have a higher likelihood of reentering in the first three years after retirement. She notes that one reason for observing this higher likelihood of unretirement among younger workers is the earnings test repeal of 2000. Friedberg and Webb provide a visual description of reentry rates of older individuals in the CPS; they find a temporary spike in the reentry rate of individuals between NRA and age 70 in the year immediately following the earnings test repeal.

A look at Figure 2.2 suggests that the immediate changes in the labor force participation of men between ages 62-69 following the earnings test repeal are led by changes in full-time work. Men below the NRA are less likely to remain at full-time work, while men above the NRA are more likely to work full-time after the repeal relative to not working. This preliminary descriptive finding of an increase in full-time work among older men directly affected by the repeal lends support to the presence of hours or fixed costs constraints that may inhibit the labor supply of older workers by limiting their ability to restrict their hours of work below the exempt amount. By raising the net wage of men above NRA and below age 70, the earnings test repeal encourages constrained workers to reenter the labor force and work full-time. The upward trends in full-time work relative to not working mirror the upwards trends in the labor force participation discussed earlier, suggesting that in years prior to and following the repeal the movement into and out of labor force among older men is from older workers leaving or joining full-time work.

Previous researchers have studied the influence of loosening the earnings test on full-time work of older individuals. In their analysis, most previous researchers study the response of full-time work relative to not working or working part-time. It is possible that individuals who leave the labor force face different constraints than those who work part-time, so, the responsiveness to loosening of the earnings test (either due to the repeal or increases in the DRC adjustments) may differ based on whether individuals are currently working part-time or not in labor force. Figure 2.3 highlights two main differences in the response of full-time work when compared to part-time work as opposed to being in the labor force. First, the trends in full-time work relative to part-time work for each age group are not similar to the trends in full-time work relative to nonparticipation; this is particularly the case for individuals below the NRA. Second, individuals below the NRA and those between ages 70-74 experience a rise in the likelihood of working full-time relative to working part-time in the period immediately following the repeal. The direction of change in full-time work and the magnitude of the change differ depending on whether the comparison is made relative to nonparticipants or part-time workers. These differences suggest that combining nonparticipants with part-time works may not accurately reflect the response in full-time work of older individuals to either the loosening of the earnings test or changes in the DRC.

2.6. REGRESSION FINDINGS

I now present the findings from the estimation of labor supply models for older men and women. In presenting these findings, I begin by describing the results from the basic labor supply models for the full sample of individuals between ages 62-74, and then perform robustness checks. Later I attempt to identify the effect for subgroups of the population.

2.6.1. FINDINGS FOR OLDER MEN

Labor Force Participation: Table 2.5 reports the findings from the estimation of the empirical labor force participation model specified earlier. The dependent variable is a simple binary measure indicating labor force participation. I provide the estimates from both a linear and logit fixed-effects model. The first specification does not control for the differential effect of the DRC adjustments on the labor supply of older individuals ages 62-74. In panel data fixed-effect models, the earnings test in place dummy captures the effect on labor force participation of being in an age group where the earnings test applies relative to an age group for which the earnings test was repealed. I estimate that the earnings test reduces labor force participation among older individuals age NRA-69, but the effect is not statistically significant.

Although the fixed-effects specification implicitly controls for the common effect of the DRC at all ages, the DRC directly applies only to individuals between ages NRA-69. When individuals between NRA-69 postpone receipt of their benefits (explicitly or due to the earnings test) their future benefits increase by the amount of the DRC. It is possible, therefore, that changes in the DRC adjustments may affect individuals between ages NRA-69 who are directly affected by the adjustments, in a manner different from older individuals who are not directly affected. The second specification includes a DRC interaction term to control for any differential effect. With the inclusion of the DRC interaction term, the earnings test effect is now marginally significant indicating that the earnings test reduces labor force participation between ages NRA-69 by 2.3 percentage points, which is about 7 percent of the average labor participation rate of older individuals between ages NRA-69. I find that the DRC interaction has an unexpected negative sign indicating older individuals directly affected by the DRC are less likely to be in the labor force than individuals not directly affected, the estimated response is statistically insignificant. The estimates from the conditional logit models in the last two columns of Table 2.5 are consistent in sign with those from the linear fixed-effects models; the DRC interaction term is marginally significant.

Gruber and Orszag (2003) document strong age-group specific trends in labor supply of older individuals. I address this concern in Table 2.6 where I check the robustness of my findings to the inclusion of age-group specific time trends.[26] I observe that in the second specification in which I control for the differential effect of the DRC changes the earnings test exerts a negative statistically significant effect on the labor force participation of individuals age NRA-69. I find the earnings test reduces labor force participation of individuals age NRA-69 by 2.9 percentage points, about a 9 percent decline relative to the average labor force participation rate of this age group. After the inclusion of the age-specific trends the DRC interaction term increases in magnitude and is marginally statistically significant suggesting that excluding age-specific trends biases the coefficient upwards. The sign of the DRC interaction term is still negative with the implication that individuals directly affected by the changes are less responsive to them than those not directly affected.

[26] These age-specific trends are obtained by interacting age dummies (age in years) with a linear trend in time. The coefficient on the age-specific trend for age 62 for instance, captures the change in labor force participation of individuals of age 62 as a linear function of time.

I make two simplifying assumptions in specifying the labor force participation models that I estimate. First I estimate the earnings test effect by using the within age-group variation in the earnings test, but in doing so I am restricted to estimating a common earnings test response for ages NRA-69, 62-64 and 70-74. I would like to assess separate age effects in the earnings test response because the behavior of ages 62-64 may differ from those of 70-74. The main difference between these two possible control groups is that individuals between ages 62 and 64 face a stronger earnings test penalty relative to individuals NRA-69, while individuals between ages 70-74 are exempt from the earnings test. Second, while estimating the differential effect of the DRC changes, I also assume that the response to the DRC changes will be the same for age groups 62-NRA and ages 70-74. This may not be the case, because as described above unlike the other age-groups, individuals between 70 and 74 are exempt from the earnings test penalty and the DRC does not apply to them, so they respond to the DRC changes only through an income effect. I relax these assumptions in Table 2.7, where I provide three sets of estimates: the first set includes estimates from the entire sample of older individuals between ages 62-74 (these estimates were earlier reported in Table 2.6), the second set focuses on behavior of individuals ages 62-69, and the third set focuses on behavior of individuals ages NRA-74. The estimates are from my preferred specification that includes both the DRC interaction and age-specific trends. Here, I find the magnitude of the estimated earnings test effect for the sample of 62-69-year-olds is fairly close to that observed in the full sample; the earnings test effect is, however, more imprecisely estimated as indicated by the relatively higher standard errors. Surprisingly, the magnitude of the DRC interaction term for older individuals does not vary when I consider the response of individuals directly affected by the DRC change relative to either ages 62-69 or ages 70-74, suggesting that as the DRC increases individuals between NRA and age 69 are less likely to be in the labor force than either those aged 62-64 or those aged 70-74. It is not clear how to interpret this finding.

Hours of Work: I now present the results from the estimation of models for hours of work. The top part of Table 2.8 shows the findings from the basic specification which does not include age-specific trends, while the bottom part does include these trends. I observe that the earnings test reduces both full-time and part-time work relative to nonparticipation among older individuals ages NRA-69. The suggestion is that older workers employed in both full-time and part-time jobs are penalized by the earnings test, and respond to its presence by leaving the labor force. I find no evidence that the earnings test affected the hours of work decisions among individuals who continued working. Among working individuals, I observe that increases in the DRC exert a positive influence on hours of work for individuals directly affected by them, but with the inclusion of age-specific trends the effect is statistically insignificant.

In Table 2.9, I report similar models estimated for different age-groups. The top part shows the estimates for full-time work relative to nonparticipation and the bottom part shows the estimates for full-time work relative to part-time work. I observe a reduction in full-time and part-time work relative to nonparticipation in response to the earnings test; the details provided in Table 2.9 allow me to see that the earnings test effect observed for both full-time and part-time work relative to nonparticipation is stronger in the middle column in which the control group consists of younger individuals 62-NRA. In particular, the earnings test effect on part-time work is not discernible when older individuals 70-74 serve as the control group. These findings suggest that older individuals 70-74 (although exempt from the earnings test) may also be influenced by the earnings test that was in place for individuals 65-69. I find no effect of the earnings test on hours of work among working individuals, for the linear models the estimated coefficients reported in columns 2 and 3 are equal in magnitude but opposite in sign.[27]

Evidence for Subgroups: The above findings imply that even after controlling for the influence of the DRC changes, older individuals perceive the earnings test as a tax and respond to it by reducing their labor force participation and not hours of work. As discussed earlier, older individuals may perceive the earnings test as a tax if they are unaware of the adjustments or face liquidity constraints. I probe whether the response to the earnings test repeal and changes in the DRC adjustment is concentrated among subgroups of individuals who are more likely to face borrowing constraints and/or be less aware. In particular, I define subgroups of individuals based on their education level: those with a high school education or less and those who studied beyond high school. I expect to find a stronger response to the earnings test among the less educated.

The estimates for the full sample of older men ages 62 to 74 are reported in Table 2.10; the top part shows the findings for labor force participation while the bottom part shows the response in hours of work. The evidence suggests that the earnings test reduces labor force participation of men ages NRA-69 in a similar manner regardless of their

[27] In Appendix Table B.2, I explore the possibility that the influence of changes in the DRC on labor supply may differ based on whether the earnings test is in place or not by including a three-way interaction of the earnings test in place, changes in the DRC relative to the mean (6.2 percent), and whether an individual is within an age range directly affect by the DRC changes. I find some evidence that in the absence of the earnings test, changes in the DRC raise full-time relative to part-time work by working men who are directly affected by the policy changes relative to those not directly affected.

education, as the magnitude of the earnings test effect for both groups is similar to that reported in Tables 2.6 and 2.8 (though the standard errors have increased). I also find that the DRC exerts a greater influence on working men ages NRA-69, who are more educated, the effect is marginally significant. There is some evidence that less educated men who are directly affected by the DRC changes are more likely to work part-time relative to not being in the labor force, but the effect is statistically insignificant.[28]

2.6.2. FINDINGS FOR OLDER WOMEN

Most studies evaluating the influence of changes in social security policies on labor supply have empirically assessed the labor supply response among older men. Reimers and Honig (1996) note that the response to changes in the earnings test and the DRC may vary between men and women for two reasons. First, they note that even though both men and women face the same exempt amount for the earnings test, women may have more flexibility in adjusting their hours of work relative to men. They suggest this could be due to the greater availability of part time jobs in occupations which employ more females. They hypothesize that loosening of the earnings test may affect hours of work among women, but will not affect their labor force participation; while the participation decision of men will be affected because of the relatively fewer

[28] It is also possible to assess the response of liquidity constrained individuals by separating them into subgroups based on net-worth, I do not rely on this categorization in the main analysis because net-worth of an individual is endogenous. Information on net-worth is available in the SIPP topical files (Assets and Liabilities); SIPP administers the topical Assets and Liabilities questionnaire at an annual frequency. I have used the topical modules to assign individuals into subgroups by net-worth, those who are at and above the median, and those who fall below the median; the estimates from the labor supply models by net-worth are reported in Appendix Tables B.3 and B.4 for men and women respectively. For older men, I find some support that as the DRC increases it reduces the labor force participation of wealthy men with above median net-worth who are directly affected by the policy relative to individuals who are not affected.

opportunities for part time work available to them. Second, the earnings test reduces the return to work more for men than women; this difference arises due to the shorter life expectancy of men relative to women. If this is the case, men may respond more strongly to the earnings test repeal than women. To examine the possibility of differential responses in my empirical analysis I estimate separate models for both men and women.[29]

The main set of findings for women are reported in Tables 2.11-2.13. There are four things to note regarding women's labor supply response to the earnings test. First, unlike Reimers and Honig's hypothesis, I observe some evidence that the earnings test reduces labor force participation of women, the magnitude of the effect is almost equivalent to that observed for men; the earnings test reduces the labor force participation of women ages NRA-69 by 2.4 percentage points. Unlike men, in columns 2 and 3, of Table 2.11, I observe that the earnings test effect on labor force participation is similar in magnitude when assessed by age-group.[30] Second, in line with predictions I observe that when I restrict the sample to older individuals between NRA-age 74, women directly affected by the DRC changes are more likely to be in the labor force than women aged 70-74, the effect is marginally significant and sensitive to the choice of specification. Third, I find some evidence suggesting that the earnings test affects women by lowering their likelihood of working at full-time and part-time jobs relative to nonparticipation.

[29] Figinski (2013) estimates separate models for men and women, and further separates women into primary and spousal beneficiaries to assess whether the response to the earnings test repeal is similar between primary beneficiaries (men and women).
[30] This finding suggests that either both control groups are adequate or both of them responded to the earnings test in a similar manner, so no difference can be detected. The first suggestion seems more plausible.

The third column of Table 2.12 indicates that unlike men, working women respond to the earnings test by reducing their hours of work, the effect is statistically significant. This finding suggests that I observe a response in hours of work by women possibly because of the greater flexibility in the choice of hours of work that is available to them relative to men. Fourth, Table 2.13, which reports the findings from the analysis by educational subgroups, indicates that the labor force participation and hours of work response to the earnings test is concentrated among more educated women. Interestingly, I observe some evidence that less educated women who are directly affected by the DRC are more likely to be in the labor force. I observe stronger evidence of the DRC effect for educated women, but it is difficult to interpret. I find that more educated women who are directly affected by the DRC are less likely to be in the labor force than similar women who are not directly affected by the DRC changes.

To summarize, the evidence for women suggests that women do not differ from men in their response to the earnings test on the extensive margin. Like men, they perceive the earnings test as a tax and reduce their labor force participation in response to this penalty on work. Unlike men, however, working women also respond to the earnings test tax by lowering their hours of work. The reduction in hours of work observed among working women in response to the earnings test possibly indicates the greater flexibility in hours of work available to women. This finding highlights the constrained nature of the labor supply adjustments available to men, which might be missed in models which only focus on either full-time work relative to nonparticipation or use continuous measures of hours. Finally, the labor supply response of women is driven by more educated women. This finding can potentially be attributed to the lower average earnings of women than men, such that higher educated women are more likely to have earnings near the earnings test threshold.

2.7. SUMMARY

Since the inception of the Social Security System, the earnings test has been an unpopular feature of the program because of the view that it penalizes older individuals who continue to work while receiving their Social Security benefits. Over the years, numerous amendments and new features to the existing rules have been introduced. One such feature is the DRC adjustments which compensate older individuals above the NRA whose benefits were withheld due to the earnings tax. This feature complicates the analysis of the earnings test. In the presence of perfect capital markets, if people are aware of the DRC adjustments and if these adjustments fully compensate older individuals for their forgone benefits, the earnings test is not a tax and should not affect the labor supply decisions of older workers. Most previous researchers have ignored the DRC adjustments, however, claiming that either older workers are unaware of or misunderstand these adjustments, and have thus analyzed older workers' response to the earnings test using static labor supply models. In recent years, however, there has been a substantial increase in the size of these adjustments. It is possible that as the DRC adjustments rise the penalty imposed by the earnings test is reduced, and older individuals may be less likely to view the earnings test as a tax. As previous researchers have stressed, whether older individuals perceive the earnings test as a tax or not is, thus, an empirical question.

Unlike previous researchers, I address this question using a life-cycle model that accounts for the potential influence of the DRC changes. Moreover, I use fixed-effects analysis which allows me to implicitly control for birth cohort effects in a flexible manner, thus accounting for the strong trends in labor supply of older individuals that may differ by cohort. Unlike most previous researchers, I also control for age specific trends in labor supply. Previous studies have assessed the labor supply response using either a continuous measure of hours or a dichotomous measures of full-time work relative to nonparticipation, which do not full capture the constrained nature of hours choices available to older workers. In the empirical analysis I use a modeling framework which allows me to account for the discrete and constrained nature of the labor supply choices that exist for older workers. Finally, I assess whether the labor supply response to the earnings test is concentrated among subgroups of individuals who are more or less likely to be liquidity constrained or unaware of features of the earnings test rules.

Even after accounting for the DRC changes, I find evidence in support of the view that both men and women perceive the earnings test as a tax. I find evidence that older men and women above the NRA and below age 70 respond to the earnings test by reducing their labor force participation. For men, I am unable to find a differential response to the earnings test by educational subgroups. For working women, I observe a reduction in the hours of work in response to the earning test, which indicates the greater flexibility in the choice of hours of work that may be available to women relative to men. I also find that the labor supply response of women is concentrated among those who are better educated, suggesting their relatively higher earnings make them more vulnerable to the earnings test penalty than less educated women whose earnings may be much lower than the annual threshold amount beyond which the earnings test applies.

Table 2.1 Features of Earnings Test and Actuarial Adjustments

	Ages 62 - NRA[1]			Ages NRA[2] - 69			Ages 70 - 74		
Year	Threshold Amount ($)	Tax Rate (%)	Actuarial Reduction Factor (ARF) (%)	Threshold Amount ($)	Tax Rate (%)	Delayed Retirement Credit (DRC) for those reaching NRA (approximate) (%)	Threshold Amount ($)	Tax Rate (%)	Delayed Retirement Credit (DRC) (%)
1996	8,280	50	6.67	12,500	33.33	5.0			
1997	8,640	50	6.67	13,500	33.33	5.0	Earnings Test does not apply for individuals		
1998	9,120	50	6.67	14,500	33.33	5.5	above age seventy.		
1999	9,600	50	6.67	15,500	33.33	5.5			
2000	10,080	50	6.67	Earnings test eliminated in		6.0	Delayed retirement creadit adjustments for		
2001	10,680	50	6.67	2000 for those at and		6.0	postponed benefits also do not apply for		
2002	11,280	50	6.67	above normal retirement		6.5	individuals above age seventy.		
2003	11,520	50	6.67			6.5			
2004	11,640	50	6.67			7.0			
2005	12,000	50	6.67			7.0			
2006	12,480	50	6.67			7.5			
2007	12,960	50	6.67			7.5			
2008	13,560	50	6.67			7.5			
2009	14,160	50	6.67			8.0			
2010	14,160	50	6.67			8.0			
2011	14,160	50	6.67			8.0			
2012	14,640	50	6.67			8.0			
2013	15,120	50	6.67			8.0			

Note: Source -- U.S. Social Security Administration.
The threshold amount is in nominal dollars. As the normal retirement age for a specific birth cohort changes, the above rules extend to the new higher normal retirement age.

[1] Since 2000, a looser earnings test applies in the year that an individual attains his normal retirement age. The looser earnings test applies only in months before the normal retirement age; during these months the individual faces a higher exempt amount and a lower tax rate of 33 percent.

[2] This set of rules applies to an individual beginning from the month he attains normal retirement age.

Table 2.2 Normal Retirement Age and Delayed Retirement Credit

Year of birth	NRA	DRC per year
1921-24	65	3.00%
1925-26	65	3.50%
1927-28	65	4.00%
1929-30	65	4.50%
1931-32	65	5.00%
1933-34	65	5.50%
1935-36	65	6.00%
1937	65	6.50%
1938	65 and 2 months	6.50%
1939	65 and 4 months	7.00%
1940	65 and 6 months	7.00%
1941	65 and 8 months	7.50%
1942	65 and 10 months	7.50%
1943-54	66	8.00%

Note: Source -- U.S. Social Security Administration.

Table 2.3 Impact on Labor supply due to changes in the Earnings Test and DRC

	Labor Force Participation (LFP)	Hours of Work
Effect of Earnings Test Repeal (Short-Run effect) or Changes in DRC in presence of Earnings		
Below normal retirement age (ages 62-NRA)	↓ (S.E.) ↓ (I.E.)	↓ (S.E.) ↓ (I.E.)
Overall Effect	↓	↓
Above normal retirement age (ages NRA-69)	↑ (S.E.) (re-entry)	↑ (S.E.) ↓ (I.E.)
Overall Effect	↑	Ambiguous
Ages 70 - 74	↓ (I.E.)	↓ (I.E.)
Overall Effect	↓	↓

Note: The above predictions regarding earnings test repeal or DRC changes in presence of earnings test apply only if the earnings test is perceived as a tax. S.E. denotes substitution effect, and I.E. denotes income effect.

Table 2.4 Descriptive Statistics for Men

		All ages (62-74)	Ages 62 - NRA	NRA - 69	Ages 70 - 74
Age in years		67.71 (4.02)	63.62 (0.98)	67.56 (1.38)	72.3 (1.49)
Labor Force Participation Rate		0.35	0.50	0.33	0.21
1996 panel	(Dec. 1995 - Feb. 2000)	0.31	0.45	0.30	0.20
2001 panel	(Oct. 2000 - Dec. 2003)	0.34	0.49	0.32	0.20
2004 panel	(Oct. 2003 - Dec. 2007)	0.35	0.49	0.33	0.22
2008 panel	(May 2008 - Jul. 2013)	0.39	0.52	0.35	0.23
Full-Time Work relative to not in Labor Force		0.29	0.45	0.25	0.13
1996 panel	(Dec. 1995 - Feb. 2000)	0.24	0.40	0.20	0.11
2001 panel	(Oct. 2000 - Dec. 2003)	0.27	0.45	0.23	0.12
2004 panel	(Oct. 2003 - Dec. 2007)	0.29	0.44	0.26	0.14
2008 panel	(May 2008 - Jul. 2013)	0.34	0.47	0.28	0.15
Full-Time Work relative to Part-Time work		0.59	0.71	0.52	0.40
1996 panel	(Dec. 1995 - Feb. 2000)	0.52	0.69	0.42	0.34
2001 panel	(Oct. 2000 - Dec. 2003)	0.58	0.75	0.52	0.37
2004 panel	(Oct. 2003 - Dec. 2007)	0.60	0.71	0.56	0.42
2008 panel	(May 2008 - Jul. 2013)	0.63	0.71	0.57	0.44

Table 2.5 Estimates of Models for Older Men's Labor Force Participation: Age 62-74

Model	Linear (Fixed-effects)		Conditional Logit	
	(1)	(2)	(3)	(4)
Earnings test in place dummy	-0.005	-0.023*	-0.077	-0.554*
	(0.008)	(0.013)	(0.189)	(0.311)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit		-0.008		-0.211*
		(0.005)		(0.117)
Age 62 to normal retirement age dummy	0.004	-0.036	0.010	-1.087*
	(0.009)	(0.027)	(0.196)	(0.645)
At normal retirement age dummy	-0.001	-0.000	0.006	0.016
	(0.008)	(0.008)	(0.167)	(0.167)
Linear age trends	no	no	no	no
Number of observations	146,572	146,572	31,668	31,668
Number of individuals	21,066	21,066	3395	3395

All models include controls for marital status, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.6 Estimates of Models for Older Men's Labor Force Participation: Age 62 - 74 with Age-Specific Trends

Model	Linear (Fixed-effects)		Conditional Logit	
	(1)	(2)	(3)	(4)
Earnings test in place dummy	-0.019	-0.029**	-0.370	-0.696**
	(0.012)	(0.013)	(0.283)	(0.329)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit		-0.019*		-0.550**
		(0.010)		(0.230)
Age 62 to normal retirement age dummy	0.027*	-0.098	0.368	-3.253**
	(0.014)	(0.067)	(0.330)	(1.500)
At normal retirement age dummy	-0.000	0.001	-0.002	0.032
	(0.008)	(0.008)	(0.166)	(0.167)
Linear age trends	yes	yes	yes	yes
Number of observations	146,572	146,572	31,668	31,668
Number of individuals	21,066	21,066	3395	3395

All models include controls for marital status, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7 Estimates of Models for Older Men's Labor Force Participation: By Age-Group

Age-range	62 - 74		62 - 69		NRA - 74	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
Earnings test in place dummy	-0.029**	-0.696**	-0.034	-0.684	-0.012	-0.478
	(0.013)	(0.329)	(0.025)	(0.495)	(0.017)	(0.481)
Delayed retirement credit (%) * Within age range	-0.019*	-0.550**	-0.020	-0.505*	-0.019	-0.599
directly affected by delayed retirement credit	(0.010)	(0.230)	(0.014)	(0.292)	(0.015)	(0.435)
Linear age trends	yes	yes	yes	yes	yes	yes
Number of observations	146,572	31,668	98,960	23,157	99,382	17,774
Number of individuals	21,066	3395	15,639	2624	15,122	2008

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies. Age 62-NRA dummy is not included in models estimated for individuals between ages NRA-74.

Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.

*** p<0.01, ** p<0.05, * p<0.1

Table 2.8 Estimates of Models for Hours of Work for Older Men Age 62-74

Model	Full-time work relative to Nonparticipation		Part-time work relative to Nonparticipation		Full-time relative to Part-time work	
	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: No age-specific trends						
Earnings test in place dummy	-0.022**	-0.975**	-0.017	-0.641	-0.007	0.004
	(0.011)	(0.495)	(0.011)	(0.407)	(0.027)	(0.299)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.007*	-0.240	-0.007*	-0.259*	0.018*	0.190*
	(0.004)	(0.187)	(0.004)	(0.155)	(0.010)	(0.109)
Linear age trends	no	no	no	no	no	no
B: With age-specific trends						
Earnings test in place dummy	-0.029***	-1.430***	-0.022*	-0.757*	-0.004	0.031
	(0.011)	(0.530)	(0.012)	(0.428)	(0.028)	(0.313)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.012	-0.787**	-0.003	-0.133	0.029	0.264
	(0.008)	(0.374)	(0.009)	(0.303)	(0.019)	(0.217)
Linear age trends	yes	yes	yes	yes	yes	yes
Number of observations	122,760	13,015	111,866	15,384	49,664	20,653
Number of individuals	19,746	1729	17,887	2060	8992	2694

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9 Estimates of Models for Hours of Work for Older Men: By Age-Group

Age-range	62 - 74		62 - 69		NRA - 74	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: Full-time work relative to Nonparticipation						
Earnings test in place dummy	-0.029***	-1.430***	-0.030	-0.734	-0.017	-1.552*
	(0.011)	(0.530)	(0.022)	(0.743)	(0.011)	(0.852)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.012	-0.787**	-0.016	-0.479	-0.007	-1.290
	(0.008)	(0.374)	(0.013)	(0.463)	(0.011)	(0.801)
Number of observations	122,760	13,015	81,964	10,238	83,411	6,029
Number of individuals	19,746	1729	14,547	1385	14,027	882
B: Part-time work relative to Nonparticipation						
Earnings test in place dummy	-0.022*	-0.757*	-0.031***	-0.800**	-0.006	-0.335
	(0.012)	(0.428)	(0.011)	(0.376)	(0.015)	(0.610)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.003	-0.133	-0.002	-0.067	-0.018	-0.440
	(0.009)	(0.303)	(0.002)	(0.053)	(0.014)	(0.538)
Number of observations	111,866	15,384	123,881	18,027	83,797	10,052
Number of individuals	17,887	2060	19,870	2432	13,742	1359
C: Full-time relative to Part-time work						
Earnings test in place dummy	-0.004	0.031	0.026	0.449	-0.026	-0.348
	(0.028)	(0.313)	(0.045)	(0.484)	(0.040)	(0.475)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	0.029	0.264	0.034	0.348	0.038	0.370
	(0.019)	(0.217)	(0.024)	(0.270)	(0.041)	(0.444)
Number of observations	49,664	20,653	39,457	15,556	26,702	11,464
Number of individuals	8992	2694	7600	2150	5150	1562

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, state unemployment rate, home ownership, number of household members, children under age 18, region, age-specific trends, age, and quarter dummies.
Age 62-NRA dummy is not included in models estimated for individuals between ages NRA-74.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10 Estimates of Models for Older Men's Labor Supply by Education: Age 62-74

	More than high school		High school or less	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: Labor Force Participation				
Earnings test in place dummy	-0.031	-0.600	-0.025	-0.771
	(0.020)	(0.460)	(0.019)	(0.476)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.021	-0.624**	-0.016	-0.437
	(0.014)	(0.313)	(0.015)	(0.342)
Number of observations	76,509	17,114	70,063	14,540
Number of individuals	10,772	1801	10,351	1593
B: Full-time relative to Nonparticipation				
Earnings test in place dummy	-0.027	-0.800	-0.027*	-2.049***
	(0.017)	(0.799)	(0.014)	(0.760)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.007	-0.490	-0.018	-1.242**
	(0.012)	(0.519)	(0.011)	(0.558)
Number of observations	62,488	7,257	60,272	5,750
Number of individuals	9990	924	9806	805
C: Part-time relative to Nonparticipation				
Earnings test in place dummy	-0.027	-0.772	-0.019	-0.893
	(0.019)	(0.599)	(0.016)	(0.617)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.012	-0.500	0.004	0.186
	(0.012)	(0.409)	(0.012)	(0.454)
Number of observations	54,780	7,994	57,086	7,383
Number of individuals	8780	1079	9153	980
D: Full-time relative to Part-time work				
Earnings test in place dummy	-0.004	0.190	-0.011	-0.129
	(0.037)	(0.430)	(0.044)	(0.467)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	0.047*	0.545*	0.005	-0.153
	(0.025)	(0.279)	(0.030)	(0.363)
Number of observations	30,600	12,766	19,064	7,869
Number of individuals	5323	1618	3680	1074

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies, and age-specific trends.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.11 Estimates of Models for Older Women's Labor Force Participation

Age-range	62 - 74		62 - 69		NRA - 74	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
Earnings test in place dummy	-0.024**	-0.513	-0.020	0.076	-0.024*	-0.979*
	(0.012)	(0.350)	(0.021)	(0.505)	(0.014)	(0.518)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.005	-0.391*	-0.014	-0.243	0.023*	0.096
	(0.009)	(0.231)	(0.012)	(0.285)	(0.013)	(0.463)
Number of observations	177,790	31,618	117,346	23,913	123,128	16,659
Number of individuals	25,514	3323	18,433	2665	18,617	1831

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies and age-specific trends. Age 62-NRA dummy is not included in models estimated for individuals between ages NRA-74.

Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.

*** p<0.01, ** p<0.05, * p<0.1

Table 2.12 Estimates of Models for Older Women: Ages 62-74

Model	Full-time work relative to Nonparticipation		Part-time work relative to Nonparticipation		Full-time relative to Part-time work	
	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
Earnings test in place dummy	-0.013	-0.131	-0.017*	-0.553	-0.074**	-0.733*
	(0.009)	(0.708)	(0.010)	(0.424)	(0.030)	(0.391)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.003	-0.757*	-0.010	-0.613**	-0.017	-0.167
	(0.007)	(0.428)	(0.007)	(0.301)	(0.022)	(0.270)
Number of observations	151,632	10,891	152,123	17,157	42,681	16,337
Number of individuals	23,710	1423	23,285	2150	8059	2153

Refer to notes for Table 11

Table 2.13 Estimates of Models for Women's Labor Supply by Education: Age 62-74

	More than high school		High school or less	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: Labor Force Participation				
Earnings test in place dummy	-0.052***	-1.378***	-0.0002	0.401
	(0.020)	(0.506)	(0.014)	(0.483)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement	-0.031**	-1.063***	0.019*	0.353
	(0.014)	(0.326)	(0.011)	(0.327)
Number of observations	78,407	16,786	99,383	14,810
Number of individuals	10,998	1734	14,575	1590
B: Full-time relative to Nonparticipation				
Earnings test in place dummy	-0.034**	-1.043	0.004	0.657
	(0.017)	(1.039)	(0.010)	(0.960)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement	-0.018*	-1.354**	0.009	-0.262
	(0.011)	(0.630)	(0.008)	(0.596)
Number of observations	64,121	5,848	87,511	5,030
Number of individuals	10,037	727	13,727	695
C: Part-time relative to Nonparticipation				
Earnings test in place dummy	-0.037**	-1.391**	-0.003	0.339
	(0.018)	(0.625)	(0.012)	(0.590)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement	-0.035***	-1.459***	0.011	0.426
	(0.013)	(0.423)	(0.009)	(0.428)
Number of observations	63,348	8,949	88,775	8,201
Number of individuals	9653	1121	13,681	1030
D: Full-time relative to Part-time work				
Earnings test in place dummy	-0.157***	-1.736***	0.002	0.185
	(0.044)	(0.561)	(0.042)	(0.559)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement	-0.064**	-0.743**	0.035	0.429
	(0.029)	(0.360)	(0.035)	(0.427)
Number of observations	24,265	9,317	18,416	7,008
Number of individuals	4482	1196	3592	955

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies, and age-specific trends .
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

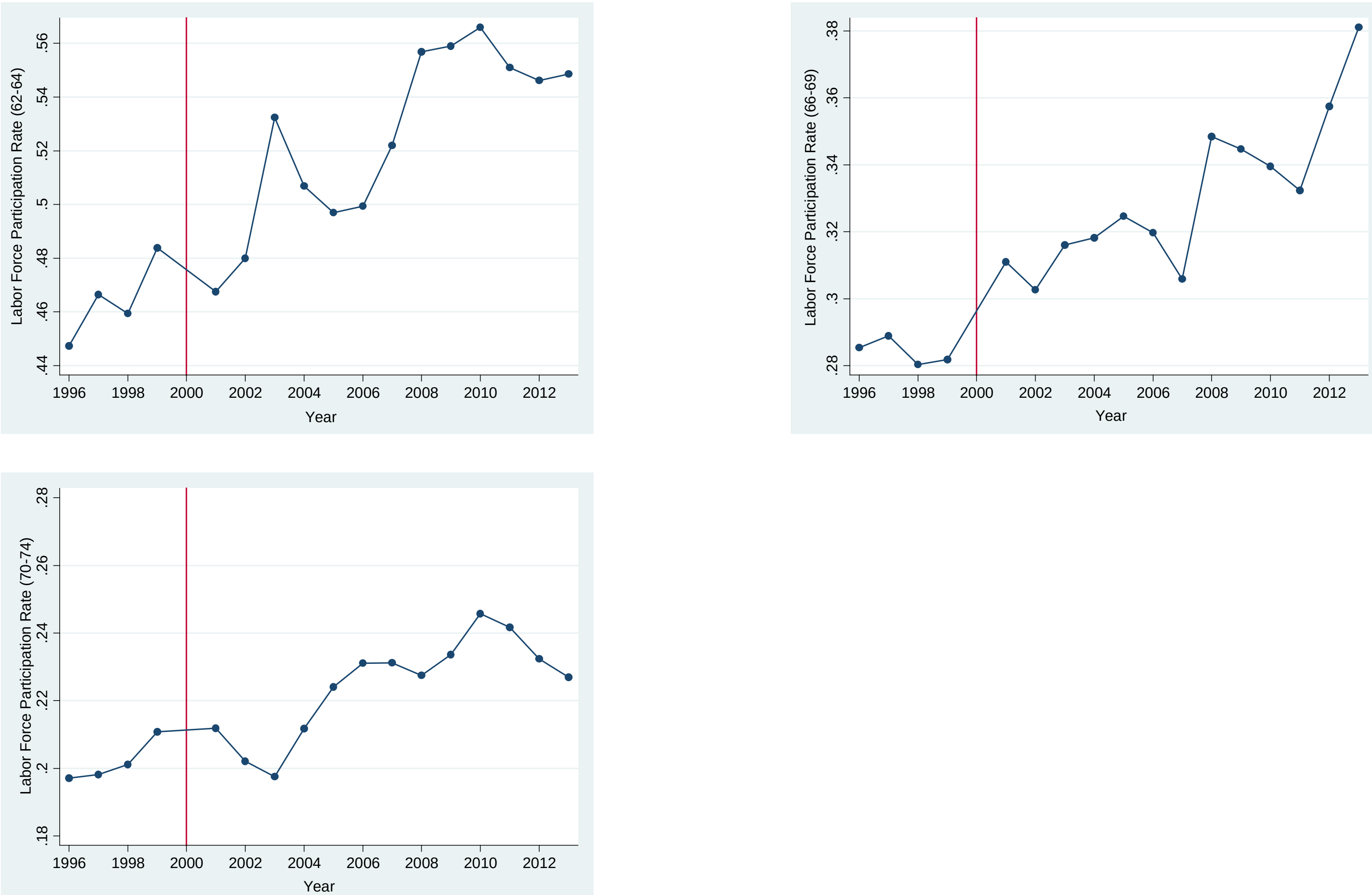


Figure 2.1 Trends in Labor Force Participation of Older Men by age group

Figure 2.2 Trends in Full-time work relative to Nonparticipation of Older Men by age group

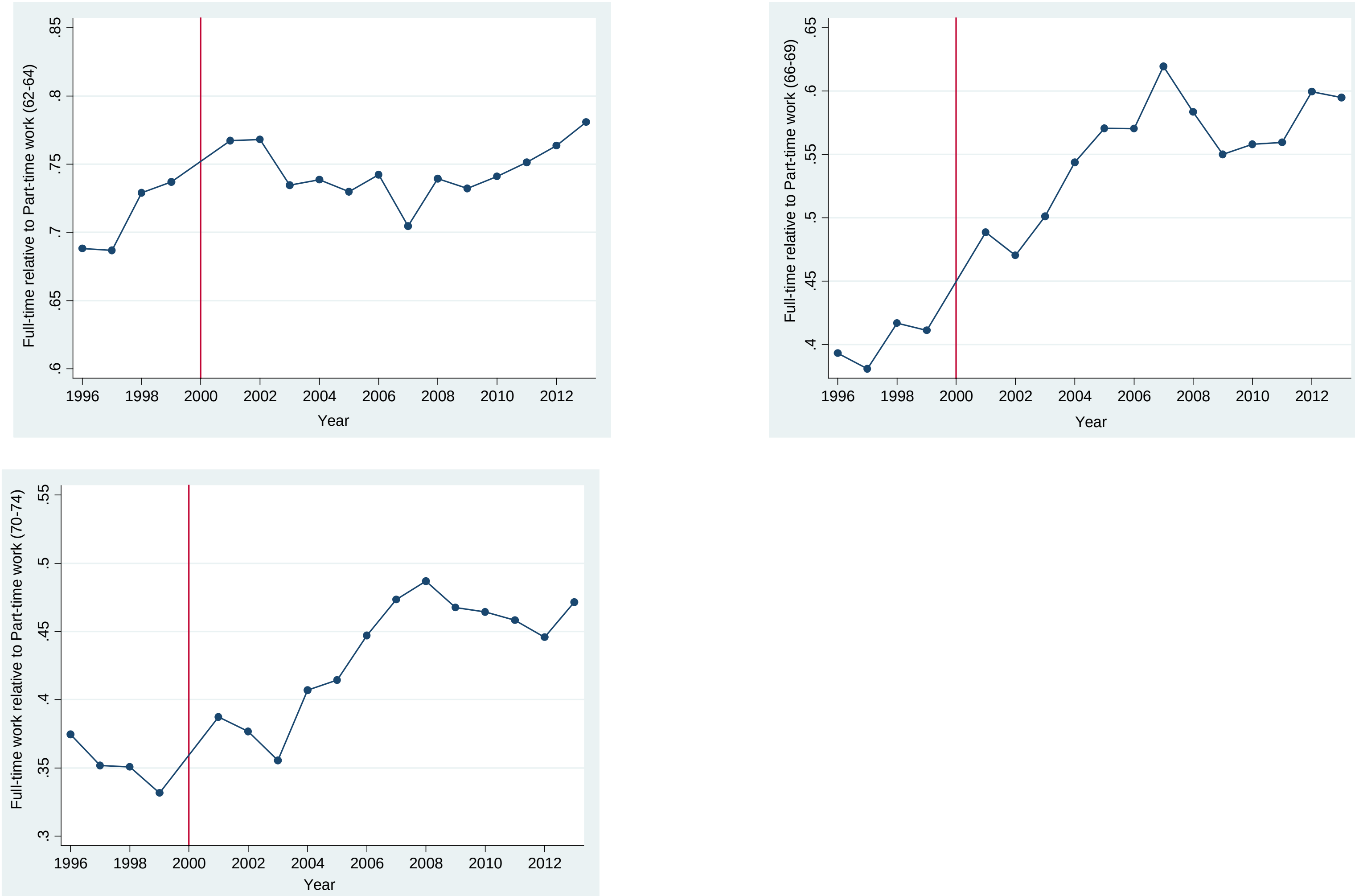


Figure 2.3 Trends in Full-time work relative to Part-time work of Older Men by age group

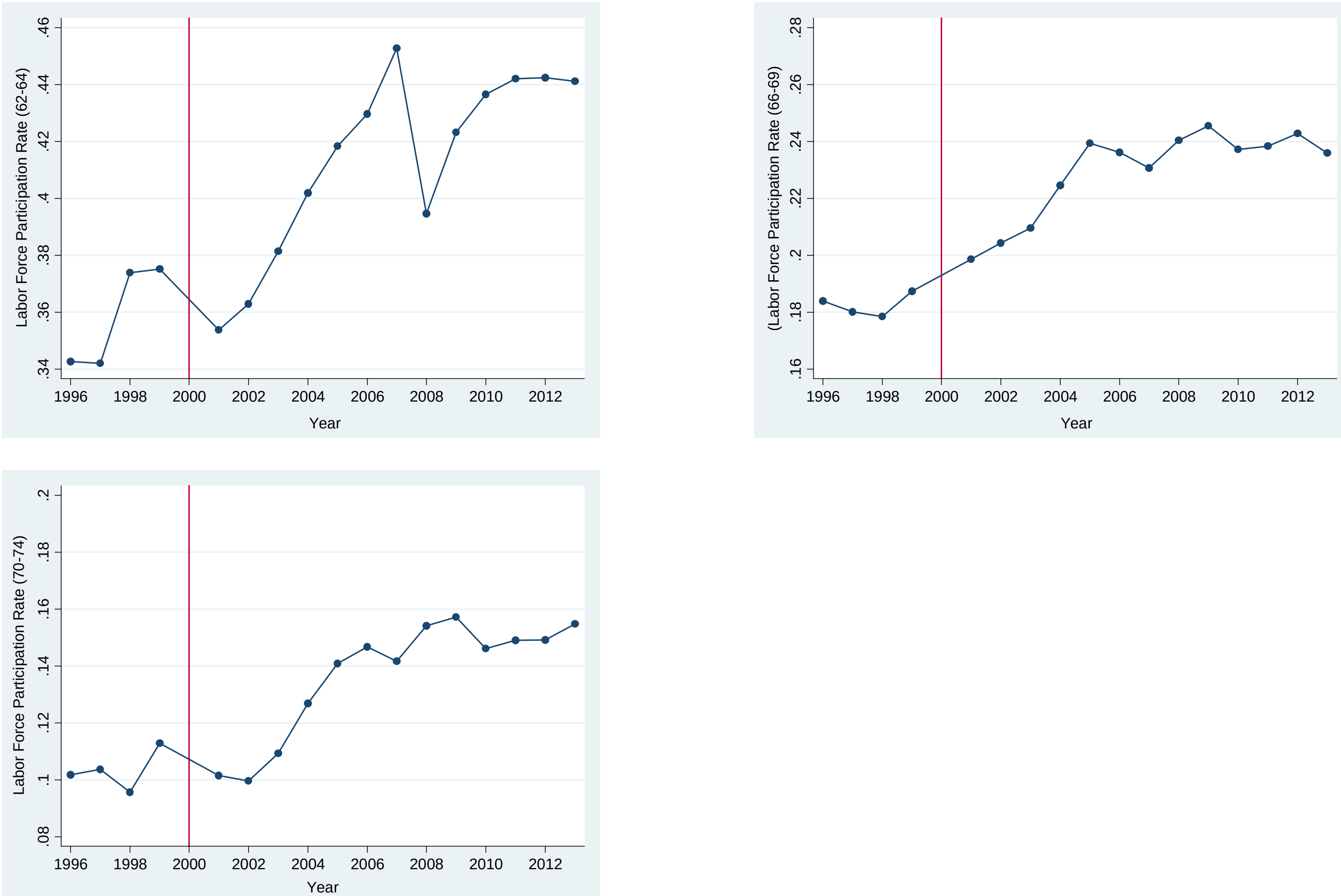


Figure 2.4 Trends in Labor Force Participation of Older Women by age group

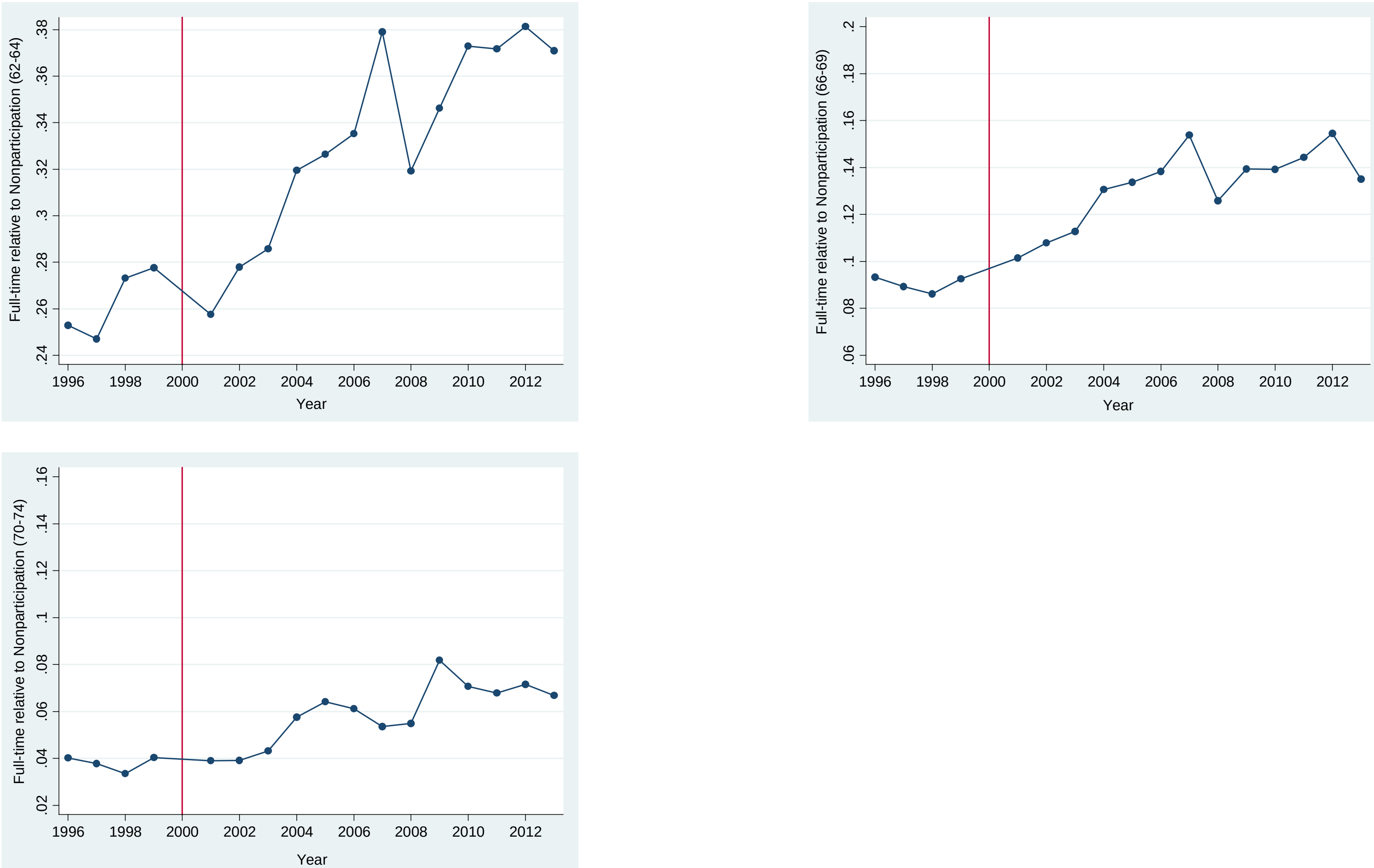


Figure 2.5 Trends in Full-time work relative to Nonparticipation of Older Women by age group

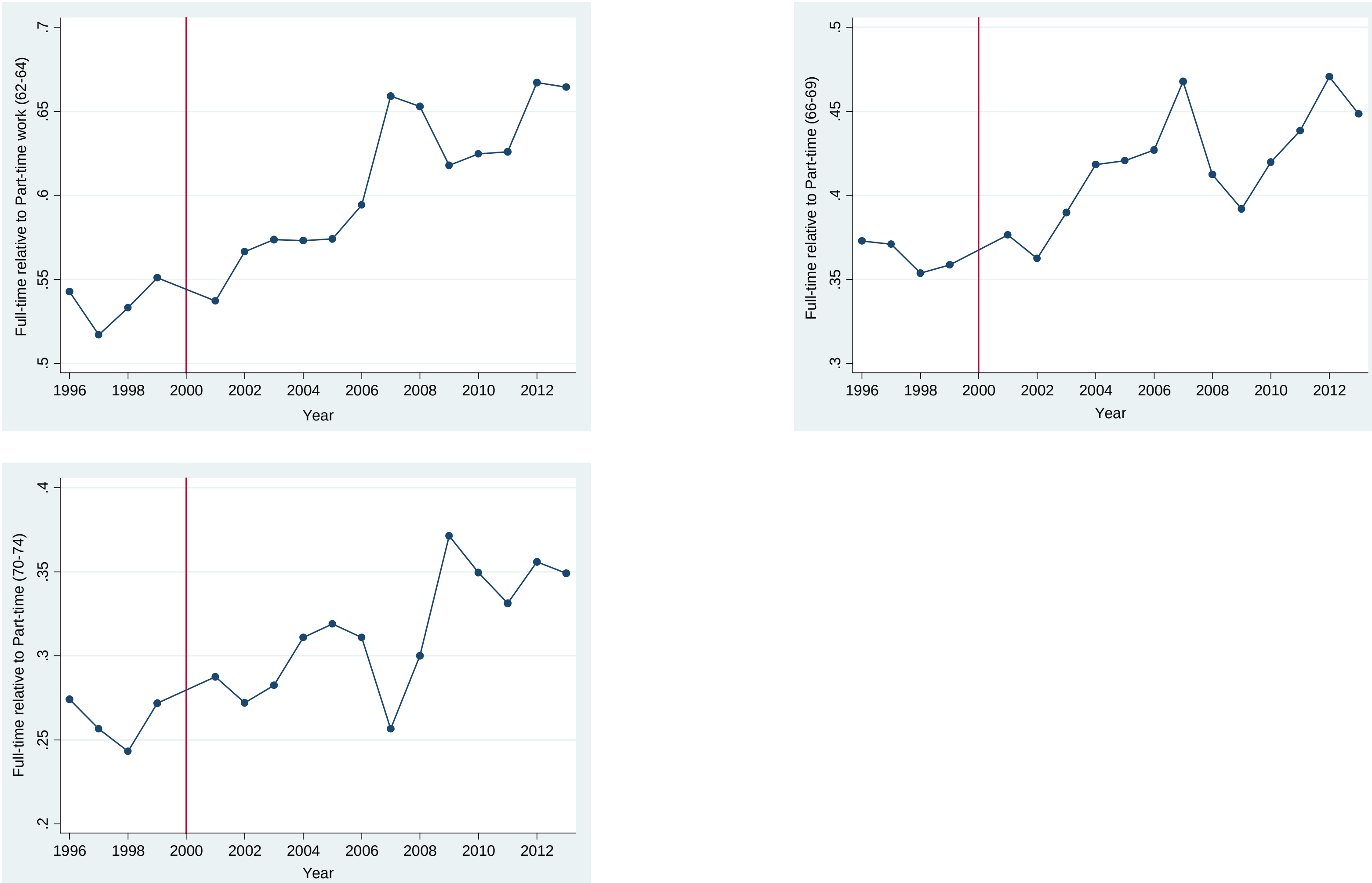


Figure 2.6 Trends in Full-time work relative to Part-time work of Older Women by age group

CHAPTER 3

LOCAL LABOR MARKET CONDITIONS AND LIVING ARRANGEMENTS OF YOUNG PEOPLE

3.1. INTRODUCTION

In a study that predates the 2007-2009 recession, Card and Lemieux (2000) document the declining economic condition of youth in the U.S. (particularly the less educated). They show that youth adapt in diverse ways to adverse economic conditions, changing their living arrangements, employment, and school enrollment behavior. A Pew Research Center (2013) report covering the period 2007 to 2012 echoes the findings of Card and Lemieux. The report shows that between the years 2007 and 2010 the young adult unemployment rate rose from 6.2 to 12.4 percent. Between the same time period the fraction of young adults (18 to34-year-olds) living with their parents rose from 22 to 24 percent.[1] The surge in the fraction of young adults living with their parents during the recent recession has motivated three recent studies to analyze the extent to which young people respond to a period of prolonged economic distress by living with their parents. Similar to Card and Lemieux, each of these studies (Kaplan (2012), Matsudaira (2015), and Lee and Painter (2013)) find that poor economic conditions—as assessed by the local

[1] These numbers are taken from the updated report by Pew Research Center (2015). These numbers exclude 18 to 24-year-old college students who are enrolled full-time. The report also notes the decline in the marriage rate among young adults (18-31) from 30 % in 2007 to 25 % in 2012.

labor market conditions, and personal unemployment status—exert a significant positive impact on the likelihood of an adult child residing with a parent.

Early research carried out in the 1980s and 1990s by demographers, sociologists, and economists identified personal and family background characteristics of young adults as important determinants of their living arrangements. Haurin et al. (1993) expanded the demographic model used in the previous studies by including economic factors such as the cost of housing and the potential wage of young adults as other possible determinants of their living arrangements. The early literature, however, relied mostly on cross-sectional data; studies using longitudinal data relied on a few cohorts and included small sample sizes.[2] Influence of economic determinants of living arrangements was often identified through the use of personal unemployment status, which is potentially jointly determined with living arrangements.

Card and Lemieux (2000) emphasized exogenous variation in young people's local labor market conditions as a key source of identification of the influence exerted by labor markets on living arrangements. One limitation of their work is their inability to separately control for the influence of business cycles on young adults and their parents.[3] Moreover, their analysis is based on repeated cross-sectional group-level data; this framework poses some additional limitations. First, within the group-level framework they were unable to account for persistent unobserved preferences of young people living with their parents. Second, within their framework, they are unable to study the influence

[2] Goldscheider and DaVanzo (1989) and (1990), and Aquilino (1990).
[3] The work of Rosenzweig and Wolpin (1993) and Ermisch (1999) stresses the importance of parental resources on young people's living arrangements.

of economic conditions on young people's movements into and out of the parental home. In the fifteen-year period following Card and Lemieux's work, researchers have attempted to address these two concerns by using individual-level analysis that controls for personal characteristics of young adults, and by using longitudinal data to examine how changes in local labor markets affect the transitions in living arrangements. A valuable insight from Kaplan (2012) is the importance of unobserved individual specific preferences in accounting for the majority of variation in living arrangements. Kaplan's work underscores the need for high frequency longitudinal data in isolating the influence of labor market conditions on living arrangements of young people.

Drawing on the insights from the earlier research I use longitudinal data from the Survey of Income Program and Participation (SIPP) for years 1996 to 2013 to study how changes in young people's labor markets affect their living arrangements, and whether this influence is stronger during the period of the recent recession. In my work I identify the effect of labor market conditions by using age-group, education, and gender specific variation in unemployment rate and employment-population ratio of young people. This measure of local labor market conditions faced by young people offers two advantages relative to that of Card and Lemieux. One, it allows greater variation, and, two, it allows me to separately account for the influence of business cycles on young adults and their parents. I also assess the ability of labor market conditions to explain the living arrangements of young adults at a point in time relative to transitions in living arrangements over time. I find no robust evidence that poor labor markets conditions affect the living arrangements of young adults.

3.2. THEORETICAL AND EMPIRICAL FRAMEWORK

Most previous researchers interested in understanding the economic determinants of parent-child coresidence decisions proposed their analysis within a utility comparison framework without explicitly developing the underlying theory. Following in a similar vein, I study the influence of economic factors on coresidence behavior by relying on a reduced form approach. However, I specify the empirical model by drawing insights from the theory developed in McElroy (1989), Rosenzweig and Wolpin (1993), and Ermisch (1999). In particular, I utilize the theoretical framework and predictions derived in Ermisch (1999) to motivate my empirical work and to interpret my findings. To facilitate the discussion in later sections, I summarize key features of the theoretical models in the parent-child coresidence literature that have guided my work.

3.2.1. THEORETICAL BACKGROUND AND FRAMEWORK

McElroy (1989) builds a theory of parent-child coresidence in which the young adult's labor supply and coresidence decisions are jointly determined.[4] In her model a young person compares the utility received while living apart to that while living with parents and chooses that combination of labor supply, consumption, and living arrangements that yields the highest level of utility. Parents' resources affect the young adult only while living at home as McElroy does not model the potential for financial transfers from parents to children. She uses a Nash bargaining model to jointly derive the young adult's indirect utility function, labor supply, and reservation wage in each living

[4] McElroy also notes the joint determination of the schooling and marriage decisions along with living arrangement decision. In addition, Haurin et al. (1993) emphasize the joint determination of living arrangements and a young adult's decision to have children.

arrangement. In specifying the empirical model, I draw on McElroy's work that emphasizes the simultaneous determination of coresidence and labor supply decisions of young people.

Rosenzweig and Wolpin (1993) extend the analysis of McElroy by incorporating the possibility of financial transfers from altruistic parents to children. In their work parents provide transfers to children within an intergenerational support framework. They model these financial transfers from parents as a form of support distinct from transfers provided by parents through shared residence with child. They note two distinguishing features of transfers provided via coresidence. First, such transfers are cheaper for parents than equivalent financial transfers because of the public good characteristic of shared housing. Second, unlike financial transfers, coresidence imposes a privacy cost on parents. Thus, Rosenzweig and Wolpin's model allows for offsetting effects of parental income on the likelihood of transfers provided via coresidence. Unlike McElroy's work, in their theoretical model parents, through their influence on the level of financial transfers, make the coresidence decision by comparing their utility in the two living arrangement scenarios, while children only choose the level of human capital investment (taking as given the level of parental transfers). I describe the Rosenzweig and Wolpin model to show how the possibility for making financial transfers influences the coresidence decision through the effect on parents' utility.

I motivate my empirical model by utilizing the theoretical framework proposed by Ermisch (1999), who built on the theory developed by McElroy and Rosenzweig and Wolpin to explicitly derive the effect of housing costs on the parent-child coresidence decision. Parents are assumed to be altruistic; their utility depends on the utility of their child as well as their own consumption of housing and other goods. Following Ermisch's notation, parents' utility can be written as:[5]

$$U^P\left[x_p, h_P, U^C(x_c, h_c, i), i\right] \qquad i = r, a$$

where U^P, U^C represent the parent and child's utility respectively, i allows utility of parents and child to differ based on their preference for coresidence (r) and living apart (a), and x_j and h_j are the parents' and child's respective consumption of housing and other goods.

Ermisch allows for the possibility of financial transfers from parents to children while living apart and together, but total transfers from parents to children are cheaper when coresiding due to joint consumption of housing.[6] In each living arrangement parents make financial transfers to their child based on a comparison of their income relative to the child's. The model has a two stage game structure, in the second stage of which the child chooses that combination of living arrangement, housing, and other consumption which yields the highest utility, taking as given transfers chosen by parents in the first stage.[7]

3.2.2. THEORETICAL PREDICTIONS

Based on his theoretical model Ermisch makes three predictions regarding the effect of changes in economic factors such as housing prices, child's income, and

[5] In his analysis Ermisch derives the theoretical model and its predictions for a single child family.
[6] Unlike Rosenzweig and Wolpin, Ermisch does not explicitly model the privacy cost that parents may experience when coresiding with a child.
[7] The theoretical model, however, does not account for the joint determination of labor supply and coresidence decisions of young people.

parents' income on the likelihood of parent-child coresidence. First, a higher cost of housing raises the probability of a child residing with parents, as the child's consumption of housing increases at the parental home relative to living apart.[8] Second, higher income of the child reduces the probability of a child residing with parents, as it raises the child's utility in the living apart situation. Third, a higher income for parents raises the probability of the child residing with parents regardless of whether parents make financial transfers (if privacy costs are ignored).[9]

3.2.3. FRAMEWORK FOR EMPIRICAL ANALYSIS

Ermisch (1999) notes that an implication of the theoretical model is that separate measures of parents' and child's income along with the cost of housing need to be included in any empirical model analyzing the effects of economic conditions on parent-child coresidence. Based on the theory outlined above, he derives the indirect utility function for a young adult *i* while living with and apart from parents at time *t*, as follows:

$$Living\ with\ Parents\colon\ U_{it}^{R}[\,y_{it}^{c}, y_{it}^{P}, p_{it}^{h}, age_{it}, u_{it}^{R}\,]$$

$$Living\ apart\ from\ Parents\colon\ U_{it}^{A}[\,y_{it}^{c}, y_{it}^{P}, p_{it}^{h}, age_{it}, u_{it}^{A}\,]$$

[8] This prediction holds only if parents' do not respond to the higher cost of housing by changing their own demand for housing. Ermisch (1996) estimates that the demand for housing by older individuals in Britain is inelastic, and so based on this evidence he believes that higher price of housing will reduce the probability that young adults live apart from their parents'.

[9] In the scenario in which parents make no financial transfers, higher parental income raises the child's joint consumption of housing at the parents' home, thus inducing the child to live at home. If parents make financial transfers to the child when living apart, higher parental income may motivate parents to make more transfers and as transfers via coresidence are cheaper, parents will choose to make more transfers by coresiding with their child.

where U^R and U^A are the indirect utility functions for the young adult while coresiding and while living apart. y_{it}^c and y_{it}^P are measures of child's and parents' income respectively, and p_{it}^h is a measure of rental costs. These three variables indicate the economic determinants of a young adult's living arrangements, while u_{it}^R and u_{it}^A represent the child's preferences for living with and apart from parents, respectively.

At any given point in time (a static model) we observe the parent and child coresiding if:

$$U_{it}^R[\, y_{it}^c, y_{it}^P, p_{it}^h, age_{it}, u_{it}^R \,] \;\; > \;\; U_{it}^A[\, y_{it}^c, y_{it}^P, p_{it}^h, age_{it}, u_{it}^A \,]$$

while in Ermisch's dynamic extension of the static model, we observe a young adult returning to parents' home if:

$$U_{it}^R[\, y_{it}^c, y_{it}^P, p_{it}^h, age_{it}, u_{it}^R \,] \;\; > \;\; U_{it}^A[\, y_{it}^c, y_{it}^P, p_{it}^h, age_{it}, u_{it}^A \,]$$

$$U_{it}^R[\, y_{it-1}^c, y_{it-1}^P, p_{it-1}^h, age_{it-1}, u_{it-1}^R \,] \;\; \leq \;\; U_{it}^A[\, y_{it-1}^c, y_{it-1}^P, p_{it-1}^h, age_{it-1}, u_{it-1}^A \,]$$

In the above framework business cycles can affect both child's and parents' income, while changes in attitude towards living apart can affect the young adult's living arrangements through their influence on preferences. The aim of the empirical analysis is to assess the degree to which changes in economic determinants affect the probability of young child residing at home (and the transitions in and out of the home) over time.

3.3. REVIEW OF PREVIOUS LITERATURE

Many previous researchers have empirically analyzed the contribution of labor market conditions and housing costs to explaining the long run rise in young adult's living with their parents. In summarizing the previous research in the economic literature, I categorize studies into early and recent research based on whether the time period of analysis in the study covered the 2007-2009 recession. I make this distinction for two reasons. First, the range of analytical methods employed in the earlier studies highlight key issues that researchers need to be mindful of when specifying their empirical model. Second, the findings in the earlier studies regarding the effect of economic conditions on long run trends in living arrangement are mixed, with some finding statistically significant but not economically meaningful effects of economic conditions. Recent studies, on the other hand, tend to find a significant and economically substantial influence of economic conditions on the long run changes in living arrangements of young people.[10]

3.3.1. EARLY RESEARCH (PRE-2007 DATA)

The early studies vary along multiple dimensions, as summarized in Tables 3.1 A and B. Three key differences among these studies are: one, the extent to which observed changes in the labor markets and housing/rental markets are identified through exogenous variation; two, the extent to which personal and parental controls are included in the

[10] One reason for this divergence is that the time period of analysis in the earlier studies is restricted to the mid-1990s or early 2000's, before the recent sharp decline in independent living of young people that began in 2005. In his descriptive work, Matsudaira (2015) illustrates that the rise in the tendency for young people to live at home began between 1970 and 1980, after which it rose substantially between 1980 and 1990, and fell slightly in the 1990s.

model specification; and three, the extent to which the researcher tries to distinguish between influences exerted at a point in time (static analysis) relative to transitions in living arrangements over time (dynamic analysis).

Relying on a comparative perspective, Card and Lemieux (2000) contribute to the literature by identifying age and gender specific regional labor market conditions as an important source of exogenous variation to estimate the degree to which parent-child coresidence, employment, and school enrollment rates co-varied within the U.S. and Canada over time.[11] They use the March Current Population Survey (CPS) from the U.S., and the Census and Survey of Consumer Finance data from Canada to concentrate on the period 1971-94. They perform reduced form group-level analysis of living arrangements using two key region and gender specific exogenous variables: the employment-population ratios of 25 to 45-year-olds, and the average wage of 16 to 24-year-olds.[12] A limitation of the employment/ population measure used is that it is unclear whether the change in the employment-population rate of 25 to 45-year-olds reflects the economic conditions faced by the young person or their parent. This limitation applies to all studies using aggregate labor market variables (either for all ages or ages other than the age-group directly being studied). For both young men and women, Card and Lemieux find a stronger response in parent-child coresidence to improvements in labor

[11] Card and Lemieux note that they rely on regional variation in the labor market instead of using state variation because the broader classification greatly increases the number of observations for young people in each age group. They illustrate that the labor markets in the U.S. and Canada differ across regions, and during periods of national or secular shocks, these markets vary in the time and strength at which they are affected, as well as in their path of recovery.

[12] They interpret the youth wage as exogenous to supply side factors noting previous research on youth labor markets that views youth employment as being determined on the demand side. Youth refers to young people below age 25.

market conditions for Canada—which had experienced a prolonged recession in the 1990s—than for the U.S. In results that foreshadow the findings from other studies, they are unable to explain differences in living arrangements between the two countries for the 1971-1991 period, but are able to fully attribute the relatively higher degree of parent-child coresidence that occurred in Canada between 1991 and 1994 to its relatively poor labor market conditions in the 1990s.

Within their empirical setup, Card and Lemieux are unable to control for changes in housing costs or factors indicating young people's preference for living at home that may vary over time. Yelowitz (2007) uses U.S. census individual level data from 1980 to 2000, and makes two contributions to the literature. First, he emphasizes the influence of the cost of housing, and second, he controls for some personal characteristics and accounts for other alternative living arrangements available to young adults. Specifically, he focuses on the changes in housing and rental costs as a possible explanation for the decline in independent living among young people.[13] He employs within-MSA variation in housing and rental costs to isolate their impact on the probability of a young adult living with a parent. He finds that higher housing prices raise the likelihood that young people live with their parents or in an economic arrangement where they share their living arrangements, while higher rents lower the likelihood of living with parents, but raise the likelihood of sharing housing with others.[14] But, Yelowitz does not find housing

[13] Yelowitz uses two other measures of economic conditions in an MSA: time to commute, and the average wage of child care workers. But due to the lack of high quality data on these two measures, he places less emphasis on the findings for these two variables.

[14] Yelowitz notes that the effect of rental costs on shared housing would not be apparent in an analysis which does not separately consider the differential effect of rental costs on the route out of the parental home.

costs or labor market conditions (as assessed by the aggregate state level unemployment rate) to be economically meaningful in explaining the trend towards living with parents between 1980 and 2000.

Using the National Longitudinal Survey of Youth (NLSY-79 and NLSY-97), Hill and Holzer (2007) move the literature forward by probing the extent to which changes in the labor market and a rich set of personal and parental characteristics explain the trends in young adult's living arrangements for two cohorts of youth aged 20 to 22 in 1984 and 2002 respectively. They harness the detailed information provided in the NLSY to assess the influence of personal attitudes and behaviors that reflect the youths' relative level of maturity, and expectations about future labor market success. They report two main findings. First, the long run decline in labor market opportunities for less-skilled young workers cannot explain the trends in living arrangements, which vary little by gender, race, or education. They do not address the potential endogeneity of their key labor supply measures and caution that these findings are not causal. Second, at a given point in time personal attitudes and behaviors that reflect maturity and independence of the young individual can explain later living arrangements, but these attitudes cannot explain the change in living arrangements over time. This latter finding emphasizes the need to appropriately control for underlying attitudes in static models used to analyze living arrangements, a point that is supported by Kaplan (2012).

Kaplan (2012) exploits the high frequency and longitudinal nature of the NLSY97 data to assert the relative importance of changes in labor market conditions in explaining the variance in living arrangements of young people at a point in time (static analysis) versus transitions over time (dynamic analysis). He is able to use the NLSY97 data at a monthly frequency because of a special set of retrospective questions about monthly coresidence that were asked only between 1998 and 2002. For this time interval, Kaplan follows a group of young men age 17 to 23 who do not go to college to estimate reduced form and structural models.[15] In the reduced form analysis, he relies on both static and dynamic methods to study the link between an individual's potentially endogenous contemporaneous earnings, employment status, and living arrangements. To some extent, he controls for the correlation between current labor supply measures and unobserved preference for living away from parents by using fixed effects (static) model. Kaplan's structural model estimates yield two main insights. First, he finds that preference shocks explain the majority of differences in living arrangements at a point in time. Second, labor market shocks experienced by the individual influence the timing of when they leave or return to the parental home; labor market shocks end up explaining the majority of the transitions in living arrangements. These two findings stress the importance of a dynamic model in examining the association between labor market changes and transitions in living arrangements.

3.3.2. RECENT RESEARCH (INCLUDING POST-2007 DATA)

Recent research, summarized in Tables 3.2 A and B, encompasses three studies that add data from the recent recession in their consideration of the significance of labor market changes in accounting for the long run rise in young adult's residing at home.

[15] Kaplan does not study the coresidence behavior of young women, citing their eligibility for benefits as possibly influencing their decision regarding living with or apart from parents. Because the monthly coresidence questions which Kaplan uses to construct his high frequency dataset were discontinued in 2002, the data restrict his ability to study individuals outside the age range 17-23. Specifically, Kaplan is unable to study the coresidence changes for young people who attend college.

Kaplan (2012) is part of both the earlier and recent research because he performs two separate sets of analysis. The first set of estimates is based on NLSY-97 data from 1998 to 2002, while the second set of estimates is based on CPS data from 1979 to 2010. His estimates from the CPS data allow him to observe changes in living arrangements that took place in the 2000s, particularly during the recent recession, so I include these findings in this section. Kaplan uses the CPS data to perform a group-level analysis using within state variation in age-group specific employment-population ratios and hours of work to assess the impact on young adults living arrangements. This analysis allows him to address the endogeneity problem posed by his use of young adults' personal employment status in the NLSY data, and to extend his findings to a broader age range. Unlike previous researchers, Kaplan's key labor market variables directly pertain to young people, but he does not separately control for the effect of labor market conditions for parents. Moreover, because of the nature of his analysis, he does not assess the effect of preferences on living arrangements. He finds significant and meaningful effects of labor market conditions on parent-child coresidence rates.

Matsudaira (2015) applies Card and Lemieux (2000)'s identification strategy to individual level data from the U.S. Census and ACS, covering a lengthy time period from 1960 to 2011. The two key variables capturing labor market conditions are the state level employment-population ratios of 35 to 44-year-olds and the state level average wages for 19 to 34-year-olds. Matsudaira finds that economic conditions exert a strong influence on young peoples' living arrangements, and they alone can explain 70 to 80 percent of the rise in coresidence experienced by young men. For women, however, he finds that economic conditions explain a smaller proportion of the changes, as living arrangements of women are slightly less sensitive to employment rates than those of young men. Similar to Hill and Holzer (2007), he confirms the absence of a strong relationship between education level and the extent to which economic conditions impact living arrangements. As mentioned earlier a limitation of the employment-population ratios of 35 to 44-year-olds is that it is unclear whether it reflects the labor markets conditions faced by the young person or the parents. Another concern with Matsudaira's analysis, is that he is unable to adequately address the problem posed by the joint determination of living arrangements, school enrollment, and marital status.[16]

Addressing the problem from the perspective of changes in household formation, Lee and Painter (2013) employ PSID data to study the effect of economic and housing market conditions on household formation and housing tenure during the period 1975-2009. A key variable in Lee and Painter's analysis is a categorical measure of whether a particular year is a recession year. This measure allows them to focus on the additional effect of recessionary periods due to uncertainty regarding future job opportunities as distinct from that implied by the current unemployment rate.[17] They also estimate dynamic models that study the effect of changes in economic conditions on the decision to leave or return home. The findings from their static and dynamic analysis reveal that the effect of economic conditions on young peoples' household formation during the recent 2007-2009 recession is much stronger than that observed during previous

[16] Matsudaira resolves this problem by assessing the sensitivity of his findings to different specifications of the model with and without controls for school enrollment and marital status.
[17] They classify a year as recession year, based on the dates released by National Bureau of Economic Research (NBER).

recessions.[18] Further exploratory analysis through simulations suggests that recessions affect independent living of young people primarily by influencing their demand for rental housing.

As Lee and Painter's (2013) analysis is closest to my work, I summarize four main concerns with their analysis which I attempt to address in my work. First, because of their focus on housing markets Lee and Painter do not closely model labor market conditions of young people. They do approximate these by including an aggregate measure of unemployment rate and a potentially endogenous employment status variable. Second, like Matsudaira (2015), they include controls for potentially endogenous school enrollment status. In their work Lee and Painter do not address the endogeneity problem posed by either the personal employment or school enrollment decisions. Third, the static analysis controls for a large set of observed personal and parental characteristics, but does not control for unobserved persistent attitudes or tastes. Fourth, the dynamic models rely on data that is measured at infrequent annual or biennial intervals, which possibly clouds the true relationship between economic conditions and living arrangements of young people.

I limit my empirical analysis to changes observed in living arrangements of young people ages 20 to 29 during 1996-2013. I also focus on whether the relatively poor labor market conditions of 2008-13 provide stronger evidence of their influence on young peoples' living arrangements (after accounting for factors affecting individual

[18] Lee and Painter define household formation as a young individual's decision to form a household independent of their parents. In their static model, they further consider whether the young individual forms a separate household via owning or renting.

preferences). This time window allows me to examine changes in living arrangements during two recessions, and in particular observe changes that took place during the recent recession and the subsequent slow recovery. I perform the analysis with high frequency data from the Survey of Income and Program Participation (SIPP).

Unlike previous researchers, my key variable for assessing the effect of labor market conditions on living arrangements is the age-group, education, and gender specific unemployment rate (or alternatively the employment-population ratio) in the young person's state of residence. Given the disparity in the labor market opportunities faced by young workers based on their educational attainment, I incorporate this additional source of variation in the unemployment rate measure. Importantly by focusing on the unemployment rate that specifically applies to the age-group of the affected young person, I am also able to separately control for the effect of labor market conditions for their parents. To relate back to earlier research, I also consider the effect that the level of aggregation has on the findings. Finally, I use dynamic models in an attempt to disentangle how changing economic conditions affect the rate of leaving relative to returning home. In both the static and dynamic models I address the problem of joint determination of schooling and marital status by conditioning on the young person's enrollment and marital status at the time they are first observed in the sample.[19]

[19] Matsudaira (2015) notes the bias that may arise in estimates of economics determinants of living arrangements when conditioning on marital or school enrollment status, if economic conditions affect the likelihood of marriage and enrollment differently for different types of individuals. This may be an important concern for his analysis, as he considers the effect of economic conditions on living arrangements over a period of fifty years, during which marriage and enrollment rates changed substantially.

3.4. DATA AND DESCRIPTIVE FINDINGS

3.4.1. DATA

I perform the empirical analysis by using data from the Survey of Income Program and Participation (SIPP).[20] The data are from the four most recent panels, which cover years 1996 to 2013. Details are summarized in Table 3.3. Each panel interviews a set of new households who are followed for a period of 3 to 5 years; the longitudinal nature of the SIPP panels allows me to observe the same individuals over multiple years.[21] The SIPP has a rotating panel design in which panel members are randomly assigned to one of the four rotation groups. Each month members of one of the rotating groups are interviewed. The survey oversamples households from areas with a high concentration of poverty.[22] Previous researchers have relied on CPS and Census data to analyze changes in living arrangements as they allow researchers to examine changes over long periods of time and include very large sample sizes. Because of its longitudinal nature, however, the SIPP is better suited for my analysis. The panel dataset enables me

[20] Other researchers have used the SIPP in the past to study changes in living arrangements. Avery et al. (1992) use the SIPP 1984 panel to study the influence of parental and young adults' income on the route taken out of the parents' home. Recently Wiemers (2014) analyzed the change in living arrangements of young people in response to personal unemployment with SIPP data. Her work, however, examines changes in living arrangements of young people through doubling up with others, a much broader group than my work which studies changes in living arrangements of young people that occur primarily through coresidence with parents.

[21] I can follow an individual as long as he stays within the age range of the sample and does not leave the sample due to attrition.

[22] Due to Census budget cuts the sample for the 2004 panel was cut in half at the end of the eighth wave (reference period June 2006-December 2007). After the eighth wave fifty percent of the sample was dropped and not interviewed in subsequent months. The data for waves 1 through 8 were collected for the full sample. Moreover, due to the government shutdown in October 2013 members of rotation group 2 in the SIPP panel 2008 were not interviewed in wave 16.

to control for unobserved preferences of individuals in the static analysis, and pursue my aim to understand the effect of economic conditions on transitions in living arrangements.

Three datasets - the Survey of Income Program and Participation (SIPP), Panel Survey of Income Dynamics (PSID), and National Longitudinal Survey of Youth (NLSY) - are longitudinal surveys that can be used to study transitions in young peoples' living arrangements over time. In comparison to the PSID and NLSY, the SIPP has both strengths and weaknesses. The main advantage in using the SIPP data is its high frequency data collection process. Individuals in the SIPP are interviewed every four months; they are asked detailed questions regarding their demographic information, labor force activity, and living arrangements in each of the previous four months. Since the mid-1980s, however, there has been evidence documenting the presence of "seam bias" in SIPP.[23] This refers to the tendency for transitions or reported changes in status to be higher "between reference periods" than "within a reference period."[24] Lemaître (1992) notes that seam bias appears to be a general problem of longitudinal surveys, regardless of differences in design and in length of the reference period. [25] I address the seam bias present in the SIPP by adopting an approach used by Grogger (2004) and Ham and

---

[23] Burkhead and Coder (1985).

[24] Moore (2008) reports that changes measured across the "seam" between two successive reference periods can exceed changes estimated with a single reference period by a factor of 10 or more. He also notes that beginning with the 2004 SIPP panel the Census Bureau in an attempt to significantly reduce the amount of seam bias adopted a more focused and extensive use of dependent interviewing (DI) procedures which use substantial answers from previous interviews to tailor the wording and routing of questions. He observes the use of such procedures substantially lowered the seam bias in 2004 panel relative to previous panels, but that it still continues to be a problem for the SIPP.

[25] Ham et al. (2014) note that seam effects have been documented for various surveys in many North American and European countries such as: the SIPP, the CPS, the National Longitudinal Survey, the PSID, the Canadian Survey of Labour and Income Dynamics, and the British Household Panel Survey.

Shore-Sheppard (2005). I use data from the fourth reference month (the month directly preceding the interview month) as it is likely to have the least seam bias, and drop the observations from all other months. As Kaplan (2012) highlights, these high frequency data are valuable in closely linking changes in economic conditions to young peoples' movement in and out of the parental home.[26] In contrast, individuals in the NLSY and the PSID were initially interviewed annually, but in recent years are being interviewed biannually.[27]

The main weaknesses in using the SIPP relative to the PSID and the NLSY are its inability to incorporate parental characteristics of individuals who are away from home, its shorter panel length, and the unavailability of detailed personal background and parental wealth information.[28] Both PSID and NLSY contain family information for the young person's parents regardless of their current living arrangement. In the SIPP, however, I can obtain parental information only when a young person is observed living with either parent. I attempt to address this issue to some extent in the static analysis through the use of fixed effects which control for unobserved persistent influence of

[26] In the empirical analysis, I use data from the fourth reference month (the month directly preceding the interview month) as it is likely to have the least recall bias.

[27] The NLSY-79 consists of young adults born between 1957-64 (ages 14-22 in 1979). During the period that most of these youth are between ages 20 and 29 the NLSY-79 interviews were conducted annually (they became biennial in 1994). While the NLSY-97 consists of young adults born between 1980 and 1984 (ages 12-17 in 1997). During the period that most of these youth are between ages 20 and 29 the NLSY-97 interviews were also conducted annually (beginning in the year 2013 the NLSY-97 interviews became biennial). The monthly coresidence questions used by Kaplan (2012) were only asked in NLSY97 between 1998 and 2002. The PSID, on the other hand, conducted annual interviews between 1975 and 1997 and biennial interviews from 1999 onwards.

[28] The SIPP contains detailed information on assets for parents who live with their children in its topical wave modules which are administered at annual intervals in a panel. The data on assets are available in the following topical modules of the SIPP: Assets, Liabilities, and Eligibility. I do not include these variables because of the infrequency of the topical modules and the associated decrease in sample size.

parental characteristics on the young adult's living arrangements, but am unable to control for any parental characteristics in the analysis of young adults' transition to returning home. Another difference is the treatment of young adults enrolled in college and living in dormitories; SIPP and NLSY treat such young adults as members of their parents' household, while they are classified as living apart from parents in the PSID.

I primarily restrict my analysis to SIPP panels 1996-2008, as with the 1996 panel the SIPP underwent a major redesign that introduced important changes to improve the quality of the estimates. First, the redesign substantially increased the sample size and the length of the SIPP panels. Second, the overlapping panel structure of earlier SIPP panels was abandoned. Third, a computer assisted interviewing procedure was introduced which permitted automatic checks. As a result, the imputation procedure was adapted to rely on historical information reported in prior waves. Fourth, specifically for my purpose, beginning with the 1996 panel the SIPP data provided two sets of variables that allow me to separately identify the young adult's mother and father, while the earlier panels included a single identifier for primary parent. Moreover, after the redesign I can also identify the type of parent-child relationship (biological, adopted, or stepchild). The advantage of these new sets of variables is that for each young adult I can identify the living arrangement status by linking him or her to either or both parents and use detailed information on family structure in the dynamic model for leaving home.[29] Using data

[29] There are two other advantages for using data from panels 1996 onwards. First, the earlier panels did not provide separate information for nine U.S. states, where following the redesign data was unavailable for only five U.S. states. Moreover, I use CPS data to estimate the labor market measures, and a major redesign of the CPS was also implemented in 1994 to improve the precision of labor force estimates.

from SIPP panels following the redesign will allow for compatibility among the post 1996 panels.

3.4.2. DESCRIPTIVE FINDINGS

To better understand the long run trends in living arrangements and appreciate the recent pronounced changes in these trends, I present some descriptive results using the SIPP panels from 1985 to 2008, covering the extended period 1984 to 2013. For the graphical analysis I include only young people ages 20 to 29 who are not enrolled in school at the time they are first observed in the sample.[30] I classify young people as living away from their parents' home if they reside in a household which does not include either parent (or in-laws), and living with a parent if they reside with at least one parent (or in-law) in a household that is not headed by them or their spouse.[31]

I begin with Figure 3.1, which describes the evolution in living arrangements of young men and women respectively over the SIPP panels.[32] The graphical analysis supports trends in living arrangements observed by other researchers. First, similar to

---

[30] I exclude individuals who serve in the armed forces. In the SIPP, only individuals who are primary sample members (those present at the time of the first interview of a household) are followed, so the sample for this analysis is restricted to primary sample members. Sample members can enroll in school at a later date.

[31] Parents are defined as biological/natural, step, or adoptive. A relatively small percentage of parents reside with their child in a household where the head of the household is the child, I exclude such individuals from the analysis. In the empirical analysis I consider individuals regardless of their marital status, but I restrict the sample for the regression analysis to individuals who are single at the time they are first observed in the sample. Because of the presence of married individuals in the sample, for the graphical analysis I add the possibility of a young person living at the in-laws' home (this accounts for about 6 percent of young people living at home and does not vary much over time).

[32] I summarize the changes in living arrangements by SIPP panel and not calendar year because over time the percentage of young adults living with and away from parents may vary for two reasons: the natural ageing of the young individuals in the panel and the underlying trends in living arrangements.

most researchers I find throughout the span of my analysis women are more likely to live away from home than men. Second, both men and women display similar trends in their living arrangements; the percentage of young adults living away from home declines smoothly at a relatively slow pace between the 1985 and 1996 panels, after which it declines sharply between the 2004 and 2008 panels. This finding concurs with that of Bitler and Hoynes (2015) who using the CPS document a decline in independent living among young adults that began in 2005.

To illuminate these findings, I consider the changes in the flows to and from home. In Figure 3.2, I categorize the movements into those returning home and those leaving home. Here, I observe important differences in the return and leave behavior of young adults. Throughout the analysis women are more likely to leave home than men, but the trends in leaving home are similar for both men and women, suggesting that similar factors are affecting their leaving home behavior. There was a sharp decline in the percentage of men and women leaving home between the 1996 and 2001 panels which stabilized in later panels. I observe a much smaller percentage of young adults returning home, moreover, there is no discernable difference in return behavior of men and women. There is a slight decline in the percentage of young adults returning home between the 2001 and 2004 panels but it levels off in later panels. A surprising aspect of these findings is the lack of change in the pattern of young adults returning home during the recent recession. In Figure 3.1 I observe a greater proportion of young adults living with their parents during the 2008 panel relative to the 2004 panel, but I do not observe any noticeable change in the rate of returning or leaving parental home between the two panels. The suggestion is that much of the increase in the proportion of young individuals living with their parents occurred prior to the beginning of the 2008 SIPP panel.

For individuals at risk of leaving home, I also consider the changes in the route taken out of the parental home. Women are more likely to leave home via marriage than men; between the 1985 and 1996 panels I observe a decline in the percentage of young adults who leave home for marriage. In the regression analysis, however, I do not separately analyze the route taken out of the parental home, because in the last four SIPP panels I do not detect much change in the patterns for the route taken out of the home. Among both sets of individuals, those returning and those leaving home, I notice a majority of individuals make such moves within the same state, with only a small fraction of the moves involving a state change. Therefore, in the regression analysis I examine the behavior of individuals who do not change their state of residence when they make the move to or from home.

Card and Lemieux (2000) find that young people adapt to changing economic circumstances by modifying their labor supply, living arrangements, and schooling decisions. In the SIPP, young people residing with a parent are considered to be a member of their household even if they leave home to live in a college dormitory. A change in the young person's enrollment status may, thus, prolong his or her stay at home. In Table 3.4, I probe how the enrollment status of young people in the SIPP changes with time in the four most recent panels; I separate the analysis for young adults at risk of leaving home and returning home. Unlike the sample for the graphical analysis, I study the trends in school enrollment by restricting the sample to young adults 20 to 29 who are single at the time they are first observed in the sample. The first row indicates the percentage of young adults who are enrolled in school at the time they are initially observed in the sample (in the regression analysis I exclude these individuals). The second row indicates the percentage of young adults who were initially unenrolled, but changed their enrollment status at a later date. I observe that a higher percentage of young adults living at home are enrolled in school relative to those away from home. In both the sample at risk of leaving and returning home, women are more likely to be enrolled in school than men. Between the 2004 and 2008 panels I notice a slight increase in the percentage of young adults enrolled in school at the time they are first observed in the sample. The increase in enrollment is more pronounced among both men and women who were initially unenrolled.[33] This finding provides suggestive evidence that young people adapt to poor economic conditions by changing their enrollment behavior.

3.5. EMPIRICAL METHODS

3.5.1. MODEL SPECIFICATION

3.5.1.1. STATIC MODEL SPECIFICATION

My aim is to estimate the effect of changing labor market conditions and cost of housing on living arrangements of young people while controlling for the effect of various factors that may influence young peoples' preference for living at home. The regression estimates are estimated for individuals aged 20 to 29, who are not enrolled in school and are single at the time they first enter the sample.[34] I estimate static models at

[33] Among young adults previously unenrolled in school, I observe a fall in the percentage who re-enroll between 1996 and 2001 panels.

[34] The sample includes only those young individuals who are primary SIPP sample members, that is they were present at the time of the first interview wave in the SIPP panel, because only primary sample members are followed.

different levels of aggregation. To consider the importance of this choice to the results, in reporting the findings from regression models the initial set of estimates pertain to the aggregated analysis. My preferred specification, however, makes use of the large individual level data available in the SIPP to estimate disaggregated models. My preferred specification includes an individual specific effect; the linear fixed-effect specification can be written:

$$\begin{aligned} Y_{ist} = \ & \theta_1\, Unemployment\ rate\ for\ young\ adult_{st} \\ & + \theta_2\, Unemployment\ rate\ for\ parents_{st} \\ & + \theta_3 \ln(Annual\ median\ fair\ market\ rent_{st}) \\ & + \theta_4 \ln(Minimum\ wage_{st}) + \theta_5\, X_{ist} + \eta_a + \lambda_i + \tau_t + \psi_s\, t + u_{ist} \end{aligned}$$

where *i* indexes individual and *t* indexes time. Y_{ist} is a dummy variable equal to one for a young person living with their parents', and zero otherwise, and X_{ist} is the state level average per capita personal income.

The theoretical framework outlined earlier signifies the role of three factors in determining the effect of economic conditions on a young adult's living arrangements: young adult's income, parents' income, and cost of housing. Previous studies have approximated labor market conditions of young adults using state or region level employment-population ratios for older age groups (as in Matsudaira (2015) and Card and Lemieux (2000)), or the employment-population ratios of directly affected young adults whose living arrangement behavior is being studied (as in Kaplan (2012)). Both these approaches are unable to separately account for the influence of business cycles on parents' resources. I attempt to address this concern by utilizing two variables to separately identify the effect of exogenous changes in labor market conditions on young adults and their parents.[35]

Card and Lemieux (2000) document the varying trends in labor market opportunities faced by young adults based on their gender and age, and the increasing economic hardship faced by less-skilled young adults. Their work emphasizes the different dimensions along which economic opportunities of young people vary. Unlike previous researchers, I attempt to incorporate these different sources of variations into my key measure of labor market conditions faced by young adults. Using CPS data, I estimate average age-group, education, and gender specific employment-population ratio and unemployment rate for young adults in each state over time. Such rich sources of variation allow me to more closely approximate local labor market conditions experienced by subgroups of individuals within the same state. I also estimate state specific average unemployment rates for prime-age adults (35 to 60-year-olds) which I interpret as representing labor market conditions for parents of young adults. Details regarding the estimation of the labor market measures for young adults and their parents are described in Appendix C.1. With these two variables I am able to separately account for the effect of business cycles on the contemporaneous resources of both young adults and their parents. In the state panel analysis, I use aggregated versions of these two variables; details are described in Appendix C.2.

[35] Another possible way to address this concern is to use variation in industry-specific unemployment rates that may result from a recession as suggested by Bartik (2013), but this approach can be applied only for college educated young adults.

Previous researchers assessed the effect of housing costs by using median house prices and/or rental data. Yelowitz (2007) and Lee and Painter (2013) include both measures, while Matsudaira (2015), Haurin et al. (1993), and Whittington and Peters (1996) include only a measure for rental costs. Lee and Painter (2013) note the absence of any guidance in the previous literature regarding whether economic conditions primarily affect young adults' demand for owner occupied or rental housing. They reason that as younger households are more likely to rent before owning their house, they expect a stronger effect on living arrangements from the cost of rental housing. Based on the same reasoning, I control for the effect of the cost of housing by including a measure of rental costs faced by young adults. In particular, I use the annual state level fair market rent for a two-bedroom apartment as my measure for rental costs.[36] The annual fair market rents are published by the U.S. Department of Housing and Urban Development (HUD); details are provided in Appendix C.3.

Card and Lemieux (2000) also note the possibility of utilizing minimum wages to instrument for young peoples' average wage across states, though they do not pursue this strategy themselves. A higher minimum wage can lead to disemployment effects among young adults, and may thus induce them to live with their parents.[37] It is also possible, however, that higher minimum wages increase the resources available to young adults, (especially if the disemployment effect is negligible), and these individuals will be less

[36] Other researchers have also relied on the fair market rents as a measure of cost of housing. For instance, in his empirical analysis Yelowitz (2007) approximates rental costs by using MSA level fair market rents, and Lee and Painter (2013) use two sources of rental data, that derived from census and annual ACS data and MSA level fair market rents.
[37] There is a voluminous literature assessing the disemployment effects of minimum wages; see Brown et al. (1982) and Neumark and Wascher (2007) for a review.

likely to reside with parents. In my work I control for the possible effect of minimum wages on living arrangements, as no other researchers have used this measure before.[38] A concern in using minimum wages is the inclusion of young adults between ages 25 and 29; minimum wages may be more relevant for 20 to 24-year-olds than for those above 25. I address this concern by estimating separate models by age group.

I also include a few additional controls. It is possible that state level rental costs are correlated with state level average income, so I include a measure for state level average per capita personal income provided by the Bureau of Economic Analysis (BEA). Also, I control for the effect of age of young adults by including age dummies. The main advantage of using individual level data is my ability to control for unobserved time persistent attributes and preferences specific to young people and their parents so as to effectively account for the influence these factors exert on young peoples' living arrangements. I also include time dummies to capture the effect of unobserved changes in business cycle, rental market conditions or societal attitudes that are common to all states. I allow for the possibility of heterogeneous trends across states in living arrangements of young people that are not correlated with changes in labor market, rental costs, and minimum wages by using state specific linear trends.[39] Angrist and Pischke (2015)

[38] The state level minimum wages are taken from the data compiled by the Urban Institute and Brookings Institution's Tax Policy Center and available at (http://www.taxpolicycenter.org/statistics/state-minimum-wage-rates-1983-2014). At any given point in time the minimum wage in the state refers to the effective minimum wage in the state on January 1st of that year.

[39] Researchers studying the employment effects of minimum wages have debated the specification of controls for spatial heterogeneity. Neumark et al. (2014) question the inclusion of linear state specific trends in analyzing sample periods with a recession at endpoints; they find linear state specific trends to be too restrictive in capturing the variation in employment across states that is brought about by recessions and suggest the inclusion of higher order polynomial trends. Addressing concerns raised by Neumark et al., Addison et al. (2015) evaluate the sensitivity of their own prior work to allowing for higher order polynomial time trends and

observe that after controlling for state specific linear trends in employment and/or living arrangements the identification of the effect of economic conditions comes from stark changes in living arrangements relative to the smoothly evolving trends that differ by states.

3.5.1.2. DYNAMIC MODEL SPECIFICATION

As described earlier I also separately analyze young adults' decisions to return and leave home. I carry out both these analyses using a hazard model framework. Here, I focus on describing the specification for the hazard of leaving home because I am able to include parent specific demographic and socioeconomic characteristics only for individuals who are observed living at their parents' home. The specification of the return hazard model is analogous to that of the leaving home hazard without parent specific demographic and socioeconomic characteristics. Any young person of age 20 to 29 who is residing in parents' household at the time when he or she first enters the sample is at risk of leaving home. The discrete-time home leaving hazard at any age is the probability of leaving at that age conditional on not having already left.[40] The hazard of leaving

---

extending their sample period. They find small employment effects of minimum wages, and considerable sensitivity of estimated minimum wage effects in response to the specification of polynomial trends and the sample period used for analysis. Since, my sample period does not include recessions at the end points, I present my initial set of regression estimates with linear state specific trends.

[40] An individual either continues living with parents or leaves through any of the possible routes out of the home. Different destinations considered in the previous literature: leaving home to – live in small or large groups, marriage, independent living alone or with a cohabiting partner or roommates, independent living as a homeowner, independent living as a renter. I have performed descriptive analysis of the various routes out of the parental home (in results that I do not show) I find that the majority of moves out of the home are not through marriage but through the "other" route for which I do not have enough information in my data to categorize further. Moreover, there is not much change in the pattern for these two routes over the two decades I examine, so I do not distinguish between the various paths taken out of the home.

home at a particular age depends on various explanatory variables that make living at home more or less appealing than staying away from home, I discuss these variables in detail below. The discrete-time hazard of leaving home h_{it} for an individual *i* at any age *t* (in the interval (t-1, t])

$$h_{it} = \Pr(Leave\ Home_{it} = 1 \mid Leave\ Home_{it-1} = 0\ \ x_{it}, y_i)$$
$$= \Pr(T_i \in (t-1, t] \mid T_i > t-1, x_{it}, y_i)$$

where *T* is the random variable denoting the age at which an individual leaves home, and $x_{it},\ y_i$ are a set of time varying and time constant explanatory variables respectively that affect the hazard rate. Following Allison (1982), I use a logit model to specify the dependence between the leaving home hazard and the explanatory variables.

$$\log\left(\frac{h_{it}}{(1-h_{it})}\right) = \alpha_t + \beta' x_{it} + \gamma' y_i$$

α_t is a set of constants (age dummies) denoting the non-parametric baseline hazard; this specification allows the hazard to vary by age while holding other explanatory variables constant. The initial specification is:

$$\log\left(\frac{h_{it}}{(1-h_{it})}\right)$$

$$= \alpha_t + \theta_1\, Unemployment\ rate\ for\ young\ adult_{st}$$

$$+ \theta_2\, Unemployment\ rate\ for\ parents_{st}$$

$$+ \theta_3 \ln(Annual\ median\ fair\ market\ rent_{st}) + \theta_4 \ln(Minimum\ wage_{st}) + \theta_5\, X_{ist}$$

$$+ \theta_6\, Young\ adult's\ demographic\ and\ socioeconomic\ controls$$

$$+ \theta_7\, Parents'demographic\ and\ socioeconomic\ controls$$

$$+ \delta_s + \tau_t + \psi_s\, t + u_{ist}$$

where X_{ist} controls for the effect of average per capital personal income. I also include variables that control for the effect of young person's gender, race, and educational attainment on the hazard of leaving home, but am unable to control for unobserved tastes and preferences of the young person that may be correlated with other variables and which may make him or her more likely to live with parents.

In the sample of individuals initially observed to be living at their parents' home, I am able to control for some parent specific attributes that may affect the young person's hazard of leaving home. Previous research by sociologists and demographers indicates the importance of many parent level characteristics that can affect young peoples' home leaving behavior. From that list I am able to control for the following: parents' level of education[41], whether both natural parents are still married and at home, parents' age,

[41] I use years of education completed by a parent if one parent is present, and average years of education if both parents are present. The SIPP data, like the CPS, provides categorical information on highest degree received or grade completed and not the actual years of education completed. Jaegar (1997) proposes three different ways to linearize responses from the categorical measure of education provided in the CPS. I use the average years of education imputed values provided in his third "assigned" measure which addresses the overestimation of average imputed values relative to observed values in his two other proposed measures.

whether parents own their home, and number of household members in the parental home. Parents' education can be used to control for permanent components of parental income, or quality of education received by the young adult. Aquilino (1990) emphasizes the importance of family structure, so I include a categorical variables indicating the presence of both natural parents at home. This may capture the level of stability at home, or the amount of supervision in a two parent household. Since, parents are less likely to face borrowing constraints they may be less responsive to changes in their current income; following Ermisch (1999) I control for parents' housing tenure as a rough measure of their wealth. Moreover, the benefit of living at home may depend on the amount of available space, which may in turn be affected by the number of household members, so, I include a control for the number of household members as well. However, Haurin et al. (1993) note that interpretation of some of these variables is not clear, as they can pick up other unobserved effects that are correlated with the variable and the young adult's decision to live at the parents' home.

3.5.2. SUMMARY STATISTICS

Table 3.5 presents, summary statistics from the data used. In the sample to which I apply static models, forty percent of young adults are observed living with parents during 1996-2013. A larger fraction of sample members are men, white, and those with a high school degree or less.[42] The age-group, education, and gender specific unemployment rate of young adults is twice as high as their parents and displays a much broader range over which it varies. I also summarize the aggregated version of the unemployment rates

[42] The estimation sample includes individuals who are not currently enrolled in school and single at the time they first enter the SIPP sample.

for young adults and their parents that is used in the aggregate level analysis; these aggregated measures display considerably less variation. For the dynamic model of leaving home, I show summary information for parental characteristics. Average education of both parents is about eleven years, young adults living with parents are equally likely to be at home if both biological parents are married or not, and about seventy-five percent of parents own their home. The hazard of leaving home is about four times as high as that of returning home; the lower likelihood of young adults returning home was also evident in the descriptive analysis.

3.5.3. METHOD OF ESTIMATION

3.5.3.1. STATIC MODEL

I estimate the static model using panel data fixed-effects estimation method because it allows me to control for time invariant unobserved individual specific tastes for work that may be correlated with other explanatory variables. To check the sensitivity of the findings to the functional form specifying the relationship between the dependent and the explanatory variables, I also estimate conditional logit model of Chamberlain (1980). An advantage of the linear fixed-effects model relative to the conditional logit is that it provides reasonable estimates of the average partial effects. Wooldridge (2010) notes that we cannot estimate the average partial effects in the conditional logit model without specifying a distribution for unobserved tastes.[43]

[43] Wooldridge (2010) observes that for the linear fixed effects model we need to make inference robust to heteroscedasticity and serial correlation. Therefore, I cluster the standard errors by state.

3.5.3.2. DYNAMIC MODEL

I estimate the discrete-time hazard model for leaving home using maximum likelihood. Following Jenkins (1995) I arrange the data for all individuals initially observed to be living at home and at risk of leaving home into person wave observations representing the time at risk. I assume a flexible non-parametric specification for the baseline hazard, and treat the time varying variables as constant within each discrete-time interval.

Suppressing the conditioning on covariates, the log likelihood function as derived by Jenkins for a stock sample of *n* individuals is as follows:

$$\log L = \sum_{i=1}^{n} \sum_{t=\tau}^{\tau+s_i} y_{it} \,.\log\left(\frac{h_{it}}{(1-h_{it})}\right) + \sum_{i=1}^{n} \sum_{t=\tau}^{\tau+s_i} \log(1-h_{it})$$

where τ represents the age at which an individual i was selected into the sample for leaving home and first became at risk of leaving.[44] s_i represents number of waves in the sample for each individual. For individuals who are observed leaving the parental home in the survey (uncensored), $\tau + s_i$ represents the SIPP wave at the time the individual is first observed living apart from parents. Those individuals who have not left home at the time of the last interview wave of the SIPP survey, or who have not left home at the time they attrit from the survey are viewed as censored, because their age at the time of leaving home is not known. For censored individuals, $\tau + s_i$ represents their last wave of

[44] τ may differ by individuals for two reasons. First, only individuals who are 20 to 29 years of age are included in the estimation sample, so we will observe a different τ for younger individuals as they attain age 20 at different points and enter the sample. Second, individuals falling in this age range may enter the actual SIPP survey sample at different waves.

observation in the survey. y_{it} represents the living at home status of the individual; the last observation of an individual who has left the parental home in the sample (uncensored) is set to $y_{it} = 1$ (if $t = \tau + s_i$). All other observations (other than the last observation) of uncensored individuals have $y_{it} = 0$, and it is also set to zero for all observations of censored individuals.

3.6. ESTIMATION RESULTS

With my regression analysis, I aim to explore four main concerns raised by previous studies. First, unlike previous researchers I assess the influence of business cycles on young adults and their parents by including two separate measures of local labor market conditions faced by each of them. Second, Hill and Holzer (2007) and Kaplan (2012) emphasize the need to control for underlying behaviors and attributes specific to young adults that influence their decision regarding whether to live with their parents; this concern is particularly important in static models where a great deal of the variation in living arrangements originates from cross sectional differences in preferences. To address this concern, in the static analysis I estimate separate models at varying levels of aggregation. This allows me to examine the sensitivity of the findings to different degrees of variation in labor market conditions and inclusion of personal characteristics.[45] Third, as stressed by Kaplan (2012) changes in labor market conditions are more likely to affect movement of young adults into and out of the parental home, hence, a dynamic analysis may yield stronger evidence for the effect of economic

[45] A similar approach was taken by Navratil and Doyle (1977). Their analysis, however, was performed in the context of assessing the relative importance of observable personal characteristics in understanding migration behavior.

conditions on living arrangements. So, I estimate hazard models for young adults' likelihood of leaving or returning home. Finally, given the mixed evidence in previous studies regarding the influence of economic conditions on living arrangements, in both the static and dynamic models I examine whether local labor market conditions of young adults exerted a stronger influence on living arrangements during the period 2008-13, when the economy experienced a severe recession and a slow subsequent recovery.

3.6.1. STATIC MODELS

3.6.1.1. STATE PANEL ANALYSIS

I begin by describing the findings from the aggregate state panel analysis presented in Table 3.6; this set of estimates pertain to the aggregated analysis. I report these estimates to relate my work to earlier research, particularly that of Card and Lemieux (2000) and Kaplan (2012) and to consider the effect that the level of aggregation has on the findings.[46] The reported coefficients are estimates from linear probability models. The dependent variable is the fraction of young adults observed staying at their parents' home in each state over time. [47] Like Card and Lemieux (2000), the first column includes a control for the prime-age adults unemployment rate as a proxy for local labor market conditions, like Kaplan (2012) the second column controls for the aggregate state level unemployment rate for young adults aged 20-29, the next two columns contain findings from models which use both the aggregate state level unemployment rates for young adults aged 20-29 and prime-age adults. The last three columns use the employment-

[46] Here I am referring to Kaplan's work with the CPS data.

[47] I am unable to use data for all 50 U.S. states in the regression analysis, because in panels 1996 and 2001 of the SIPP five smaller states are not separately identified due to confidentiality reasons.

population ratio of young adults aged 20-29 instead. The second and the third specifications allow me to assess the importance of controlling separately for young and prime-age adults labor market conditions. I include an interaction term in the fourth specification which allows the effect of the unemployment rate of young adults to vary during the recent recession. In all seven specifications of the model, I find an insignificant effect of local labor market conditions of young adults on their living arrangements. I also fail to find any evidence regarding the influence of local labor market conditions faced by parents, rental costs, or minimum wages on living arrangements.

The signs of the coefficients for young adults' local labor market measures are in line with predictions of the theoretical model, but the magnitude of the effect is quite small. I find young adults are more likely to live with their parents in states where they experience an increase in the unemployment rate or a decrease in the employment-population ratio. At a given point in time 40 percent of young adults are observed living at home across different states, so the estimate from the third column indicates a one percentage point increase in the unemployment rate of young adults would increase their probability of living with parents by .11 percentage points. The point estimates, however, are consistent with unemployment rates exerting a weaker influence on living arrangements during the recent recession relative to the earlier years. Although small in magnitude, the point estimates suggest that parents' unemployment rates exert a relatively stronger influence in raising the probability of young adults' living at home. Contrary to theoretical predictions, I estimate that a 10 percent increase in fair market rent reduces the likelihood of staying home by 0.3 percentage points, a small effect.[48] The magnitude of the estimate for minimum wages is stronger than that of any other economic factor, a 10 percent increase in minimum wages raises the probability of living with parents by 1.66 percentage points. But as noted above, these effects are statistically insignificant.

3.6.1.2. INDIVIDUAL LEVEL ANALYSIS

Table 3.7 reports the findings from using individual level data; in this set of findings I do not control for individual effects. There are two differences between Tables 3.6 and 3.7; first, is the use of individual-level data, and, second, is the use of group-specific unemployment rate and employment-population ratio.[49] I find that the group-specific unemployment rate measure of young adults now is statistically significant, but in an unexpected direction. An increase in the unemployment rate for young adults lowers their probability of living with parents, but the magnitude of the effect is

[48] Ermisch (1999) notes that the effect of housing costs on young adults is sensitive to the price elasticity of demand for housing by parents. As I show in the descriptive statistics, in the sample for leaving home, approximately 75 percent of parents are homeowners, they are less likely to respond toc changes in rental markets.

[49] The estimates presented in Table 3.7 differ from those of Table 3.6 in two ways, one they are based on individual level data and two, the group-specific unemployment rate and employment-population ratio incorporate greater variation than the aggregated unemployment and employment measures used in Table 3.6. To compare my results to earlier studies that do not make use of the greater variation in unemployment and employment measures, I estimate an intermediate set of individual-level models in which I use the aggregate unemployment rate and employment - population ratio for young adults age 20 to 29 (the same as used in Table 3.6). These results are presented in Appendix Table C.1, I observe that relative to Table 3.6 the estimate for the unemployment rate measure for young adults 20 to 29 declines in magnitude, it now has a negative sign indicating a higher unemployment rate reduces young adults' probability of living with their parents'. I observe the same effect in Table 3.7, but now the estimate for group-specific unemployment rate is statistically significant. Taken together the findings from Table 3.7 and Appendix Table C.1 suggest that the lack of evidence of a response of living arrangements to changes in the labor market conditions of young people is not being driven by my use of individual data or greater variation in the measures for local labor market conditions.

negligible. The findings suggest that the higher group-specific unemployment rate reduce the likelihood of young adults living with their parents even during the recent recession, but the effect is not statistically significant.[50] All the other estimated effects are statistically insignificant. In the individual level analysis, I also observe that the magnitude of the estimated coefficients declines such that none of the estimates exerts a meaningful effect on living arrangements.

I now make use of the longitudinal nature of the SIPP to control for persistent unobserved individual and parent specific variables that may affect young adults' preference for living with their parents. The findings are reported in Tables 3.8 A and B; Table 3.8 A shows the estimates from the linear probability model with individual fixed effects, and Table 3.8 B shows the estimates from the conditional logit model. I find no evidence that changes in the group-specific employment-population ratio or the unemployment rate influence young adults' living arrangements. These findings are not sensitive to the choice of specification. I do find that poor labor market conditions faced by parents have a positive impact on young adults living arrangements, but the strength of this evidence is sensitive to the functional form as it is only strong in the conditional logit models. I also find a higher probability that young people live with their parents in states with higher minimum wages. The effect is statistically significant, but small in

[50] I also estimated individual-level fixed effect models that separate the recent recession period into two subperiods (2007-09 and 2010-13) and found stronger evidence of a negative effect of labor market conditions of young people on their living arrangements for the earlier period (faced with poor labor market conditions young people are less likely to live with their parents during the 2007-09 period).

magnitude—a 10 percent increase in the minimum wage would increase the probability of living with parents by only 0.4 percentage points.

Unlike the earlier study by Card and Lemieux (2000) and recent studies by Kaplan (2012), Lee and Painter (2013), and Matsudaira (2015), I find a negligible influence of young people's labor market conditions on their living arrangements. This may be due to three reasons. First, I differ from these studies in controlling for persistent unobserved individual specific effects, as a result the identification of the effect of changes in the labor markets of young adults in my models comes from individuals who change their living arrangement status. Second, I differ from previous studies in using greater variation in the key measures for local labor market conditions and in controlling for the effect of business cycles on parents. Third, I also control for the effect of both the state level rents and the minimum wages. The results from Tables 3.6-3.8 and Appendix Table C.1, however, indicate that none of these factors are driving the results. I am unable to find any effect of local labor market conditions on young adults' living arrangements regardless of the level of aggregation, the degree of variation in the unemployment rates or employment-population ratios, the presence of controls for local labor market conditions faced by parents, the presence of individual effects, or controls for rental market conditions and minimum wages.[51] This set of results suggests that the findings from the high-frequency SIPP data are not consistent with those of the studies

[51] In results that I do not report here, I have estimated models excluding the fair market rents and minimum wages. Excluding these variables from the analysis changes the magnitude of the estimated coefficients for the unemployment rate and employment-population ratio of young adults slightly, but the effect is statistically insignificant.

listed above.[52] The results from my work instead support the findings of Hill and Holzer (2007) who also observe that declining wages and employment opportunities of young people over a twenty year period explain very little of their increased tendency to live with their parents.

In Tables 3.9A-B and 3.10A-B , I explore the possibility that subgroups of individuals by age or gender may be differently affected.[53] The overall evidence from these sets of tables is that changes in local labor market conditions of young people do not exert a strong influence on their living arrangements. When I analyze the living arrangements by age-group and gender separately, I do observe some additional but unintuitive results. First, I find weak evidence that an increase in the employment-population ratio of young adults 20-24 raises their probability of living with their parents during the recent recession. Second, there is weak evidence that young adults 25 to 29 years of age are more responsive to their parents' measure of the unemployment rate, as it exerts a stronger influence on their likelihood of living with their parents than any other economic factor. Third, faced with a higher rent, adults aged 25 to 29 with at most a high school degree are relatively less likely to live with their parents than those with more than a high school degree. The effect is statistically significant. I also find strong evidence that higher rents reduce the likelihood of women living with their parents. Fourth, the minimum wage effect is larger and statistically significant mostly for men and young

[52] To further probe whether the lack of a response in living arrangements to changes in the labor market conditions could be due to the oversampling of low income individuals in the SIPP, I estimated the individual fixed effects models using weights. The results from the weighted analysis are similar to those reported in Table 3.8A (unweighted analysis).

[53] I also, estimated separate models by education where I fail to find any evidence of a differential response in living arrangements to labor market conditions.

adults 25-29. For men, a 10 percent increase in the minimum wage raises the probability of living with parents by 1.2 percentage points. The minimum wage literature has concentrated on studying the disemployment effects of minimum wages on teenagers and adults age 20-24 as a relatively larger fraction of individuals in these groups are likely to be affected. My findings, then, are puzzling in that respect.

To focus on the influence of labor markets on living arrangements during the most recent recession and recovery in Table 3.11, I restrict my sample to the years 2008-13. The data for these years come from the SIPP panel 2008, which started out with a much larger sample size relative to the previous panels and covered an extended period of five years (the largest and the longest SIPP panel, since the inception of the survey); it contains about one third of my entire sample. I find that measures of labor market conditions faced by young adults continue to be statistically insignificant, of unexpected sign, and quite small in magnitude. I also observe that although the rent variable is statistically insignificant, it now has a positive sign.

In Tables 3.12 A and B, I check the robustness of my findings to two concerns that may possibly influence the previous estimates. First, as I explain in Appendix C.3 the fair market rent data provided by the HUD contains estimates for annual median rent beginning from the year 2001; for the years 1996-2000 only rent estimates at the fortieth percentile are provided which I converted to the fiftieth percentile. It is possible that these rent adjustments do not accurately measure the same rents in the 1996-2000 period as those provided in the rest of the series. Second, it is also possible that the welfare reform of 1996 may have affected the living arrangements of young unmarried women.[54] To assess the sensitivity of my findings to these two concerns, I exclude data from the 1996 panel and restrict the data to years 2001 to 2013. As seen from Tables 3.12 A and B, I find no evidence that changes in local labor markets for young adults affect their living arrangements, even when the sample is restricted to cover the period after 2001.[55]

3.6.2. DYNAMIC MODELS

3.6.2.1. LEAVING HOME

In my static models I was unable to uncover a response in the living arrangements of young people to changes in their labor market conditions. I now rely on the high frequency and longitudinal nature of the SIPP data to estimate dynamic models of leaving from and returning to parental home to examine whether changes in young peoples' labor market conditions around the time of their move shed more light on the determinants of their living arrangements. Tables 3.13 A-B report the findings from the estimation of discrete time hazard models for leaving home; the top part of the table contains the coefficient estimates, and the bottom contains the marginal effects.[56] The smaller number

[54] In her analysis of doubling up behavior of young adults during the recent recession, Wiemers (2014) uses SIPP data covering a period after 1998 when the welfare reform had been fully implemented.

[55] Interestingly, the fair market rents now exert a negligible influence in the linear model specifications, and a positive influence in the estimates from the conditional logit models. These finding together with those from Tables 3.9-3.11, provide some suggestive evidence that can explain the unintuitive results for the effect of higher rental costs on women and less educated 25 to 29-year-olds. The suggestion is that the negative effect of fair market rents may be driven by the effect of welfare reforms on women, particularly women age 25 to 29 with at most a high school degree. The direction of the effect is not from rents to living arrangements, but from a decline in demand for rental housing driven by young women that may have led to lower rents in states where more women chose to live at home. For more details regarding the 1996 welfare reform see Appendix C.4.

[56] I estimate the marginal effects using the Stata software package; the marginal effects of continuous variable are estimated using the Margins Dydx option. The marginal effect for the

of observations relative to the static analysis are for two reasons: one, they reflect that the estimation sample consists of only individuals who are residing with parents at the time they are first observed and are at risk of leaving home, and two, unlike the static analysis, all observations for an individuals after he or she has left home are excluded. The dependent variable is a categorical variable that is set to zero in periods in which the young adult has not left home, and one in the first wave when a person has left home. I find no effect of changes in young adults' labor market conditions on their hazard for leaving home, even during the years 2008-13. I test for the effect of employment-population ratio in 2008-2013 and find that it is statistically insignificant. The marginal effects for the influence of the young adult unemployment rate and employment-population ratio are almost identical in magnitude, they both represent a negligible influence of labor market conditions relative to the mean hazard of leaving home, which is 2 percent.[57] I do not find any evidence of an effect of the other economic determinants on the hazard for leaving home.

group-specific unemployment rate gives the average effect of a unit increase in the unemployment rate for young adults on their hazard of leaving home while holding all other variables constant. The marginal effect of the 2008-13 dummy interaction term gives the average effect of a one-unit increase in the group-specific unemployment rate of young adults on their living arrangements in the 2008-13 period relative to that in previous periods.

[57] Two related concerns cloud the interpretation of the findings from the estimation of the home leaving hazard. First, the pool of individuals at risk of leaving home includes those who have never left home, and those who have some experience living away from home (for reasons other than attending college), but returned home prior to their first interview at their time of entry in the SIPP sample. I am unable to distinguish between these two types of individuals. As a result, the findings from the home leaving hazard only conveys information regarding the change in the rate at which young people, who are observed residing with their parents for some length of time (regardless of their past experience of independent living), leave home as economic conditions change; they do not imply a change in the timing of first exit from parents' home to independent living. Second, Greenwood (1997) notes that studies of migration distinguish between primary (those migrating/moving for the first time) and repeat migrants, because the two kinds of migrants may differ in their propensity to move, and may, thus, be affected differently by changes in their circumstances. I am unable to account for such differences.

I further explore how the effect of labor market conditions on the hazard of leaving home varies by gender and age-group; the findings are reported in Tables 3.14 and 3.15. Here, the basic finding is that of a lack of evidence regarding the influence of labor market conditions of young adults on their hazard of leaving home. There is also an unintuitive result in one specification for the effect of the employment-population ratio for both 20 to 24-year-olds and men.[58] In the disaggregated analysis by age, I find evidence that accords with intuition in that higher rents lower the home leaving hazard for 20 to 24-year-olds.

3.6.2.2. RETURNING HOME

To illuminate the findings from the dynamic models of home leaving, I supplement them with an analysis of the effect of labor market conditions on the hazard of returning home, while keeping in mind the relatively fewer returns observed in my sample. Tables 3.16 A and B report the findings from estimation of hazard models for returning home. Here again, I am unable to uncover any evidence linking changes in the labor markets for young people to their hazard of returning home. All the estimated coefficients are statistically insignificant and small in magnitude.

In Tables 3.17 and 3.18, I consider the hazard for returning home by subgroups. In an unexpected finding I observe that higher rents raise the likelihood of more educated 20 to 24-year-olds and women to returning home relative to those less educated; the effect is marginally significant. I also find some evidence that higher minimum wages

[58] For men, I also test for the effect of employment-population ratio in 2008-2013 and find that it is statistically insignificant.

lower the hazard of returning home for 20 to 24-year-olds. The stronger effect of minimum wages on 20 to 24-year-olds is in line with the fact that the younger age-group is more likely to be directly affected by the minimum wages.

3.6.3. MIGRATION

In the analysis thus far, I have restricted my attention to individuals who do not change their state of residence; it is possible that I am unable to uncover a response in living arrangements of young people to changes in their labor market conditions because poor labor markets influence a young person's decision to migrate across states in search of better opportunities. To explore this possibility, I estimate whether a young person's likelihood of changing their state of residence since the last interview is responsive to changes in the labor markets in their previous state; the estimates are presented in Table 3.19. I do not find any effect of local labor market conditions on a young person's likelihood of changing their state of residence. I also re-estimate the likelihood of a young adult to leave and return home since last interview while including young people who change their state of residence in the sample at risk, the results are reported in Tables 3.20 and 3.21. Similar to the estimates presented in Table 3.13 and 3.16 where the sample at risk does not include young people who change their state of residence, I find that poor labor market conditions do not affect the likelihood of leaving or returning home. I observe that higher rents raise the likelihood of young people to return home, the effect is statistically significant. I also re-estimated static models of living arrangements; the estimates are similar to those reported in Table 3.8 (for the linear fixed effects model). To summarize, in both the dynamic and static models of living arrangements the estimated influence of labor markets on living arrangements is not affected by the inclusion of individuals who change their state of residence.

3.7. SUMMARY

Recent studies assessing the importance of labor market changes in determining the living arrangements of young adults have found strong evidence indicating young adults in depressed labor markets are more likely to live with their parents. These studies have employed a variety of methods to uncover the link between labor markets and living arrangements. Studies which used pooled cross-sectional data relied on exogenous variation in local labor market conditions to identify their effect on living arrangements, but these studies could not control for the effect of personal or parental characteristics and could not disentangle the effect of changes in the labor markets on the return or leave behavior of young adults. Studies which used longitudinal data, on the other hand, used endogenous measures such as personal employment status to assess the effect of the labor markets on the hazard of returning or leaving home. I use the high-frequency longitudinal Survey of Income Program and Participation data to study changes in young adults living arrangements in response to changes in their labor market conditions.

There are three main differences between my work and that of previous researchers. First, my key measure for local labor market conditions faced by young adults are the age-group, education, and gender specific unemployment rate or the employment-population ratio of young adults. This measure encompasses much greater variation than those used by other researchers, thus allowing me to more accurately capture labor market conditions experiences of young people. Second, unlike previous researchers I am able to separately control for the effect of business cycles on both young adults and their parents as living arrangements can be influenced because of a change in resources of either young adults or their parents. Third, I complement the work of previous researchers by exploring the extent to which findings from static models are sensitive to the level of aggregation and the inclusion of controls for unobserved individual specific tastes or preferences of young adults for living with their parents. I supplement the findings from the static models by considering the influence of changes in labor market conditions on the rate of leaving and returning home. But, I find that none of these differences are really important to the results. There is no robust influence of changes in the labor markets of young individuals on their living arrangements. My results indicate that the findings from the high-frequency SIPP data are not consistent with those provided by other studies. There is some evidence that higher minimum wages lower the hazard of returning home among young adults ages 20 to 24, and that higher rents lower the hazard of leaving home among young adults ages 20 to 24.

As noted by Hill and Holzer (2007), one possible reason for the inability to uncover an effect for labor market conditions of young adults on their living arrangements is that the measures employed (in my study and theirs's[59]) might be capturing the effect of transitory changes in labor market opportunities of young adults, while it is the more permanent changes that might be affecting living arrangements of young adults. Danziger and Ratner (2010) document how changes since the mid-1970s have affected the extent to which young adults achieve financial independence. In particular, they note the decline in the labor market prospects of less-educated young men

[59] Although their measures for labor market conditions are potentially endogenous.

who now find it more difficult to earn enough to support a family than they did during the mid-1970s. They note a series of labor market changes—computerization and other kinds of labor-saving technology that lowered demand for less-educated workers, decline in the real minimum wage, decline in importance of unions, and increased globalization—that created hardships for workers, particularly men with no more than a high school degree. These changes have resulted in young men with no more than a high school degree having lower employment rates, lower real wages, and less access to employer-provided health insurance and pensions than did similar workers in the mid-1970s. Based on these changes in labor markets for young people, Danziger and Ratner argue that without a sufficient and steady income, a young adult today might be less likely to live independently than in earlier years. But, Hill and Holzer (2007) contend that the observance of similar trends over time in living arrangements for young adults across education and gender groups casts some doubt on the hypothesis that these trends are primarily driven by changes in labor market opportunities, which as Danziger and Ratner document have diverged greatly across education and gender groups in the past three decades.

Table 3.1A Early Research (pre-2007 data)

	Data	Time-period covered (frequency)	Main outcome of interest	Method	Economic variables	Findings
Card and Lemieux (2000)	U.S. March Current Population Survey (CPS) Canada Census and Survey of	Data for years 1971,1981,1991, 1994	Proportion of youth in a given age group who are: a) employed b) living with parents[1] c) enrolled in school	Grouped linear probability model with region and time fixed effects	1) Region and gender specific employment population ratio of 25-45 year olds 2) Region and gender specific index of wages for 16-24 year olds	In areas with depressed local demand conditions, young men are more likely to live with parents and also, more likely to attend school.
Yelowitz (2007)	Census	Descriptive analysis 1970 - 2000 Regression analysis 1980 - 2000	Whether the young adult lives in one of the following arrangements: a) non-independent b) economic arrangement c) independent	Separate probit model for each living arrangement with MSA and time fixed effects	1) MSA level Housing Market Conditions: a) Median housing prices b) Monthly housing payments c) Median rent (FMR) 2) State level unemployment rate	Higher housing costs reduce independent living, but they explain little of the aggregate change in young people's living arrangements over time. Higher housing costs can explain about 15 percent of the total change in independent living arrangements between 1980 and 2000.
Hill and Holzer (2007)	National Longitudinal Survey of Youth (NLSY79 and 97)	1979 and 1997 cohorts of NLSY observed in years: 1984 and 2002	Whether the young adult lives in one of the following arrangements: a) at home with parents b) married c) cohabiting with a partner	Linear probability model with cohort fixed effects	1) Current hours worked by individual 2) Hourly wage of the individual 3) Hours worked by individual in high school.	Long term declines in labor market opportunities for less educated young wrokers cannot account for much of the growing tendency of young adults to live at home. Suggestive evidence that personal attitudes and behaviors that reflect independence and maturity are associated with later living arrangements, but not with changes over time.

[1] Card and Lemieux's definition of living with parents is broader than that used in most of the parent-child coresidence literature. For the U.S. data they define young adults' to be living with a parent if they live with one or both parents or a related family member (uncle, aunt, or grandparents).

Table 3.1B Early Research (pre-2007 data)

	Data	Time-period covered (frequency)	Main outcome of interest	Method	Economic variables	Findings
Static Analysis						
Kaplan (2012)	National Longitudinal Survey of Youth (NLSY97)	1998 - 2002 Monthly data created from annual interviews	Whether individual lives away from either parent	1) Logit 2) Conditional-logit	1) Monthly employment status of individual 2) Monthly earnings of the individual	Employed young adults are more likely to live away from parents, and among employed youth, those with higher earnings are more likely to live away. The fixed-effects estimates are not statistically significant, but
Dynamic Analysis						
Kaplan (2012)	National Longitudinal Survey of Youth (NLSY97)	1998 - 2002 Monthly data created from annual interviews	1) moving back with either parent (Return home) 2) moving out again (Leave home conditional on having returned)	Discrete-time proportional hazard model with random effects	1) Monthly employment status of individual 2) Monthly earnings of individual 3) Whether individual recently stopped working	A youth who has recently stopped working (transition from employment to nonemployment in previous three months) is more likely to return home than a similar youth who has not undergone such a transition. For employed youth, reductions in earnings also raise the probability of returning home.

Table 3.2A Recent Research (including post 2007-data)

	Data	Time-period covered (frequency)	Age-range (sample included)	Main outcome of interest	Method	Economic variables	Findings
Static Analysis							
Kaplan (2012)	Curent Population Survey (CPS)	1979 - 2010 (monthly and quarterly)	Men and women pooled analysis by age groups: 16 - 24 16 - 34	Parent-child coresidence rate in each state	State-panel fixed-effects	1) Age-group specific state Employment rate 2) Age-group specific state level average hours worked[1] 3) State level housing price index	A one percentage point increase in employment rate increases the coresidence rate by 0.15 to 0.18 of a percentage point.
Matsudaira (2015)	Census and American Community Survey (ACS)	1960 - 2011 (Decennial Census: 1960-2000 and annual ACS 2001-11)	3 age groups separate for men and women: 19 - 24 25 - 29 30 - 34	Whether individual lives with either parent	Linear probability model with state and time fixed-effects	1) State level employment population ratio of 35-44 year olds 2) Estimate of state level wages for 19-34 year olds 3) State level average rental costs	Areas with lower employment rates, lower wage rates, and higher housing costs are associated with larger increases in the fraction of young adults living at home. For men, economic factors alone can explain 70 to 80 percent of the total change in living arrangements from 1970 to 2011, and for women economic conditions can explain 50 to 60 percent of the total changes.

[1] Kaplan also controls for a set of two year lagged labor market variables to allow for the possibility of lagged effects on living arrangements.

Table 3.2B Recent Research (including post 2007-data)

	Data	Time-period covered (frequency)	Age-range (sample included)	Main outcome of interest	Method	Economic variables	Findings
Lee and Painter (2013)	Panel Study of Income Dynamics (PSID)	1975 - 2009 annual interviews 1975-97 biennial interviews 1999-2009	Men and women pooled and separate analysis as well: 18 - 34	a) individual continues to live with parent b) individual forms a separate household as a renter c) individual forms a separate household as a homeowner	Multinomial Logit (MNL)	1) Employment status of individual 2) Economic Conditions: a) Dummy for NBER recession year b) State unemployment rate c) State average real wages 3) Housing Market Conditions: a) State median gross rent b) State median house value	Find that recessions lowered household formation rates, which in turn, depressed housing demand, particularly in the rental sector. Household formation rates are reduced by up to 22 percentage points when young adults are not employed, and by up to 19 percentage points during a recession.
Dynamic Analysis							
Lee and Painter (2013)	Panel Study of Income Dynamics (PSID)	1975 - 2009 annual interviews 1975-97 biennial interviews 1999-2009	Men and women pooled and separate analysis as well: 18 - 34	Decision to a) form an independent household b) retun to parent's home	Proportional hazard model	1) Employment status of individual 2) Economic Conditions: a) Dummy for NBER recession year b) State unemployment rate c) State average real wages 3) Housing Market Conditions: a) State median gross rent b) State median house value	Higher real wages increase the likelihood of independent living. Recessions, and higher state level unemployment rate lower the likelihood of independent living. Higher rents delay household formation while housing prices have an insginificant effect.

Table 3.3 Survey of Income Program and Participation (SIPP) Panel Details

Panel	Date of First Interview	Date of Last Interview	Data Available for Reference Period	Number of Waves	Length of Panel (years)
1996	Apr. 1996	Mar. 2000	Dec. 1995 - Feb. 2000	12	4
2001	Feb. 2001	Jan. 2004	Oct. 2000 - Dec. 2003	9	3
2004	Feb. 2004	Jan. 2008	Oct. 2003 - Dec. 2007	12	4
2008	Sep. 2008	Aug. 2013	May 2008 - Nov. 2013	16	5

Note: Date of first interview refers to the date of interview for rotation group 1 which was the first group to be interviewed, while the date of last interview refers to the date of interview for rotation group 4 which was the last group to be interviewed. The data is available for four months preceding the date of each interview. Length of the panel refers to the number of years that members of a specific rotation group were interviewed.

Table 3.4 Young Men and Women's School Enrollment

		1996 Dec 95 - Feb 00	2001 Oct 00 - Dec 03	2004 Oct 03 - Dec 07	2008 May 08 - Nov 13
Sample of Young Adults Living with Parents and at Risk of Leaving Home[a]					
Men (%)					
	Enrolled in school at the time when first interviewed in sample[b]	37.96 (1135 / 2990)	38.47 (896 / 2329)	39 (1126 / 2887)	41.1 (1468 / 3572)
	Enrolled in school after first interview[c] (among those previously unenrolled)	25.55 (474 / 1855)	20.45 (293 / 1433)	19.31 (340 / 1761)	27.38 (576 / 2104)
Women (%)					
	Enrolled in school at the time when first interviewed in sample	49.44 (1198 / 2423)	49.87 (966 / 1937)	49.61 (1201 / 2421)	52.61 (1607 / 3081)
	Enrolled in school after first interview (among those previously unenrolled)	34.04 (417 / 1225)	29.35 (285 / 971)	25.9 (316 / 1220)	33.51 (494 / 1474)
Sample of Young Adults Living Away from Parents and at Risk of Returning Home					
Men (%)					
	Enrolled in school at the time when first interviewed in sample	16.71 (372 / 2226)	20.67 (491 / 2375)	18.46 (458 / 2481)	19.67 (512 / 2603)
	Enrolled in school after first interview (among those previously unenrolled)	13.75 (255 / 1854)	8.97 (169 / 1884)	8.5 (172 / 2023)	12.29 (257 /2091)
Women (%)					
	Enrolled in school at the time when first interviewed in sample	20.81 (521 / 2504)	24.5 (618 / 2522)	24.09 (693 / 2877)	25.03 (696 / 2781)
	Enrolled in school after first interview (among those previously unenrolled)	18.20 (361 / 1983)	14.23 (271 / 1904)	13.92 (304 / 2184)	17.94 (374 / 2085)

Note: For each category listed in a row, I provide the percentage of individuals who satisfy that category, and list the number of individuals who satisfy the condition relative to the total in the parentheses.
The initial sample consists of all individuals ages 20 to 29 who are single at the time when they first enter the SIPP sample. I exclude individuals who serve in the armed forces. In the SIPP, only individuals who are primary sample members (those present at the time of the first interview of a household) are followed, so the sample for this analysis is restricted to primary sample members.
I declare a person as living away from his parent's home in any four-month interval during which he does not live with either parent, and as living at his parents home if he is observed to live with either of his parents' in a household not headed by him.

[a] The sample at risk of leaving home consists of all single young adults living with parents at the time of their first interview in sample and the sample at risk of returning home consists of all single adults living away from parents at the time of their first interview in sample.

[b] Indicates the percentage of young adults enrolled in school at the time of their first interview.

[c] Indicates the percentage of young adults who enrolled anytime after they are first observed in sample (given that they were unenrolled at the time of first interview).

Table 3.5 Descriptive Statistics

		Mean (Standard deviation)	Minimum	Maximum
	Fraction living with parents	0.4		
	Hazard of leaving home at any given wave	0.02 (0.15)		
	Hazard of returning home at any given wave	0.005 (0.76)		
Static Model				
Personal characteristics of young adults	Age of young person (years)	24.09 (3.08)	20	29.92
	Women	0.45	0	1
	High school or less	0.58	0	1
	Black	0.17	0	1
	American Indian or Asian	0.07	0	1
Economic variables	Individual Level Analysis			
	Group-specific unemployment rate for young adults	0.1	0	0.56
	Group-specific employment-population ratio for young adults	0.72	0.19	1
	Unemployment rate for prime-age adults	0.05	0.01	0.14
	State Panel Analysis			
	Unemployment rate for ages 20 to 29	0.09	0.01	0.22
	Employment population-ratio for ages 20 to 29	0.72	0.52	0.86
	Minimum wage[a] ($)	7.19 (0.78)	5.79	9.28
	Monthly median rent ($)	944.77 (225.93)	582.46	1758.41
	Number of individuals	37,318		
	Number of observations	193,996		
Dynamic Model				
Parental characteristics affecting hazard of leaving home	Age of parent (years)[b]	50.71 (6.8)	35.08	85.33
	Education (years)	11.96 (3.06)	0	18
	Both natural parents married to each other	0.5	0	1
	Ownership of home	0.75	0	1
	Number of members in household	4.23 (1.7)	1.22	22
	Number of individuals	10,757		

[a] All dollar values are are adjusted for inflation using CPI-U and expressed in 2013 dollars.

[b] The mean and standard deviation for time varying variables are calculated by first averaging over all the observations available for each individual (person wave information).

Table 3.6 Estimates from State Panel Analysis of Living Arrangements

Linear Probability Model

Dependent Variable: Proportion of Young Adults Living with Parents

Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment rate for ages 20 to 29		0.160	0.113	0.184			
		(0.256)	(0.235)	(0.296)			
2008-2013 dummy * unemployment rate for ages 20 to 29				-0.145			
				(0.393)			
Employment-population ratio for ages 20 to 29					-0.237	-0.221	-0.220
					(0.142)	(0.144)	(0.191)
2008-2013 dummy * employment-population ratio for ages 20 to 29							-0.005
							(0.310)
Unemployment rate for prime-age adults	0.469		0.416	0.426		0.396	0.395
	(0.585)		(0.553)	(0.547)		(0.593)	(0.587)
Annual median fair market rent in state (in logs)	-0.026	-0.014	-0.030	-0.033	-0.017	-0.034	-0.034
	(0.073)	(0.068)	(0.074)	(0.072)	(0.067)	(0.072)	(0.072)
Minimum wage (in logs)	0.166	0.161	0.166	0.164	0.162	0.166	0.166
	(0.147)	(0.145)	(0.147)	(0.147)	(0.144)	(0.146)	(0.147)
Observations	2,254	2,254	2,254	2,254	2,254	2,254	2,254
Number of clusters	46	46	46	46	46	46	46

Note: All specifications inlude controls for average per capita income by state, average age of young adults in state, state and SIPP wave dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.

Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.7 Estimates from Individual-Level Analysis of Living Arrangements (Without Individual Fixed Effects)

Linear Probability Model

Dependent Variable: Whether Young Adult Lives with Parents

Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Group-specific unemployment rate		-0.076**	-0.076**	-0.134**			
		(0.031)	(0.031)	(0.056)			
2008-2013 dummy * Group-specific unemployment rate				0.093			
				(0.077)			
Group-specific employment-population ratio					0.022	0.022	0.039
					(0.023)	(0.024)	(0.027)
2008-2013 dummy * Group-specific employment-population ratio							-0.053
							(0.039)
Unemployment rate for prime-age adults	-0.067		0.001	-0.046		-0.049	0.102
	(0.215)		(0.217)	(0.205)		(0.224)	(0.285)
Annual median fair market rent in state (in logs)	0.025	0.028	0.028	0.027	0.022	0.025	0.027
	(0.092)	(0.091)	(0.092)	(0.092)	(0.091)	(0.092)	(0.093)
Minimum wage (in logs)	0.059	0.059	0.059	0.056	0.059	0.059	0.065
	(0.045)	(0.045)	(0.045)	(0.045)	(0.044)	(0.045)	(0.046)
p-Value: joint test for no effect of state-specific trend terms	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Observations	193,996	193,996	193,996	193,996	193,996	193,996	193,996
Number of clusters	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, race, gender, educational attainment, age-year, quarter, and state dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.8A Estimates from Individual Level Analysis of Living Arrangements (With Individual Fixed Effects) Linear FE

Dependent Variable: Whether Young Adult Lives with Parents									
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Linear Fixed Effects									
Group-specific unemployment rate		-0.016	-0.018	-0.015	-0.014				
		(0.016)	(0.016)	(0.016)	(0.021)				
2008-2013 dummy * Group-specific unemployment rate					-0.006				
					(0.031)				
Group-specific employment-population ratio						0.005	0.005	0.004	-0.003
						(0.015)	(0.015)	(0.015)	(0.016)
2008-2013 dummy * Group-specific employment-population ratio									0.020
									(0.022)
Unemployment rate for prime-age adults	0.120		0.127	0.127	0.127		0.122	0.122	0.122
	(0.102)		(0.102)	(0.102)	(0.102)		(0.101)	(0.100)	(0.100)
Annual median fair market rent in state (in logs)	-0.075	-0.070	-0.075	-0.080	-0.075	-0.071	-0.075	-0.080	-0.075
	(0.050)	(0.049)	(0.049)	(0.051)	(0.049)	(0.050)	(0.050)	(0.051)	(0.050)
High school or less * Annual median fair market rent in state (in logs)				0.010				0.010	
				(0.008)				(0.008)	
Minimum wage (in logs)	0.044*	0.043*	0.044*	0.062**	0.044*	0.043*	0.044*	0.062**	0.044*
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
High school or less * Minimum wage (in logs)				-0.036				-0.036	
				(0.028)				(0.028)	
p-Value: joint test for no effect of state-specific trend terms	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Observations	193,996	193,996	193,996	193,996	193,996	193,996	193,996	193,996	193,996
Number of clusters	46	46	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.

Cluster robust standard errors in parentheses, where the clustering is done by state *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.8B Estimates from Individual Level Analysis of Living Arrangements (With Individual Fixed Effects) Conditional Logit

Dependent Variable: Whether Young Adult Lives with Parents									
Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Conditional Logit									
Group-specific unemployment rate		-0.290	-0.349	-0.218	-0.291				
		(0.436)	(0.438)	(0.451)	(0.545)				
2008-2013 dummy * Group-specific unemployment rate					-0.115				
					(1.030)				
Group-specific employment-population ratio						0.066	0.091	0.007	-0.140
						(0.421)	(0.420)	(0.414)	(0.401)
2008-2013 dummy * Group-specific employment-population ratio									0.552
									(0.744)
Unemployment rate for prime-age adults	6.588*		6.746**	6.644*	6.742**		6.622*	6.546*	6.646**
	(3.415)		(3.411)	(3.400)	(3.404)		(3.380)	(3.369)	(3.376)
Annual median fair market rent in state (in logs)	-1.981	-1.741	-1.974	-2.266	-1.972	-1.752	-1.983	-2.271	-1.976
	(1.505)	(1.483)	(1.500)	(1.559)	(1.502)	(1.489)	(1.506)	(1.564)	(1.508)
High school or less * Annual median fair market rent in state (in logs)				0.405*				0.405*	
				(0.231)				(0.230)	
Minimum wage (in logs)	1.970**	1.929*	1.968**	2.702***	1.967**	1.933*	1.971**	2.706***	1.967**
	(0.991)	(1.002)	(0.990)	(0.993)	(0.990)	(0.998)	(0.987)	(0.993)	(0.988)
High school or less * Minimum wage (in logs)				-1.433*				-1.440*	
				(0.799)				(0.791)	
p-Value: joint test for no effect of state-specific trend terms	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Observations	25,586	25,586	25,586	25,586	25,586	25,586	25,586	25,586	25,586
Number of clusters	46	46	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.

Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.9A Estimates from Individual Fixed Effects Analysis of Living Arrangements: Ages 20-24

Dependent Variable: Whether Young Adult Lives with Parents								
Model	Linear Fixed Effects				Conditional Logit			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ages 20-24								
Group-specific unemployment rate	-0.014	-0.011			-0.402	-0.402		
	(0.022)	(0.028)			(0.589)	(0.595)		
2008-2013 dummy * Group-specific unemployment rate		-0.012				-0.222		
		(0.036)				(1.019)		
Group-specific employment-population ratio			0.005	-0.016			0.017	-0.494
			(0.019)	(0.024)			(0.477)	(0.556)
2008-2013 dummy * Group-specific employment-population ratio				0.057*				1.564*
				(0.029)				(0.811)
Unemployment rate for prime-age adults	0.091	0.091	0.086	0.085	3.301	3.502	3.064	3.221
	(0.166)	(0.166)	(0.165)	(0.165)	(5.054)	(5.007)	(5.038)	(4.998)
Annual median fair market rent in state (in logs)	-0.062	-0.047	-0.063	-0.047	-2.324	-1.710	-2.345	-1.649
	(0.075)	-0.07	(0.075)	(0.070)	(1.919)	(1.753)	(1.930)	(1.764)
High school or less * Annual median fair market rent in state (in logs)	0.022		0.022		0.698*		0.701**	
	(0.013)		(0.013)		(0.359)		(0.356)	
Minimum wage (in logs)	0.051	0.002	0.052	0.001	1.934*	0.437	1.955*	0.407
	(0.041)	(0.034)	(0.041)	(0.034)	(1.118)	(1.131)	(1.116)	(1.135)
High school or less * Minimum wage (in logs)	-0.077*		-0.078*		-2.411**		-2.437**	
	(0.045)		(0.045)		(1.229)		(1.223)	
Observations	94,553	94,553	94,553	94,553	13,154	13,154	13,154	13,154
Number of clusters	46	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year dummies, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.9B Estimates from Individual Fixed Effects Analysis of Living Arrangements: Ages 25-29

Dependent Variable: Whether Young Adult Lives with Parents								
Model	Linear Fixed Effects				Conditional Logit			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Ages 25-29								
Group-specific unemployment rate	0.008	0.007			1.209	0.941		
	(0.015)	(0.026)			(0.814)	(1.273)		
2008-2013 dummy * Group-specific unemployment rate		-0.009				-0.370		
		(0.043)				(2.075)		
Group-specific employment-population ratio			0.017	0.018			0.253	0.290
			(0.013)	(0.017)			(0.653)	(0.818)
2008-2013 dummy * Group-specific employment-population ratio				0.010				0.911
				(0.031)				(1.749)
Unemployment rate for prime-age adults	0.145	0.148	0.154	0.157	6.512	6.765	6.872	7.296
	(0.121)	(0.121)	(0.122)	(0.122)	(5.641)	(5.680)	(5.635)	(5.741)
Annual median fair market rent in state (in logs)	-0.093	-0.105	-0.093	-0.104	-4.790*	-5.229**	-4.770*	-5.255**
	(0.062)	(0.063)	(0.061)	(0.062)	(2.485)	(2.533)	(2.486)	(2.531)
High school or less * Annual median fair market rent in state (in logs)	-0.028**		-0.028**		-0.993**		-0.991**	
	(0.011)		(0.011)		(0.458)		(0.454)	
Minimum wage (in logs)	0.029	0.067**	0.029	0.068**	4.660***	5.751***	4.667***	5.787***
	(0.035)	(0.030)	(0.035)	(0.030)	(1.524)	(1.362)	(1.509)	(1.361)
High school or less * Minimum wage (in logs)	0.089**		0.090**		2.856*		2.897*	
	(0.041)		(0.041)		(1.627)		(1.610)	
Observations	99,443	99,443	99,443	99,443	8,606	8,606	8,606	8,606
Number of clusters	46	46	46	46	45	45	45	45

Note: All sepecifications include controls for average per capita income in state, age-year dummies, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.10A Estimates from Individual Fixed Effects Analysis of Living Arrangements by Gender: Men

Dependent Variable: Whether Young Adult Lives with Parents

Model	Linear Fixed Effects				Conditional Logit			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Men								
Group-specific unemployment rate	-0.019	-0.003			-0.522	0.013		
	(0.024)	(0.030)			(0.716)	(0.741)		
2008-2013 dummy * Group-specific unemployment rate		-0.020				-1.015		
		(0.034)				(1.101)		
Group-specific employment-population ratio			-0.001	-0.014			-0.038	-0.517
			(0.018)	(0.022)			(0.534)	(0.537)
2008-2013 dummy * Group-specific employment-population ratio				0.031				1.372
				(0.032)				(1.115)
Unemployment rate for prime-age adults	0.038	0.033	0.030	0.028	3.296	3.327	3.109	3.211
	(0.117)	(0.116)	(0.114)	(0.113)	(3.546)	(3.544)	(3.473)	(3.462)
Annual median fair market rent in state (in logs)	-0.034	-0.023	-0.035	-0.023	0.265	0.800	0.247	0.828
	(0.066)	(0.063)	(0.066)	(0.064)	(1.914)	(1.801)	(1.916)	(1.812)
High school or less * Annual median fair market rent in state (in logs)	0.020		0.020		0.601		0.605	
	(0.013)		(0.013)		(0.374)		(0.373)	
Minimum wage (in logs)	0.146***	0.108**	0.146***	0.108**	4.761***	3.596***	4.770***	3.581***
	(0.051)	(0.044)	(0.051)	(0.044)	(1.677)	(1.388)	(1.676)	(1.385)
High school or less * Minimum wage (in logs)	-0.065		-0.066		-2.017		-2.047	
	(0.047)		(0.047)		(1.299)		(1.293)	
Observations	105,522	105,522	105,522	105,522	13,339	13,339	13,339	13,339
Number of clusters	46	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.10B Estimates from Individual Fixed Effects Analysis of Living Arrangements by Gender: Women

Dependent Variable: Whether Young Adult Lives with Parents								
Model	Linear Fixed Effects				Conditional Logit			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Women								
Group-specific unemployment rate	-0.014	-0.024			-0.568	-0.846		
	(0.020)	(0.024)			(0.587)	(0.751)		
2008-2013 dummy * Group-specific unemployment rate		-0.002				0.020		
		(0.048)				(1.543)		
Group-specific employment-population ratio			0.001	0.003			0.222	0.283
			(0.016)	(0.018)			(0.506)	(0.537)
2008-2013 dummy * Group-specific employment-population ratio				0.021				0.418
				(0.041)				(1.241)
Unemployment rate for prime-age adults	0.228	0.233	0.222	0.226	9.421*	9.675*	9.170*	9.456*
	(0.162)	(0.163)	(0.160)	(0.161)	(5.163)	(5.244)	(5.087)	(5.170)
Annual median fair market rent in state (in logs)	-0.137**	-0.136**	-0.138**	-0.137**	-5.342***	-5.122**	-5.355***	-5.147**
	(0.056)	(0.055)	(0.056)	(0.055)	(2.058)	(2.012)	(2.067)	(2.023)
High school or less * Annual median fair market rent	0.002		0.002		0.328		0.326	
in state (in logs)	(0.013)		(0.013)		(0.398)		(0.401)	
Minimum wage (in logs)	-0.023	-0.029	-0.023	-0.029	0.781	0.224	0.785	0.228
	(0.033)	(0.036)	(0.033)	(0.035)	(1.272)	(1.323)	(1.271)	(1.326)
High school or less * Minimum wage (in logs)	-0.013		-0.013		-1.239		-1.239	
	(0.046)		(0.046)		(1.387)		(1.385)	
Observations	88,474	88,474	88,474	88,474	12,214	12,214	12,214	12,214
Number of clusters	46	46	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.11 Estimates from Individual Fixed Effects Analysis of Living Arrangements: 2008 – 2013

Dependent Variable: Whether Young Adult Lives with Parents+C4:O31

Model	Linear Fixed Effects				Conditional Logit			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Group-specific unemployment rate	-0.017	-0.014			-0.356	-0.278		
	(0.022)	(0.020)			(0.724)	(0.704)		
Group-specific employment-population ratio			0.012	0.010			0.210	0.155
			(0.019)	(0.017)			(0.667)	(0.644)
Unemployment rate for prime-age adults	0.081	0.082	0.079	0.081	2.481	2.427	2.441	2.394
	(0.096)	(0.096)	(0.095)	(0.096)	(3.372)	(3.384)	(3.378)	(3.388)
Annual median fair market rent in state (in logs)	0.100	0.086	0.098	0.085	4.633	4.412	4.605	4.387
	(0.075)	(0.079)	(0.075)	(0.079)	(2.950)	(3.023)	(2.950)	(3.022)
High school or less * Annual median fair market rent in state (in logs)		0.025		0.026		0.422		0.427
		(0.023)		(0.022)		(0.815)		(0.811)
Minimum wage (in logs)	0.043	0.089	0.043	0.090	0.284	1.069	0.303	1.096
	(0.061)	(0.076)	(0.061)	(0.076)	(3.015)	(3.611)	(3.007)	(3.595)
High school or less * Minimum wage (in logs)		-0.086		-0.087		-1.430		-1.449
		(0.075)		(0.075)		(2.690)		(2.673)
Observations	65,033	65,033	65,033	65,033	8,742	8,742	8,742	8,742
Number of clusters	46	46	46	46	45	45	45	45

Note: All sepecifications include controls for average per capita income in state, age-year dummies, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.

Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.12A Estimates from Individual Fixed Effects Analysis of Living Arrangements: 2001-2013 (Linear FE)

Dependent Variable: Whether Young Adult Lives with Parents

Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Linear Fixed Effects						
Group-specific unemployment rate	-0.013	-0.014	0.001			
	(0.017)	(0.016)	(0.023)			
2008-2013 dummy * Group-specific unemployment rate			-0.022			
			(0.032)			
Group-specific employment-population ratio				0.006	0.006	-0.005
				(0.015)	(0.015)	(0.016)
2008-2013 dummy * Group-specific employment-population ratio						0.020
						(0.022)
Unemployment rate for prime-age adults	0.037	0.038	0.037	0.034	0.034	0.035
	(0.080)	(0.079)	(0.080)	(0.079)	(0.079)	(0.078)
Annual median fair market rent in state (in logs)	-0.008	-0.015	-0.007	-0.008	-0.015	-0.008
	(0.045)	(0.047)	(0.045)	(0.046)	(0.047)	(0.046)
High school or less * Annual median fair market rent in state (in logs)		0.014			0.015	
		(0.011)			(0.011)	
Minimum wage (in logs)	0.032	0.056**	0.032	0.032	0.057**	0.032
	(0.023)	(0.026)	(0.023)	(0.023)	(0.026)	(0.023)
High school or less * Minimum wage (in logs)		-0.048			-0.049	
		(0.037)			(0.037)	
Observations	145,994	145,994	145,994	145,994	145,994	145,994
Number of clusters	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.12B Estimates from Individual Fixed Effects Analysis of Living Arrangements: 2001-2013 (Conditional Logit)

Dependent Variable: Whether Young Adult Lives with Parents						
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Conditional Logit						
Group-specific unemployment rate	-0.089	-0.035	0.269			
	(0.503)	(0.504)	(0.661)			
2008-2013 dummy * Group-specific unemployment rate			-0.583			
			(1.032)			
Group-specific employment-population ratio				0.032	-0.011	-0.277
				(0.445)	(0.433)	(0.463)
2008-2013 dummy * Group-specific employment-population ratio						0.582
						(0.744)
Unemployment rate for prime-age adults	2.707	2.660	2.698	2.679	2.642	2.733
	(2.764)	(2.781)	(2.756)	(2.757)	(2.775)	(2.736)
Annual median fair market rent in state (in logs)	0.275	0.025	0.288	0.274	0.025	0.283
	(1.428)	(1.494)	(1.429)	(1.433)	(1.498)	(1.434)
High school or less * Annual median fair market rent in state (in logs)		0.356			0.356	
		(0.289)			(0.287)	
Minimum wage (in logs)	1.963**	2.561**	1.958**	1.963**	2.563**	1.959**
	(0.962)	(1.054)	(0.962)	(0.960)	(1.056)	(0.963)
High school or less * Minimum wage (in logs)		-1.225			-1.229	
		(0.980)			(0.967)	
Observations	18,564	18,564	18,564	18,564	18,564	18,564
Number of clusters	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, and quarter dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.13A Dynamic Model: Discrete Time Hazard for Leaving Home (Coefficient Estimates)

Dependent Variable: Whether Young Adult Left Home						
	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Estimates						
Group-specific unemployment rate	0.582	0.615	1.362			
	(0.820)	(0.827)	(1.159)			
2008-2013 dummy * Group-specific unemployment rate			-1.634			
			(1.423)			
Group-specific employment-population ratio				-0.587	-0.598	-0.986*
				(0.542)	(0.547)	(0.595)
2008-2013 dummy * Group-specific employment-population ratio						1.531**
						(0.709)
Unemployment rate for prime-age adults	2.013	1.915	2.068	2.075	1.993	2.391
	(5.671)	(5.681)	(5.683)	(5.682)	(5.683)	(5.703)
Annual median fair market rent in state (in logs)	-1.498	-1.402	-1.497	-1.483	-1.384	-1.443
	(1.140)	(1.144)	(1.148)	(1.144)	(1.147)	(1.159)
High school or less * Annual median fair market rent in state (in logs)		-0.293			-0.300	
		(0.455)			(0.448)	
Minimum wage (in logs)	-0.211	0.129	-0.170	-0.221	0.113	-0.219
	(0.900)	(0.873)	(0.908)	(0.902)	(0.870)	(0.923)
High school or less * Minimum wage (in logs)		-0.688			-0.678	
		(0.870)			(0.860)	
Observations	35,774	35,774	35,774	35,774	35,774	35,774
Number of clusters	44	44	44	44	44	44

Note: All sepecifications include controls for average per capita income in state, age-year dummies for young adult, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy. I also include the following lagged parental controls: average education of parents, average age of parents, whether both biological parents were present, home ownership status of parents and number of household members.

The analysis includes young adults 20-29 who are single, not enrolled in school and living with parents at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.

Cluster robust standard errors in parentheses, where the clustering is done by state *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.13B Dynamic Model: Discrete Time Hazard for Leaving Home (Marginal Effects)

Dependent Variable: Whether Young Adult Left Home						
	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Marginal Effects						
Group-specific unemployment rate	0.01	0.01	0.03			
2008-2013 dummy * Group-specific unemployment rate			-0.03			
Group-specific employment-population ratio				-0.01	-0.01	-0.02
2008-2013 dummy * Group-specific employment-population ratio						0.03
Unemployment rate for prime-age adults	0.04	0.04	0.04	0.04	0.04	0.05
Annual median fair market rent in state (in logs)	-0.03	-0.03	-0.03	-0.03	-0.03	-0.03
High school or less * Annual median fair market rent in state (in logs)		-0.006			-0.006	
Minimum wage (in logs)	-0.004	0.003	-0.003	-0.004	0.002	-0.004
High school or less * Minimum wage (in logs)		-0.01			-0.01	

Note: All sepecifications include controls for average per capita income in state, age-year dummies for young adult, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy. I also include the following lagged parental controls: average education of parents, average age of parents, whether both biological parents were present, home ownership status of parents and number of household members.
The analysis includes young adults 20-29 who are single, not enrolled in school and living with parents at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.14 Dynamic Model: Estimates of Hazard for Leaving Home: By Age-Group

Dependent Variable: Whether Young Adult Left Home

	Ages 20-24				Ages 25-29			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Group-specific unemployment rate	0.449	0.367			0.833	2.012		
	(1.009)	(1.277)			(1.812)	(2.682)		
2008-2013 dummy * Group-specific unemployment rate		0.039				-2.077		
		(1.561)				(3.264)		
Group-specific employment-population ratio			-1.575**	-1.639**			-0.097	-0.435
			(0.742)	(0.827)			(1.138)	(1.299)
2008-2013 dummy * Group-specific employment-population ratio				0.425				1.373
				(0.994)				(1.322)
Unemployment rate for prime-age adults	3.687	3.807	2.803	3.064	-4.742	-4.615	-4.528	-4.068
	(7.833)	(7.682)	(7.844)	(7.814)	(10.034)	(10.151)	(9.978)	(10.157)
Annual median fair market rent in state (in logs)	-4.393**	-4.256**	-4.280**	-4.093**	2.105	1.969	2.132	2.029
	(1.865)	(1.728)	(1.868)	(1.725)	(2.538)	(2.468)	(2.551)	(2.488)
High school or less * Annual median fair market rent in state (in logs)	0.227		0.260		-0.897		-0.918	
	(0.591)		(0.581)		(0.715)		(0.720)	
Minimum wage (in logs)	0.482	-0.063	0.484	-0.116	-0.041	-0.086	-0.084	-0.116
	(1.440)	(1.391)	(1.409)	(1.382)	(1.237)	(1.251)	(1.234)	(1.251)
High school or less * Minimum wage (in logs)	-0.838		-0.915		-0.295		-0.223	
	(0.933)		(0.932)		(1.704)		(1.673)	
Observations	21,211	21,211	21,211	21,211	13,231	13,231	13,231	13,231
Number of clusters	43	43	43	43	40	40	40	40

Refer to notes for Table 3.13

Table 3.15 Dynamic Model: Estimates of Hazard for Leaving Home: By Gender

Dependent Variable: Whether Young Adult Left Home								
	Women				Men			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Group-specific unemployment rate	-0.215	0.514			1.378	2.191		
	(1.168)	(1.640)			(1.364)	(1.766)		
2008-2013 dummy * Group-specific unemployment rate		-1.479				-1.512		
		(2.267)				(1.650)		
Group-specific employment-population ratio			0.046	-0.447			-0.853	-1.672*
			(1.112)	(1.168)			(0.809)	(0.941)
2008-2013 dummy * Group-specific employment-population ratio				1.422				2.249**
				(1.398)				(1.022)
Unemployment rate for prime-age adults	2.516	2.918	2.395	2.939	2.827	2.778	3.333	3.645
	(10.732)	(10.782)	(10.808)	(10.924)	(8.273)	(8.268)	(8.200)	(8.209)
Annual median fair market rent in state (in logs)	-1.541	-1.666	-1.548	-1.631	-1.388	-1.495	-1.339	-1.346
	(1.611)	(1.630)	(1.607)	(1.630)	(2.088)	(2.076)	(2.097)	(2.099)
High school or less * Annual median fair market rent in state (in logs)	-0.359		-0.354		-0.207		-0.210	
	(0.557)		(0.556)		(0.553)		(0.545)	
Minimum wage (in logs)	1.859	0.896	1.868	0.910	-1.461	-1.070	-1.506	-1.169
	(1.431)	(1.277)	(1.422)	(1.261)	(1.203)	(1.050)	(1.216)	(1.061)
High school or less * Minimum wage (in logs)	-2.112		-2.119		0.648		0.684	
	(1.472)		(1.476)		(1.306)		(1.303)	
Observations	13,226	13,226	13,226	13,226	21,992	21,992	21,992	21,992
Number of clusters	42	42	42	42	43	43	43	43

Refer to notes for Table 3.13

Table 3.16A Dynamic Model: Discrete Time Hazard for Returning Home (Coefficient Estimates)

Dependent Variable: Whether Young Adult Returned Home

	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Estimates						
Group-specific unemployment rate	-0.141	-0.154	-0.387			
	(0.926)	(0.868)	(1.127)			
2008-2013 dummy * Group-specific unemployment rate			0.492			
			(1.785)			
Group-specific employment-population ratio				-0.025	-0.019	-0.122
				(0.673)	(0.666)	(0.643)
2008-2013 dummy * Group-specific employment-population ratio						0.353
						(0.878)
Unemployment rate for prime-age adults	8.649	8.687	8.607	8.555	8.590	8.675
	(9.137)	(9.125)	(9.121)	(9.166)	(9.171)	(9.193)
Annual median fair market rent in state (in logs)	0.591	0.846	0.582	0.587	0.839	0.613
	(1.688)	(1.677)	(1.690)	(1.687)	(1.676)	(1.696)
High school or less * Annual median fair market rent in state (in logs)		-0.529			-0.523	
		(0.515)			(0.519)	
Minimum wage (in logs)	-0.652	-0.339	-0.658	-0.649	-0.330	-0.645
	(1.472)	(1.551)	(1.467)	(1.473)	(1.560)	(1.475)
High school or less * Minimum wage (in logs)		-0.560			-0.574	
		(1.134)			(1.155)	
Observations	63,674	63,674	63,674	63,674	63,674	63,674
Number of clusters	44	44	44	44	44	44

Note: All sepecifications include controls for average per capita income in state, age-year, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy.
The analysis includes young adults 20-29 who are single, not enrolled in school and living away from parents at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.16B Dynamic Model: Discrete Time Hazard for Returning Home (Marginal Effects)

Dependent Variable: Whether Young Adult Returned Home						
	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Marginal Effects						
Group-specific unemployment rate	-0.001	-0.001	-0.003			
2008-2013 dummy * Group-specific unemployment rate			0.003			
Group-specific employment-population ratio				-0.0002	-0.0001	-0.001
2008-2013 dummy * Group-specific employment-population ratio						0.002
Unemployment rate for prime-age adults	0.06	0.06	0.056	0.06	0.06	0.06
Annual median fair market rent in state (in logs)	0.004	0.01	0.004	0.004	0.01	0.004
High school or less * Annual median fair market rent in state (in logs)		-0.003			-0.003	
Minimum wage (in logs)	-0.004	-0.002	-0.004	-0.004	-0.002	-0.004
High school or less * Minimum wage (in logs)		-0.004			-0.004	

Note: All sepecifications include controls for average per capita income in state, age-year, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy.
The analysis includes young adults 20-29 who are single, not enrolled in school and living away from parents at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.17 Dynamic Model: Estimates of Hazard for Returning Home: By Age-Group

Dependent Variable: Whether Young Adult Returned Home

	Ages 20-24				Ages 25-29			
Covariates	(1)	(2)	(4)	(5)	(1)	(2)	(4)	(5)
Group-specific unemployment rate	-1.502	-1.494			3.735*	3.364		
	(1.117)	(1.591)			(1.938)	(2.675)		
2008-2013 dummy * Group-specific unemployment rate		0.325				0.187		
		(2.678)				(2.948)		
Group-specific employment-population ratio			1.100	0.809			-1.398	-1.575
			(0.888)	(0.958)			(1.212)	(1.129)
2008-2013 dummy * Group-specific employment-population ratio				0.971				0.776
				(1.871)				(1.447)
Unemployment rate for prime-age adults	14.298	14.352	13.800	14.297	7.017	7.019	8.080	8.353
	(12.941)	(12.916)	(12.967)	(12.961)	(14.526)	(14.518)	(14.426)	(14.418)
Annual median fair market rent in state (in logs)	1.222	0.704	1.175	0.764	-0.096	-0.117	0.035	0.017
	(1.984)	(1.979)	(1.997)	(1.984)	(3.312)	(3.339)	(3.309)	(3.329)
High school or less * Annual median fair market rent	-1.003*		-0.986*		0.093		0.003	
in state (in logs)	(0.598)		(0.599)		(0.794)		(0.805)	
Minimum wage (in logs)	-3.001*	-2.562*	-2.847*	-2.511*	2.238	1.149	2.152	1.216
	(1.636)	(1.420)	(1.633)	(1.427)	(2.301)	(2.373)	(2.284)	(2.359)
High school or less * Minimum wage (in logs)	0.715		0.585		-2.285		-2.012	
	(1.281)		(1.306)		(1.654)		(1.676)	
Observations	22,060	22,060	22,060	22,060	36,738	36,738	36,738	36,738
Number of clusters	43	43	43	43	40	40	40	40

Refer to notes for table 3.16.

Table 3.18 Dynamic Model: Estimates of Hazard for Returning Home: By Gender

Dependent Variable: Whether Young Adult Returned Home								
	Women				Men			
Covariates	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Group-specific unemployment rate	-1.539	-2.819*			0.950	2.141		
	(1.114)	(1.493)			(1.672)	(2.188)		
2008-2013 dummy * Group-specific unemployment rate		3.191				-2.456		
		(2.248)				(2.503)		
Group-specific employment-population ratio			-0.239	0.271			1.972	0.963
			(0.839)	(0.850)			(1.322)	(1.453)
2008-2013 dummy * Group-specific employment-population ratio				-1.523				2.497*
				(1.172)				(1.474)
Unemployment rate for prime-age adults	9.946	9.390	8.835	8.408	12.510	12.450	13.716	14.081
	(13.242)	(13.314)	(13.305)	(13.320)	(12.213)	(12.175)	(11.997)	(11.849)
Annual median fair market rent in state (in logs)	1.225	0.671	1.152	0.611	1.244	1.218	1.348	1.477
	(2.650)	(2.687)	(2.630)	(2.666)	(2.394)	(2.318)	(2.379)	(2.291)
High school or less * Annual median fair market rent	-1.098*		-1.044*		-0.116		-0.220	
in state (in logs)	(0.598)		(0.601)		(0.773)		(0.751)	
Minimum wage (in logs)	0.091	0.093	0.202	0.202	-0.703	-1.313	-0.810	-1.268
	(1.627)	(1.625)	(1.620)	(1.632)	(2.243)	(1.995)	(2.227)	(1.991)
High school or less * Minimum wage (in logs)	0.164		0.084		-1.003		-0.674	
	(1.314)		(1.334)		(1.568)		(1.590)	
Observations	31,032	31,032	31,032	31,032	27,924	27,924	27,924	27,924
Number of clusters	42	42	42	42	40	40	40	40

Refer to notes for Table 3.16

Table 3.19 Dynamic Analysis of Changing State of Residence

Dependent Variable: Whether Young Adult Changed State of Residence						
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Group-specific unemployment rate	1.519		1.165	1.545		
(in previous state for state changers)	(1.362)		(1.146)	(1.809)		
2008-2013 dummy * Group-specific unemployment rate				-0.646		
(in previous state for state changers)				(1.622)		
Group-specific employment-population ratio					-1.549	-1.786*
(in previous state for state changers)					(0.971)	(1.041)
2008-2013 dummy * Group-specific employment-population ratio						0.752
(in previous state for state changers)						(0.674)
Unemployment rate for prime-age adults		19.631	18.871	18.899	18.768	18.932
(in previous state for state changers)		(23.614)	(23.409)	(23.441)	(23.494)	(23.556)
Annual median fair market rent in state (in logs)	0.509	-0.382	-0.394	-0.406	-0.465	-0.459
(in previous state for state changers)	(13.436)	(13.399)	(13.380)	(13.393)	(13.366)	(13.379)
Minimum wage (in logs)	0.791	0.905	0.899	0.902	0.872	0.873
(in previous state for state changers)	(4.887)	(5.032)	(5.023)	(5.024)	(5.032)	(5.033)
Observations	133,498	133,498	133,498	133,498	133,498	133,498
Number of clusters	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, state, SIPP panel, and quarter dummies, and linear state trends.
The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.20 Discrete Time Hazard for Leaving Home (Includes those who Change State of Residence)

Dependent Variable: Whether Young Adult Left Home						
	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Group-specific unemployment rate	1.076**		1.029**	1.094		
(in previous state for state changers)	(0.535)		(0.522)	(0.675)		
2008-2013 dummy * Group-specific unemployment rate				-0.127		
(in previous state for state changers)				(1.004)		
Group-specific employment-population ratio					-1.035**	-1.317***
(in previous state for state changers)					(0.439)	(0.485)
2008-2013 dummy * Group-specific employment-population ratio						1.037*
(in previous state for state changers)						(0.542)
Unemployment rate for prime-age adults		3.725	3.008	3.012	3.122	3.369
(in previous state for state changers)		(6.675)	(6.658)	(6.658)	(6.624)	(6.629)
Annual median fair market rent in state (in logs)	-1.004	-1.132	-1.156	-1.158	-1.162	-1.142
(in previous state for state changers)	(2.855)	(2.827)	(2.831)	(2.833)	(2.826)	(2.828)
Minimum wage (in logs)	-1.080	-1.050	-1.045	-1.043	-1.064	-1.064
(in previous state for state changers)	(0.882)	(0.910)	(0.909)	(0.908)	(0.907)	(0.915)
Observations	47,492	47,492	47,492	47,492	47,492	47,492
Number of clusters	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, age-year, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy. The analysis includes young adults 20-29 who are single, not enrolled in school and living away from parents at the time they are first observed in sample. I also include the following lagged parental controls: average education of parents, average age of parents, whether both biological parents were present, home ownership status of parents and number of household members.
Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

Table 3.21 Discrete Time Hazard for Returning Home (Includes those who Change State of Residence)

Dependent Variable: Whether Young Adult Returned Home						
	Specification					
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Group-specific unemployment rate	0.388		0.185	-0.826		
(in previous state for state changers)	(1.422)		(1.407)	(1.324)		
2008-2013 dummy * Group-specific unemployment rate				1.854		
(in previous state for state changers)				(1.521)		
Group-specific employment-population ratio					-0.211	-0.062
(in previous state for state changers)					(0.981)	(0.911)
2008-2013 dummy * Group-specific employment-population ratio						-0.479
(in previous state for state changers)						(1.172)
Unemployment rate for prime-age adults		11.211	11.079	10.969	11.100	10.986
(in previous state for state changers)		(10.490)	(10.452)	(10.467)	(10.509)	(10.493)
Annual median fair market rent in state (in logs)	6.573***	5.955***	5.953***	5.980***	5.932***	5.931***
(in previous state for state changers)	(1.700)	(1.794)	(1.793)	(1.802)	(1.777)	(1.778)
Minimum wage (in logs)	-0.248	-0.199	-0.199	-0.218	-0.199	-0.205
(in previous state for state changers)	(1.821)	(1.788)	(1.788)	(1.780)	(1.788)	(1.786)
Observations	68,832	68,832	68,832	68,832	68,832	68,832
Number of clusters	45	45	45	45	45	45

Note: All sepecifications include controls for average per capita income in state, age-year, quarter, state, and panel dummies, and linear state trends. Controls for young people include race, high school or less and gender dummy. The analysis includes young adults 20-29 who are single, not enrolled in school and living away from parents at the time they are first observed in sample.

Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1

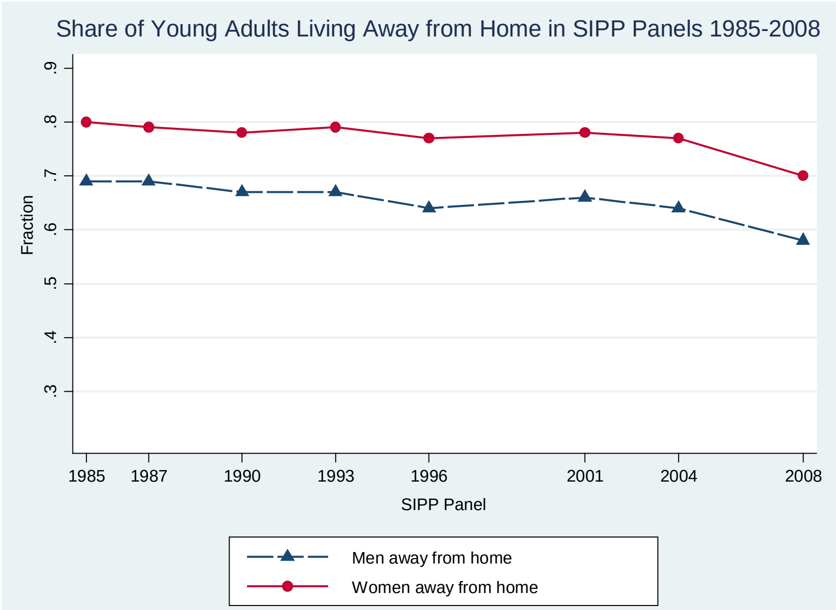


Figure 3.1 Share of Young Adults Living Away from Home in SIPP Panels 1985-2008

Note: The initial sample consists of all individuals ages 20 to 29 who are not enrolled in school at the time when they first enter the SIPP sample. I declare a person as living away from his parents' home in any four-month interval during which he does not live with either his parents or in-laws, and as living at his parents' home if he is observed living with either his parents or in laws in a household not headed by him or his spouse.

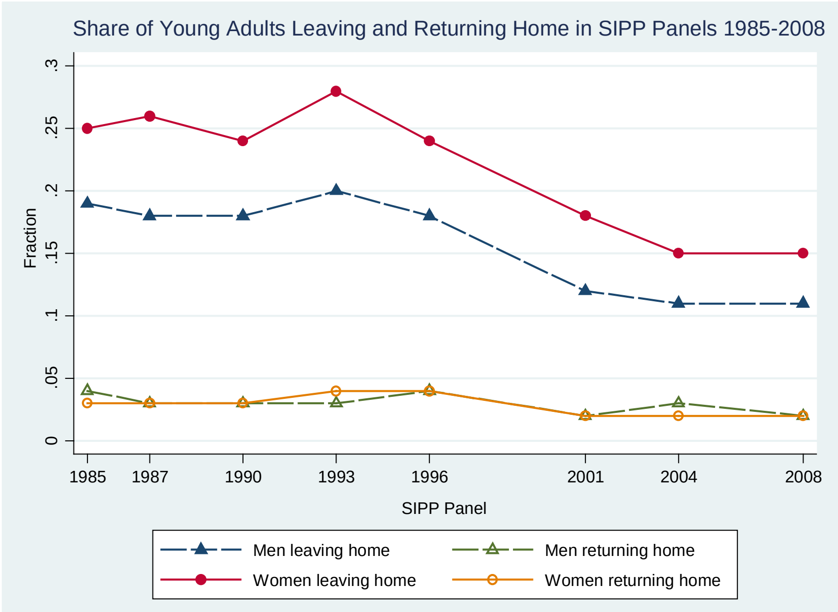


Figure 3.2 Share of Young Adults Leaving and Returning Home in SIPP Panels 1985-2008

Note: Moves observed in sample show the percentage of individuals who return home from among those who were at risk of returning home, and the percentage of individuals who leave home from among those who were at risk of leaving. A person is described as having left the parental home when he no longer lives at his parents' home (as indicated by a change of address and the absence of parents at new address).

REFERENCES

Addison, John T., McKinley L. Blackburn, and Chad D. Cotti. (2015). On the Robustness of Minimum Wage Effects: Geographically-Disparate Trends and Job Growth Equations. *IZA Journal of Labor Economics*. 4, 1-16.

Allison, Paul. (1982). Discrete-time Methods for the Analysis of Event Histories. In Leinhardt, S. (ed.), *Sociological Methodology*, Jossey-Bass Publishers, San Francisco. (13), 61-98.

Angrist, Joshua D. and Jörn-Steffen Pischke. (2015). *Mastering Metrics: The Path from Cause to Effect*. Princeton: Princeton University Press.

Altonji, Joseph, and Christina Paxson. (1988). Labor Supply Preferences, Hours Constraints, and Hours-Wage Trade-offs. *Journal of Labor Economics* 6 (2) (April), 254–76.

Aquilino, William S. (1990). The Likelihood of Parent-Adult Child Coresidence: Effects of Family Structure and Parental Characteristics. *Journal of Marriage and Family*. 52(2), 405-19.

Avery, Roger, Frances Goldscheider, and Alden Speare Jr. (1992). Feathered Nest/Gilded Cage: Parental Income and Leaving Home in the Transition to Adulthood. *Demography*. 29 (3), 375-88.

Baker, Michael and Dwayne, Benjamin. (1999). How do retirement tests affect the Labour Supply of Older men? *Journal of Public Economics.* 71, 27-51.

Bartik, Timothy. (2013). How Effects of Local Labor Demand Shocks Vary with Local Labor Market Conditions. Working paper. Upjohn Institute for Employment Research.

Behaghel, Luc, and David Blau. (2012). Framing Social Security Reform: Behavioral Responses to the Changes in the Full Retirement Age. *American Economic Journal: Economic Policy*. 4(4), 41-67.

Benítez-Silva, H. and Dwyer, D. S. (2005). The Social Security Earnings Test and Work Incentives, *Journal of Policy Analysis and Management*. 26, 527–55.

Bitler, Marianne, and Hilary Hoynes. (2015). Living Arrangements, Doubling Up, and the Great Recession: Was This Time Different? *American Economic Review: Papers and Proceedings*. 105(5), 166-70.

Blank, Rebecca M. (1997). Policy Watch: The 1996 Welfare Reform. *The Journal of Economic Perspectives*. 11(1), 169-77.

Blau, David M. (1994). Labor Force Dynamics of Older Men. *Econometrica.*62 (1), 117-56.

Blau, David, and Ryan Goodstein. (2010). Can Social Security Explain Trends in Labor Force Participation of Older Men in the United States?" *Journal of Human Resource*. 45(2), 328-63.

Blau, Francine D., Lawrence M. Kahn, and Jane Waldfogel. (2004). The Impact of Welfare Benefits on Single Motherhood and Headship of Young Women: Evidence from the Census. *The Journal of Human Resources*. 39 (2), 382-404.

Blinder, Alan, Roger Gordon, and Donald Wise. (1980). Reconsidering the Work Disincentive Effects of Social Security. *National Tax Journal*. 33, 431-42.

Burkhauser, Richard and John Turner. (1981). Can Twenty-Five Million Americans Be Wrong? A Response to Blinder, Gordon and Wise, *National Tax Journal*. 34, 467-72.

Burtless, Gary and Robert A. Moffitt. (1985). The Joint Choice of Retirement Age and Postretirement Hours of Work. *Journal of Labor Economics*. 3 (2), 209-36.

Brown, Charles, Curtis Gilroy, and Andrew Kohen. (1982). The Effect of Minimum Wage on Employment and Unemployment. *Journal of Economic Literature.* 20 (2), 487-528.

Burkhead, D. and J. Coder. (1985). Gross Changes in Income Recipiency from the Survey of Income and Program Participation. *Proceedings* of the American Statistical Association, Social Statistics Section. 351-56.

Card, David and Thomas Lemieux. (2000). Adapting to Circumstances (The Evolution of Work, School, and Living Arrangements among North America Youth). In *Youth Employment and Joblessness in Advanced Countries*. David G. Blanchflower and Richard B. Freeman eds. Chicago. University of Chicago Press. 171-214.

Chamberlain, Gary. (1980). Analysis of covariance with qualitative data. *Review of Economic Studies*. 47, 225–38.

Cogan, John. (1981). Fixed Costs and Labor Supply. *Econometrica,* 49, 945–963.

Coile, Courtney, Peter Diamond, Jonathan Gruber and Alain Jousten. (2002). Delays in Claiming Social Security Benefits, *Journal of Public Economics*. 84, 357-85

Colombino, Ugo. (2003). A Simple Intertemporal Model of Retirement Estimated On Italian Cross-Section Data. *Labour*. 17(1), 115-37.

Current Population Survey. Chapter 1. Labor Force Data Derived from the Current Population Survey. *BLS Handbook of Methods*. (August 4, 2016). http://www.bls.gov/opub/hom/pdf/homch1.pdf.

Danziger, Sheldon and David Ratner. (2010). Labor Market Outcomes and the Transition to Adulthood. *The Future of Children*. 1, 133-158.

DaVanzo, Julie and Frances Kobrin Goldscheider. (1990). Coming Home Again: Returns to the Parental Home of Young Adults. *Population Studies*. 44 (2), 241-55.

Diamond, Peter, and Jonathan Gruber. (1999). Social Security and Retirement in the United States. *Social Security and Retirement around the World*. Jonathan Gruber and David A. Wise eds. Chicago. University of Chicago Press. 437-73.

Diamond, Peter, and Jerry Hausman. (1984). The Retirement and Unemployment Behavior of Older Men. *In Retirement and Economic Behavior*, ed. Henry Aaron and Gary Burtless, 97 – 132. Washington, D.C.: The Brookings Institution.

Diamond, Peter, and Jerry Hausman. (1984b). Individual Retirement and Savings Behavior" *Journal of Public Economics*. 23, 81-114.

Disney, Richard, and Sarah Smith. (2002). The Labor Supply Effects of the Abolition of the Earnings Rule for Older Workers in the United Kingdom. *The Economic Journal*. 112(478), C136-52.

Ellwood, David, and Mary Jo Bane. (1985). The Impact of AFDC on Family Structure and Living Arrangements. *Research in Labor Economics*. Ed: Ronald Ehrenberg. Volume 7. Greenwich, CT.: JAI Press.

Engelhardt, Gary, and Anil Kumar. (2009). The Repeal of the Retirement Earnings Test and the Labor Supply of Older Men. *Journal of Pension Economics and Finance*. 8 (4), 429-450.

Engelhardt, Gary, and Anil Kumar. (2014). Taxes and the Labor Supply of Older Americans: Recent Evidence from the Social Security Earnings Test. *National Tax Journal*. 67 (2), 443-58.

Ermisch, John. (1996a). The Demand for Housing in Britain and Population Ageing: Microeconometric Evidence. *Economica*. 63 (251), 383-404.

Ermisch, John. (1999). Prices, Parents, and Young People's Household Formation. *Journal of Urban Economics*. 45, 47-71.

Feinstein, Jonathan. (1993). The Relationship between Socioeconomic Status and Health: A Review of the Literature. *The Milbank Quarterly*. 71 (2), 279-322.

Figinski, Theodore. (2012). Women and the Social Security Earnings Test. Unpublished manuscript. University of California-Irvine, Irvine, CA.

Freeman, Richard. (1985). Unions, Pensions and Pension Funds. In *Pensions, Labor, and Individual Choice*, ed. David Wise; 89-122. Chicago: University of Chicago Press.

Friedberg, Leora. (1998). The Social Security Earnings Test and Labor Supply of Older men." *Tax Policy and the Economy*. 12, 121-50.

Friedberg, Leora. (2000). The Labor Supply Effects of the Social Security Earnings Test. *The Review of Economics and Statistics*. 82 (1), 48-63.

Friedberg, Leora, and Anthony Webb. (2009). New Evidence on the Labor Supply Effects of the Social Security Earnings Test. *Tax Policy and the Economy*. 23, 1-35.

Goldscheider, Frances and Julie DaVanzo. (1989). Pathways to Independent Living in Early Adulthood: marriage, Semiautonomy, and Premarital Residential Independence. *Demography*. 26 (4), 597-614.

Greene, William. (2010). Testing Hypotheses about Interaction Terms in Nonlinear Models. *Economic Letters*. 107, 291-6.

Greenwood, Micahel J. (1997). Internal Migration in Developed Countries, in *Handbook of Population and Family Economics*. Vol. 1B, edited by Mark R. Rosenzweig and Oded Stark. New York: North Holland. 647-720.

Grogger, Jeffrey. (2004). Welfare Transitions in the 1990s: The Economy, Welfare Policy, and the EITC. *Journal of Policy Analysis and Management*. 23(4), 671-95.

Gruber, Jonathan, and Brigitte Madrian. (1993). Health Insurance Availability and the Retirement Decision. Working paper 4469.

Gruber, Jonathan, and Brigitte Madrian. (2004). Health Insurance, Labor Supply, and Job Mobility: A Critical Review of the Literature. McLaughlin, Catherine (ed.) Health Policy and the Uninsured. Washington, D.C.: Urban Institute Press.

Gruber, Jonathan, and Peter Orszag. (1999). What to Do About the Social Security Earnings Test? Issue in Brief Number 1, Center for Retirement Research. July, 1999.

Gruber, Jonathan, and Peter Orszag. (2003). Does the Social Security Earnings Test affect Labor Supply and Benefits Receipt? *National Tax Journal*. 4(56), 755-73.

Gustman, Alan, and Thomas Steinmeier. (1984a). Partial Retirement and the Analysis of Retirement Behavior. *Industrial and Labor Relations Review*. 37 (3), 403-15.

Gustman, Alan L. and Thomas L. Steinmeier. (1985). The Effect of Partial Retirement on Wage Profiles of Older Workers. *Industrial Relations*, 24(2), 257-265.

Gustman, Alan, and Thomas Steinmeier. (2000). Retirement Outcomes in the Health and Retirement Study. *Social Security Bulletin.* 63 (4), 57-71.

Gustman, Alan, and Thomas Steinmeier. (2004). Understanding Patterns of Social Security Benefit Receipt, Pensions Incomes, Retirement and Saving by Race, Ethnicity, Gender and Marital Status: A Structural Approach. Working Paper 2004-082.

Hurd, Michael. (1996). The Effect of Labor Market Rigidities on the Labor Force Behavior of Older Workers. *In Advances in the Economics of Aging*, ed. David Wise; 11-58. Chicago: University of Chicago Press.

Ham, John and Lara Shore-Sheppard. (2005). The Effect of Medicaid Expansions for Low-Income Children on Medicaid Participation and Private Insurance Coverage: Evidence from the SIPP. *Journal of Public Economics*. 89(1), 57-83.

Ham, John, Xianghong Li, and Lara Shore-Sheppard. (2014). The Employment Dynamics of Disadvantaged Women: Evidence from the SIPP. Working paper available at: http://econweb.umd.edu/~ham/seam_bias_2014_03_17jh_ls_accept.pdf.

Haider, Steven and David Loughran. (2008). The Effect of the Social Security Earnings Test on Male Labor Supply: New Evidence from Survey and Administrative Data. *The Journal of Human Resources*. 43, 57–87.

Ham, John C. (1982). Estimation of a Labor Supply Model with Censoring Due to Unemployment and Underemployment. *The Review of Economic Studies*. 49 (3), 335-54.

Haurin, Donald, Patric H. Hendershott, and Dongwook Kim. (1993). The Impact of Real Rents and Wages on Household Formation. *The Review of Economics and Statistics*. 75 (2), 284-93.

Hill, Carolyn and Harry J. Holzer. (2007). Labor Market Experiences and Transition to Adulthood, in *The Price of Independence: The Economics of Early Adulthood*, edited by Sheldon Danziger and Cecilia Elena Rouse (New York: Russell Sage Foundation): 107–37.

Hoyne, Hilary Williamson. (1997). Does Welfare Play Any role in Female Headship Decisions? *Journal of Public Economics*. 65, 89-117.

Hurd, Michael. (1996). The Effect of Labor Market Rigidities on the Labor Force Behavior of Older Workers. *In Advances in the Economics of Aging*, ed. David Wise; 11-58. Chicago: University of Chicago Press.

Jaegar, David A. (1997). Reconciling the Old and New Census Bureau Education Questions: Recommendations for Researchers. *Journal of Business and Economic Statistics*. 15 (3), 300-09.

Jenkins, Stephen. (1995). Easy Estimation Methods for Discrete-Time Duration Models. *Oxford Bulletin of Economics and Statistics*. (57), 129-38.

Kahn, James. (1988). Social Security, Liquidity, and Early Retirement. *Journal of Public Economics*. 35, 97-117.

Kaplan, Greg. (2012). Moving Back Home: Insurance against Labor Market Risk. *Journal of Political Economy*. 120 (3), 446-512.

Krueger, Alan B. and Bruce D. Meyer. (2002). Labor Supply effects of Social Insurance. Handbook of Public Economics s, in: A. J. Auerbach & M. Feldstein (ed.), *Handbook of Public Economics*, edition 1, volume 4, chapter 33, 2327-92.

Krueger, Alan, and Jorn-Steffen Pischke. (1992). The Effect of Social Security on Labor Supply: A Cohort Analysis of the Notch Generation. *Journal of Labor Economics*. 10(4), 412-37.

Lee, Kwan Ok and Gary Painter. (2013). What happens to household formation in a recession? *Journal of Urban Economics*. 76, 93-109.

Lehrer, Evelyn L. (1992). The Impact of Children on Married Women's Labor Supply: Black-White Differentials Revisited. *The Journal of Human Resources*. 27 (3), 422-44.

Lemaître, Georges. (1992). Dealing with the Seam Problem for the Survey of Labor and Income Dynamics. SLID Research Paper Series, Np. 92-05. Ottawa: Statistics Canada.

Leonesio, Michael V. (1990). The Effects of the Social Security Earnings Test on the Labor-Market Activity of Older Americans: A Review of the Evidence. *Social Security Bulletin*. 53 (5), 2-21.

Liebman, Jeffrey B., and Erzo F. P. Luttmer. (2012). Would People Behave Differently if They Better Understood Social Security? Evidence from a Field Experiment. NBER Working Paper No. 17287. National Bureau of Economic Research, Cambridge, MA.

Lumsdaine, Robin, James Stock, and David Wise. (1996). Why Are Retirement Rates So High at Age 65? *Advances in the Economics of Aging*. David Wise ed. 61-82.

Madrian, Brigitte, and Nancy Beaulieu. (1998). Does Medicare Eligibility Affect Retirement? *Inquiries in the Economics of Aging.* David A. Wise ed. University of Chicago Press. 109-31.

Maestas, Nicole. (2010). Back to Work: Expectations and Realizations of Work after Retirement. *Journal of Human Resources*. 45 (3), 718-48.

Matsudaira, Jordan. (2015). Economic Conditions and the Living Arrangements of Young Adults: 1960 to 2011. *Journal of Population Economics*. 1-29.

Michaud, Pierre-Carl, and Arthur van Soest. (2008). How Did the Elimination of the Earnings Test above the NRA Affect Retirement Expectations? *Fiscal Studies* 29 (2), 197-231.

Mastrobuoni, Giovanni. (2006). Labor Supply Effects of the Recent Social Security Benefit Cuts: Empirical Estimates Using Cohort Discontinuities. CEPS Working Paper No. 136.

Mastrobuoni, Giovanni. (2009). Labor Supply Effects of the Recent Social Security Benefit Cuts: Empirical Estimates Using Cohort Discontinuities. *Journal of Public Economics*. 93(11-12), 1224-33

McElroy, Marjorie B. (1985). The Joint Determinants of Household Membership and Market Work: The Case of Young Men. *Journal of Labor Economics*. 3 (3), 293-316.

Mitchell, Olivia and Fields, Gary. (1982). The Effects of Pensions and Earnings on Retirement: A Review. *Research in Labor Economics*. (5), 115-55.

Moffitt, Robert. (1987). Life Cycle Labor Supply and Social Security: A Time Series Analysis. *In Work, Health and Income among the Elderly*, edited by Gary Burtless. Washington, D.C.: Brookings: 183-227.

Moffitt, Robert. (1992). Incentive Effects of the U.S. Welfare System: A Review. *Journal of Economic Literature*. 30(1), 1-61.

Moffitt, Robert. (1994). Welfare Effects on Female Headship with Area Effects. *The Journal of Human Resources*. 29(2), 621-36.

Moore, Jeffery C. (2008). Seam Bias in the 2004 SIPP Panel: Much Improved, but Much Bias Still Remains. U.S. Census Bureau Statistical Research Division Survey Methodology Research Report Series #2008-3. Available at https://www.census.gov/srd/papers/pdf/rsm2008-03.pdf.

Navratil, Frank and James J. Doyle. (1977). The Socioeconomic Determinants of Migration and The Level of Aggregation. Economics and Finance. *Southern Economic Journal*. 43, 1547-59.

Neumark, David J., J.M. Ian Salas, and William Wascher. (2014). Revisiting The Minimum Wage-Employment Debate: Throwing Out the Baby with The Bathwater? *Industrial and Labor Relations Review*, ILR Review, Cornell University, ILR School. 67(2.5), 608-48.

Neumark, David and William Wascher. (2007). Minimum Wages and Employment, *Foundations and Trends in Microeconomics* (January), 1–182.

Pingle, Jonathan. (2006). Social Security Delayed Retirement Credit and the Labor Supply of Older Men. Working Paper 2006-37, Washington, D.C.: Board of Governors of the Federal Reserve.

Quinn, Joseph, Richard Burkhauser, and Daniel Myers. (1990). *Passing the Torch: The Influence of Economic Incentives on Work and Retirement*. Kalamazoo, Mich.: Upjohn Institute for Employment Research.

Reimers, Cordelia, and Marjorie Honig. (1993). The Perceived Budget Constraint under Social Security: Evidence from Reentry Behavior. *Journal of Labor Economics*. 11 (1), 184-204.

Reimers, Cordelia, and Marjorie Honig. (1996). Responses to Social Security by Men and Women: Myopic and Far-Sighted Behavior. *Journal of Human Resources*. 31 (2), 359-82.

Rosenzeweig, Mark, and Kenneth Wolpin. (1993). Intergenerational Support and the Life-Cycle Incomes of Young Men and Their Parents: Human Capital Investments, Coresidence, and Intergenerational Financial Transfers. *Journal of Labor Economics*. 11 (1), 84-112.

Ruhm, Christopher. (1990). Bridge Jobs and Partial Retirement. *Journal of Labor Economics.* 8(4), 482-501.

Rust, John, and Christopher Phelan. (1997). How Social Security and Medicare Affect Retirement Behavior in a World of Incomplete Markets. *Econometrica*. 65(4), 781-831.

Shoven, John, and Sita Slavov. (2014). Does It Pay to Delay Social Security, *Journal of Pension Economics and Finance*. 13(2), 121-44.

“A Brief History”. Social Security Online. (September, 20, 2015). Page-17 http://www.ssa.gov/pubs/EN-21-059.pdf

Survey of Income and Program Participation. Users’ Guide. (2001). (August 4, 2016). http://www.census.gov/programs-surveys/sipp/guidance/users-guide.html

Survey of Income and Program Participation. Users’ Guide. (2009). (July 21, 2014). http://www.census.gov/programs-surveys/sipp/methodology/users-guide.html

"Social security Benefits. Normal Retirement Age." Social Security Online. (July 21, 2014). http://www.ssa.gov/oact/progdata/nra.html

"Social Security Fact Sheet. Delayed Retirement Credit." Social Security Online. (July 21, 2014). http://www.ssa.gov/pressoffice/DlyRetCrdtFactSheet.html

"Exempt Amounts Under the Earnings Test." Social Security Online. (July 21, 2014). http://www.ssa.gov/oact/cola/rtea.html

Tran, Bac. (2002). The Effect of the Repeal of the Retirement Earnings Test on the Labor Supply of Older Workers. Manuscript, University of Maryland.

Whittington, Leslie and Elizabeth Peters. (1996). Economic Incentives for Financial and Residential Independence. *Demography*. 33 (1), 82-97.

Wiemers, Emily. (2014). The Effect of Unemployment on Household Composition and Doubling Up. *Demography*. 51, 2155-78.

Wooldridge, Jefferey M. (2010). *Econometric Analysis of Cross Sectional and Panel Data*. 2nd edition. Cambridge, MA: MIT Press.

Yelowitz, Aaron S. (2007). Young Adults Leaving the Nest: The Role of the Cost of Living, in *The Price of Independence: The Economics of Early Adulthood*, edited by Sheldon Danziger and Cecilia Elena Rouse (New York: Russell Sage Foundation), 170-206.

Zabalza. A., C. Pissarides, and M. Burton. (1980). Social Security and the Choice Between Full-Time Work, Part-Time Work and Retirement. *Journal of Public Economics*. 14, 245-76.

APPENDIX A – RETIREMENT MODEL

Model A: Fully Anticipated Changes

Optimization of utility function (1) with respect to the budget constraint (2) results in the following first order conditions:

$$dz: \qquad U_{Wz}(C_z) = U_{Rz}(C_z) - \mu \left[W_z - B_z(z) + \int_z^D e^{r(z-t)} \frac{\partial B_t(z)}{\partial z} dt \right]$$

$$dC: \qquad \frac{\partial U_{Wt}(C_t)}{\partial C_t} = \mu \;\; if \;\; t < z \qquad and \qquad \frac{\partial U_{Rt}(C_t)}{\partial C_t} = \mu \;\; if \;\; t \geq z$$

The equilibrium is defined by the following two equations. The first equation is derived by simplifying the budget constraint in equation (2).

$$C = \frac{1 - e^{-rz}}{1 - e^{-rD}} W + \frac{e^{-rz} - e^{-rD}}{1 - e^{-rD}} R\,[1 + g(z - NRA)] \qquad (4)$$

$$\varepsilon C = W - R[1 + g(z - NRA)] + Rg\left(\frac{1}{r} - \frac{1}{r} e^{r(z-D)}\right) \qquad (5)$$

I first analyze the impact of a change in the delayed retirement credit (actuarial adjustment factor 'g'). As the changes in the actuarial adjustments were implemented only for individuals above their normal retirement age, in the analysis that follows I will focus on the case where z > NRA only.

Totally differentiating the two equations defining the equilibrium:

$$\begin{pmatrix} 1 & \frac{re^{-rz}}{1-e^{-rD}}\left[\left(1+g(z-NRA)\right)R-W\right]-gR\frac{e^{-rz}-\ e^{-rD}}{1-\ e^{-rD}} \\ \varepsilon & gR\left(1+\ e^{r(z-D)}\right) \end{pmatrix}\begin{pmatrix} dC \\ dz \end{pmatrix}$$

$$=\begin{pmatrix} \frac{e^{-rz}-\ e^{-rD}}{1-\ e^{-rD}}R(z-NRA) \\ -R(z-NRA)+R(\frac{1}{r}-\ \frac{1}{r}e^{r(z-D)}) \end{pmatrix}\ dg$$

Simplifying the above expression:

$$\frac{dz}{dg}$$

$$=\frac{R[(\varepsilon(e^{-rz}-\ e^{-rD})+1-\ e^{-rD})\ (z-NRA)-\frac{1}{r}\left(1-e^{r(z-D)}\right)(1-\ e^{-rD})]}{\Delta} \quad (6)$$

where $\Delta=gR\left[\left(1+e^{-r(-z+D)}\right)(-1+e^{-rD})+\varepsilon e^{-rz}\left(r(z-NRA)+e^{r(z-D)}-1\right)\right]$

$$-\ \varepsilon re^{-rz}(W-R)$$

Given that W > R, the sign of Δ depends on the terms $\left(r(z-NRA)+e^{r(z-D)}-1\right)$.

If, r > 0, then the term $e^{r(z-D)}-1$ < 0, and the sign of Δ depends only on the term $r(z-NRA)$. This term is always negative if z < NRA, which implies $\Delta<0$. But if z > NRA then $r(z-NRA)$ is negative if the date of death D is not close to the age of retirement and the interest rate is between (0,10) percent.

If $\Delta<0$ then the sign of $\frac{dz}{dg}$ depends on the numerator of equation (6). If z > NRA then the sign of the numerator is ambiguous because $-\frac{1}{r}\left(1-e^{r(z-D)}\right)(1-\ e^{-rD})<0$ for all r > 0, while $(\varepsilon(e^{-rz}-\ e^{-rD})+1-\ e^{-rD})\ (z-NRA)>0$ for all z > NRA.

Thus, the sign of $\frac{dz}{dg} \gtrless 0$ is ambiguous.

The model predicts that as the actuarial adjustment factor g is increased by one unit the impact on the optimal retirement age among individuals above their NRA is ambiguous.

The impact of an increase in the normal retirement age can be derived by totally differentiating equations (4) and (5).

$$\begin{pmatrix} 1 & \frac{re^{-rz}}{1-e^{-rD}}\left[\left(1+g(z-NRA)\right)R-W\right]-gR\frac{e^{-rz}-\ e^{-rD}}{1-\ e^{-rD}} \\ \varepsilon & gR\left(1+\ e^{r(z-D)}\right) \end{pmatrix} \begin{pmatrix} dC \\ dz \end{pmatrix}$$

$$= \begin{pmatrix} -gR\ \frac{e^{-rz}-\ e^{-rD}}{1-\ e^{-rD}} \\ gR \end{pmatrix} dNRA$$

Simplifying the above expression

$$\frac{dz}{dNRA} = \frac{gR[-\varepsilon(e^{-rz}-\ e^{-rD})-1+\ e^{-rD}]}{\Delta} \qquad (7)$$

The numerator in equation (7) is always negative, so the sign of $\frac{dz}{dNRA}$ depends on the sign of Δ.

If $\Delta < 0$, then $\frac{dz}{dNRA} > 0$

Thus, under the conditions specified above which ensure that $\Delta < 0$, an increase in the normal retirement age raises the optimal retirement age.

Model B: Unanticipated Changes

If the changes in social security policy variables are unanticipated, and the worker learns about these changes at time z then the decision problem faced by an individual after time z can be modified as follows:

$$\max_{z',C_t'} V(z) = \int_z^{z'} e^{-rt} U_W(C_t') dt + \int_{z'}^{D} e^{-rt}\, U_R(C_t') dt \tag{8}$$

subject to the budget constraint

$$\int_0^z e^{-rt}\, C_t dt + \int_z^D e^{-rt}\, C_t' dt = \int_0^{z'} e^{-rt} W_t dt + \int_{z'}^{D} e^{-rt} B_t dt \tag{9}$$

The following two equations define the equilibrium when the actuarial adjustment factor g changes to g'. The first equation is derived by simplifying the budget constraint in equation (9).

$$C(1 - e^{-rz}) + C'(e^{-rz} - e^{-rD}) = \left(1 - e^{-rz'}\right)W + R\left(e^{-rz'} - e^{-rD}\right)$$

$$[1 + g'(z' - NRA)]$$

$$\varepsilon C' = W - R[1 + g'(z' - NRA)] + Rg'\left(\frac{1}{r} - \frac{1}{r}\, e^{r\left(z' - D\right)}\right)$$

To derive how an unanticipated change in the actuarial adjustment factor g' impacts the optimal retirement age, I totally differentiate the two equations defining the equilibrium.

$$\begin{pmatrix} 1 & \frac{-re^{-rz'}}{e^{-rz} - e^{-rD}} W + \frac{re^{-rz'}}{e^{-rz} - e^{-rD}} \left[1 + g'^{(z'-NRA)}\right] R - g'R \frac{e^{-rz'} - e^{-rD}}{e^{-rz} - e^{-rD}} \\ \varepsilon & g'R\left(1 + e^{r(z-D)}\right) \end{pmatrix}$$

$$\begin{pmatrix} dC' \\ dz' \end{pmatrix}$$

$$= \begin{pmatrix} \frac{e^{-rz'} - e^{-rD}}{e^{-rz} - e^{-rD}} R(z' - NRA) \\ -R(z' - NRA) + R(\frac{1}{r} - \frac{1}{r} e^{r(z\prime - D)}) \end{pmatrix} dg'$$

Simplifying the above expression

$$\frac{dz'}{dg'} = \frac{R\left[(z' - NRA)\left(\varepsilon\left(e^{-rz'} - e^{-rD}\right) + e^{-rz} - e^{-rD}\right)\right]}{\Delta'}$$

$$\frac{-R\,\frac{1}{r}\left(1 - e^{r\left(z'-D\right)}\right)\left(e^{-rz} - e^{-rD}\right)}{\Delta'} \quad (10)$$

where $\Delta' = g'R\ [\left(1 + e^{r(z-D)}\right)\left(e^{-rD} - e^{-rz'}\right) + \varepsilon e^{-rz'}$

$$\left(r(z - NRA) + e^{-r\left(D-z'\right)} - 1\right)\Big] - \varepsilon r e^{-rz}(W - R)$$

Given that W > R, the sign of Δ' depends on the terms $\left(r(z - NRA) + e^{-r(D-z\prime)} - 1\right)$.

$\Delta' < 0$ under the same conditions that ensured $\Delta < 0$ in Model A.

If $\Delta' < 0$ then the sign of $\frac{dz\prime}{dg\prime}$ depends on the numerator of equation (10). If z' > NRA then the sign of the numerator is ambiguous because $-\frac{1}{r}\left(1 - e^{r\left(z'-D\right)}\right)\left(e^{-rz} - e^{-rD}\right) < 0$ for all r > 0, while $(z' - NRA)\left(\varepsilon\left(e^{-rz'} - e^{-rD}\right) + e^{-rz} - e^{-rD}\right) > 0$ for all z > NRA.

Thus, the sign of $\frac{dz\prime}{dg\prime} \gtrless 0$ is ambiguous.

To derive how an unanticipated change in the normal retirement age NRA' impacts the optimal retirement age, I again totally differentiate the two equations defining the equilibrium.

$$\begin{pmatrix} 1 & \frac{-re^{-rz'}}{e^{-rz} - e^{-rD}} W + \frac{re^{-rz'}}{e^{-rz} - e^{-rD}} \left[1 + g(z' - NRA')\right] R - gR \frac{e^{-rz'} - e^{-rD}}{e^{-rz} - e^{-rD}} \\ \varepsilon & g'R\left(1 + e^{r(z-D)}\right) \end{pmatrix}$$

$$\begin{pmatrix} dC' \\ dz' \end{pmatrix}$$

$$= \begin{pmatrix} -gR \frac{e^{-rz'} - e^{-rD}}{e^{-rz} - e^{-rD}} \\ gR \end{pmatrix} dNRA'$$

Simplifying the above expression

$$\frac{dz'}{dNRA'} = \frac{gR\left[-\varepsilon\left(e^{-rz'} - e^{-rD}\right) - e^{-rz} + e^{-rD}\right]}{\Delta'} \qquad (11)$$

The numerator in equation (11) is always negative, so the sign of $\frac{dz\prime}{dNRA\prime}$ depends on the sign of Δ'.

If $\Delta' < 0$, then $\frac{dz\prime}{dNRA\prime} > 0$

The above models highlight that regardless of whether the social security policy changes were anticipated or unanticipated by the individuals, the change in the normal retirement age (under certain conditions) raises the optimal retirement age, while the change in the delayed retirement credit for individuals above their normal retirement age has an ambiguous effect on the optimal retirement age.

APPENDIX B – LABOR SUPPLY MODELS BY SUBGROUPS

Table B.1 Table of Acronyms

Acronym	
NRA	Normal Retirement Age
DRC	Delayed Retirement Credit
SIPP	Survey of Income and Program Participation
SSA	Social Security Administration

Table B.2 Estimates of Models for Older Men's Labor Supply By Education: Age 62 - 74

	More than high school		High school or less	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: Labor Force Participation				
Earnings test in place dummy	-0.097*	-1.437	-0.059	-1.898**
	(0.053)	(0.983)	(0.040)	(0.913)
Earnings test in palce * (Delayed retirement credit - 6.2) * Within age range directly affected by delayed retirement	-0.055	-0.608	-0.036	-1.029
	(0.044)	(0.968)	(0.042)	(1.051)
Earnings test in place * (Delayed retirement credit - 6.2)	-0.015	-0.634	-0.007	-0.300
	(0.020)	(0.562)	(0.023)	(0.618)
(Delayed retirement credit - 6.2) * Within age range directly affected by delayed retirement credit	0.013	0.034	0.014	0.204
	(0.025)	(0.631)	(0.028)	(0.690)
Number of observations	76,509	17,114	70,063	14,540
Number of individuals	10772	1801	10351	1593
B: Full-time relative to Part-time work				
Earnings test in place dummy	-0.169*	-1.238	-0.028	-0.513
	(0.099)	(1.069)	(0.096)	(1.053)
Earnings test in palce * (Delayed retirement credit - 6.2) * Within age range directly affected by delayed retirement	-0.198**	-1.702*	0.018	-0.093
	(0.090)	(0.963)	(0.096)	(1.074)
Earnings test in place * (Delayed retirement credit - 6.2)	0.130**	1.237**	-0.019	-0.270
	(0.052)	(0.567)	(0.062)	(0.702)
(Delayed retirement credit - 6.2) * Within age range directly affected by delayed retirement credit	0.111*	0.957	-0.030	-0.145
	(0.058)	(0.633)	(0.067)	(0.749)
Number of observations	30,600	12,766	19,064	7,869
Number of individuals	5323	1618	3680	1074

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies, and age-specific trends.
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table B.3 Estimates of Models for Older Men's Labor Supply By Wealth: Age 62 - 74

	Greater than or equal to median net wealth		Less than median net wealth	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
	A: Labor Force Participation			
Earnings test in place dummy	-0.018	-0.632	-0.039*	-0.996
	(0.024)	(0.685)	(0.021)	(0.632)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.029*	-0.812**	-0.008	-0.441
	(0.016)	(0.405)	(0.016)	(0.457)
Number of observations	54,361	9,480	54,356	8,992
Number of individuals	9161	1259	9826	1222
	B: Full-time relative to Part-time work			
Earnings test in place dummy	-0.044	-0.130	-0.004	0.203
	(0.052)	(0.610)	(0.048)	(0.605)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	0.051	0.599	-0.014	-0.144
	(0.034)	(0.394)	(0.035)	(0.430)
Number of observations	17,872	6,506	17,870	6,297
Number of individuals	3423	988	3842	1038

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies, and age-specific trends .
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

Table B.4 Estimates of Models for Older Women's Labor Supply By Wealth: Age 62 - 74

	Greater than or equal to median net wealth		Less than median net wealth	
Model	Linear-FE	Conditional Logit	Linear-FE	Conditional Logit
A: Labor Force Participation				
Earnings test in place dummy	-0.035*	-0.947	-0.021	-0.750
	(0.020)	(0.657)	(0.019)	(0.614)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.009	-0.688*	-0.015	-0.819**
	(0.014)	(0.414)	(0.014)	(0.416)
Number of observations	66,201	10,185	66,193	8,863
Number of individuals	11089	1322	11896	1178
B: Full-time relative to Part-time work				
Earnings test in place dummy	-0.138**	-1.171	0.001	0.013
	(0.055)	(0.727)	(0.046)	(0.700)
Delayed retirement credit (%) * Within age range directly affected by delayed retirement credit	-0.020	-0.080	0.018	0.176
	(0.036)	(0.459)	(0.039)	(0.542)
Number of observations	15,389	5,380	15,386	4,919
Number of individuals	2993	824	3280	781

All models include controls for age 62-NRA dummy, at normal retirement age dummy, marital status, age, state unemployment rate, home ownership, number of household members, children under age 18, region, age, and quarter dummies, and age-specific trends .
Cluster robust standard errors are reported in parentheses, where the clustering is done by individuals.
*** p<0.01, ** p<0.05, * p<0.1

APPENDIX C – MEASURES OF LOCAL LABOR AND RENTAL MARKETS AND DISCUSSION OF WELFARE REFORM

C.1 Measures of Local Labor Market Conditions

I estimate these average age-group, education, and gender specific unemployment rate for each state over time from the basic monthly CPS data. I first categorized young individuals in the CPS into two age groups, those ages 20 to 24 and those ages 25 to 29. I also classified individuals into two categories based on education, those who completed high school or less and those who completed some level of education beyond high school. Based on these groupings I then estimate age-group, education, and gender specific employment-population ratio and unemployment rate. Since I include observations for only the fourth reference month from the SIPP, I average the group-specific employment-population ratio and unemployment rate measures created from the CPS data over the four-month period directly preceding the fourth reference month of the SIPP data. This allows the employment-population ratio and the unemployment rate measures to incorporate the average circumstances in the young person's state of residence during the four months prior to the month for which he or she provides information about living arrangements.

I also make use of CPS data to estimate the state level average unemployment rates for individuals age 35 to 60 and interpret this measure as potentially the unemployment rates for parents. This measure assumes that parents live in the same state as the young adult. In the descriptive analysis I see very few young people moving across states, which suggests that for individuals at risk of returning or leaving home this measure of economic conditions for parents may be adequate.

C.2 Specification of Static State Panel Analysis

I perform the aggregate-level analysis with the following specification:

$$
\begin{aligned}
Y_{st} = \ & \theta_1 \, Aggregate\ unemployment\ rate\ for\ young\ adult_{st} \\
& + \theta_2 \, Aggregate\ unemployment\ rate\ for\ parents_{st} \\
& + \theta_3 \ln(Annual\ median\ fair\ market\ rent_{st}) \\
& + \theta_4 \ln(Minimum\ wage_{st}) + \theta_5 \, X_{st} + \theta_6 \, Age_{st} + \delta_s + \tau_t + \psi_s \, t \\
& + u_{st}
\end{aligned}
$$

where the dependent variable Y_{st} represents the proportion of young individuals in a state who live with their parents.

There are two main differences between the disaggregated individual level analysis and the aggregated analysis. First, in the aggregated state panel analysis I am unable to make full use of the variation in the average age-group, education, and gender specific unemployment rate of young people. Using data from the CPS, I average the estimated unemployment rate measure used in the individual data over gender and education to obtain an aggregate measure reflecting the average state level unemployment rates for young people 20 to 29 years old. I also estimate the aggregate unemployment rate for individuals 35 to 60 years old in a similar manner. I control for the effect of average age of young adults, as this can vary over time across states. Second, I am unable to control for detailed demographic controls for young adults.

C.3 Fair Market Rents

I estimate the effect of rental costs by using the median fair market rents provided by the U.S. Department of Housing and Urban Development (HUD). The HUD annually estimates fair market rents for 530 metropolitan and 2,045 nonmetropolitan FMR areas. I use these rent data for FMR areas to estimate the average annual median fair market rent by state.[1] The fair market rents are gross rent estimates, that is, they include the shelter rent plus the cost of all tenant-paid utilities except telephone, cable or satellite television, and internet. In preparing the fair market rent estimates the HUD uses rent data for

[1] Yelowitz (2007) also uses the FMR data for his measure of rental costs. His measure differs from mine in two ways. First, he uses MSA level rents, and second he uses the 40th percentile rents, while I make use of the 50th percentile rents that became available beginning in 2000.

typical non-substandard rental units that are occupied by renters who moved to their current residence within the last 15 to 22 months.[2]

The HUD relies on three sources of survey data to develop the fair market rent base year estimates: the decennial Census, the American Community Survey (ACS), and Random Digit Dialing (RDD) telephone surveys. Because of lags in processing the survey data, most Census and ACS based data are put into effect approximately 3 years after they are collected. To account for this lag, the base year estimates are updated using CPI (FMR or regional) data and trended forward by applying a national trend factor based on forecasts of gross rent changes. The trend factor projects the estimates forward to the midpoint of the fiscal year for which the fair market rents are being calculated, but there is a lag of fifteen months in the use of most recent CPI and the midpoint of the fair market rent. Therefore, in the regression analysis my specifications include a lagged measure of fair market rents.

Fair market rents are expressed as a percentile point within the distribution of standard-quality rental units; the current FMR definition uses the 40th percentile rents for most areas, but some areas are assigned 50th percentile rent. Fiftieth percentile FMRs were introduced by a rule announced in 2000 that also set the eligibility criteria that would be used to select and assign 50th percentile rather than the 40th percentile FMRs to certain areas. The rule also stated that areas assigned 50th percentile FMRs were to be re-evaluated after three years, and that the 50th percentile rents would be cancelled if the areas did not meet certain requirements. Beginning in 2001, rents at the 50th percentile are also calculated and made available for all FMR areas. Because the 40th percentile rents are not consistently available for all FMR areas, I make use of the 50th percentile rent data. For the years 2001 to 2013, I use these median rent estimates to create my measure of rental costs in each state. The 50th percentile fair market rents, however, are not available for years 1996 to 2000. Since for the fiscal year 2001, both the 40th and the 50th percentile rents are available, I use this information to convert the FMRs from 1996 to 2000 into the 50th percentile. I make this conversion by multiplying the FMR from

[2] The following units are excluded: public housing, rental units built in last two years, rental units considered substandard in quality, seasonal rentals, and rental units on ten or more acres.

each of these years with the corresponding ratio of rents in the 50th and 40th percentile from 2001.

The next step is to use the 50th percentile FMRs to estimate the state level median rents. The fair market rent estimates are made available by FMR area, county, or county subarea (New England). I use U.S. census data to estimate population weighted state level median rents. With the exception of New England area, I estimate state level medial rents by averaging the population weighted median rent across counties in the state. For new England, I first calculate the population weighted average median rent across county subareas, and then use these estimates to further calculate the population weighted median rent across counties in the New England states. Finally, I adjust the annual rents for inflation by converting them to constant 2013 dollars using the CPI-U.

C.4 1996 Welfare Reform

Until 1996, the Aid to Families with Dependent Children (AFDC) was the major welfare program serving single women with children; welfare benefits were not available to women without children and were unavailable or more difficult to obtain if a woman was married.[3] Previous researchers have observed that a single mother living with other relatives such as parents faced greater difficulty in qualifying for benefits. The implication was that by providing a large subsidy for single mothers who headed their own households, AFDC benefits exerted an unambiguous increasing effect on household headship by single mothers.[4] The Personal Responsibility and Work Opportunity

---

[3] Blau et al. (2004). Moffitt (1994) also notes that although, the Aid to Families with Dependent Children-Unemployed Parent (AFDC-UP) program has provided benefits for married families, eligibility is quite restrictive and only about 7 percent of AFDC caseloads consist of such families.

[4] In a comprehensive study Ellwood and Bane (1985) explore the effect of welfare benefits on family structure and living arrangements employing various datasets and methods of investigation. Using their preferred specification, which focuses on within-state comparison of women with different probabilities of AFDC receipt, they find that higher welfare benefits encourage single mothers to form their own households instead of living with relatives (parents). Moffitt (1992) notes that although the studies from 1980s show slightly stronger evidence regarding the effect of welfare benefits on female headship decisions, the effects are small in magnitude. Studies in the 1990s and 2000s have included state fixed effects (Moffitt (1994)), individual fixed effects (Hoynes (1997)), and MSA fixed effects and MSA specific time trends (Blau et al. (2004)) to assess the robustness of findings from the earlier studies conducted in the 1970s and 1980s; no clear consensus emerges from this new set of studies. Moffitt (1994) finds that including state fixed effects reduces the magnitude of the welfare effect on female headship

Reconciliation Act of 1996 substantially reformed the U.S. welfare system, the AFDC program was replaced by the Temporary Assistance for Needy Families (TANF) program which eliminated unrestricted family entitlement to cash assistance, increased work requirements for welfare recipients, and placed time limits on total benefit receipt. [5] It is possible that the welfare reform of 1996 may have influenced the living arrangements of single mothers in my sample.

---

for whites and the estimated coefficients become statistically insignificant, but in some of his specifications with state fixed effects he continues to find a positive statistically significant effect of welfare benefits for black women. After controlling for individual fixed effects, Hoynes (1997) is unable to find any significant relationship between welfare benefits and female headship for either whites or blacks. Blau et al. (2004) conclude that for black women, particularly the less educated, limiting welfare benefits will raise the likelihood of single mothers living with their relatives (parents).

[5] Blank (1997).

Table C.1 Estimates from Individual-Level Analysis of Living Arrangements (Without Individual Fixed Effects)

Linear Probability Model Dependent Variable: Whether Young Adult Lives with Parents						
Covariates	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment rate for ages 20 to 29	-0.059	-0.054	0.016			
	(0.099)	(0.101)	(0.177)			
2008-2013 dummy * unemployment rate for ages 20 to 29			-0.112			
			(0.191)			
Employment-population ratio for ages 20 to 29				-0.007	-0.015	-0.015
				(0.071)	(0.072)	(0.071)
2008-2013 dummy * employment-population ratio for ages 20 to 29						0.011
						(0.043)
Unemployment rate for prime-age adults		-0.026	0.022		-0.079	-0.108
		(0.222)	(0.233)		(0.217)	(0.251)
Annual median fair market rent in state (in logs)	0.026	0.027	0.028	0.020	0.024	0.024
	(0.091)	(0.092)	(0.092)	(0.090)	(0.092)	(0.092)
Minimum wage (in logs)	0.059	0.059	0.061	0.059	0.059	0.057
	(0.044)	(0.045)	(0.045)	(0.044)	(0.045)	(0.046)
p-Value: joint test for no effect of state-specific trend terms	0.0	0.0	0.0	0.0	0.0	0.0
Observations	193,996	193,996	193,996	193,996	193,996	193,996
Number of clusters	46	46	46	46	46	46

Note: All sepecifications include controls for average per capita income in state, race, gender, educational attainment, age-year, quarter, and state dummies, and linear state trends. The analysis includes young adults 20-29 who are single, and not enrolled in school at the time they are first observed in sample. I focus on individuals who do not change their state of residence when they change their living arrangements. Cluster robust standard errors in parentheses, where the clustering is done by state *** p<0.01, ** p<0.05, * p<0.1