An Empirical Evaluation of Recent Social Security Reforms

Giovanni Mastrobuoni

A Dissertation
Presented to the Faculty
of Princeton University
in Candidacy for the Degree
of Doctor of Philosophy

Recommended for Acceptance
by the Department of
Economics

September 2006

Abstract

In 1995, the Social Security Administration started sending the annual Social Security Statements to workers. It contains information about the worker's estimated benefits at the ages 62, 65, and 70. In the first chapter, I use this unique natural experiment to analyze retirement and claiming decision making. Despite the previous availability of this information, the statement has a significant impact on workers' knowledge about their benefits. These findings are consistent with a model of costly information. I use this exogenous variation in knowledge to analyze the optimality of workers' retirement decisions. Before the Statement was introduced to uninformed workers, who are more likely to be low–educated and black, made, on average, worse retirement decisions. The estimated Social Security benefits contained in the Statement appears to have helped low–educated workers, but not black workers.

In response to an earlier "crisis" in Social Security financing two decades ago, the Congress implemented an increase in the normal retirement age (NRA) of two months per year for cohorts born in 1938 and afterward. These cohorts reached retirement age in 2000, and in the second chapter, I study the effects of these benefit cuts on recent retirement behavior. The evidence strongly suggests that the mean retirement age of the affected cohorts has increased by around half as much as the increase in the NRA. If these increases in work effort by older workers continue, it will have extremely important implications for the estimates of Social Security trust fund exhaustion that have played such a major role in recent discussions of Social Security reform.

Beneficiaries of Social Security face restrictions on how much they can earn without incurring the earnings test (ET). In 2000, President Clinton eliminated the ET between age 65 and 70. In the last chapter I evaluate how this removal impacts the long–term finances of the Trust Fund. I find that the Social Security Administration in the long run is actually saving money and that the removal appears to be Pareto–efficient. A removal of the remaining part of the ET is likely to be even less costly

and to produce larger increases in labor supply and contributions.

Acknowledgements

I would like to thank Orley Ashenfelter for his continuous support. Wioletta Dziuda has been of invaluable help. I would also like to thank Bo Honoré, Alan Krueger, Franco Peracchi, and Harvey Rosen and all participants at the Princeton Labor seminar, the Princeton Public Finance seminar, the 2006 SOLE conference, and the Office of Research seminar at the Social Security Administration, in particular Jane Fortson, Radha Iyengar, Joyce Manchester, Nisreen Salti, Martin Schonger, Perry Singleton, Matt Weinberg, and Luke Willard. A special thanks goes to Jae Song.

Dedicated to Wiola and to my parents.

Contents

Abstract iii

Acknowledgements v

1 Do better–informed workers make better retirement choices? A test based on the Social Security Statement 1

1.1 Introduction 1

1.2 Data 7

1.3 The Social Security Statement 9

1.4 Workers' knowledge about their benefits and the Statement 11

1.4.1 Modeling the optimal time for getting informed 12

1.4.2 The effect of the Statement on workers' knowledge about retirement benefits 16

1.5 The effect of the Statement on retirement and Social Security benefit claiming decisions 26

1.5.1 The effect of the Statement on workers' expected claiming behavior 28

1.5.2 Social Security incentives 30

1.5.3 The effect of the Statement on claiming and retirement behavior 34

1.6 Conclusions 47

Bibliography 52

.1 Summary statistics 53

2 Labor Supply Effects of the Recent Social Security Benefit Cuts: Empirical Estimates Using Cohort Discontinuities **60**

2.1 Introduction 60

2.2 A simple intertemporal model of retirement 63

2.3 Empirical Strategy 64

2.4 Estimation Results 70

2.4.1 Alternative explanations 72

2.5 Conclusions 73

Bibliography 77

.1 Data 78

.2 The inter–temporal model or retirement 79

3 The Social Security Earnings Test Removal. Money Saved or Money Spent by the Trust Fund? **93**

3.1 Introduction 93

3.2 The impact of the ETR on the Trust Fund 94

3.2.1 Changes in claiming behavior 95

3.2.2 The Social Security Wealth 99

3.2.3 The long–term budgetary impact 103

3.3 Conclusions 108

Bibliography 111

.1 Data 112

.2 How the “65 spike” became the “NRA spike” 112

.3 Cohort-specific annuity tables 113

Chapter 1

Do better–informed workers make better retirement choices? A test based on the Social Security Statement

1.1 Introduction

Social Security is the largest expenditure program in the United States. The 70-year-old system provides more than half of the income for two thirds of the elderly population. For 34 percent of them, Social Security benefits represent 90 percent of their income (SSA, 2004). Due to demographic changes, the Social Security system faces a fiscal imbalance and is in urgent need of reform. This is clearly acknowledged by the Social Security Administration (SSA) in the Statement that is sent yearly to all workers:

> "Your estimated benefits are based on current law. Congress has made changes to the law in the past and can do so at any time. The law gov-

erning benefit amounts may change because, by 2042, the payroll taxes collected will be enough to pay only about 73 percent of scheduled benefits."

While reforms are necessary, the nature of these reforms is a subject of controversy. In order to evaluate different proposals, it is critical to understand how people make their retirement decisions.

Standard economic theory assumes that agents base their retirement decisions on forward-looking variables, such as the present discounted value of the agents' Social Security benefits (the income effect) and its changes due to working an additional year (the substitution effect). Hurd (1990) and Krueger and Meyer (2002) provide a comprehensive survey of studies that have tried to measure these effects.

The income effect due to an increase in the present discounted value of Social Security benefits, called the Social Security wealth (SSW) should induce early retirement. Numerous empirical studies have found this effect. There is no consensus, however, on the size of the effect, in other words on how much of the trend towards lower labor force participation is attributable to the expanding Social Security system and how much to changes in preferences for leisure. The main empirical issue is that Social Security is a federal program, and thus any cross-sectional variation in benefits arises from cross-sectional variation in life-time earnings, marital status, and number of dependents, and all these factors may, as well, have independent effects on labor supply decisions (Krueger and Meyer, 2002).[1]

Postponing retirement by one year can generate considerable changes in SSW (the SSW accrual). Positive accruals generate an incentive to work, though the size of this effect, the substitution effect, has been disputed as well. There are two pronounced

[1]Krueger and Pischke (1992) try to overcome this problem using "double-indexing" for the "notch-generation," a generation that faced sudden great reductions in SSW. The authors note that labor force participation continued falling during this time, casting doubt on previously estimated income effects.

retirement rate spikes: at the early retirement age (ERA) and at the normal retirement age (NRA). Around 60 percent of people claim their Social Security benefits at the age of 62, and among those who do not claim before age 65, 80 percent claim at age 65. Some factors can partially explain this clustering: large disutility from work, large discount rate (ERA spike), and discontinuities in the actuarial adjustment rates (NRA spike). However, as pointed out by Panis et al. (2002) and Lumsdaine et al. (1996), most structural models are unable to account for the size of these spikes. One plausible explanation for the existence of spikes is provided by Phelan and Rust (1997), who attribute part of the 62-spike to liquidity constraints and part of the 65-spike to lock-in effects due to Medicare when workers lack alternative health insurance in retirement.

Their explanation is at odds, however, with the evidence from the 1961 change in the early retirement age from 65 to 62. While the ERA has changed suddenly, the spike in retirement has moved very slowly (over 30 years, Burtless, 1999). Based on this evidence, Axtell and Epstein (1999) suggest that spikes may not be entirely the product of rational decision making but resemble some herd behavior. Additional support for a behavioral explanation of the spikes is provided by the recent increase in the NRA suggested by the 1983 Greenspan Commission. Mastrobuoni (2005a) shows that the entire 65-spike at which the workers claim their Social Security benefits moved together with NRA. This contradicts the Medicare explanation as the Medicare eligibility at age 65 remained unchanged.[2]

Economic models of retirement implicitly assume that workers know their future benefits as a function of their retirement age and are able to compare future streams of benefits. Empirical evidence, however, suggests that these are strong assumptions. When asked, only around 50 percent provide an estimate of their expected

[2]The Social Security Statement contains the advice that, "even if you do not retire at age 65, be sure to contact Social Security three months before your 65th birthday to enroll in Medicare."

Social Security benefits (Bernheim and Levin 1989 , Gustman and Steinmeier 2001).[3] Gustman and Steinmeier show that less than 30 percent of respondents are able to estimate their future benefits to within about $1,500 per year. Moreover, Lusardi and Mitchell (2006) show that financial illiteracy is widespread among older Americans. Only half of the age 50+ respondents can correctly answer two simple questions regarding interest compounding and inflation. Is it then reasonable to assume those same respondents are able to compute their retirement incentives, which typically involve relatively complex calculations?

Despite very little knowledge about retirement incentives, the fact that people seem to respond to incentives when making their retirement decisions has been called by Chan and Stevens (2003) an "important empirical puzzle in the retirement literature."

Gustman and Steinmeier try to test the robustness of retirement models when a measure of knowledge about benefits is added to the retirement regression. They find that knowledge does not affect workers' responsiveness to incentives. Chan and Stevens go one step further and analyze how the interaction of knowledge and accruals affects workers' decisions. The authors find that the responsiveness to pension incentives is entirely driven by the 20 percent of workers who perceive them correctly.[4] The validity of using measures of knowledge in the regressions, however, is questionable as knowledge is endogenous: workers gather information when they approach their expected retirement age.

I make use of a unique natural experiment to shed light on these issues: In 1995, the Social Security Administration started sending out the annual Social Security

---

[3]In my data that focuses on workers aged 55 and above the 2/3 of workers are able to provide an estimate.

[4]They do not find any link between knowledge and Social Security incentives, which they consider a result of data limitations. The first limitation is that they can measure if workers correctly perceive their Social Security benefits, but not if they correctly perceive their Social Security accruals. The second limitation is that the match between the Health and Retirement Survey and the administrative records is available only up to the 1992 survey year, and is likely to introduce measurement error in the benefit calculations for the subsequent years.

Statement. The Statement is a concise, easy-to-read personal record of past earnings and a summary of the estimated benefits for the worker and his or her family as a function of his or her retirement age. The Statement has been sent out in phases, starting with workers who were 60 years and older. In later years it has been sent according to the following (year,age) combinations: (1996, 58+), (1997, 53+), (1998, 47+), (1999, 44+), (2000, 25+).

The introduction of the Statement provides an exogenous source of variation in the information about Social Security benefits. This change is used to analyze workers' retirement and claiming decisions. First, I model how workers gather information about their Social Security benefits. The empirical evidence is consistent with a model of retirement where information is costly. The Statement allows us to look at the effect of moving from a system in which information is freely available, but the worker has to show some initiative and either call the SSA or learn the Social Security benefit rules to know about the Social Security incentives he or she faces, to a system where the cost of gathering information is basically zero. I show that these two systems produce significantly different levels of knowledge.[5] I identify workers who know little or nothing about their future Social Security benefits before they receive the Statement and find that they benefit the most from the information contained in the Statement. I find that, for these workers, the effect of the Statement on knowledge is strong even when they are close to their retirement date. Respondents from the Health and Retirement survey are less likely to say that they do not know their benefits and their expected benefits are closer to the actual benefits that they end up getting in later waves. Uninformed workers, though, are a very selective sample of the population. In order to value the information, workers need to be able to

[5] Duflo and Saez (2003) is similar in spirit to my analysis in that it also deals with the endogeneity problem of information. The authors use a randomized experiment to study the role of information in the employees' decisions to enroll in a Tax Deferred Account retirement plan. They conclude that "the important decision about how much to save for retirement can be affected by small shocks such as a very small financial reward and/or the influence of peers, and thus does not seem to be the consequence of an elaborate decision process."

use the information and need to be free to choose their retirement age. It is known that workers who face health problems or are liquidity constraints tend to retire as soon as possible. Consistent with this, I find that wealthier and healthier workers are significantly more likely to get informed. I find that even after controlling for labor market experience, occupation, wealth, and health, black workers and workers with low levels of education are significantly less likely to know their benefits. One possible explanation for this persistent gap is that these workers are also more likely to be financially illiterate (Lusardi and Mitchell, 2006).

Later, I measure how the additional information about Social Security incentives affects retirement and claiming behavior. I look at changes in workers' expectations about their claiming age, and I find only limited evidence that receiving the first Statement generally induces some workers to update their expectations.

Then, I use the exogenous variation in information to test whether retirement and claiming decisions become more sensitive to Social Security incentives. Workers who are not well informed before receiving the Statement, namely blacks and low educated workers, are also the ones for whom Social Security accruals play the smallest role in claiming decisions. But this is not necessarily inconsistent with the theory, because those workers are also more likely to be liquidity constraint and in bad health.

The introduction of the Statement, instead, generates mixed results. Low educated workers show a small and insignificant increase in the responsiveness to the Social Security incentives, but black workers show, because of early claiming, a large and significant reduction. One possible explanation for this is that upon receiving a Statement black workers, who tend to have a significantly lower life–expectancy, realize that SSA's actuarial adjustment is not large enough to be actuarially fair for them.

Two other findings are puzzling, namely that: 1) workers whose spouse is eligible to receive dependent benefits become more likely to take these additional benefits into consideration when deciding about retirement (this may be due to the lack of information about the existence of spouse's and survivor's benefits, an additional information contained in the Statement); 2) workers aged 62 and 65 become less sensitive to Social Security Incentives. Age 62 and 65 are the two ages at which the retirement benefits are reported in the Statement. This is puzzling and suggests that some people retiring at 62 and 65 make this decision based on simple rules of thumb and not Social Security incentives.

Summing up, it seems that for some groups, namely low–educated workers the lack of knowledge is the product of a maximization process, while for others, mostly blacks, lack of knowledge is more difficult to be justified.

1.2 Data

I use the Health and Retirement Survey (HRS) to evaluate how the Statement affects workers' knowledge about their future benefits, and to evaluate what determines whether workers are informed even before receiving the Statement. Later I use the Survey of Income and Program Participation (SIPP) to evaluate the effect of the Statement on retirement decisions.

The HRS is a longitudinal, biennial, nationally representative survey of older Americans. I use waves 1 to 6 (1992–2002), and restrict the analysis to workers older than age 55 who are not receiving Social Security disability benefits. I also use a special module added to the 2004 survey to analyze financial literacy. To measure the actual effect on retirement decisions, I use the 1990, 1991, 1993, and 1996 SIPP surveys matched with information on benefit receipt and earnings histories from the Social Security Administration's administrative records. Since workers who reach the early retirement age of 62 after the 1983 Social Security amendments face conceptually similar benefit rules and since Statements were introduced in 1995, I restrict my analysis to workers born after 1922. Seventy percent of married women are eligible for spousal benefits that exceed their own benefits; therefore, when analyzing retirement behavior, I focus the analysis on male workers. The main advantage of using the SIPP data is that information on earnings is available up to 2003; that is, the data cover the period after the introduction of the Statement. In the HRS, on the other hand, only the first wave (1992) is matched to administrative records. While it would be possible to use the survey information for the years after 1992, it is only available every two years. Another main advantage of the SIPP over the HRS is that the sample size is five times larger, which allows us to better control for observed heterogeneity.

After restricting the sample to male workers born between 1922 and 1940, the SIPP data contain around 14,000 observations. Since I cannot control for health status workers who at any time claim for disability benefits are excluded from the sample.[6] Workers are matched with their spouses' information. Two percent of male workers have expected benefits that are smaller than half of the benefits of their spouse. These workers are excluded from the analysis since they are better off by claiming for their spouses' benefits, and are unlikely to respond to changes in their own SSW.

Using cross-sectional information from the SIPP data, I construct a panel that ranges from age 55 to either age 72 or the year 2003. Since information from the SIPP survey is used for both years before and years after the survey, there is a potential measurement error problem. While the error is likely to be small for characteristics that change little over time (gender, marital status, education, wealth), there are time-varying factors that have been shown to influence retirement decisions. There are two important factors that are time-varying, but that I cannot control for: health status and private pensions. Previous studies have found that the elasticity of retirement with respect to Social Security incentives is robust to the exclusion of both

[6] Some further deletions are made mostly for reasons of miscellaneous data inconsistencies.

health status (Panis et al., 2002) and private pensions (Coile and Gruber, 2001). Nevertheless, I control for whether the worker is covered or receives a private pension, and whether he has health insurance. Table 13 in the Appendix shows the summary statistics for the main SIPP sample used later in the regressions.

1.3 The Social Security Statement

The introduction of the Statements was phased in starting in 1995. The SSA was required to mail the annual Statement—then named the Personal Earnings and Benefit Estimate Statement—to all workers age 60 and older.[7] Younger workers have been added to the recipient list in subsequent years, and since 2000 almost all workers not claiming benefits receive the Statement. Workers usually receive their Statement one month before their birthdays.[8] In fact, this seems to be a good timing since 65 percent of all workers claim immediately after their birthdays (15 percent of the claims occur in January and the remaining workers tend to claim uniformly across the year).

The main purpose of the Statement is to inform the public about benefits under SSA programs, to aid in financial planning, and to ensure the worker's earnings records are complete and accurate. The Statement contains expected Social Security benefits at the early (62), the normal (usually 65, though increasing since 2003), and the late (70) retirement age as well as the worker's entire earnings history. The Statement also informs workers about spouse's benefits, survivors' benefits, and disability benefits. The Statement does not report the SSW. Later, I evaluate how this additional information affects workers' retirement behavior assuming that workers are able to compute their SSW.

Beside the Government Accountability Office (GAO) that has tried to evaluate their understandability, economists have not paid much attention to the introduction

[7] In the Appendix I provide a sample of the Social Security Statement. Earlier versions of the Statement can be found in reports by the GAO, although they changed little over time.

[8] In 2000 the SSA started sending the Statement three months before the worker's birthday.

of the Statements.[9] This has prompted Jackson (2005) to conclude that: "Given the importance of Social Security benefits to so many Americans, it is surprising how little academic attention has been given to the content and implications of Social Security benefits" and "..., what is clear is that the Social Security Statement is one of the most important communication that the federal government sends out to the general public each year, and as such the document deserves much more attention from public official and academic writers than it has received to date."

According to the GAO reports the overall public reaction to receiving an unsolicited Statement has been favorable. The reports cite a nationally representative survey in which (as predicted by Bernheim, 1987) "the majority of the respondents indicated they were glad to receive their Statements and 95 percent of them said the information provided was helpful to their families." The April 2005 report finds that 66 percent of workers remember receiving a Statement (unfortunately they do not provide this number by age groups), and that 90 percent of those who remember receiving a Statement say that they remember the amount of estimated Social Security benefits. The results of a Gallup survey, undertaken at the request of the SSA, revealed that individuals who had received a Statement had a significantly increased basic understanding of Social Security, and an increased understanding of some important basic features of Social Security: the amount of Social Security benefits depends on how much people earned; Social Security pays benefits to workers who become disabled; Social Security provides benefits to dependents of workers who die.[10] According to the 2004 Retirement Confidence Survey, 80 percent of workers use retirement benefit Statements (not necessarily only Social Security Statements) and 20 percent find them the most helpful tool in retirement and claiming decision making (Helman and Paladino, 2004). Jackson analyzes the content of the Social

[9]See GAO/T-HEHS-96-210, GAO/HEHS-97-19, GAO/HEHS-98-228, GAO/T-HEHS-00-101, GAO-05-192 on www.gao.gov

[10]See www.ssa.gov.

Security Statement, and reports how because of various cognitive biases workers may misinterpret the value of their benefits. He then suggests that including the present discounted value of Social Security benefits may facilitate the comparison with other sources of income and minimize labor market distortions.

1.4 Workers' knowledge about their benefits and the Statement

In all six available waves of the HRS (1992–2002), workers are asked about their expected retirement age and their expected Social Security benefits. Upon receiving the Statement, workers should be less likely to answer that they do not know the benefit amount they expect to receive once they retire. Also, for workers who provide an estimate, I expect the forecast error, that is the difference between the expected Social Security benefits and the actual benefits, to be smaller.[11]

It is important to note that workers have always had the option to ask the SSA to compute their expected benefits (it would usually take 4 to 6 weeks to receive an estimate). According to the HRS around 50 percent of the respondents contacts the SSA by age 62. Given the complexity of the benefit formula this it isn't too surprising. The Statement is likely to provide new information mainly to those who have not contacted the SSA. We can think of them as the treatment group that actually receives a treatment. Since receiving a Statement influences the probability of contacting the SSA, I need to correct for this endogeneity if I want to measure

[11]Because of the panel structure of the survey, I can compare these expectations with the reported actual benefits received in later waves. Although later on in the analysis I focus on male workers, here, in order to gain precision, I use both the male and the female samples. Using only the male sample does not substantially alter any of the results.

the effect of Statement on those workers who wouldn't have contacted the SSA.[12] Fortunately it is possible to correct for this endogeneity bias using pre–Statement information on who contacted the SSA.

Because of this selection the group that contacts the SSA is not a random sample, and so it is useful to formalize what influences the decision to contact the SSA.

1.4.1 Modeling the optimal time for getting informed

A worker will acquire new information about his retirement benefits when, based on his prior $f(b)$ over the whole distribution of his retirement benefits (which are function of the retirement age $b = (b_{62}, ..., b_{70})$) he believes that the expected gains of information outweigh the cost of information. Retirement affects utility through its consequences on consumption and leisure. Defining the retirement decision as $R \in \{0, 1\}$, it's optimal to gather information when

$$\int max_R U[R(b)]f(b)db - max_R \int U[R(b)]f(b)db > c. \tag{1.1}$$

Intuitively information matters when better knowledge about the benefits can influence the retirement decision, in other words, when variation in benefit patterns generate variations in utility $U[R(b)]$. If, for example, the prior is such that the worker strongly believes that it is optimal to retire as soon as possible, it might not be optimal for him to collect additional information. Factors that can generate such a boundary solution are high discount rates, high disutility from work (health issues), high mortality, and low risk aversion. Notice that I am implicitly assuming that workers are able to evaluate their retirement incentives (complicated functions of their benefits). Financially illiterate workers, unable to compute those incentives,

[12]The 1992 and 1994 waves of the HRS contain information about whether the respondent contacted the SSA to calculate his benefits (in the 2000 wave only a subset of around 200 people were asked this question). The exact formulation of the question is: Have you ever had the Social Security Administration calculate what your Social Security retirement benefit will be?"

might also choose not to get informed.

The main effect of the Statement is to considerably reduces c, which should help workers to make better retirement choices. But if workers select into the unknowledgeable state changes in retirement behavior are expected to be lower than in a situation where knowledge were randomly assigned. Before analyzing the effect of the Statement it is therefore important to analyze the selection issue.

Column (1) in Table 1.1 shows that, apart from age (multiplied by 1/2 for a reason that will be clear shortly), the two strongest predictors for contacting the SSA are the level of education and race. Both, having less than a high school degree and being black, reduce the probability of contacting the SSA by around 15 percentage points. Consistent with the theory wealthier workers, therefore workers that are less likely to be liquidity constraint, are more likely to contact the SSA (column 2). The effects are very large. Compared to workers that are in the first wealth quartile, workers with wealth above the median are 15 percentage points more likely to contact the SSA. Healthy workers are, compared to workers in fair and poor health, more likely to contact the SSA. Health and wealth do also capture around 30 percent of the differences that in the first column were attributed to race and education.

In column (3) I additionally control for the subjective life–expectancy and for labor market experience.[13] While more experienced workers are significantly more likely to contact the SSA, the coefficient on the subjective life-expectancy is not significant. Since the SSA's actuarial adjustments for postponing retirement are based on the average life-expectancy workers with a low subjective life–expectancy should be less likely to get informed if they know that they should follow the simple rule of retiring and claiming the benefits as soon as possible. On the other hand, workers with a high life–expectancy should do the opposite, claim as late as possible (70). Checking

[13] The subjective life–expectancy is measured as the self-reported probability of surviving age 75 divided by the implied probability from the Vital Statistics life tables that someone of the respondent's age and gender will live to be 75.

for non-linearities does reveal that workers in the first and the last quartile of the distribution of subjective life–expectancy are less likely to get informed, but the effects are not significant.[14]

Around 35 percent of workers age 65 receive a private pension. The incentives of getting informed might differ by whether workers receive a pension or participate in a defined benefit or defined contribution plan, both because pension change the liquidity constraint and because pensions change the overall retirement incentives. When I control for these factors, I indeed find that workers who already receive a pension are significantly more likely to have contacted the SSA.[15] Participating in a pension plan does not significantly change the probability of contacting the SSA, even when I focus on those who do not yet receive a pension income. Do to data limitation I was unable to test whether the relative importance of pension benefits to Social Security benefits matters. Controlling for private pensions does not reduce the effects of race and education.

In column (5) I control for the respondents financial planning time horizon, information available from the HRS's first wave (no information on pensions). How far in advance workers are planning is certainly related to their time preference. Consistent with this I find that the longer the planning time horizon the more likely it is workers contact the SSA. It is important to notice that even after controlling for health, wealth, mortality, and proxies of time preference workers without a high school degree and black workers are 10 percentage points less likely to contact the SSA. In the last column I additionally control for occupation fixed effects. While this reduces by another 30 percent the differences across levels of education, the coefficient on race drops by only 1 percentage point.

Summing up, workers who didn't contact the SSA before the introduction of the

[14] Results available upon request.

[15] The sample size is lower because the information on whether the respondent receives a pension isn't available in the first wave.

Table 1.1: Linear probability model of contacting the SSA.

		(1)	(2)	(3)	(4)	(5)	(6)
$age \times 1/2$		8.32	8.06	7.55	7.08	7.97	8.22
		(0.54)**	(0.54)**	(0.57)**	(0.85)**	(0.78)**	(0.81)**
Female		-1.54	-1.09	2.86	8.28	1.35	0.52
		(1.49)	(1.47)	(1.73)	(2.43)**	(2.29)	(2.64)
Below high school		-14.87	-10.53	-9.03	-9.31	-8.98	-6.26
		(1.71)**	(1.75)**	(1.88)**	(2.74)**	(2.36)**	(2.52)*
Some college		6.02	5.18	4.90	5.68	3.25	0.87
		(2.11)**	(2.09)*	(2.15)*	(2.87)*	(2.78)	(2.91)
College		10.29	7.26	8.20	9.09	7.44	5.17
		(2.14)**	(2.20)**	(2.26)**	(2.99)**	(2.80)**	(3.32)
Single		-7.55	-3.14	-4.20	-7.03	-4.15	-2.89
		(1.61)**	(1.67)	(1.78)*	(2.38)**	(2.32)	(2.39)
Black		-13.87	-10.60	-10.31	-13.24	-10.06	-9.16
		(1.74)**	(1.75)**	(1.93)**	(2.75)**	(2.37)**	(2.49)**
Self–r. health:	very good		-0.63	-1.32	-1.02	-0.83	-1.05
			(1.87)	(1.92)	(2.80)	(2.61)	(2.69)
	good		-1.42	-2.17	1.07	-2.56	-2.17
			(1.93)	(2.00)	(2.92)	(2.67)	(2.74)
	fair		-5.05	-4.11	-2.50	-3.89	-3.90
			(2.29)*	(2.51)	(3.95)	(3.43)	(3.55)
	poor		-6.66	-5.11	-2.00	-8.13	-9.23
			(2.94)*	(3.60)	(8.09)	(5.03)	(5.13)
Wealth percentiles:	25-50		6.48	5.61	8.84	4.25	3.69
			(1.79)**	(1.94)**	(2.93)**	(2.59)	(2.71)
	50-75		15.70	14.56	13.75	12.38	11.43
			(2.04)**	(2.17)**	(3.14)**	(2.89)**	(3.02)**
	75-100		16.72	15.64	11.62	16.63	16.39
			(2.34)**	(2.48)**	(3.39)**	(3.29)**	(3.44)**
Subjective P_{75}				-2.40	0.44	-5.10	-4.64
				(1.92)	(2.87)	(2.56)*	(2.65)
Experience				0.43	0.48	0.35	0.32
				(0.07)**	(0.12)**	(0.09)**	(0.10)**
Pension on current job					2.38		
					(2.63)		
Defined benefit plan					1.11		
					(2.77)		
Receives a pension					12.42		
					(3.51)**		
Financial time horizon	few months					-11.17	-10.32
						(3.92)**	(4.02)*
	year					-7.24	-6.67
						(4.25)	(4.39)
	few years					-7.78	-6.89
						(3.59)*	(3.69)
	5-10 years					-5.26	-4.96
						(3.68)	(3.77)
Occupation dummies			no	no	no	no	yes
Observations		5466	5466	4990	2018	2346	2190
R-squared		0.12	0.14	0.14	0.14	0.16	0.18

Notes: Clustered (by individual) standard errors in parentheses. *Sample:* HRS 1992-1994, age 55-65. The excluded categories are workers with a high school (HS) degree, in excellent health, with net wealth in the first quartile, and a financial time horizon of more than 10 years. The subjective probability of surviving until age 75, P_{75}, is divided by the implied probability from the Vital Statistics life tables that someone of the respondent's age and gender will live to be 75.

Statement tend to be younger, with lower levels of education, single, black, in poor health, poor, with fewer labor market experience, and less likely to plan many years in advance. Next I show that these workers are more likely to improve their knowledge about their benefits upon receiving a Statement, which is consistent with the idea that information is costly.

1.4.2 The effect of the Statement on workers' knowledge about retirement benefits

Column (1) in Table 1.2 shows the effect of the Statement on the probability of reporting Social Security benefits,[16] estimated using a linear probability model. I control for age, age squared, year, gender, level of education, marital status, race, and labor market experience (number of years with positive earnings). When I control for a quadratic term of age and a linear term for years the introduction of the Statement reduces the probability of not reporting an estimate by 5 percentage points. Controlling for age and year fixed effects (column 2) doesn't alter the effects. This 16 percent drop in the probability of being uniformed can be interpreted as an average treatment effect. Being black and not having a high school degree are both very strong predictors for not knowing the future amount of the benefits. Controlling for health and wealth does not alter this results, and the reason is that controlling for age and time the introduction of the Statement tends to be orthogonal to the other variables.

In order to evaluate the effect of the Statement on workers who didn't contact the SSA before receiving the Statement I need to control for the fact that some workers would have shown an improvement even without the Statement (they would have contacted the SSA). Define the event "contacting SSA" as $C \in \{0, 1\}$ and "not being

[16]The dependent variable is equal to one when workers respond that they "don't know" their Social Security benefits. The very few workers who refuse to respond are not included in the regressions.

Table 1.2: Linear probability (in percent) model of being unable to provide a benefit estimate.

	(1)	(2)	(3)	(4)	(5)	(6)
	Does not report and expected Social Security benefit amount					
Post–Statement	-5.37	-5.21	-5.13	0.28	-2.14	-2.18
	(1.26)**	(1.93)**	(1.93)**	(1.51)	(2.17)	(2.17)
No SSA contact				29.79	30.08	29.59
				(1.65)**	(1.65)**	(1.65)**
Post× no SSA c.				-10.51	-10.90	-11.10
				(1.86)**	(1.86)**	(1.86)**
Female	4.69	4.66	4.72	6.52	6.62	6.88
	(1.08)**	(1.08)**	(1.08)**	(1.30)**	(1.31)**	(1.30)**
Below high school	9.43	9.48	7.61	7.84	7.86	6.52
	(1.37)**	(1.37)**	(1.39)**	(1.62)**	(1.62)**	(1.64)**
Some college	-1.81	-1.80	-1.48	-0.01	0.05	0.22
	(1.28)	(1.28)	(1.28)	(1.49)	(1.49)	(1.50)
College	-1.74	-1.78	-0.70	1.39	1.39	1.96
	(1.30)	(1.30)	(1.33)	(1.50)	(1.50)	(1.52)
Single	3.93	3.89	2.66	2.37	2.26	1.14
	(1.03)**	(1.02)**	(1.05)*	(1.27)	(1.27)	(1.33)
Black	10.43	10.38	8.94	5.91	5.85	4.97
	(1.39)**	(1.40)**	(1.42)**	(1.68)**	(1.68)**	(1.71)**
Wealth	no	no	yes	no	no	yes
Health	no	no	yes	no	no	yes
Age effects	no	yes	yes	no	yes	yes
Year effects	no	yes	yes	no	yes	yes
Observations	14493	14493	14493	10237	10237	10237
R-squared	0.06	0.06	0.07	0.11	0.11	0.11

Notes: The non-numbered column reports the sample means. The excluded educational category is high school. Clustered (by individual) standard errors in parentheses; Bootstrapping (using 200 rep.) the standard errors by individual to account for both clustering, and also for the variation due to the first-step estimation of the probabilities of misclassification of contacting the SSA has negligible effects on the standard errors (results available upon request). * significant at 5 percent; ** significant at 1 percent. *Sample:* HRS 1992-2002, age 55-65.

able to provide an estimate" as $N \in \{0,1\}$. I need to estimate the improvement in $\Pr(N = 1)$ that would have happened independently of the Statement $T \in \{0,1\}$: $\Pr(N_t = 1|C_{t-2} = 0, T = 0) - \Pr(N_{t-2} = 1|C_{t-2} = 0, T = 0)$. Having in mind that I am always conditioning on $T = 0$, by the law of total probability: $\Pr(N_t = 1|C_{t-2} = 0) = \Pr(N_t = 1|C_t = 0)\Pr(C_t = 0|C_{t-2} = 0) + \Pr(N_t = 1|C_t = 1)\Pr(C_t = 1|C_{t-2} = 0)$. One way to estimate $\Pr(C_t = 1|C_{t-2} = 0)$ is to use the cross–sectional information using age as a measure of time. Our estimate of $\Pr(C_t = 1|C_{t-2} = 0)$ is going to be equal to the coefficient on age$\times 1/2$ from Table 1.1. Age is multiplied by 1/2 in order to estimate the probability over a 2-year period (the HRS is biennial). When I control for sex, education, race and marital status the estimate is 0.0832 with a standard deviation of 0.0054.

Although I do not know $\Pr(N_t = 1|C_t = 1) = E(N_t|C_t = 1)$ and $\Pr(N_t = 1|C_t = 0) = E(N_t|C_t = 0)$ for the years after 1994, I can estimate these probabilities using data from the 1992 and 1994 waves assuming that the probability of contacting SSA and the effects from contacting SSA wouldn't have changed over time. Given these assumptions the overstatement of the effect of the Statement for workers who didn't contact SSA is approximately equal to 2.4 percentage points (30 percent) when using data up to 1996: $[E(N_{t-2}|C_{t-2} = 1) - E(N_{t-2}|C_{t-2} = 0)]P(C_t = 1|C_{t-2} = 0) = 0.30 \times 0.08$.

A similar conclusion is reached when, in order to use the whole data, I estimate a regression model with known probabilities of misclassification of the variable C. Defining C^* as the true event and C as the misclassified one, the true effect of the Statement for group x is proportional to the misclassified one

$$\begin{aligned}
&[E\left(N|C = 0, T = x\right) - E\left(N|C = 1, T = x\right)] \\
&= [E(N|C^* = 0, T = x) - E(N|C^* = 1, T = x)] \\
&\times \Pr\left(C^* = 0|C = 0\right), \quad x = 0, 1
\end{aligned}$$

where the factor of proportionality is the probability of correctly classifying $1 - C$. Controlling for other X's, it can be shown that the estimated true effect of the Statement is equal to $\widehat{\beta}_{11}$ in the following linear model:[17]

$$N = \beta_{00} + \beta_{01}\left(1 - C\right)\Pr\left(C^{*} = 0|C = 0, X\right) + \beta_{10}T_{1}$$
$$+\beta_{11}\left(1 - C\right)\Pr\left(C^{*} = 0|C = 0, X\right)T_{1} + X'\gamma + \epsilon. \tag{1.2}$$

This is the specification used from column (4) on, where I interact the probability of not having contacted the SSA and the post–Statement variable. This way I measure the treatment effect on the treated, and indeed the entire effect of the Statement is concentrated among those who never contacted the SSA (66 percent of the sample). Column (4) shows that not having contacted the SSA increases the initial probability of not reporting an estimate in the pre–Statement period by 30 percentage points, a very large effect. Notice also that this additional variable captures half of the effect of being black and reduces the differences due to the level of education. This means that blacks and workers with low levels of education are not only less likely to contact SSA in order to get informed, but are also less likely to get informed using other channels.

For those that do not contact the SSA, the Statement reduces the probability of not reporting an estimate by 10 percentage points, approximately one third of the initial difference. Columns (5) and (6) show that controlling for age and year fixed effects and for health and wealth does not change the estimated effects of the Statement.[18]

[17] In order to control for the variation that is due to the first step, I can either use a modified version of Murphy and Topel (1985)'s two-step estimator that accounts for the panel structure (dependence over time), or I can simply bootstrap clusters of individuals and than run the first and second step. Since doing so has negligible effects on the standard errors (mainly due to the precision of the estimate of $\Pr(C_t = 1|C_{t-2} = 0, X)$), the analysis is carried out conditional on the estimate from the first stage.

[18] The results are not different when, disregarding an endogeneity problem, I also control for the time left from the expected retirement date (results available upon request).

Table 1.3: Linear probability (in percent) model of not being able to provide a Social Security benefits estimate by age.

Age	Contacted SSA	Contacted SSA Pre-SSS	Contacted SSA Pre-Post	Contacted SSA Pre-Post	Did not contact SSA Pre-SSS	Did not contact SSA Pre-Post	Did not contact SSA Pre-Post
55	0.23	23.54	-3.43	-8.11	47.77	-7.33	-9.57
		(2.84)**	(6.24)	(6.29)	(2.40)**	(6.65)	(6.67)
56	0.27	18.97	3.74	1.96	52.06	-7.25	-8.13
		(2.62)**	(5.13)	(5.11)	(2.53)**	(4.92)	(4.86)
57	0.25	20.53	1.90	-0.22	49.13	-5.91	-6.45
		(2.92)**	(4.74)	(4.74)	(2.56)**	(4.30)	(4.20)
58	0.36	22.00	-3.60	-3.87	53.30	-7.14	-7.66
		(3.39)**	(4.08)	(4.16)	(3.71)**	(4.45)	(4.46)
59	0.34	19.71	0.95	1.94	57.46	-16.08	-15.89
		(3.41)**	(4.10)	(4.15)	(3.68)**	(4.34)**	(4.33)**
60	0.37	15.22	-0.65	-0.56	57.82	-15.57	-16.06
		(3.06)**	(3.55)	(3.77)	(4.08)**	(4.65)**	(4.85)**
61	0.47	10.00	4.89	6.12	57.50	-22.17	-20.91
		(2.31)**	(2.87)	(3.17)	(4.52)**	(4.99)**	(5.02)**
62	0.55	12.15	-1.78	-0.39	56.34	-24.01	-23.21
		(3.16)**	(3.76)	(4.01)	(5.90)**	(6.53)**	(6.78)**
63	0.59	11.43	0.98	0.99	54.05	-20.86	-20.95
		(3.81)**	(4.57)	(4.67)	(8.21)**	(8.87)*	(8.85)*
64	0.64	14.29	-0.69	-5.09	33.33	2.59	-4.28
		(9.37)	(9.80)	(9.50)	(15.75)*	(16.28)	(16.24)
Other Xs			no	yes		no	yes

Notes: The first column reports the fraction contacting the SSA. “Pre” columns report the fraction of workers who do not provide an estimate during the Pre–Statement period. Pre–Post columns report changes in the probability of providing a benefit estimate. Fractions are computed separately for workers who contacted (first three columns) and those who didn’t contact the SSA (last three columns). Clustered (by individual) standard errors in parentheses. Bootstrapping (using 200 rep.) the standard errors by individual to account for both clustering, and for the variation due to the first-step estimation of the probabilities of misclassification of contacting the SSA has negligible effects on the significance level (results available upon request). * significant at 5 percent; ** significant at 1 percent. *Sample:* HRS 1992-2002, age 55-64.

The effect on knowledge could be different at different ages, and thus could have very different effects on retirement behavior. The effect could be concentrated among younger workers, this way having only the effect of anticipating the information, with a small potential of changing retirement behavior. In order to capture how the Statement can differently affect different age groups, the first column in Table 1.3 reports for each age the fraction of workers who have contacted the SSA. Since almost all workers claim by age 65, the table is truncated at age 64. Most workers contact the SSA when they are close to retirement. Around 30 percent call in their 50s, while an additional 20 percent call when they approach the early retirement age.

In the remaining columns of Table 1.3, I analyze how at different ages the probability of reporting a benefit estimate changes upon receiving a Statement.[19] The sample is split into those who did and those who didn't contact the SSA (using again a model with misclassification and known probabilities of misclassification). Among those who contacted the SSA there is a clear reduction in the probability of not reporting an estimate as we approach the early retirement age. There is no such pattern for those who didn't contact the SSA in the pre–Statement period. In the post–Statement period, there is a clear improvement around the early retirement age. The effect of the introduction of the Statement can be seen by looking at the $Pre - Post$ columns. There are 2 $Pre - Post$ columns, the first does not control for other regressors (gender, education, experience, and veteran status), the other does. Among those who contacted the SSA the differences are not significantly different from zero. On the other hand, among workers who didn't contact the SSA, the Statement reduced the fraction by around 10 percentage points up to age 58 and 20 percentage points afterwards. In relative terms, the effect around the early retirement age is to reduce the fraction of workers that are unable to provide a benefit estimate by almost one half.[20]

[19] I performed a similar analysis using instead of age the expected number of remaining years from retirement, and the results were very similar.

[20] The effect at even earlier ages are small. Workers in their 40s and early 50s are only 3-6 percentage points more likely to provide an estimate as a consequence of receiving the Statement

After age 58 the differences are significant at the 1 percent level (except at age 64 where the sample size is also very small).

The Statement has a significantly larger impact at ages close to the early retirement age. It generates little additional information for workers who are far from retiring.

Up until now I haven't considered the possibility that a worker's knowledge about the benefits may be positively influenced when someone else in the household receives a Statement. The HRS allows us to analyze how worker's knowledge changes when the spouse receives a Statement. In Table 1.4 I compute the probability of being unable to provide an estimate by the worker's own Statement status and the spouse's Statement status, separately by gender and by whether the worker contacted the SSA. For those workers who contacted the SSA there are no changes due to their own or the spouse's Statement. Among workers who didn't contact the SSA, the effect of the own Statement (vertical comparison) tends to be larger among men than among women, while the effect of the spouse's Statement is around 2 percentage points for men and at least 5 times as large for women.[21] This is consistent with the Social Security rules about dependent spouse benefits: A spouse receives the highest amount between her own benefits and one half of the worker's benefits. Since the majority of women are better off claiming through their husband's account, the husbands' Statements tend to carry more information.

Once I established that the Statement reduces the probability that workers are unable to provide an estimate of their future benefits, I can analyze whether those who provide an estimate improved their forecasts. Figure 1.1 shows the density of the

(results available upon request). This cast some doubt on the utility of sending the Statements to young workers that seem to show little interest for them. The estimated cost of sending each Statement is about 56 cents. Given that around 136 million Statements are sent out every year, the total cost is approximately $75 million. More than half of this amount could be saved by sending Statements to older workers only.

[21]Notice that the value of 64.5 that corresponds to the case where the female worker received a Statement but her younger spouse didn't is due to the limited sample size imprecisely estimated.

Table 1.4: Spillover effects on the probability (in percent) of not being able to provide a Social Security benefits estimate by age.

	Spouse's Statement period							
	Did not contact SSA				Contacted SSA			
	Women		Men		Women		Men	
Own Statement	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Pre	58.4	49.1	45.7	43.5	26.2	24.1	15.2	12.9
	(1.3)	(2.6)	(1.3)	(4.5)	(1.7)	(3.7)	(1.1)	(4.3)
	[1354]	[375]	[1440]	[124]	[699]	[137]	[1076]	[62]
Post	64.5	40.9	32.8	29.7	17.6	20.7	13.1	12.8
	(8.7)	(1.0)	(2.7)	(1.0)	(9.5)	(1.3)	(2.5)	(0.9)
	[31]	[2361]	[311]	[2109]	[17]	[1025]	[176]	[1246]

Notes: Sample: HRS 1992-2002, age 55-64. Standard errors in parentheses and sample size in squared brackets.

forecast error (the difference between the expected and the actual benefits) for those workers who did and didn't contact the SSA.[22] [23] Errors seem to be approximately distributed symmetrically around zero, which suggests that, on average, there is no prediction bias. In the pre–Statement period (solid line) the variability of the errors for workers who didn't contact the SSA is much larger than for those who contacted the SSA; this difference seems to disappear once the Statement is introduced (dashed line). As before, this change in the distribution of the error term is likely to be upward biased by the fact that some workers would have contacted the SSA in the absence of the Statement. Substituting workers who didn't contact the SSA with workers who contacted the SSA with probability equal to the probability of contacting the SSA over a two-year period,[24] and plotting the corresponding pre–Statement density

[22]Benefits are expressed in 2003 dollars using the CPI. I take into account that actual Social Security benefits refer to the year before the interview. Results using the relative forecast error are similar.

[23]Note that to highlight the distributional differences I truncated the distribution of the error at ±$1000 (3 percent of the sample).

[24]These graphs use only information up to 1996 and therefore the probability is simply equal to 8 percent.

allow us to judge the expected improvement that is not attributable to the Statement (dotted line).

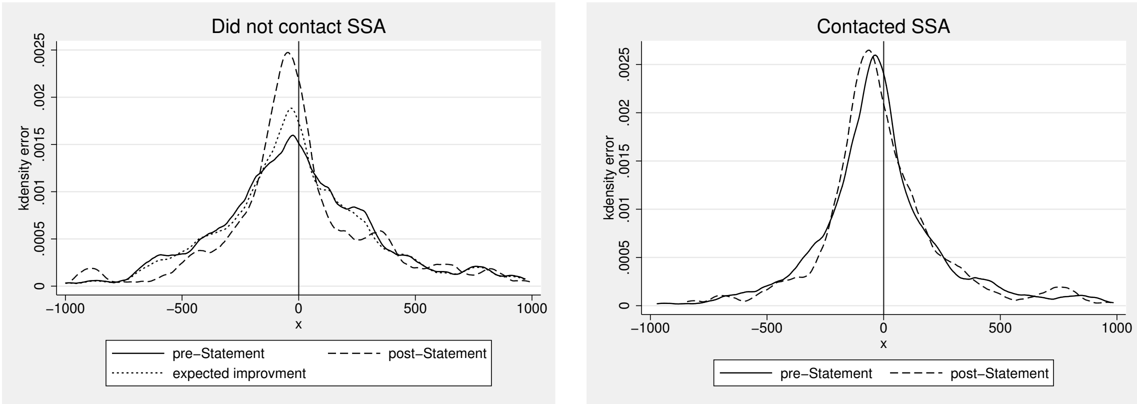

Figure 1.1: Monthly forecast error. Epanechnikov kernel estimate using a \$35 bandwidth. *Sample:* HRS 1992-1996, age 55-65.

In Table 1.5, I test whether the distributional differences in Figure 1.1 are significant. For workers who didn't contact the SSA I use the pre–Statement density that controls for the expected improvements (dashed line). Most of the improvement seems to lie within one standard deviation from the mean, which is why I test if the ratio of the pre–Statement to the post–Statement variance is larger than one, truncating the error at ±\$1000, ±\$500, and ±\$300.[25] The p-value of this one-sided test for those who didn't contact the SSA is equal to 10 percent for the \$1000 truncation but quickly drops to being significant as I concentrate the analysis to errors that are closer to the median. For those who contacted the SSA I can reject the hypothesis that the variance decreased after the introduction of the Statement. It is worth noting that although the variance of the forecast error decreased for those who were previously uninformed, similarly to what we observed before for the probability of reporting an estimate, their post Statement errors are still larger compared to the other group.

[25]The reason to use truncated values is that variances are highly sensitive to outliers. Without truncation the variance of the error is even larger in the pre–Statement period. In the HRS, respondents can report weekly, monthly, biyearly, and yearly values. The big discrepancies seem to be due to the few observations with measurement errors in the variable that reports this "frequency" variable.

Table 1.5: Variance ratio test

	Did not contact SSA			Contacted SSA		
	Standard Dev.		p-value	Standard Dev.		p-value
	Pre-SSS	Post-SSS	Pre/Post	Pre-SSS	Post-SSS	Pre/Post
Forecast error truncated at:						
$\|e\| < \$1000$	342.67	324.81	0.109	270.73	265.15	0.299
	[781]	[416]		[1350]	[449]	
$\|e\| < \$500$	225.89	204.28	0.016	189.13	181.00	0.141
	[661]	[364]		[1240]	[415]	
$\|e\| < \$300$	158.53	131.35	0.000	132.36	128.97	0.278
	[527]	[299]		[1072]	[366]	

Notes: Standard deviation of the errors and p-value of a variance ratio test with null hypothesis $H_0 : V_{pre}/V_{post} < 1$. Estimates control for the improvement in the standard deviation of the forecast error that is independent of the Statement by using the dashed line version of Figure 1.1 for the pre–Statement period. Since variances are highly sensitive to outliers I test the null using three truncated versions of the forecast error. Numbers of observations in square brackets. *Sample:* HRS 1992-1996, age 55-65.

The above analysis suggests that thanks to the Statement some workers became more knowledgeable about their Social Security benefits. The workers for whom I observe an improvement didn't contact the SSA before. The profile of those workers is consistent with the idea that information is costly. Controlling for various factors I am able to reduce educational gaps by around one half and racial gaps by around one third. While the remaining differences could be due to different preferences over leisure, another possible reason might be financial illiteracy.[26] Lusardi and Mitchell (2006) show that black workers and workers with low levels of education are significantly less likely to respond correctly to simple questions about compound interest, inflation, and portfolio management.

The important lesson is that the free availability of information is not sufficient to get informed. Obtaining information seems to be costly and prevents workers who think that information to be less valuable to become knowledgeable. Stimulating workers by directly providing them with information reduces that cost and has the

[26]Another explanation may be that some workers prefer to procrastinate (O'Donoghue and Rabin, 1999).

predictable effect of improving workers' knowledge. In the next section, I test whether and how the new information affected workers' retirement decisions.

1.5 The effect of the Statement on retirement and Social Security benefit claiming decisions

The additional information provided by the Statement can influence workers' behavior in many ways. There may be a "surprise" effect: workers who overestimated their expected Social Security benefits should react by working and saving more, while those who underestimated their benefits should do the opposite. Although changes in labor supply may also happen at the intensive level (hours), I focus on changes at the extensive level (participation). Since forecast errors are approximately symmetrically distributed around zero, these changes may go in both directions. Also, as over time the age at which workers received their first Statement decreases, we should expect these "surprise" effects to weaken. In addition, even if the decision of becoming informed is the sole product of a maximization process with costly information, at the margin the Statement should strengthen the link between Social Security incentives and retirement.

Because of liquidity constraints and the earnings test (ET), the retirement decision is strongly related to the claiming decision. According to the HRS data, half of the time the monthly self-reported retirement date and the monthly self-reported claiming date are not more than 12 months apart from each other. When the difference between the two dates is larger than one year, the difference is mainly due to early retirement. Among those who retire at or after age 62, 75 percent claim and retire within a year. Since the administrative records do not have information about self-reported retirement status, for those workers who show positive earnings in the previous year, I measure retirement based on claiming Social Security benefits. Alternatively, I could define retirement based on some given changes in earnings.

There is very little analysis of the claiming decision for those workers who have already retired and therefore face a financial decision. The decision to postpone claiming is equivalent to the decision to purchase additional annuities. Coile et al. (2002) show that for some male workers, typically those who are married and face long "joint" life expectancies, delaying claiming of Social Security benefits after age 62 can generate substantial gains, and that these gains may actually be 10 or more times greater when risk aversion is taken into account.

Before moving to the analysis, I need to mention the other major Social Security reforms that happen around the time of the introduction of the Statement. The most important reform is the 2000 earnings test removal for workers above the normal retirement age (usually 65). Earnings of Social Security beneficiaries above the earnings test threshold, up to their benefit amount, are taxed away at a 50 percent rate between age 62 and 65, and, before 2000, at a 33 percent rate between 65 and 69. Although the earnings tax is only that high for myopic workers, the reason being that benefits that are taxed away increase future benefits at an almost actuarially fair rate through the so-called recomputation, workers are sensitive to the tax. The removal had the effect of increasing the fraction of workers who claim their Social Security benefits at the normal retirement age, the age at which the tax was removed (Mastrobuoni, 2005a).

The other two reforms changed the benefit formula and will be included in my benefit calculations. In response to an earlier "crisis" in Social Security financing two decades ago, the US Congress implemented both a reduction in the Normal Retirement Age (NRA) of two months per year for cohorts born in 1938 and afterward, and, staring in 1986, an increase in the delayed retirement credit (DRC),[27] that is the actuarial adjustment to the benefits when retirement is postponed beyond the

[27] See Mastrobuoni (2005b).

normal retirement age. The DRC has been increased by half a percent every other year from its original 3 percent. It is going to reach its final value of 8 percent for workers born in 1943 or later.

1.5.1 The effect of the Statement on workers' expected claiming behavior

Before looking at the actual retirement and claiming behavior, I can analyze whether at the time workers received the Statements they change when they expect to retire.[28]

We should expect workers to be more likely to change their expectations when they receive their first Statement, and less likely afterwards. Using the panel structure of the HRS, I estimate the effect of the Statement on the probability that the expected claiming age stays constant.[29] All regressions include age fixed effects, levels of education, marital status and race. I also control for a linear time trend and for the 2000 earnings test removal. In Table 1.6, I report the marginal effects of the Statement on the probability of keeping the same expected claiming age. The first column allows for just a one-time effect, which is small and not significantly different from zero. Column (2) shows that those who did not contact the SSA are significantly more likely to change their expected claiming age.[30] The estimates in both of these columns are contaminated by the fact that the first Statement should have the opposite effect than subsequent Statements. In column (3), I include an indicator variable equal to one when the person already received a Statement in the previous wave. The coefficient has a positive sign, meaning that receiving a second Statement increases the probability of maintaining the same expected age, though the effect is

[28]See Chan and Stevens (2004), who estimate a model of expected retirement.

[29]I tried to replicate the same analysis with respect to the expected retirement age, though only a few workers are asked about their expected retirement date, and so the sample size was too small to estimate any effect.

[30]I control for the fact that contacting SSA is endogenous by estimating the model using the probabilities of misclassification in same manner as when I dealt with the probability of providing a benefit estimate.

only significant at the 10 percent level. In column (4), I interact both Statement effects with the "No SSA contact" dummy. Both, the effect of the first Statement and the effect of additional Statements is not significantly different for the two groups.

Table 1.6: Marginal effects (in percent) on the probability of keeping the same expected age of claiming.

	P(same expected claiming age)			
	(1)	(2)	(3)	(4)
Post-statement	-2.10	-4.14	-1.88	-3.66
	(2.39)	(3.05)	(2.41)	(3.15)
No SSA contact		-4.68		-4.81
		(2.30)*		(2.30)*
Post-st.× No SSA cont.		-0.23		0.03
		(2.98)		(3.27)
Additional statements			4.42	4.17
			(2.45)	(3.30)
Additional st.× No SSA cont.				-0.25
				(3.85)
Post-ET removal	-3.54	-6.16	-5.02	-7.02
	(2.66)	(2.97)*	(2.78)	(3.02)*
Year	-0.19	0.64	-0.63	0.14
	(0.63)	(0.72)	(0.690)	(0.80)
Observations	5961	5022	5961	5022
R-squared	0.02	0.03	0.02	0.03
Mean	66.72	67.58	66.72	67.58

Notes: The marginal effects are estimated using a linear probability model. I additionally control for age, age squared, education, marital status, race, and veteran status. Clustered (by individuals) standard errors in parentheses; * significant at 5 percent; ** significant at 1 percent. *Sample:* HRS 1992-2002, age 55-65.

Workers may not pay attention to the first Statement they receive, so there is a potential measurement error problem. This may explain why the effects are generally small and not significant. This measurement error problem is less salient when analyzing actual retirement behavior. Each Statement should have the effect of improving workers response to retirement incentives.

1.5.2 Social Security incentives

In order to analyze whether workers became more responsive to Social Security incentives, I need to forecast earnings and compute future benefits as a function of the retirement age. Below I briefly review the main provisions of the benefit formula and the assumptions needed to compute the SSW.

In order to compute Social Security benefits $B_t(a)$ for each retirement age I need to forecast earnings. To best approximate the information contained in the Statement I use the same assumptions the SSA uses in calculating the benefits for the Social Security Statement. The Statement assumes that if the worker doesn't retire he is likely to earn the same amount he earned last year (or the year before if last year's earnings are zero). In other words, real earnings are assumed to follow a random walk, so that the previous year's earnings are the best predictor for future earnings. This assumption is not very different from Coile and Gruber (2001), who assume that real earnings are expected to grow by one percent. Every year, benefits are then computed as a function of age (from age 55 to 70) and as a function of the retirement age (from the worker's actual age to age 70). The benefit rules are held constant, and it is assumed that promised benefits are going to be paid. Workers who retire before age 62 are assumed to claim at age 62.

I do not model the spouse's retirement decision, and I assume that the spouse claims at the earliest possible age.[31] A spouse is defined as "independent" when her own benefits at age 62 are larger than 50 percent of her husband's benefits at age 62. In this case her SSW is not added to her husbands SSW but enters the regression independently.

Benefits are a function of the weighted average of the highest 35 years of average wage-indexed earnings, called the AIME. Since workers tend to have lower earnings at the beginning of their career than at the end working an additional year normally

[31]Most of the times it is age 62, which also represents the median claiming age

increases future benefits even at age 62, which generates an additional incentive to work (Blinder et al., 1981). However, between age 55 and 61 the increase in Social Security benefits is modest. Its median ranges between 1 percent and 2 percent, and the 75th percentile between 1 percent and 4 percent (Table 1.7). Starting at age 62 instead, the increase is substantial. An 8 percent actuarial adjustment has to be added to the median 1 percent increase that is due to current earnings. The 75th percentile reaches almost 10 percent. Looking at benefits only doesn't take into account that working an additional year means that benefits are not collected in that year, and that Social Security taxes are paid on the additional earnings up to the maximum taxable threshold. Whether workers think that future benefits make up for this loss depends on the number of years that they, and possibly their spouses, expect to collect benefits. It also depends on their discount rate. In other words, it depends on changes in the expected present discounted value of the Social Security benefits net of contributions. The SSW is a function of time t and retirement age a:

$$SSW_t(a) = PDV_t(B(a)) = \sum_{t=s}^{T} \beta^{t-s} p_t(s) B_t(a) \tag{1.3}$$

Following the literature I use a real discount rate of 3 percent ($\beta = 1.03$).[32] $B_t(a)$'s are expressed in 2003 dollars using the CPI, and the conditional probabilities of survival, $p_t(s)$, are based on the SSA's cohort-specific life tables.[33] Since I lack

[32]There is some evidence that discount rates may actually be larger than 3 percent (Samwick, 1998). On the other hand, Blinder et al. (1981) argue that in the absence of borrowing constraints it is more appropriate to use a real interest rate instead, which can be assumed to be very low (they use 1 percent). I follow the mainstream literature and use a 3 percent discount rate, though the reduced form model estimated controlling for age seems to be robust to the use of different discount rates. The reason is that controlling for age the effect of the accrual is mostly identified by the accrual's cross-sectional variation within age, while the use of different discount rates generates mainly large differences across age.

[33]The life tables are prepared by the Office of the Chief Actuary in the Social Security Administration. Projected death rates and life tables are based on Alternative II forecasts for the 1998 Trustees report (taken from the Berkeley Mortality Database). To compute total Social Security benefits (including spouse's benefits and survivors' benefits) when using the tables I am implicitly assuming that the couple's individual mortalities are independent.

precise information on dependent children, the benefits include dependent benefits and survivors' benefits, related only to the spouse. In that case $p_t(s)$ is a column vector where the entries are: the probability that only the worker survives, the probability that only his wife survives, and the probability that both survive. $B_t(a)$ is a row vector containing the worker's own benefits, the survivors's benefits, and the sum of the worker's own benefits and the dependent spouse's benefits.

The Social Security accrual is the expected gain in SSW from waiting an additional year before retiring,

$$ACC_t(a) = SSW_t(a+1) - SSW_t(a), \tag{1.4}$$

while the peak-value (PV) is the difference between the maximum SSW and the current SSW (Coile and Gruber, 2001),

$$PV_t(a) = \max_x SSW_t(x) - SSW_t(a). \tag{1.5}$$

Retirement decisions based on PV's and ACC's differ whenever ACC's are not monotonic relative to the retirement age. An additional complication comes from Social Security payroll taxes and income taxes. I also compute the accrual net of Social Security taxes, $tW_t(a)$, assuming, like in Diamond and Gruber (1998), that workers bear the entire payroll tax, t (12.4 percent since 1990). Since I do not observe income I do not attempt to try to simulate income taxes, though in the regressions the different tax treatment of Social Security benefits should in part be absorbed by the coefficient on earnings.[34]

In Table 1.7 I show the median (and some 75th percentiles) of the expected growth

[34] If a beneficiary files a federal tax return as "an individual," ("a couple") and the combined income is between \$25,000 and \$34,000 (\$32,000 and \$44,000) in 2004, he or she pays taxes on 50 percent of the Social Security benefits. If the combined income is more than \$34,000 (\$44,000), up to 85 percent of the Social Security benefits are subject to income tax.

rates in Social Security benefits and SSW for male workers at different ages. There is significant heterogeneity in expected increases in benefits from postponing retirement. This heterogeneity is mainly due to eligibility criteria to different types of benefits (i.e., dependent spouse's benefits), to differences in earnings histories, and to differences in current earnings. Men who evaluate the future streams of Social Security benefits taking only their own benefits into consideration (either because they have no dependents, or because their spouses are better off by claiming their own benefits) generally face negative or null increases in SSW from additional work.

Table 1.7: Median expected growth rates of Social Security benefits and social security wealth as a function of age.

	$\frac{B(t+1)}{B(t)} - 1$	$SSW(t+1)/SSW(t) - 1$					
		Own Benefits		Own+Dependent spouse			
		Median		Median		75th percentile	
		Pre-tax	After-tax	Pre-tax	After-tax	Pre-tax	After-tax
55	2.0%	2.0%	-1.4%	2.0%	-0.5%	3.6%	0.2%
56	1.8	1.8	-1.4	1.8	-0.6	3.1	0.0
57	1.3	1.3	-1.6	1.3	-0.9	2.3	0.0
58	1.1	1.1	-1.6	1.1	-1.0	1.9	0.0
59	1.0	1.0	-1.6	1.0	-1.0	1.6	0.0
60	0.8	0.8	-1.5	0.8	-0.9	1.4	0.0
61	0.6	0.6	-1.3	0.6	-0.9	1.2	0.0
62	8.9	1.0	-0.6	2.2	0.5	4.2	2.8
63	8.0	0.0	-1.1	1.1	-0.2	3.3	2.1
64	7.3	-0.9	-1.6	0.3	-0.9	2.5	1.2
65	5.1	-3.3	-4.3	-2.2	-3.0	-0.7	-1.6
66	4.7	-3.8	-4.7	-2.9	-3.5	-1.5	-2.2
67	4.3	-4.6	-5.2	-3.6	-4.0	-2.3	-2.9
68	4.0	-5.1	-5.6	-4.2	-4.5	-3.0	-3.4
69	3.8	-5.6	-6.0	-4.8	-5.0	-3.6	-3.8

Notes: The After-tax columns represent the changes in SSW net of Social Security payroll taxes, assuming that workers carry the whole tax burden. *Sample:* SIPP linked to administrative data.

1.5.3 The effect of the Statement on claiming and retirement behavior

Next I look at the difference between the pre–Statement and the post–Statement claiming hazards, and I do it separately for workers who retired before age 62 (they face only a financial decision) and for workers who are working at age 62.[35] The hazard is defined as the probability of claiming within a year, conditional on not having claimed before. Figure 1.2 shows that among the working sample 50 percent claim at the early retirement age. There is also a pronounced spike at age 65. There seem to be some differences between the pre and the post–Statement period, mainly after age 64, though part of these differences could be due to the earnings test removal (Song and Manchester).

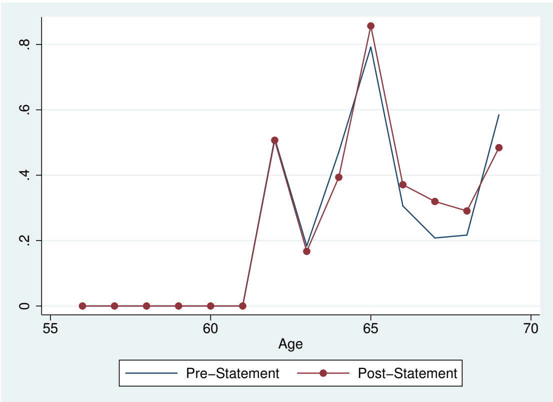

Figure 1.2: Claiming hazards for the working sample

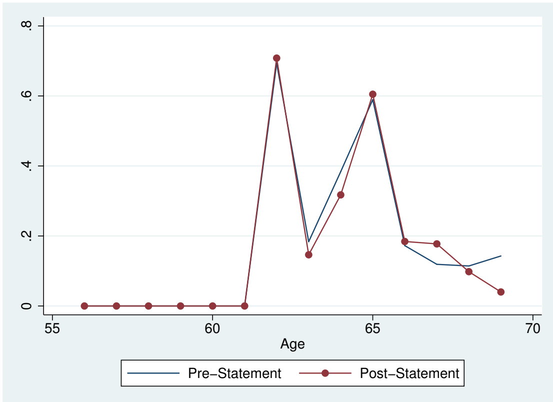

Figure 1.3: Claiming hazards for the retired sample

The hazard rates for the sample of workers who are already retired show a different pattern. More than 70 percent of them claim immediately at age 62, while the spike at 65 reaches 60 percent. Overall, the Statement seems to haven't changed the 62 and 65 hazards, and to have slightly decreased the 63 and 64 hazards. While there are some changes in the hazard rates I am not controlling for many factors that may

[35] A person is assumed to be working when his forecasted earnings are different from zero. This represent an almost absorbing state. Among workers aged 62 to 69 only 3.6 percent experiences positive earnings after having zero forecasted earnings, with average earnings of $6745.

generate these differences. More importantly, these differences do not tell us anything about optimality. Even if there were no differences in the hazards, it could still be possible that with the Statement workers sort themselves in a more optimal way across retirement/claiming ages.

In order to be able to define optimality I need to introduce a simple theoretical framework. For clarity of exposition assume there are only two periods. Worker i can either work ($R_i = 0$) earning y_i and retire the next period or, retire immediately ($R_i = 1$) and receive reduced benefits today (b_{i1}). $k_i > 1$ measures his disutility from work. The worker chooses the retirement date that maximizes his utility:

$$V_i = \max_{R_i} y_i \left(1 - R_i\right) + k_i b_{i1} R_i + k_i b_{i2} \left(1 + a \left(1 - R_i\right)\right). \tag{1.6}$$

It is optimal to retire immediately when

$$OV_i = y_i + k_i b_{i2} \left(1 + a\right) - k_i \left(b_{i1} + b_{i2}\right) = y_i + k_i ACC_i \leq 0, \tag{1.7}$$

where $ACC_i = b_{i2} \left(1 + a\right) - \left(b_{i1} + b_{i2}\right)$ represents the Social Security accrual. This expression tells us that the worker should retire whenever his Option Value (OV) is either zero or negative. Notice that the disutility from work (k_i) is individual-specific.

This simplified model has been used extensively in the retirement literature. Coile and Gruber (2000) estimate a probit reduced form model of retirement that incorporates forward-looking Social Security incentives. Their concept is based on the Option Value model of Stock and Wise (1990), a model that resembles a dynamic programming model although it introduces some important simplifications. As we saw in Table 1.7, accruals tend to be decreasing with age *except* between ages 61 and 62. Since workers may be forward-looking and incorporate future accruals in their retirement decisions Coile and Gruber (2000) and numerous papers that follow their approach use the peak value as the main measure of Social Security incentives. All of these papers use reduced form PV probits, and assume a constant coefficient on the PV.[36]

Since the Social Security Statement is sent to workers depending on time and age, it is extremely important to properly control for these two variables. For this reason I use a random coefficient linear probability model, though very similar results are obtained when using a proportional hazard model where the effects of the ACC are allowed to vary by age and socioeconomic characteristics (McCall, 1994).

Unlike most of the previous literature, I will define retirement almost entirely based on the claiming status. The main reason is that while claiming Social Security benefits is well-defined, there is no variable that measures precisely the retirement date. We may say that a person is retired if we observe a large drop in her earnings. This is, however, a noisy measure of retirement, and it is not obvious that it is better than the one based on claiming Social Security benefits. The third reason is that defining retirement based on benefit receipt restricts the analysis to people above age 62. Above age 62 accruals are monotonically decreasing, which allows us to focus on the accruals instead of using the PVs. When monotonic, accruals are a better measure of incentives than PVs. The reason is that two workers with the same PV may face very different incentives if the first has his peak in SSW in one year and the second has his peak in 5 years. Our model is

$$R_i = k_i ACC_i + \beta' \tilde{x}_i + \epsilon_i, \tag{1.8}$$

where $\tilde{x}_i$ denotes the other regressor, including the forecasted earnings (y_i). I generally set $\tilde{x}_i = x_i - \bar{x}$ for continuous variables and exclude the median when dealing with categorical variables.

First, I assume that k_i is constant across people and independent of the Statement

[36]Gustman and Steinmeier (1986), instead, assume that the heterogeneity in k_i depends on health, age, and year of birth.

$T \in \{0, 1\}$, while later I allow k_i to vary:

$$k_i = \alpha_0 + \alpha_1 T_i + \gamma_0' \tilde{z}_i + \gamma_1' \tilde{z}_i T_i + \nu_i. \tag{1.9}$$

α_0 is the effect of the accrual for the "baseline" worker in the pre–Statement period.

Substituting k_i into Eq. (1.8) I get,

$$R_i = [\alpha_0 + \alpha_1 T_i + \gamma_0' \tilde{z}_i + \gamma_1' \tilde{z}_i T_i] ACC_i + \beta' \tilde{x}_i + u_i, \tag{1.10}$$

where $u_i = \epsilon_i + \nu_i ACC_i$. In this setup, α_1 represents the difference between the post– and the pre–Statement period in the marginal effect of a unit (\$100,000) increase in the accrual on the probability of retirement for the baseline case:

$$\alpha_1 = \frac{\partial P\,(R=1)}{\partial ACC}|_{\tilde{z}=0, T_i=1} - \frac{\partial P\,(R=1)}{\partial ACC}|_{\tilde{z}=0, T_i=0}. \tag{1.11}$$

To ease the interpretation of the regression coefficients all z's are dichotomous variables. In such a case

$$\alpha_1 + \gamma_1 = \frac{\partial P\,(R=1)}{\partial ACC}|_{\tilde{z}=1, T_i=1} - \frac{\partial P\,(R=1)}{\partial ACC}|_{\tilde{z}=1, T_i=0} \tag{1.12}$$

represents the $Post - Pre$ effect for a worker with $z = 1$. It follows that γ_1 is equal to Eq. 1.12 minus Eq. 1.11 and represents the difference of $Post - Pre$ effects between workers with characteristics $\tilde{z} = 1$ and workers with baseline characteristics, a difference-in-difference.

To relax the assumption that workers retire and claim at the same time, I perform the regression for the entire sample first, and then separately for those who work, and for those who are retired. Later, since the results based on the whole sample are not significantly different from those of the working sample, I show only the latter.

In order to control for changes in claiming behavior that may be due to the earnings test removal, I include a post-ET removal dummy, both in X and in Z.[37] I also control for the average ET tax.[38] The higher the average tax, the higher the incentive for a worker who claims his benefits to start earning less or stop working altogether. Table 13 in the Appendix shows the summary statistics for the sample used in the regressions.

All regressions control for the worker's own SSW, his spouse's SSW, retirement status of his spouse, earnings (potential), age dummies, year, year squared, post–Statement dummy, a post-earnings test removal dummy, level of education, marital status, AIME at age 55, real estate property wealth, health insurance, difference in age relative to his spouse, SIPP panel dummies, children in the household, pension information, veteran status, experience, and experience squared.

It is likely that the same factors that determine fixed costs or fixed opportunities from work also affect the disutility from work, k_i.[39] And, even more importantly, it is very likely that the previously observed heterogeneity in the level of knowledge of Social Security benefits affects the observed k_i through some sort of individual-specific measurement error. If I observe the true accrual ACC, but workers base their decisions on their perceived and mismeasured accrual $\widehat{ACC}$, the estimated effect will be downward biased (relative to workers' actual intentions). The bias will be higher the higher the variance of measurement error $Var(\widehat{ACC} - ACC)$.

Column (1) of Table 1.8 shows the results, based on the entire sample, when I

[37]When I restrict the analysis only to the period before the ET was removed (1984–1999) the results tend to be of similar size though less significant.

[38]The average ET tax is $t_{ET} = min(benefits, (earnings - ETthreshold) \times marginaltax)/benefits$. When earnings are below the ET threshold, the marginal tax and the average tax are zero. Table 13 in the Appendix shows that the average tax is 0.60, while the average marginal tax is 0.34. Special rules apply the first year a worker claims his benefits. Under these rules, a worker can use a monthly test amount. If he claims and retires during the year, he can get a full Social Security check for any whole month he is retired, regardless of his yearly earnings. Since I do not have information on monthly earnings I cannot control for this case, which is why the average tax may be measured with some error.

[39]The weight put on leisure is also likely to depend on complementarities relative to other consumption goods.

estimate Eq. (1.8) assuming that k_i is constant. Including a post–Statement dummy, I allow the Statement to have an effect on the hazard rate, but not through the accrual.

The coefficient on the accrual tells us that a $1,000 increase in the accrual decreases the hazard rate of claiming Social Security benefits by 0.74 percentage points.[40]

Table 1.8: Linear probability model of claiming Social Security benefits.

in $100k	(1) ALL	(2) WORKING	(3) RETIRED	(4) WORKING
ACC	-0.74	-0.60	0.36	-0.54
	(0.08)**	(0.09)**	(0.28)	(0.09)**
Forecasted earnings	-0.51	-0.58		-0.26
	(0.01)**	(0.02)**		(0.03)**
SSW	0.09	0.07	0.01	0.06
	(0.01)**	(0.01)**	(0.02)	(0.01)**
Spouse's SSW	0.04	0.03	0.03	0.03
	(0.01)**	(0.01)**	(0.02)	(0.01)**
Post–ET removal	0.17	0.21	0.04	0.06
	(0.02)**	(0.02)**	(0.04)	(0.02)**
Post–Statement	-0.00	0.01	-0.06	0.01
	(0.01)	(0.01)	(0.03)*	(0.01)
Retired Spouse	0.04	0.04	0.03	0.04
	(0.01)**	(0.01)**	(0.02)	(0.01)**
Average ET tax				-0.22
				(0.01)**
Observations	29178	24694	4484	24694
R-squared	0.21	0.22	0.28	0.23

Notes: ACC and SSW are expressed in real 2003 dollars. All regressions control for age dummies, year, year squared, level of education, marital status, AIME at age 55, real estate property wealth, health insurance, difference in age relative to the spouse, SIPP panel dummies, children in the household, pension dummy, veteran status, experience, and experience squared. The baseline worker Clustered (by individual) standard errors in parentheses; * significant at 5 percent; ** significant at 1 percent. *Sample:* SIPP linked to administrative data.

The coefficient on the SSW means that a $10,000 increase in SSW increases the probability of claiming by 0.9 percentage points. Notice that male workers are twice

[40]Panis et al. (2002) estimate a similar regression based on the HRS, though they use a probit and the PV and find a marginal effect of 0.7 percent.

as responsive to their own SSW than to their spouse's SSW. The disutility from work is simply the ratio between the coefficient on the accrual and the coefficient on potential earnings for the working sample, and is equal to $0.74/0.51 = 1.45$, meaning that in retirement workers value consumption 45 percent more. When the spouse is already retired, workers are 4 percentage points more likely to retire. Restricting the analysis to the people who work (who represent 5/6th of the sample), the results are not very different.

As we saw in Figures 1.2, for the working sample there are no significant changes in the hazard rate between the pre– and the post–Statement period. The ET removal has a large effect. In columns (1) and (2) I do not control for the average ET tax (computed using the forecasted earnings), which is why the coefficient on the post–ET removal dummy is quite large and significant. In column (4) I add the average ET tax to the regression. This captures most but not all the effect that was measured by the post–ET dummy. The coefficient on that dummy drops from 0.21 to 0.06, showing that a complete removal of the ET has an effect that cannot be entirely explained by changes in the average tax.[41] Adding the average ET tax also reduces (in absolute values) both the effect of the accrual (from -0.60 to -0.54) and the effect of earnings (from -0.58 to -0.26).

For the retired sample, where the claiming decision is purely financial almost all effects are not significantly different from zero. The only effect that is significant is the one related to the post–Statement dummy. Those who face only a financial decision are 6 percentage points more likely to postpone claiming after receiving the Statement. This is a large effect, and represents in relative terms a 12 percent drop.

Next, I estimate Eq. 1.10, allowing for heterogeneity in k_i. Table 1.9 shows only the coefficients related to the accruals. The coefficients on earnings and SSW are not shown since they are almost identical to those seen in Table 1.8. The first row

[41] I also tried to include the marginal tax, though, as expected given the discrete nature of the claiming decision, controlling for the average tax the effect of the marginal tax is close to zero.

reports the result for the "baseline" worker. This worker is 62, married, white, has a dependent spouse, a high school degree, is not a veteran, and has no private pension.

I again divide the sample into those who work and those who are retired. For each of these groups, the first columns show the baseline effect (α_0), the post–Statement effect for the baseline worker (α_1) and the post–ET removal effect (θ). The remaining effects in the first columns are the estimated γ_0s. The second columns show the $Post - Pre$ effects, the estimated γ_1s.

I start by analyzing the working sample. First, it is important to notice that there is a considerable amount of heterogeneity in the responsiveness to the accruals. The effect at age 62 is more than twice as large as the overall effect we saw before, while, at least in the pre–Statement period, at other ages the effects tend to be significantly smaller. Between age 65 and 67, the effects are not different from zero. The main explanation for this is sample selection. At age 62, those who continue working do so because they face significantly larger accruals. This difference gets smaller as those with small accruals drop out the sample once they claim. Since the population gets more homogenous, accruals lose their predictive power for retirement behavior.

There is no improvement in the responsiveness to ACCs of the baseline worker (aged 62) due to the Social Security Statement. Moreover, there is a slight worsening: the coefficient of 0.41 has the interpretation that a baseline worker, aged 62, etc., shows a $0.41/1.22 = 0.33$ drop in the marginal effect of the accrual on the hazard rate.

There are differential effects relative to age. When compared to individuals at age 62, there is a significant improvement in the effect of accruals on claiming behavior at ages 63 and 64. Before the introduction of the Statement, at age 64, where almost 15 percent of worker claim, the marginal effect was $-1.22 + 0.63 = -0.59$. With the introduction of the Statement the responsiveness increases in absolute values by $0.55 - 0.41 = 0.14$. There are no significant improvements at later ages, but, since

Table 1.9: Random coefficient linear model of claiming.

	WORKING		RETIRED	
		Post – Pre		*Post – Pre*
ACC (Baseline)	-1.22		0.63	
	(0.22)**		(0.78)	
ACC×				
Post–Statement	0.41		0.43	
	(0.29)		(1.19)	
Post–ET removal	-0.82		0.04	
	(0.47)		(1.40)	
Year	0.02	0.01	0.03	-0.11
	(0.02)	(0.04)	(0.08)	(0.12)
Black	0.17	0.82	-0.27	-0.41
	(0.27)	(0.39)*	(0.64)	(1.13)
Single	-0.55	0.91	1.88	0.64
	(0.35)	(0.51)	(1.45)	(1.76)
Independent spouse	-1.13	1.17	2.61	-1.18
	(0.31)**	(0.39)**	(1.00)**	(1.33)
Below high school	-0.05	-0.26	0.50	-0.10
	(0.19)	(0.27)	(0.54)	(0.88)
Some college	-0.13	0.14	0.42	-0.05
	(0.18)	(0.23)	(0.61)	(0.85)
College	-0.39	0.16	-1.13	0.13
	(0.12)**	(0.15)	(0.45)*	(0.64)
Veteran	0.07	-0.18	0.69	-1.33
	(0.12)	(0.14)	(0.47)	(0.62)*
Pension	0.65	-0.68	0.21	0.32
	(0.15)**	(0.19)**	(0.62)	(0.90)
Missing pension info.	-0.09	0.11	-0.72	1.76
	(0.15)	(0.19)	(0.73)	(1.02)
Age 63	1.09	-0.33	-0.99	-0.29
	(0.13)**	(0.14)*	(0.78)	(0.94)
Age 64	0.63	-0.55	0.58	-0.81
	(0.19)**	(0.22)*	(1.22)	(1.50)
Age 65	1.38	0.07	-1.71	0.31
	(0.30)**	(0.52)	(1.18)	(1.65)
Age 66	1.55	0.12	-1.52	0.81
	(0.63)*	(0.85)	(1.98)	(2.17)
Age 67	1.34	-1.66	0.62	1.05
	(0.66)*	(0.89)	(1.96)	(1.91)
Age 68	1.03	-1.18	-1.33	1.18
	(0.70)	(0.93)	(1.88)	(1.84)
Age 69	0.39	0.03	-1.79	2.75
	(1.33)	(1.18)	(2.08)	(2.13)
Observations	24694		4484	
R-squared	0.23		0.29	

Notes: Additional controls as in Table 1.8. Clustered (by individuals) standard errors in parentheses, * significant at 5 percent; ** significant at 1 percent. *Sample:* SIPP linked to administrative data.

few workers claim after age 65, it is difficult to interpret these results.

It is interesting that people seem to behave less optimally at the two peak ages, 62 and 65, while the rest of the population improved their decisions after the Statement was sent out. One explanation for this may lie in the information contained in the Statement. Remember that the Statement informs workers about their future Social Security benefits if they retire at ages 62, or 65, or 70, which may have induced some workers to focus more on these ages than accruals would predict. This result seems to support the view that some workers use age 62 and age 65 as focal points for their retirement decision without paying too much attention to the Social Security incentives.

An interesting result is that in the pre–Statement period workers with a dependent spouse show smaller responsiveness to accruals than single workers. Since married workers are more likely to have contacted the SSA, this effect is somehow puzzling. In fact, married workers with an independent spouse are the most responsive group with respect to Social Security incentives. Their coefficient is almost twice as large compared to those with a dependent spouse, and this difference is highly significant. This is evidence that many married workers didn't, prior to the Statement, take spouses' benefits (and survivors' benefits!) into account. The Statement informs workers about family benefits. In fact, quite surprisingly, the Statement appears to have eliminated the gap between workers with and without a dependent spouse.

Let us now turn to those variables that were good predictors of contacting the SSA for a benefit estimate. There seems to be a small, but not significant, reduction in the differences across levels of education. Consistent with the fact that knowledge was positively correlated with education, the responsiveness to Social Security incentives is also increasing with education in the pre–Statement period. Workers with college education have, compared to workers with high school degrees, coefficients that are in absolute value 39 percentage points larger. These differences get smaller in the post–Statement period, as we would expect given that the Statement is more likely to affect those with lower levels of education.

Compared to the baseline, there is no improvement, but rather a worsening in responsiveness for blacks. While showing no improvements can be consistent with the behavior predicted by a model with costly information (as long as the additional information doesn't move workers away from their boundary solutions), a worsening in the responsiveness represents a puzzle. The worsening is due to the fact that after receiving the Statement blacks are more likely to claim and retire at the age of 62. This is clearly shown in Table 1.5.3 where I measure the unexplained change in the distribution of the retirement age due to the Statement. This is accomplished by adding the interaction of age dummies, race dummies, and a post–Statement dummy in the retirement equation used for the results shown in Table 1.8. The first column shows the result for the whole working sample, while subsequent columns restrict the sample to few years before and after the introduction of the Statement. The significance of the unexplained jump in the hazard for black workers at the age of 62 is robust to this regression discontinuity approach.

Another result is related to the information I have about private pensions. The positive coefficient on pension variables may capture the fact that for this group I am likely to mismeasure the actual accrual. The fact that the coefficient on the pension has almost no effect (0.65-0.68) in the post–Statement period may be due to changes in pension plans. There has been a dramatic transition from defined benefit plans to defined contribution plans (Munnell et al., 2003), which makes it less likely for Social Security accruals to be contaminated by pension accruals in the post–Statement period.

Let us turn now to the retired sample. Here the estimated effects are quite noisy. This is due to the sample size, but also to the fact that there is no more variation in accruals due to earnings. Also, for the retired sample the standard deviation of

Table 1.10: Unexplained Statement–effects on the hazards, by race

Post-Pre Statement	WORKING			
Control Group:	ALL	5 year	4 years	3 years
Age 62, white	0.01	0.02	0.03	0.03
	(0.01)	(0.02)	(0.02)	(0.02)
Age 62, black	**0.10**	**0.11**	**0.13**	**0.14**
	(0.03)**	**(0.05)***	**(0.05)***	**(0.06)***
Age 63, white	0.01	0.03	0.04	0.06
	(0.01)	(0.02)	(0.02)	(0.03)*
Age 63, black	0.05	0.08	0.09	0.15
	(0.04)	(0.06)	(0.07)	(0.08)
Age 64, white	-0.06	-0.03	-0.01	0.00
	(0.02)**	(0.02)	(0.03)	(0.03)
Age 64, black	-0.03	-0.02	0.03	0.07
	(0.06)	(0.08)	(0.09)	(0.11)
Age 65, white	-0.05	-0.09	-0.05	-0.03
	(0.03)*	(0.03)**	(0.03)	(0.04)
Age 65, black	0.04	-0.03	-0.05	-0.06
	(0.07)	(0.10)	(0.12)	(0.12)
Observations	24694	13293	10526	7819

Notes: All regressions control for age dummies, year, level of education, marital status, AIME at age 55, real estate property wealth, health insurance, difference in age relative to the spouse, SIPP panel dummies, children in the household, pension dummy, veteran status, experience, and experience squared. The baseline worker Clustered (by individual) standard errors in parentheses; * significant at 5 percent; ** significant at 1 percent. *Sample:* SIPP linked to administrative data.

the accruals is almost half as large as in the working sample (see Table 13 in the Appendix), which generates noisier regression estimates. The $Post - Pre$ effects are generally not significant, probably because those who face only a financial decision are more likely to have already gathered the information contained in the Statement. Nevertheless, there are some interesting results. The most striking result is that at age 62 the effect has the wrong sign and is not different from zero. The reason for this is that most workers who retire prior to age 62 claim as soon as possible, irrespective of Social Security incentives. At later ages, when those who claim as soon as possible are not in the sample anymore, the marginal effects of the accruals tend to be bigger than for the working sample. At 63, for example, the estimated coefficient is $0.63 - 0.99 = -0.36$ for a retired person, while it is only $-1.22 - 1.09 = -0.13$ for a working person. Few retired workers claim after 63, which translates into large standard errors at later ages.

The difference between high school graduates and college graduates is now even larger when compared to the difference for the working sample. Another difference between the working sample and the retired sample is related to the differential effects by marital status. While workers with a dependent spouse used to be less responsive to the accrual, among those who already retired the effect for singles and for those with an independent spouse is larger. For the retired sample, there is no variation in accruals due to current earnings, and since I control for the SSW and the AIME, the only variation that is left is due to changes over time in the actuarial adjustments, changes in the normal retirement age, and changes in the probabilities of survival. Although I control for the age difference between husband and wife, the probabilities of survival generate a considerable variation in the accruals, though only for workers with a dependent spouse. This may explain why retired workers who are either single or have an independent spouse show effects that are large and with the wrong sign.

1.6 Conclusions

There is empirical evidence that a worker's retirement decision responds to forward-looking retirement incentives. These incentives depend on current and future earnings, and on retirement benefits. Social Security benefits, which represent the most important source of retirement income, are a complicated function lifetime earnings. It is generally assumed that workers know their benefits and are able to compute their retirement incentives.

In order to understand whether this is a reasonable assumption I analyze workers' knowledge. Contacting the SSA represents the single most important channel through which workers learn about their future benefits. I model the probability of contacting the SSA and find evidence that is consistent with the existence of considerable costs of collecting (and processing) information about Social Security benefits: Workers who, for various reasons (health, liquidity, etc.), face simple retirement decisions are less likely to contact the SSA. Additional evidence confirming this result comes from the 1995 introduction of the Social Security Statements. These Statements, which contain an estimate of the worker's benefits if he retires at age 62, 65, and 70, generate an exogenous variation in the cost of obtaining information. Upon receiving a Statement workers are more likely to be able to provide a benefit estimate and their benefit estimate tends to be more precise. Controlling for the endogeneity of the decision to contact the SSA, I find that the whole improvement is concentrated among those workers who do not contact the SSA. I also find evidence of spillovers. Consistent with the importance of spouse benefits for women, female workers improve their knowledge when their husband receives a Statement, but not viceversa.

Then I turn to study how this additional information affects workers' retirement behavior. Given that the Statement reduces the cost of information the model predicts that workers who were at the margin of getting informed make better retirement decisions. I measure optimality based on the correlation between the retirement decision and the Social Security incentives. The empirical model is flexible enough to allow us to measure the sensitivity to Social Security incentives for subgroups of workers. I find that in the pre–Statement period better–informed workers respond more strongly to Social Security accruals. Although the introduction of the Statement doesn't improve the overall responsiveness to the retirement incentives, there is significant heterogeneity across age, marital status, and race. Unpredictably, upon receiving a Statement black workers are more likely to retire and claim as soon as possible. This has the effect of lowering their responsiveness to the retirement incentives. One possible explanation is that upon receiving a Statement black workers, who tend to have a lower life–expectancy, realize that the SSA's actuarial adjustment is not large enough, and therefore claim as soon as possible.

Compared to workers with a dependent spouse and workers who are single, workers with a dependent spouse seem to become more likely to take their spouse life–expectancy into account when computing their Social Security incentives.

Upon receiving a Statement workers who retire at the age of 62 or 65 become less sensitive to Social Security incentives. This is puzzling and suggests that some workers may follow simpler retirement rules and use 62 and 65 as focal points. This finding has important implications for the construction of the Statement. Providing forecasted benefits at all 9 possible claiming ages may improve the decision making for workers retiring at 62 and 65. Also, the Statement provides workers with information about their benefits, but it does not calculate a worker's SSW. If this weakens the beneficial effect of the Statement, a possible addition to the Statement could be a table that assists workers in calculating their SSW. Since the SSA cannot possibly use individual–specific mortality rates, one easy way to circumvent this problem would be to construct a table that contains "suggested" retirement ages as a function of a worker's own and his spouse's life–expectancy.

Bibliography

Fast facts & figures about social security. Technical report, Social Security Administration, August 2004.

Robert L. Axtell and Joshua M. Epstein. Coordination in transient social networks. In Henry Aaron, editor, *Behavioral Dimensions of Retirement Economics*, pages 161–183. Brookings Institution Press & Russell Sage Foundation, 1999.

Douglas B. Bernheim. Social security benefits: An empirical study of expectations and realizations. Technical Report 2257, National Bureau of Economic Research, Inc, May 1987.

Douglas B. Bernheim and Lawrence Levin. Social security and personal saving: An analysis of expectations. *American Economic Review*, 79(2):97–102, 1989.

Alan S. Blinder, Roger H. Gordon, and Donald E. Wise. Reconsidering the work disincentive effects of social security. *National Tax Journal*, 33(4):431–442, June 1981.

Gary Burtless. An economic view of retirement. In Henry Aaron, editor, *Behavioral Dimensions of Retirement Economics*, pages 7–42. Brookings Institution Press & Russell Sage Foundation, 1999.

Sewin Chan and Ann Huff Stevens. What you don't know can't help you: Pension knowledge and retirement decision making. Technical Report 10185, National Bureau of Economic Research, Inc, December 2003.

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

Courtney C. Coile and Jonathan Gruber. Social Security Incentives for Retirement. In David Wise, editor, *Themes in the Economics of Aging*, chapter 10, pages 311–341. University of Chigaco Press, 2001.

Courtney C. Coile and Jonathan Gruber. Social security and retirement. NBER Working Papers 7830, National Bureau of Economic Research, Inc, August 2000.

Courtney C. Coile, Peter Diamond, Jonathan Gruber, and Alain Jousten. Delays in Claiming Social Security Benefits. *Journal of Public Economics*, 84(3):357–385, July 2002.

Peter A. Diamond and Jonathan Gruber. Social Security and Retirement in the U.S. In Jonathan Gruber and David A. Wide, editors, *Themes in the Economics of Aging*. University of Chicago Press, Chicago, 1998.

Esther Duflo and Emmanuel Saez. The Role Of Information And Social Interactions In Retirement Plan Decisions: Evidence From A Randomized Experiment. *The Quarterly Journal of Economics*, 118(3):815–842, 2003.

Alan L. Gustman and Thomas L. Steinmeier. A Structural Retirement Model of Retirement. *Econometrica*, 54(3):555–584, May 1986.

Alan L. Gustman and Thomas L. Steinmeier. Imperfect knowledge, retirement and saving. Technical Report 8406, National Bureau of Economic Research, Inc, August 2001.

Ruth Helman and Variny Paladino. Will Americans Ever Become Savers? Issue Brief 268, Employee Benefit Research Institute, April 2004.

Michael D Hurd. Research on the elderly: Economic status, retirement, and consumption and saving. *Journal of Economic Literature*, 28(2):565–637, 1990.

Howell E. Jackson. Accounting for social security benefits. In *Behavioral Public Finance*. Russell Sage Foundation, 2005.

Alan Krueger and Bruce Meyer. Labor supply effects of social insurance. In *Handbook of Public Economics*, volume 4. North-Holland, Amsterdam, 2002.

Alan B. Krueger and Jorn-Steffen Pischke. The effect of social security on labor supply: A cohort analysis of the notch generation. *Journal of Labor Economics*, 10 (4):412–37, October 1992.

Robin Lumsdaine, James H. Stock, and David A. Wise. Why are Retirement Rates so High at Age 65? In David A. Wise, editor, *Advances in the Economics of Aging*, pages 61–82. University of Chicago Press, 1996.

Annamaria Lusardi and Olivia S. Mitchell. Financial literacy and planning: Implications for retirement wellbeing. DNB Working Papers 078, Netherlands Central Bank, Research Department, January 2006. available at http://ideas.repec.org/p/dnb/dnbwpp/078.html.

Giovanni Mastrobuoni. The Social Security Earnings Test Removal. Money Saved or Money Spent by the Trust Fund? mimeo, April 2005a.

Giovanni Mastrobuoni. Labor Supply Effects of the Recent Social Security Benefit Cuts: Empirical Estimates Using Cohort Discontinuities. mimeo, October 2005b.

Brian P McCall. Testing the proportional hazards assumption in the presence of unmeasured heterogeneity. *Journal of Applied Econometrics*, 9(3):321–34, 1994.

Alicia H. Munnell, Kevin Cahill, and Natalia Jivan. How has the shift to 401(k)s affected the retirement age? Issue in brief 13, Center for Retirement Research, October 2003.

Kevin M Murphy and Robert H Topel. Estimation and inference in two-step econometric models. *Journal of Business & Economic Statistics*, 3(4):370–79, 1985.

Ted O'Donoghue and Matthew Rabin. Procrastination in preparing for retirement. In Henry Aaron, editor, *Behavioral Dimensions of Retirement Economics*, pages 125–156. Brookings Institution Press & Russell Sage Foundation, 1999.

Constantijn Panis, Michael Hurd, David Loughran, Julie Zissimopoulos, Steven Haider, and Patricia St.Clair. The effects of changing social security administration's early entitlement age and the normal retirement age. report for the SSA, RAND, 2002.

Christopher Phelan and John Rust. How Social Security and Medicare Affect Retirement Behavior in a World of Incomplete Markets. *Econometrica*, 65(4):781–831, July 1997.

Andrew A. Samwick. New Evidence on Pensions, Social Security, and the Timing of Retirement. Working Paper 6534, NBER, 1998.

Jae G. Song and Joyce Manchester. New evidence on earnings and benefit claims following the removal of the retirement earnings test in 2000.

James H. Stock and David A. Wise. Pensions, the Option Value of Work and Retirement. *Econometrica*, Vol.58(5):1151–1180, 1990.

.1 Summary statistics

In Table 11, I report how these incentives have changed over time from 1984 to 2002 for a 62-year-old male worker. Quite surprisingly, over the last 20 years, earnings of 62-year-old male worker didn't grow in real terms and were quite low during the early 1990s. The AIME (expressed in yearly terms), on the other hand, has steadily increased over time, suggesting that most of the real growth in earnings happened at younger ages. The SSW is increasing over time as well. Starting in the mid–1990s, relative accruals and relative peak values show an increase.

Table 11: Earnings and Social Security incentives at age 62

Year	Earnings	AIME	$B_{t+1}/B_t - 1$	SSW	ACC/SSW	PV/SSW
1984	26.7	28.4	9.48%	181.1	2.89%	6.79%
1985	25.8	28.6	9.43	184.9	2.81	6.41
1986	25.8	29.5	9.39	191.6	2.79	6.29
1987	26.6	29.6	9.37	194.8	2.84	6.59
1988	24.8	29.7	9.43	195.0	2.83	6.51
1989	25.0	30.1	9.37	198.2	2.83	6.72
1990	25.6	30.7	9.34	200.1	2.77	6.32
1991	23.1	30.2	9.28	195.2	2.71	6.31
1992	22.9	31.3	9.20	200.6	2.67	6.17
1993	23.3	31.3	9.18	197.9	2.54	6.20
1994	22.4	31.8	9.12	203.7	2.54	6.26
1995	21.5	31.3	9.18	193.9	2.45	6.30
1996	22.4	32.9	9.19	205.7	2.59	6.74
1997	24.8	33.2	9.19	205.8	2.59	6.92
1998	24.3	35.4	9.12	215.6	2.54	6.74
1999	27.0	37.9	9.15	231.8	2.69	7.37
2000	26.6	36.9	9.32	223.1	2.72	7.28
2001	24.8	39.2	9.33	229.4	2.69	6.72
2002	24.1	39.6	9.46	227.9	2.80	6.92

Notes: Social security wealth (SSW), accruals (ACC) and peak values (PV) include spouse's benefits. Values are in $1,000. *Sample:* SIPP linked to administrative data.

At age 62, there are two trends that neutralize each other. The first trend, which increases accruals and especially peak values, is due to the increase in life expectancy and the increase in the delayed retirement credit (DRC), the actuarial increase beyond the normal retirement age.[42] The DRC was 3 percent for the 1922–1924 birth cohorts, and has been scheduled to reach 8 percent for workers born in 1943 or later cohorts (increasing by 0.5 percentage points every two years). The other trend, the increase in the AIME, tends to reduce the accruals. The reason is that the weight of current earnings in the benefit formula is decreasing over time. This can be seen by looking at the trend in the growth rate of benefits, and is especially pronounced among younger workers. Table 12 reports the Social Security incentives for a 55-year old worker. It is assumed that workers who retire before age 62 claim as soon as they can, meaning at age 62. The expected average growth rates of Social Security benefits in 1995 are less than half of those in 1977. Since mortality improvements are mostly concentrated at old ages, this reduction shows up in the peak values as well. At age 55, the peak value has been decreasing over time, and this may be responsible for some early withdrawals from the labor force.

[42] The increase in the DRC has an effect on the PV when the peak lies beyond age 65, which with a 3 percent discount rate happens 25 percent of the time.

Table 12: Earnings and Social Security incentives at age 55

Year	Earnings	AIME	$B_{t+1}/B_t - 1$	SSW	ACC/SSW	PV/SSW
1977	35.6	23.8	3.48%	122.5	3.48%	23.34%
1978	34.8	23.8	3.42	124.6	3.42	22.56
1979	37.0	22.4	3.29	118.2	3.29	23.84
1980	35.4	21.4	3.39	115.0	3.39	23.68
1981	34.3	21.6	3.25	115.6	3.25	22.59
1982	34.2	22.1	3.00	119.3	3.00	22.13
1983	36.5	24.2	2.76	131.0	2.76	20.74
1984	36.7	24.0	2.64	127.3	2.64	20.94
1985	36.9	25.3	2.52	132.4	2.52	19.53
1986	36.7	26.3	2.08	135.7	2.08	17.58
1987	35.5	26.3	1.96	139.0	1.96	18.11
1988	36.0	26.4	1.87	135.5	1.87	17.99
1989	37.6	28.3	1.80	145.0	1.80	17.91
1990	35.7	28.1	1.71	141.5	1.71	17.22
1991	37.1	29.2	1.63	144.9	1.63	16.68
1992	37.5	30.5	1.49	152.7	1.49	16.67
1993	34.9	29.5	1.52	145.5	1.52	16.82
1994	36.7	31.6	1.42	151.2	1.42	15.61
1995	35.0	30.3	1.47	142.2	1.47	15.97

Notes: Social security wealth (SSW), accruals (ACC) and peak values (PV) include spouse's benefits. Values are in \$1,000. *Sample:* SIPP linked to administrative data.

Table 13: Summary statistics for the whole SIPP sample (1977–2003)

	WORKING SAMPLE, N=24694				*RETIRED SAMPLE, N=4484*			
	Mean	SD	Min	Max	Mean	SD	Min	Max
P(R=1)	0.44	0.50	0.00	1.00	0.51	0.50	0.00	1.00
ACC ($100k)	0.05	0.07	-0.16	0.26	0.01	0.04	-0.14	0.18
Forecasted earn.($100k)	0.40	0.23	0.00	0.85	0.00	0.00	0.00	0.00
Average ET tax	0.70	0.40	0.00	1.00	0.00	0.00	0.00	0.00
Marginal ET tax	0.41	0.18	0.00	0.50	0.00	0.00	0.00	0.00
SSW ($100k)	2.32	0.98	0.07	5.35	1.41	0.87	0.06	4.77
Spouse SSW ($100k)	0.34	0.55	0.00	2.46	0.32	0.51	0.00	2.53
AIME ($100k)	0.27	0.10	0.00	0.50	0.18	0.12	0.00	0.50
Prop. Wealth ($1m)	0.23	0.65	-7.50	8.00	0.22	0.63	-4.91	6.16
Health Insurance	0.89	0.31	0.00	1.00	0.74	0.44	0.00	1.00
Age difference	-3.19	4.60	-30.00	10.00	-2.81	4.77	-30.00	10.00
SIPP panel 2	0.14	0.35	0.00	1.00	0.14	0.35	0.00	1.00
SIPP panel 3	0.19	0.39	0.00	1.00	0.19	0.39	0.00	1.00
SIPP panel 4	0.18	0.39	0.00	1.00	0.16	0.37	0.00	1.00
SIPP panel 5	0.29	0.45	0.00	1.00	0.28	0.45	0.00	1.00
Children	0.13	0.33	0.00	1.00	0.12	0.32	0.00	1.00
Year	1994	5.41	1984	2003	1994	5.28	1984	2003
Post–ET removal	0.04	0.19	0.00	1.00	0.06	0.24	0.00	1.00
Post–Statement	0.44	0.50	0.00	1.00	0.46	0.50	0.00	1.00
Black	0.07	0.25	0.00	1.00	0.13	0.33	0.00	1.00
Single	0.16	0.37	0.00	1.00	0.29	0.45	0.00	1.00
Retired spouse	0.23	0.42	0.00	1.00	0.22	0.41	0.00	1.00
Below high school	0.14	0.34	0.00	1.00	0.17	0.37	0.00	1.00
Some college	0.13	0.33	0.00	1.00	0.13	0.34	0.00	1.00
College	0.32	0.47	0.00	1.00	0.27	0.45	0.00	1.00
Veteran	0.61	0.49	0.00	1.00	0.66	0.47	0.00	1.00
Experience	38.56	7.48	9.00	52.00	26.45	9.31	9.00	48.00
Age	63.04	1.28	62.00	69.00	63.12	1.62	62.00	69.00
Independent spouse	0.30	0.46	0.00	1.00	0.34	0.47	0.00	1.00
Pension	0.48	0.50	0.00	1.00	0.49	0.50	0.00	1.00
Missing pen. Info	0.34	0.47	0.00	1.00	0.31	0.46	0.00	1.00

Figure 4: The Social Security Statement

Your Estimated Benefits

To qualify for benefits, you earn "credits" through your work— up to four each year. This year, for example, you earn one credit for each $900 of wages or self-employment income. When you've earned $3,600, you've earned your four credits for the year. Most people need 40 credits, earned over their working lifetime, to receive retirement benefits. For disability and survivors benefits, young people need fewer credits to be eligible.

We checked your records to see whether you have earned enough credits to qualify for benefits. If you haven't earned enough yet to qualify for any type of benefit, we can't give you a benefit estimate now. If you continue to work, we'll give you an estimate when you do qualify.

What we assumed— If you have enough work credits, we estimated your benefit amounts using your average earnings over your working lifetime. For 2004 and later (up to retirement age), we assumed you'll continue to work and make about the same as you did in 2002 or 2003. We also included credits we assumed you earned last year and this year.

We can't provide your actual benefit amount until you apply for benefits. **And that amount may differ from the estimates stated below because:**
(1) Your earnings may increase or decrease in the future.
(2) Your estimated benefits are based on current law. **The law governing benefit amounts may change.***
(3) Your benefit amount may be affected by **military service, railroad employment or pensions earned through work on which you did not pay Social Security tax. Visit** ***www.socialsecurity.gov/mystatement*** **to see whether your Social Security benefit amount will be affected.**

Generally, estimates for older workers are more accurate than those for younger workers because they're based on a longer earnings history with fewer uncertainties such as earnings fluctuations and future law changes.

These estimates are in today's dollars. After you start receiving benefits, they will be adjusted for cost-of-living increases.

▼ *Retirement You have earned enough credits to qualify for benefits. At your current earnings rate, if you stop working and start receiving benefits...

At age 62, your payment would be about...	$882 a month
If you continue working until...	
your full retirement age (67 years), your payment would be about...	$1,278 a month
age 70, your payment would be about...	$1,594 a month

▼ *Disability You have earned enough credits to qualify for benefits. If you became disabled right now,

Your payment would be about...	$1,169 a month

▼ *Family If you get retirement or disability benefits, your spouse and children also may qualify for benefits.

▼ *Survivors You have earned enough credits for your family to receive survivors benefits. If you die this year, certain members of your family **may** qualify for the following benefits.

Your child...	$911 a month
Your spouse who is caring for your child...	$911 a month
Your spouse, if benefits start at full retirement age...	$1,215 a month
Total family benefits cannot be more than...	$2,233 a month

Your spouse or minor child may be eligible for a special one-time death benefit of $255.

▼ Medicare You have enough credits to qualify for Medicare at age 65. Even if you do not retire at age 65, be sure to contact Social Security three months before your 65th birthday to enroll in Medicare.

*** Your estimated benefits are based on current law. Congress has made changes to the law in the past and can do so at any time. The law governing benefit amounts may change because, by 2042, the payroll taxes collected will be enough to pay only about 73 percent of scheduled benefits.**

We based your benefit estimates on these facts:

Your name...	Wanda Worker
Your date of birth...	May 5, 1963
Your estimated taxable earnings per year after 2003...	$35,051
Your Social Security number (only the last four digits are shown to help prevent identity theft)...	XXX-XX-2004

2

Help Us Keep Your Earnings Record Accurate

You, your employer and Social Security share responsibility for the accuracy of your earnings record. Since you began working, we recorded your reported earnings under your name and Social Security number. We have updated your record each time your employer (or you, if you're self-employed) reported your earnings.

Remember, it's your earnings, not the amount of taxes you paid or the number of credits you've earned, that determine your benefit amount. When we figure that amount, we base it on your average earnings over your lifetime. If our records are wrong, you may not receive all the benefits to which you're entitled.

- **Review this chart carefully** using your own records to make sure our information is correct and that we've recorded each year you worked. You are the only person who can look at the earnings chart and know whether it is complete and correct.

  Some or all of your earnings from **last year** may not be shown on your *Statement*. It could be that we still were processing last year's earnings reports when your *Statement* was prepared. Your complete earnings for last year will be shown on next year's *Statement*. **Note:** If you worked for more than one employer during any year, or if you had both earnings and self-employment income, we combined your earnings for the year.

- **There's a limit on the amount of earnings on which you pay Social Security taxes each year.** The limit increases yearly. Earnings above the limit will not appear on your earnings chart as Social Security earnings. (For Medicare taxes, the maximum earnings amount began rising in 1991. Since 1994, **all** of your earnings are taxed for Medicare.)

- **Call us right away** at **1–800–772–1213** (7 a.m.–7 p.m. your local time) if any earnings for years **before last year** are shown incorrectly. If possible, have your W-2 or tax return for those years available. (If you live outside the U.S., follow the directions at the bottom of page 4.)

Your Earnings Record at a Glance

Years You Worked	Your Taxed Social Security Earnings	Your Taxed Medicare Earnings
1979	474	474
1980	1,123	1,123
1981	1,983	1,983
1982	3,293	3,293
1983	4,461	4,461
1984	5,600	5,600
1985	6,950	6,950
1986	8,813	8,813
1987	10,941	10,941
1988	12,803	12,803
1989	14,520	14,520
1990	16,308	16,308
1991	17,920	17,920
1992	19,655	19,655
1993	20,534	20,534
1994	21,730	21,730
1995	23,155	23,155
1996	24,838	24,838
1997	26,806	26,806
1998	28,720	28,720
1999	30,824	30,824
2000	33,060	33,060
2001	34,237	34,237
2002	35,051	35,051
2003	Not yet recorded	

Did you know... Social Security is more than just a retirement program? It's here to help you when you need it most.

You and your family may be eligible for valuable benefits:

- When you die, your family may be eligible to receive survivors benefits.
- Social Security may help you if you become disabled— even at a young age.
- It is possible for a young person who has worked and paid Social Security taxes in as few as two years to become eligible for disability benefits.

Social Security credits you earn move with you from job to job throughout your career.

Total Social Security and Medicare taxes paid over your working career through the last year reported on the chart above:

Estimated taxes paid for Social Security:		Estimated taxes paid for Medicare:	
You paid:	$24,723	You paid:	$5,820
Your employers paid:	$24,723	Your employers paid:	$5,820

Note: You currently pay 6.2 percent of your salary, up to $87,900, in Social Security taxes and 1.45 percent in Medicare taxes on your entire salary. Your employer also pays 6.2 percent in Social Security taxes and 1.45 percent in Medicare taxes for you. If you are self-employed, you pay the combined employee and employer amount of 12.4 percent in Social Security taxes and 2.9 percent in Medicare taxes on your net earnings.

Some Facts About Social Security

About Social Security and Medicare...

Social Security pays retirement, disability, family and survivors benefits. Medicare, a separate program run by the Centers for Medicare and Medicaid Services, helps pay for inpatient hospital care, nursing care, doctors' fees, and other medical services and supplies to people age 65 and older, or to people who have been receiving Social Security disability benefits for two years or more. Your Social Security covered earnings qualify you for both programs.

Here are some facts about Social Security's benefits:

- **Retirement**— If you were born before 1938, your full retirement age is 65. Because of a 1983 change in the law, the full retirement age will increase gradually to 67 for people born in 1960 and later.

  Some people retire before their full retirement age. You can retire as early as age 62 and take your benefits at a reduced rate. If you continue working after your full retirement age, you can receive higher benefits because of additional earnings and special credits for delayed retirement.

- **Disability**— If you become disabled before full retirement age, you can receive disability benefits after six months if you have:
  - enough credits from earnings (depending on your age, you must have earned six to 20 of your credits in the three to 10 years before you became disabled); and
  - a physical or mental impairment that is expected to prevent you from doing "substantial" work for a year or more, *or* result in death.

- **Family**— If you're eligible for disability or retirement benefits, your current or divorced spouse, minor children, or adult children disabled before age 22 also may receive benefits. Each may qualify for up to about 50 percent of your benefit amount. The total amount depends on how many family members qualify.

- **Survivors**— When you die, certain members of your family may be eligible for benefits:
  - your spouse age 60 or older (50 or older if disabled, or any age if caring for your children younger than age 16); and
  - your children if unmarried and younger than age 18, still in school and younger than 19 years old, or adult children disabled before age 22.

  If you are divorced, your ex-spouse could be eligible for a widow's or widower's benefit on your record when you die.

Receive benefits and still work...

You can continue to work and still get retirement or survivors benefits. If you're younger than your full retirement age, there are limits on how much you can earn without affecting your benefit amount. The limits change each year. When you apply for benefits, we'll tell you what the limits are at that time and whether work would affect your monthly benefits. When you reach full retirement age, the earnings limits no longer apply.

Before you decide to retire...

Think about your benefits for the long term. Everyone's situation is different. For example, be sure to consider the advantages and disadvantages of early retirement. If you choose to receive benefits before you reach full retirement age, your benefits will be permanently reduced. However, you'll receive benefits for a longer period of time.

To help you decide when is the best time for you to retire, we offer a free booklet, *Social Security—Retirement Benefits* (Publication No. 05-10035), that provides specific information about retirement. You can calculate future retirement benefits on our website at *www.socialsecurity.gov* by using the *Social Security Benefit Calculators*. There are other free publications that you may find helpful, including:

- *Understanding The Benefits* (No. 05-10024)— a general explanation of all Social Security benefits;
- *How Your Retirement Benefit Is Figured* (No. 05-10070)— an explanation of how you can calculate your benefit;
- *The Windfall Elimination Provision* (No. 05-10045)— how it affects your retirement or disability benefits;
- *Government Pension Offset* (No. 05-10007)— explanation of a law that affects spouse's or widow(er)'s benefits; and
- *When Someone Misuses Your Number* (No. 05-10064)— what to do if you're a victim of identity theft.

We also have other leaflets and fact sheets with information about specific topics such as military service, self-employment or foreign employment. You can request Social Security publications at *www.socialsecurity.gov* or by calling us at **1-800-772-1213**.

If you need more information—Visit *www.socialsecurity.gov/mystatement* on the Internet, contact any Social Security office, call **1-800-772-1213** or write to Social Security Administration, Office of Earnings Operations, P.O. Box 33026, Baltimore, MD 21290-3026. If you're deaf or hard of hearing, call TTY 1-800-325-0778. If you have questions about your personal information, you must provide your complete Social Security number. If your address is incorrect on this *Statement*, ask the Internal Revenue Service to send you a Form 8822. We don't keep your address if you're not receiving Social Security benefits.

Para solicitar una *Declaración* en español, llame al 1-800-772-1213.

Chapter 2

Labor Supply Effects of the Recent Social Security Benefit Cuts: Empirical Estimates Using Cohort Discontinuities

2.1 Introduction

In 1983, the US Congress implemented an increase in the Normal Retirement Age (NRA) of two months per year for cohorts born in 1938 and afterward. Due to the way Social Security benefits are computed, each 2-months increase in the NRA translates into a little bit more than a 1 percentage point reduction in Social Security benefits. The cohorts affected by these benefit cuts reached the early retirement age (62) in 2000, and in this chapter I study the effects of these benefit cuts on recent retirement behavior.

The normal retirement age is gradually rising from 65 to 67. Workers born in 1938 face a normal retirement age of 65 years and 2 months, which means that, everything else equal, their benefits are 1 percent smaller than those paid to workers born before 1938, with a NRA of 65. The 1939 cohort has a NRA of 65 and 4 months and so on up to the 1943 cohort, whose NRA will be 66. After a 10-year break, the reform will resume with cohort 1955, reaching an NRA of 67 for workers born after 1960.

Increasing the NRA is likely to influence two important decisions that workers face at the end of their career: (a) when to start collecting Social Security benefits, and (b) when to retire. Since benefits are adjusted actuarially fairly with respect to the entitlement age, the long-term solvency of the Social Security trust fund can potentially only be improved by decisions about retirement. An increase in labor force participation generates more contributions, which are the main source of revenue for the trust fund.

In this chapter, I provide the first ex-post evaluation of what the actual effects of increases in the NRA have been. The evaluation may provide both substantive evidence to guide further reforms and a guide to calibration of structural models of retirement decisions. My results suggest that previous research may have seriously underestimated the influence of a change in the NRA on retirement behavior and raise a hustings question about how best to improve these models. Moreover, the advantage of using the change in the NRA to estimate the effect of Social Security incentives on labor supply is that the exact change in benefits is known, not prone to measurement error, and is exogenous.

Due to the timing of the reform, I treat the 1937 birth cohort as the control group and the post 1938 cohorts, which face a reduction in benefits, as the treatment groups in the analysis. Figure 2.1 plots the fraction of, respectively, male and female workers in the sample who retired form the labor force as a function of their age, both before and after the changes in the NRA. I argue that the most obvious candidate for the distance between the two CDFs of the retirement age is the increase in the NRA.

In terms of changes in the average retirement age the point estimates generally imply, depending on the model used, an increase in the actual age of retirement of around 35 percent to 75 percent of the increase in the NRA.

My estimates are more than three times as large as previous out-of-sample predictions. These predictions suggest that the labor supply response to the change in the NRA would be small, though huge potential uncertainty exists about such predictions. Coile and Gruber (2000) simulate the effects on retirement of a one year increase in the NRA. The effect in the 61 to 65 age range is estimated to reduce the age of retirement by between one-half and two months. Another recent paper that contains simulations of a one year increase in the NRA is a report for the SSA written by Panis et al. (2002). Their estimate on the average retirement age is around seven days. Both studies relied on estimates based on the cross-sectional variation in labor supply that could be related to differences in Social Security benefits. One problem is that Social Security benefits depend on the whole history of wages, and so endogeneity can be an issue. Moreover, present discounted values of future streams of benefits are likely to be measured with error, which may downward bias their estimates.

Despite the reforms passed in 1983, the trust fund is projected to become insolvent in less than forty years. While this date of insolvency is often portrayed by the news media as certain, a great deal of uncertainty surrounds these estimates. One of the most important sources of uncertainty is the behavior of future workers and retirees.[1] To make better predictions, it is important to understand how workers' behavior may have been affected by the increase in the NRA.

Section 2.2 introduces a simple intertemporal model of retirement. It's main purpose is to highlight that in theory transitional effects that arise when benefit cuts are unexpected can generate big changes in the labor supply. Section 2.3 presents the model used for the empirical estimation. Results are shown in section 2.4, while section 2.5 concludes the chapter. Appendix .1 describes the data I use.

[1] See Anderson et al., 2003

2.2 A simple intertemporal model of retirement

Life-cycle theory predicts that a worker's reaction to benefit cuts, a decrease in lifetime income, will depend on when one first learns about the reform. Attentive workers may have started reacting to the reform in 1983, and for these workers, the change in retirement behavior is likely to be small due to 20 years of consumption smoothing. Some workers may have learned about the increase of the NRA in 1995 when the SSA started mailing a Social Security Statement to all workers age 60 and over. The statement shows estimated benefits at different ages of retirement, including the first possible age of retirement and the NRA. In 2000, the SSA also added a special insert to the statement containing the changes in the NRA. Very distracted workers may learn about the benefit at the time they claim the benefits.

The purpose of the proposed model is to show what the expected reaction in terms of both consumption and retirement is as a function of the date at which the worker is informed about the benefit cut. This model is quite standard.[2] It assumes that workers maximize their utility over consumption (C) and the time of retirement (z). Retirement is an absorbing state, and workers claim benefits at the time they retire and face a perfect capital market rate of return r. There is no uncertainty about wages W and mortality. The worker's problem takes the following form:

$$\max_{z,C_t} V(z) = \int_0^z e^{-\delta t} U_W(C_t)dt + \int_z^D e^{-\delta t} U_R(C_t)dt \tag{2.1}$$

s.t.

$$\int_0^D e^{-rt} C_t dt = \int_0^z e^{-rt} W_t dt + \int_z^D e^{-rt} R_t dt \ , \tag{2.2}$$

where D is the date of death. In order to obtain closed form solutions, I assume that

[2]For example, see Colombino (2003) or the working paper version of Hurd et al. (2002).

the utility function is $U_R(C) = \ln C$. Disutility from work is captured by an additive constant $U_W = U_R - \epsilon$, where U_W is a worker's utility level and U_R is that worker's utility in retirement. In this setup, e^{ϵ} represents the factor by which the worker's consumption must be increased to generate the same level of a retiree's utility. Notice that what is called disutility from work may additionally capture the observation that retirees tend to make better consumption choices (Aguiar and Hurst, 2004) and that retirees do not have work related costs. I further assume for simplicity that the rate of preference equals the interest rate, $\delta = r$ and that real wages are constant over time, $W_t = W$. The benefit formula used by the SSA expresses benefits as a function of past wages and increases with the age of retirement, z:

$$R(z, W) \quad = \quad R(W)(1 + g(z - NRA)) \ ,$$

where g represents the actuarial adjustment factor.

In Appendix .2, I show that this simple model gives two important predictions. First, for reasonable parameters, increasing the NRA delays retirement and reduces consumption. This result implicitly assumes that the Social Security rules change at time zero, when the worker starts working. Second, for reasonable parameters, if the rules change when the worker is already working, the reaction in terms of consumption and retirement is stronger. This occurs because an early-informed worker has more time to smooth consumption over time, and thus will not postpone retirement as much as a late-informed one.

2.3 Empirical Strategy

The estimation strategy is to measure the distance between the cumulative distribution functions (CDFs) of the retirement age of workers with different NRAs. I estimate the following linear model by least squares:

$$y_i = \sum_{a=61}^{65} 1(A_i = a) \left(\alpha_a + \sum_{c \neq 1937} \beta_{a,c} 1(C_i^* = c) \right) + \gamma' X_i + \epsilon_i \ , \tag{2.3}$$

where y_i is equal to 1 when the worker is retired and zero otherwise. $1(A_i = a)$ is equal to 1 if the worker is a years old and 0 otherwise, and $1(C_i^* = c)$ is equal to 1 if the worker is born in year c and 0 otherwise.

Since I include all age dummies and I omit the 1937 cohort dummy and the constant term, the $\beta_{a,c}$ measures the difference between cohort c and cohort 1937 at age a in the CDFs of the retirement age, $\widehat{\beta}_{a,c} = E[Y|C = c, a, X = 0] - E[Y|C = 1937, a, X = 0]$.

One problem with evaluating this reform is that using CPS data the year of birth variable can sometimes be misclassified. CPS data contain a precise measure of the age of the respondent in the survey week,[3] but this information coupled with the information of the survey year provides at least an imperfect measure of the year of birth. Misclassification errors are not uncommon in empirical research. Gruber and Orszag (2003) in a paper that analyzes the impact of the earnings test on labor supply take the most conservative approach of deleting observations for which ambiguity exists about the earnings test regime. Krueger and Pischke (1992) warn the reader about the a probability of misclassification of around 20 percent when using the March CPS to establish the year of birth, but they do not correct for that.

Since months of birth are approximately uniformly distributed (Table 2.3), the

[3]CPS respondents provide their date of birth, though this information is later discarded from the public-use data. Unfortunately, because of the weak follow-up and the noisy identification of observations across waves, using the longitudinal component of the CPS allows me to get an exact measure of the year of birth for only a small number of observations. To match observations over time, I use the conservative approach of first matching by the CPS identifiers (hrhhid huhhnum hurespl), race and gender. Whenever, after this first step the standard deviation of age is bigger than one half, I additionally match by education, which for elderly people is normally constant over time (Madrian and Lefgren, 1999).

probability of misclassifying the year of birth based on the survey month is known. If I take the simple approach of generating the birth cohort as the difference between the survey year and age, $cohort = year - age$, for example in the January survey the probability of misclassifying someone who was born in 1936 as a 1937 cohort is (assuming that the survey is carried out in the last day of the month) around 11/12. The reason is that someone surveyed in January is very likely to have his birthday later in the year. The probability of misclassification is 10/12 in February, and, carrying out the calculation, zero in December. The probability of misclassification using this method would be on average equal to one half.

A better way to assign the birth year is to minimize the probability of misclassification. Adding a year to the cohort if the survey month is one of the first six months of the year causes the average probability of misclassification to drop to one quarter. When I use this definition I call it the naive method. When I use this method and I additionally restrict the sample to the January and December surveys the probability of misclassification is only one over twelve.[4] I call this method the restricted method.

There is an obvious trade-off between minimizing the probability of misclassification and maximizing the statistical power. In order to work with the whole sample, instead of minimizing the error, I can use the information about the probability distribution of the misclassification errors and estimate a model with misclassification (Aigner, 1973). The only empirical paper I am aware of that uses a similar approach is Card and Krueger (1992). Let $Y \in \{0,1\}$ be 1 if the worker is retired and define C^* to be the true cohort and C the observed cohort. The misclassification probabilities are known and assumed to depend only on the survey month m, $p(m) = \Pr(C^* = c-1|C = c, m)$. If education or other regressors are correlated with the month of birth,[5] the estimator may not be consistent. $\Pr(Y = 1|C = c, m, a, X) = E[Y|C = c, m, a, X]$ repre-

[4]To be more precise, given that the survey week always contains the 19th of the month, the probability is 19/365 in January and 11/365 in December.

[5]See Angrist and Krueger (1991).

sents the conditional probability of being retired by age a, given that a worker is observed in month m to be born in year c, while $E[Y|C^* = c, m, a, X]$ represents the probability of being retired given that a worker is truly born in year c. For ease of notation I will discard the other independent variables X, but probabilities that are not misclassification probabilities are supposed to be conditional on X.

Assuming that given the true cohort the mismeasured one is not informative, I have that

$$E[Y|C = c, C^* = c, m, a] = E[Y|C^* = c, m, a] \ .$$

By the law of total probability

$$E[Y|C = c, m, a] = (1 - p(m))E[Y|C^* = c, m, a] + p(m)E[Y|C^* = c - 1, m, a] \ .$$

The probability of being retired depends on the survey month as well, since, conditional on a birth year (the true or the observed one), later in the year workers tend to be older. Assuming that conditional on the cohort C^*, the dependency on the survey month is additively separable and does not change across cohorts, $E[Y|C^* = c, m, a] = E[Y|C^* = c, a] + g(m, a)$. If all workers were retiring only in January, $g(m, a)$ would be 0. Plugging this into the previous equation, I get that

$$E[Y|C = c, m, a] = (1 - p(m))E[Y|C^* = c, a] + p(m)E[Y|C^* = c - 1, a] + g(m, a)$$

Averaging over the different survey months after defining $p = \sum_m p(m) \Pr(M = m)$ results in

$$E[Y|C = c, a] = (1 - p)E[Y|C^* = c, a] + pE[Y|C^* = c - 1, a] + g(a) \ ,$$

where $g(a) = E(g(m))$ The main reason for specifying the dependence of retirement on the survey is to remember that in the empirical analysis it is important to keep a similar distribution of survey months when comparing different cohorts. Having this in mind, if all months of the year are included in the empirical analysis, from the definition $E[Y|C^* = c, m, a] = E[Y|C^* = c, a] + g(m, a)$, it follows that $g(a)$ is zero.

Solving for the true effect, I get a recursive formula, where the true probability of being retired for a given cohort is a function of the observed probability and the true probability of the previous cohort,

$$E[Y|C^* = c, a] = \frac{E[Y|C = c, a] - E[Y|C^* = c - 1, a]p}{1 - p} - g \ . \tag{2.4}$$

At this point, I need a starting point for the recursion. I use $E[Y|C = 1935, a] = E[Y|C^* = 1936, a]$ as a staring point, which implies that $E[Y|C^* = 1936, a] = E[Y|C = 1936, a]$. This allows me to analyze the differences in the CDF between two pre-reform cohorts, the 1936 and the 1937 one.

It can be shown that this recursion can be implemented by estimating the following linear model

$$y_i = \sum_{a=61}^{65} 1(A_i = a) \left(\sum_{c \neq 1937} \gamma_{a,c} \Pr(C_i^* = c) \right) + \gamma' X_i + \epsilon_i \ , \tag{2.5}$$

with the initial condition $\Pr(C_i^* = 1936) = \{0, 1\}$.

Conditioning on c, a, and $X = 0$:

$$E[Y|C = c, a, X = 0] = \gamma_{a,c} \Pr(C^* = c|C = c) + \gamma_{a,c-1} \Pr(C^* = c - 1|C = c)$$
$$= \gamma_{a,c}(1 - p) + \gamma_{a,c-1}p \quad (2.6)$$

Rearranging terms,

$$\gamma_{a,c} = \frac{E[Y|C = c, a, X = 0] - \gamma_{a,c-1}p}{1 - p} , \quad (2.7)$$

which resembles Eq. 2.4.

Instead of estimating $\widehat{\gamma}_{a,c}$, I estimate $\widehat{\beta}_{a,c} = \widehat{\gamma}_{a,c} - \widehat{\gamma}_{1937,c}$ using eq. 2.3 with the only difference being that $\Pr(C_i^* = c)$ substitutes for the previous $1(C_i^* = c)$.

$\beta_{a,c}$ measures the difference between the cohorts' cumulative distribution functions that I showed in the figures, and controls for differences that may be due to observable characteristics X. It is assumed that adjacent birth cohorts do not differ by unobservable factors that affect retirement behavior.

A more easily interpretable result can be obtained from the sum of the estimated coefficients. If outside the 62–65 window, the CDFs of the different cohorts are not related to the change in the NRA, this sum measures the effect of the reform on the average retirement age. For the 1938 cohort, for example,

$$\begin{aligned} E[A_{38}] - E[A_{37}] &= \sum_{a=62}^{66} a[\Pr_{38}(A = a) - \Pr_{37}(A = a)] \\ &= \sum_{a=62}^{66} a(\beta_{a,38} - \beta_{a-1,38}) \\ &= 62(\beta_{62,38} - \beta_{61,38}) + ... + 66(\beta_{66,38} - \beta_{65,38}) \\ &= 62(\beta_{62,38} - 0) + ... + 66(0 - \beta_{65,38}) \\ &= -\sum_{a=62}^{65} \beta_{a,38} \ . \end{aligned} \quad (2.8)$$

Notice that for men the differences outside of the 62–65 window are indeed smaller (Figure 2.2), while for women the difference at age 61 is quite large (Figure 2.3). By not including differences outside the 62–65 window I may underestimate the effect on the average retirement age.

Tables 2.1 and 2.2 contain the summary statistics of the two samples that are used later in the analysis. On average, the observed birth cohorts are quite similar.

2.4 Estimation Results

First, I divide workers according to whether they were born before or after 1938 based on the naive method. Figure 2.1 shows the corresponding CDFs for the female (right panel) and the male sample (left panel). The primary alternative hypothesis is that this gap is a product of a preexisting trend towards later retirement. As Quinn (1999) showed, the trend towards early retirement stopped in the late 80s and early 90s. However, if there was a trend towards later retirement that is unrelated to the benefit cuts, there would be a significant difference in labor force participation rates across all birth cohorts, while Figures 2.2 and 2.3 show that the CDFs of workers born between 1935 and 1937 are very similar. Instead, the increase in labor force participation starts with workers born in 1938 or later, and these are precisely the workers who experienced benefit cuts.

In order to see whether these differences are significant, I measure the difference between the CDF's by estimating equation (2.3), separately for men and women. The estimated distance between the cumulative distribution functions ($\widehat{\beta}_2$) are shown in Tables 2.4 and 2.5. For each of the three models columns (1), (3) and (5) contain only age and cohort dummies, while columns (2), (4) and (6) additionally control for marital status, education, race, children in the household, total members of the household, geographic region, veteran status and whether the household resides in a metropolitan area. Controlling for these variables seems to reduce the estimated changes by very little. Table 2.6 shows the sum of the estimated coefficients (multiplied by 12, to obtain monthly values), the sample equivalent of equation (2.8). These estimates represent the change of the average retirement age with respect to the 1937 cohort.

The first striking result from Tables 2.4 and 2.5 is that for all three models and for both, men and women, the estimated difference in CDFs between, on one side, the 1938, 1939, and 1940 cohorts, and on the other side, the 1937 cohort, are almost always negative, meaning that in the 61 to 65 age range, the CDF of the 1937 cohort lies above the CDF of the other three cohorts. Although not all post–reform $\widehat{\beta}$s are significant, their sums are, apart from few exceptions, highly significant (Table 2.6), which suggests that the increase in the NRA generated an increase in the reform which had a significant effect on the average retirement age. On the other hand, the 1936 and 1937 CDF are quite similar, and this translates into changes in the average retirement age that are not significantly different from 0.

Due to the misclassification error, the estimates of the naive model are generally smaller, while the sophisticated method produces effects that tend to lie in between the other two. Since the naive method is likely to underestimate the true change, I will focus on the results based on the other two methods.

Women born in 1938 have a change in their average retirement age that lies between 1 and 1.4 months. Since their NRA increased by 2 months, in relative terms the change is between 50 and 67 percent (results shown in squared brackets). The average retirement age of women born one year later, in 1939, increased by 0.9 to 1.6 months, which corresponds to a lower relative change (0.22 to 0.40). The relative change is estimated to be larger for the 1940 cohort (0.26 using the sophisticated method and 0.47 using the restricted method).

A similar pattern arises for the male sample. There are very large relative changes for the 1938 cohort (0.83 to 1), smaller changes for the 1939 cohort (0.24 to 0.37), and changes that lie in between for the 1940 cohort (0.43 to 0.47).

2.4.1 Alternative explanations

The identification is based on the assumption that the observed change in labor force participation across contiguous birth cohorts is due to the change in the NRA. The fact that for the 1938 and 1939 cohorts the estimated β_2s are negative at all ages allows me to rule out that single shocks are driving all the results. Take for example the stock market crisis of 2001. Workers with defined contribution plans may react to such shocks by working longer to make up for the financial losses. While this may explain single differences in the cumulative distribution functions, in particular the 1939–1937 one at age 62 or the 1938–1937 at age 63, it cannot explain the other differences, where cohorts are affected in a similar way. Notice also that at the time of the 2002–2003 stock market crisis the youngest cohort (1940) is already 63 years old. Unless the effect related to the stock market crisis is heterogenous across age, it will difference out when summing up the coefficients to get the effect on the average retirement age. Moreover, Coile and Levine (2004) find no evidence that changes in the stock market drive aggregate trends in labor supply. This is mainly due to the fact that, although 45 percent of all workers are covered by a pension plan, few of them have substantial stock holdings.

Another possible confounding effect is the 2000 Earnings Test removal above the NRA. Earnings of Social Security beneficiaries above the earnings test threshold, up to their benefit amount, are taxed away at a 50 percent rate between age 62 and 65, and, before 2000, at a 33 percent rate between 65 and 69. While the benefit that are taxed away due to the earnings test are not lost, but rather postponed at an actuarially fair rate, there is some evidence that people perceive the earnings test as a pure tax.[6] Since my analysis is focused on ages 61 to 65, this change may affect my results only if there are spillover effects on younger ages, in other words only if workers decide to continue working in order to reach the age at which they can work without being taxed. Analogous to the previous case, this would only affect single differences, particularly the 1939-1937 at age 61 or the 1938-1937 at age 62, and only if spillover effects reach back 4-5 years!

In order to exclude that results are driven by labor market shocks, the same equation has been estimated using weekly hours of work as the dependent variable. There are no significant differences in hours of work across these cohorts. The results are also not driven by differences in part-time work or disability status. Including disabled workers, or part-time workers (work less than 35 hours) in the retired workers also does not alter the results (results available upon request).

2.5 Conclusions

An aging population and low labor force participation rates for older workers have worsened the financial situation of the Social Security Trust Fund. Aware of this, some twenty years ago, several reforms were passed on the recommendation of the Greenspan commission. Their aim was to cut benefits and increase labor force participation. Among other changes, the reform scheduled an increase in the normal retirement age for workers born after 1938.

I find evidence that workers reacted very strongly to this increase in the NRA. The average retirement age for the 1938 and 1939 birth cohorts increased by around 1.5 months. Given that the change in the NRA was for these two cohorts, respectively, 2 and 4 months, this represents a large effect. Workers born in 1940, who face a 6 months increase in their NRA show larger changes. To calculate a precise estimate, the analysis presented in this chapter must be repeated in a few years. Given that

[6] See Gruber and Orszag (2003).

there is ongoing intense work to reform Social Security, conducting early analysis with limited data is, I believe, essential.

Previous studies, using out-of-sample predictions, have estimated much smaller effects on labor force participation. Beside the fact that these estimates are based on older cohorts that may respond differently to retirement incentives, three major factors may have downward-biased previous results. The first is that these models are not capturing any effect due to norms that may be related to the NRA. There is strong evidence that workers may look at the NRA as a focal point. The second is that estimates based on these models, since benefits are a function of past earnings, may suffer from endogeneity bias. The third is that these models, since they are estimated using cross-sectional variation in Social Security benefits and retirement status, may capture long-term effects, while the 1983 reforms may have been unexpected. Using a simple intertemporal model of retirement, I show how this can generate larger changes in the average retirement age than would otherwise be expected. The third problem is that in order to construct Social Security wealth, a component of all forward-looking incentives to retire, the researcher needs detailed information about past and future earnings, interest rates, and preferences; in short, measurement error may be an issue. The increase in the NRA generates a clear reduction in Social Security Wealth that is free of measurement error.

Despite the 1983 reform, the Social Security trust fund is projected to become insolvent in around 40 years. The Social Security projections are only one of several projections made by other institutions. A common feature of all projections is that they depend heavily on the way the future behavior is modeled. My results may help to evaluate the importance of the increase of the NRA on labor force participation.

According to the 2003 Technical Panel on Assumptions and Methods (Tru, 2003) little documentation is available on the trustees' way of forecasting labor force participation. The same panel explains that the base methodology is based on three steps: The first is to estimate autoregressive "age, sex, marital status, and presence of children" specific labor force participation rates models that control for economic, demographic, and policy variables. For older people, instead of LFPRs, hazard rates are used, and Social Security benefits (relative to past earnings) and the fraction of workers affected by the Social Security earnings test are included in the regressions, though it is not clear how big the age groups are. The second step is to subjectively adjust some estimated coefficients based on economic theory, prior beliefs, and the "full mosaic" of all estimated models. The last step is to estimate fitted values based on projections of explanatory variables. This model is likely to be accurate in cases of smooth changes over time. The problem is that the increase in the NRA may have introduced a break in the trend at the very end of the period used by the trustees. Therefore, difficult to detect, especially if age groups are merged together. According to the 2004 Trustees report (Tru, 2004) "... changes in available benefit levels from Social Security and increases in the normal retirement age, and the effects of modifying the earnings test are expected to encourage work at higher ages. Some of these factors are modeled directly."

The 2000 Technical Panel on Assumptions and Methods (Tru, 2003) recommends that "Social Security should be considered explicitly since it may result in higher participation rates." If the increase in NRA affects workers born after 1939 the way it seems to have affected the 1938, 1939, and 1940 birth cohorts, the trustees should definitely follow this recommendation.

Bibliography

Technical panel on assumptions and methods. Report to the social security advisory board, Washington D.C., 2003.

The 2004 Annual Report. Technical report, The Board Of Trustees, Federal OASDI Trust Funds, 2004.

Mark Aguiar and Erik Hurst. Consumption vs. expenditure. NBER Working Papers 10307, National Bureau of Economic Research, Inc, February 2004.

Dennis J. Aigner. Regression with a binary independent variable subject to errors of observation. *Journal of Econometrics*, 1(1):4960, 1973.

Michael Anderson, Ronald Lee, and Shripad Tuljapurkar. Stochastic Forecasts of the Social Security Trust Fund. CEDA Papers 20030005CL, Center for the Economics and Demography of Aging, University of California, Berkeley, 2003.

Joshua D. Angrist and Alan B. Krueger. Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014, November 1991.

Mary Bowler, Randy E. Ilg, Stephen Miller, Ed Robison, and Anne Polivka. Revisions to the Current Population Survey Effective in January 2003. Employment and earnings, Bureau of Labor Statistics, February 2003. http://www.bls.gov/cps/cpsoccind.htm.

David Card and Alan B. Krueger. Does school quality matter? returns to education and the characteristics of public schools in the united states. *Journal of Political Economy*, 100(1):1–40, February 1992.

Courtney C. Coile and Jonathan Gruber. Social security and retirement. NBER Working Papers 7830, National Bureau of Economic Research, Inc, August 2000.

Courtney C. Coile and Phillip B. Levine. Bulls, bears, and retirement behavior. Nber working papers, National Bureau of Economic Research, Inc, September 2004.

Ugo Colombino. A simple intertemporal model of retirement estimated on italian cross-section data. *Labour*, 17:115–137, 2003.

Jonathan Gruber and Peter Orszag. Does the social security earnings test affect labor supply and benefits receipt? *National Tax Journal*, 4(56):755, 773 2003.

Michael D. Hurd, James P. Smith, and Julie M. Zissimopoulos. The effects of subjective survival on retirement and social security claiming. NBER Working Papers 9140, National Bureau of Economic Research, Inc, September 2002.

Alan B. Krueger and Jorn-Steffen Pischke. The effect of social security on labor supply: A cohort analysis of the notch generation. *Journal of Labor Economics*, 10 (4):412–37, October 1992.

Brigitte C. Madrian and Lars John Lefgren. A note on longitudinally matching current population survey (CPS) respondents. NBER Technical Working Papers 0247, National Bureau of Economic Research, Inc, November 1999.

Constantijn Panis, Michael Hurd, David Loughran, Julie Zissimopoulos, Steven Haider, and Patricia St.Clair. The effects of changing social security administration's early entitlement age and the normal retirement age. report for the SSA, RAND, 2002.

Joseph F. Quinn. Has the early retirement trend reversed? Technical Report 424, Boston College Department of Economics, May 1999.

.1 Data

I use the CPS monthly data from January 1998 to December 2004. The CPS data contain information about the respondent's age by the end of the survey week, usually the second week of the month.[7]

I restrict the data to individuals born between 1936 and 1939, aged 61 to 65. While the upper limit 65 is chosen because it is the last available age for the 1939 cohort, the lower limit of 61 is chosen to avoid attributing differences before that age to the change in the NRA. Workers who retire early need to wait at least until age 62 in order to claim the benefits. Differences in retirement rates before 62 are therefore unlikely to be related to the increase in the NRA. Including age 61 allows for possible small spillover effects. However, these restrictions represent conservative choices and may underestimate the overall effect since, as will be shown later, differences in retirement rates under age 62 and above age 65 tend to be smaller, the bias is likely to be small. I also discard data on individuals who are not in the labor force and do not report themselves as retired. This group includes people with disabilities. The CPS has a much bigger sample size than the Health and Retirement Survey (HRS). For each 1937-1939 birth cohort, aged between 61 and 65 there are around 60.000 observations, while the HRS contains only 1000 observations for people born in 1937 and aged 61 to 63. Also, the last available HRS was collected in 2002, when workers born in 1938 were only 64.

The disadvantage of these data is that there is no information on Social Security insured status or pension benefits. Fortunately, almost all active and retired men and women above 62 are eligible for Social Security benefits (Panis et al., 2002). Thus the reform is likely to affect almost the entire cohort of workers born after 1938. This allows me to estimate labor supply responses to benefit cuts without actually

[7]The reference week for CPS is the week (Sunday through Saturday) of the month containing the 12th day.

observing individual benefits.

In the analysis I use unweighted data. When I use CPS weights, the distance between the distribution functions is slightly smaller for men. The reason for this may lie in the revision to the weighting procedures, and the switch from the 1990 to the 2000 Census, that, according to the Bureau of Labor Statistics, affected the comparability of the CPS data series over time (Bowler et al., 2003). Since this revision fell exactly in the middle of the sample, the analysis is consucted without using CPS weights.

.2 The inter–temporal model or retirement

The first order conditions of the model are:

$$\begin{aligned} dz &: U_W(C_t) = U_R(C_t) - \mu(W_z - R_z(z) + \int_z^D e^{r(z-t)} \frac{\partial R_t(z)}{\partial z} dt) \\ dC &: \frac{\partial U_x(C_t)}{\partial C_t} = \mu \quad x = W, R \end{aligned}$$

Given these assumptions, the system of equations that define the equilibrium is:

$$\epsilon C = W - R(1 + \frac{.05}{10}(z - NRA)) + R\frac{.05}{10}(\frac{1}{r} - \frac{1}{r}e^{r(z-D)})$$

$$\begin{aligned} C &= \frac{1 - e^{-rz}}{1 - e^{-rD}} W + \frac{e^{-rz} - e^{-rD}}{1 - e^{-rD}} R(1 + \frac{.05}{10}(z - NRA)) \\ &= \alpha(z)W + (1 - \alpha(z))R(1 + \frac{.05}{10}(z - NRA)) \end{aligned}$$

Totally differentiating:

$$\begin{pmatrix} 1 & \frac{re^{-rz}}{1-e^{-rD}}((1+\frac{.05}{10}(z-NRA))R-W)-\frac{.05}{10}R\frac{e^{-rz}-e^{-rD}}{1-e^{-rD}} \\ \epsilon & \frac{.05}{10}R(1+e^{r(z-D)}) \end{pmatrix} \begin{pmatrix} dC \\ dz \end{pmatrix}$$

$$= \begin{pmatrix} -\frac{.05}{10}R\frac{e^{-rz}-e^{-rD}}{1-e^{-rD}} \\ \frac{.05}{10}R \end{pmatrix} dNRA$$

and solving:

$$\begin{pmatrix} \frac{dC}{dNRA} \\ \frac{dz}{dNRA} \end{pmatrix}$$

$$= \frac{1}{\Delta} \begin{pmatrix} .005R\left(-1+e^{-rD}\right)\left(1+e^{-r(-z+D)}\right) & re^{-rz}(R-W)+.005Re^{-rz}(rz-rNRA+e^{r(z-D)}-1) \\ -\epsilon\left(-1+e^{-rD}\right) & \left(-1+e^{-rD}\right) \end{pmatrix}$$

$$\begin{pmatrix} -\frac{.05}{10}R\frac{e^{-rz}-e^{-rD}}{1-e^{-rD}} \\ \frac{.05}{10}R \end{pmatrix},$$

where

$$\Delta = \frac{.05}{10}R(\left(1+e^{-r(-z+D)}\right)\left(-1+e^{-rD}\right)+\epsilon e^{-rz}(r(z-NRA)+e^{r(z-D)}-1)$$
$$-\epsilon re^{-rz}(W-R)\ .$$

Notice that if $r(z-NRA)+e^{r(z-D)}-1<0$, then $\Delta<0$. The first expression can only be positive if the worker retires after her NRA ($z>NRA$) and the interest rate is extremely large. It follows that for reasonable parameters the retirement age increases when the NRA increases,

$$\frac{dz}{dNRA} = \frac{\frac{.05}{10}R}{\Delta}(-\epsilon\left(e^{-rz}-e^{-rD}\right)-1+e^{-rD})>0\ , \tag{9}$$

while consumption decreases if,

$$\frac{dC}{dNRA} = \frac{\left(\frac{.05}{10}R\right)^2}{\Delta} e^{-rz} \left(e^{r(z-D)} \left(1 - e^{r(z-D)}\right) + r \left(\frac{R-W}{\frac{.05}{10}R} + z - NRA \right) \right) < 0.$$

or

$$e^{r(z-D)} \left(1 - e^{r(z-D)}\right) + r \left(\frac{R-W}{\frac{.05}{10}R} + z - NRA \right) > 0 \; .$$

Notice that the first term is always positive, while the second is not. Now assume that an increase of NRA to NRA' has not been anticipated. Up to time z the worker behaves as in the previous case

$$\epsilon C = W - R(1 + \frac{.05}{10}(z - NRA)) + R\frac{.05}{10}(\frac{1}{r} - \frac{1}{r}e^{r(z-D)})$$

$$C \;\; = \;\; \frac{1-e^{-rz}}{1-e^{-rD}}W + \frac{e^{-rz} - e^{-rD}}{1-e^{-rD}}R(1 + \frac{.05}{10}(z - NRA))$$

After time z, the new objective is:

$$\max_{z,C_t} V(z) = \int_z^{z'} e^{-rt}U_W(C_t)dt + \int_{z'}^{D} e^{-rt}U_R(C_t)dt$$

s.t.

$$\int_0^z e^{-rt}C_t dt + \int_z^D C_t' dt = \int_0^{z'} e^{-rt}W_t dt + \int_{z'}^{D} e^{-rt}R_t dt$$

or simplifying as before, s.t.

$$C(1-e^{-rz}) + C'(e^{-rz} - e^{-rD}) = \left(1 - e^{-rz'}\right) W + \left(e^{-rz'} - e^{-rD}\right) R(1 + \frac{.05}{10}(z' - NRA'))$$

Combining the FOCs:

$$\epsilon C' = W - R(1 + \frac{.05}{10}(z' - NRA')) + R\frac{.05}{10}(\frac{1}{r} - \frac{1}{r}e^{r(z'-D)})$$

$$\begin{pmatrix} 1 & \frac{-re^{-rz'}}{e^{-rz}-e^{-rD}}W + \frac{re^{-rz'}}{e^{-rz}-e^{-rD}}(1+\frac{.05}{10}(z'-NRA'))R - \frac{.05}{10}R\frac{e^{-rz'}-e^{-rD}}{e^{-rz}-e^{-rD}} \\ \epsilon & \frac{.05}{10}R(1+e^{r(z-D)}) \end{pmatrix} \begin{pmatrix} dC' \\ dz' \end{pmatrix}$$
$$= \begin{pmatrix} -\frac{.05}{10}R\frac{e^{-rz'}-e^{-rD}}{e^{-rz}-e^{-rD}} \\ \frac{.05}{10}R \end{pmatrix} dNRA'$$

$$\begin{pmatrix} \frac{dC'}{dNRA'} \\ \frac{dz'}{dNRA'} \end{pmatrix} = \begin{pmatrix} 1 & \frac{-re^{-rz'}}{e^{-rz}-e^{-rD}}W + \frac{re^{-rz'}}{e^{-rz}-e^{-rD}}(1+\frac{.05}{10}(z'-NRA'))R - \frac{.05}{10}R\frac{e^{-rz'}-e^{-rD}}{e^{-rz}-e^{-rD}} \\ \epsilon & \frac{.05}{10}R(1+e^{r(z-D)}) \end{pmatrix}^{-1}$$
$$\begin{pmatrix} -\frac{.05}{10}R\frac{e^{-rz}-e^{-rD}}{1-e^{-rD}} \\ \frac{.05}{10}R \end{pmatrix}$$

Solving gives that

$$\frac{dz'}{dNRA'} = \frac{\frac{.05}{10}R}{\Delta'}\left[-\epsilon\left(e^{-rz'} - e^{-rD}\right) - e^{-rz} + e^{-rD}\right] > 0 \ ,$$

where

$$\Delta' = .005R\left(\left(1+e^{r(z-D)}\right)\left(e^{-rD} - e^{-rz'}\right) + \epsilon e^{-rz}\left(r(z-NRA) + e^{-r(D-z)} - 1\right)\right)$$
$$-\epsilon r e^{-rz}\left(W - R\right) < 0 \ .$$

To show that the myopic worker has, ceteris paribus, a higher optimal age of retirement after the an increase of NRA, I evaluate $\frac{dz}{dNRA}$ at $NRA' = NRA$ and

$z = z'$. To show that

$$\frac{dz'}{dNRA'}(NRA' = NRA, z = z') > \frac{dz}{dNRA}.$$

after some algebra, it is sufficient to show that,

$$e^{r(z-D)}\left(1 - e^{r(z-D)}\right) + r\left(\frac{R-W}{\frac{.05}{10}R} + z - NRA\right) > 0\ , \tag{10}$$

which is the same condition that determines consumption to decrease when benefits are cut.

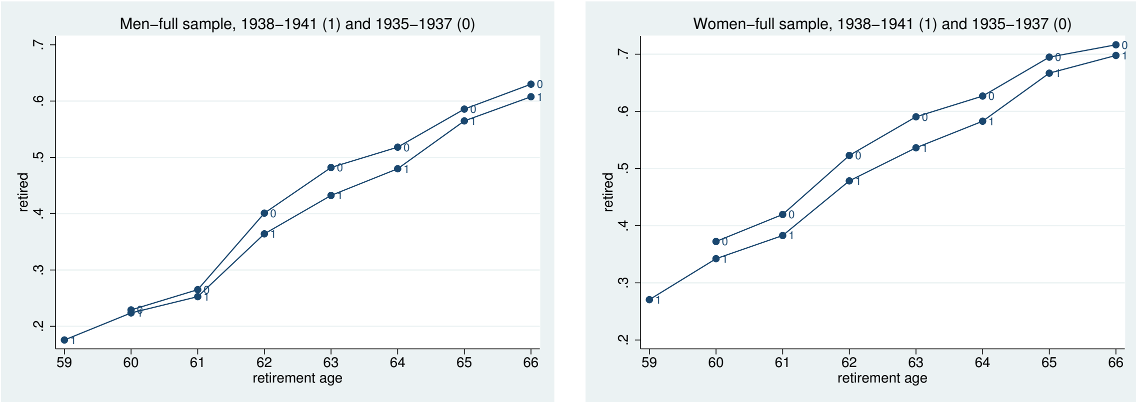

Figure 2.1: Cumulative distribution function of retirement age for workers affected (1) and not affected (0) by the reform.

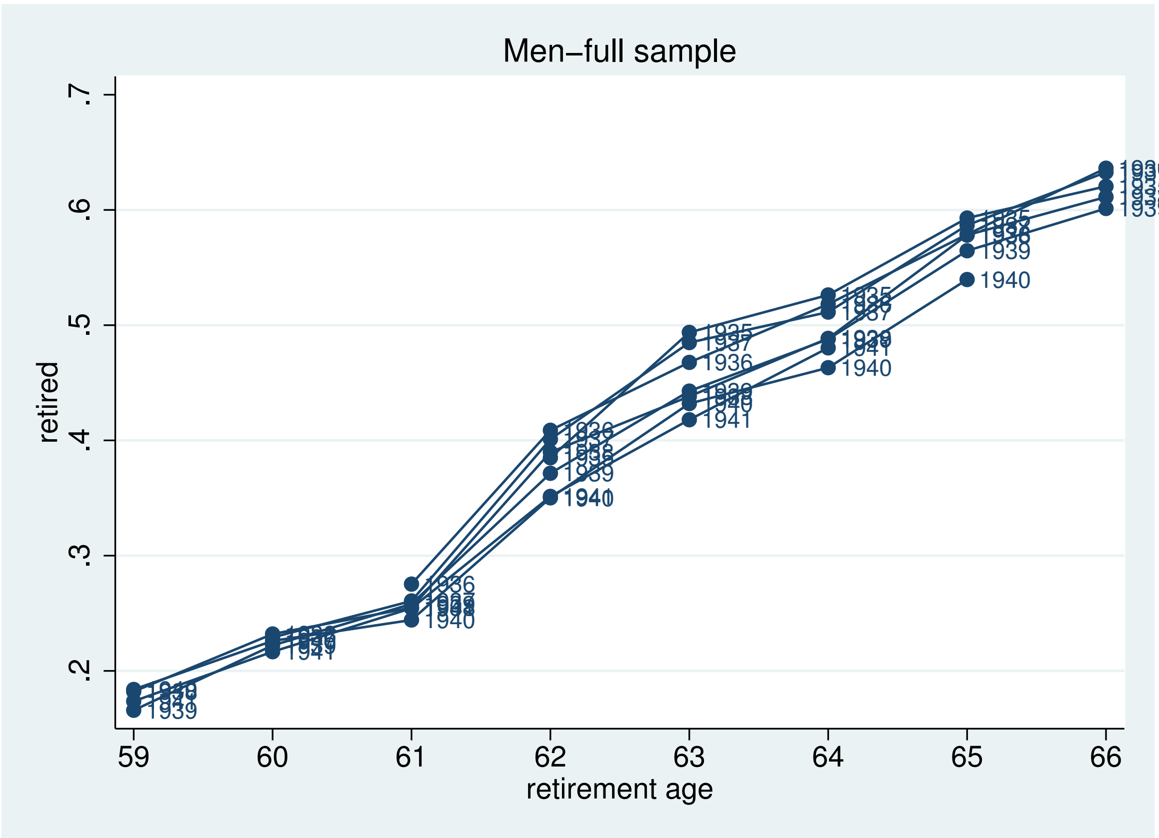

Figure 2.2: Cumulative distribution function of retirement age by year of birth. Full male sample.

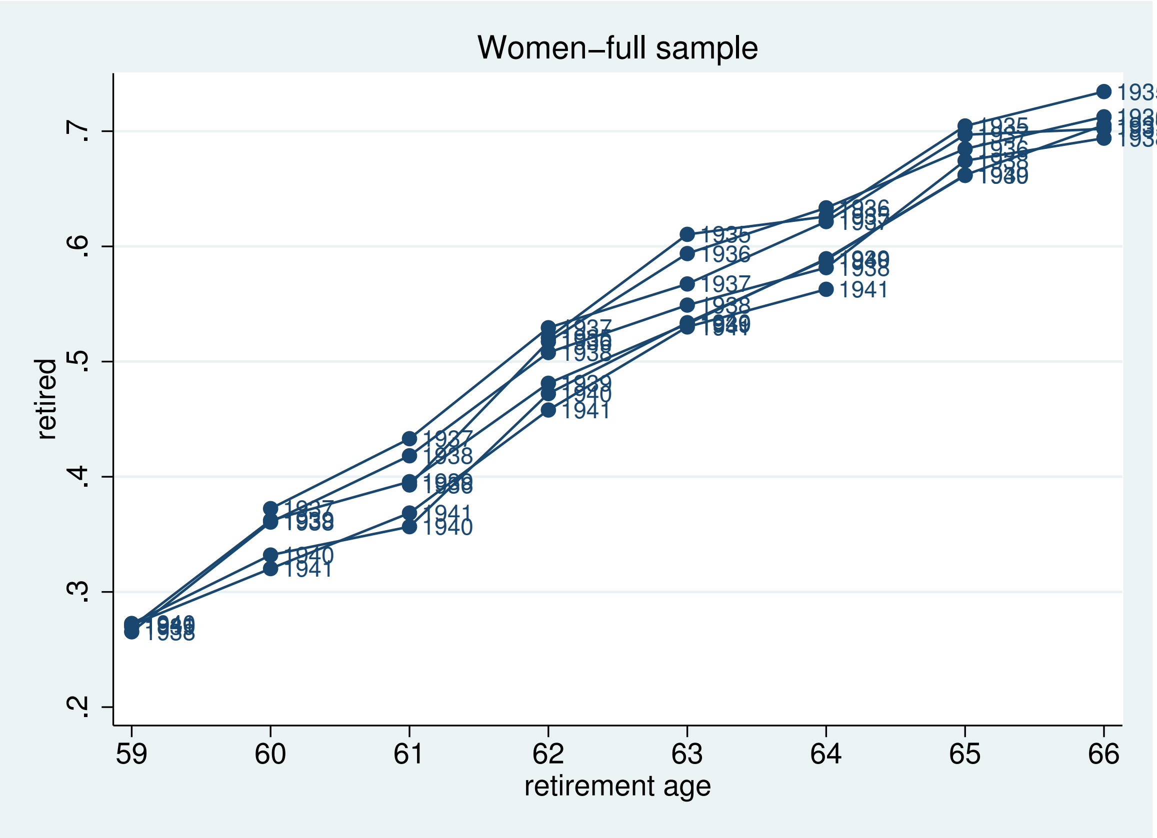

Figure 2.3: Cumulative distribution function of retirement age by year of birth. Full female sample.

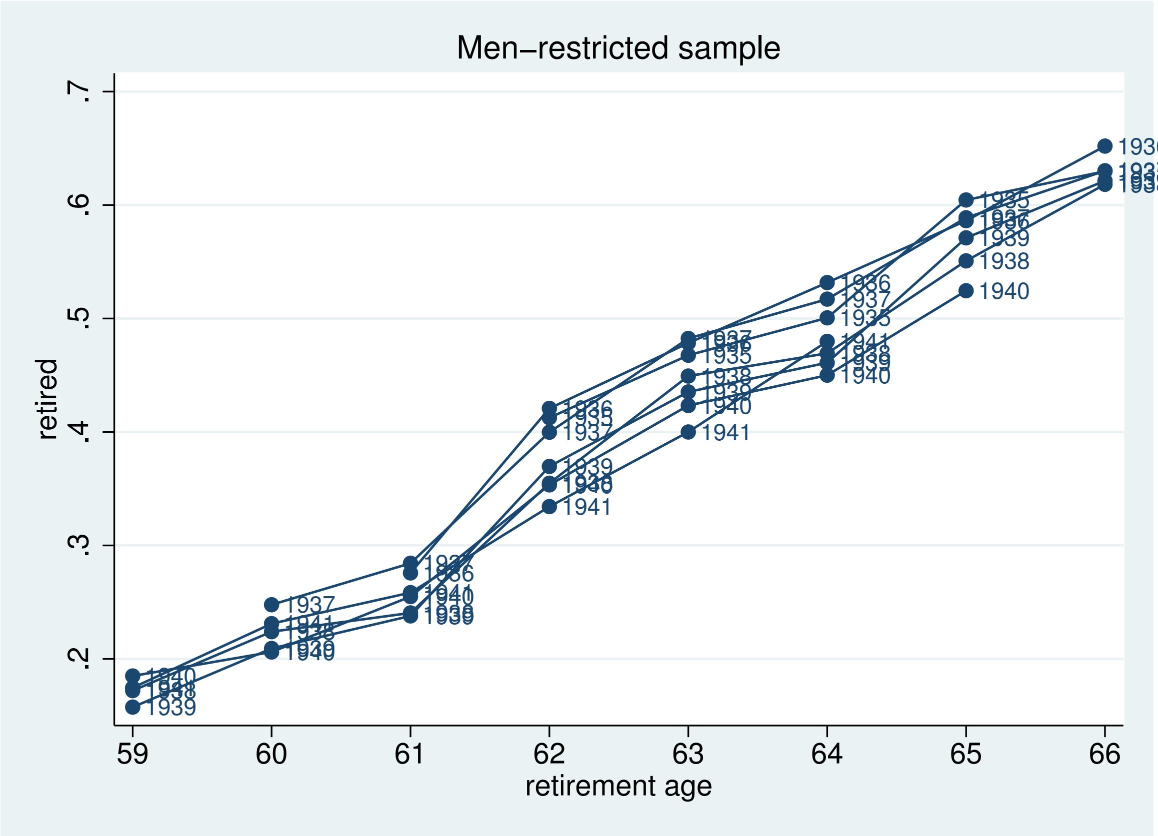

Figure 2.4: Cumulative distribution function of retirement age by year of birth. Restricted male sample.

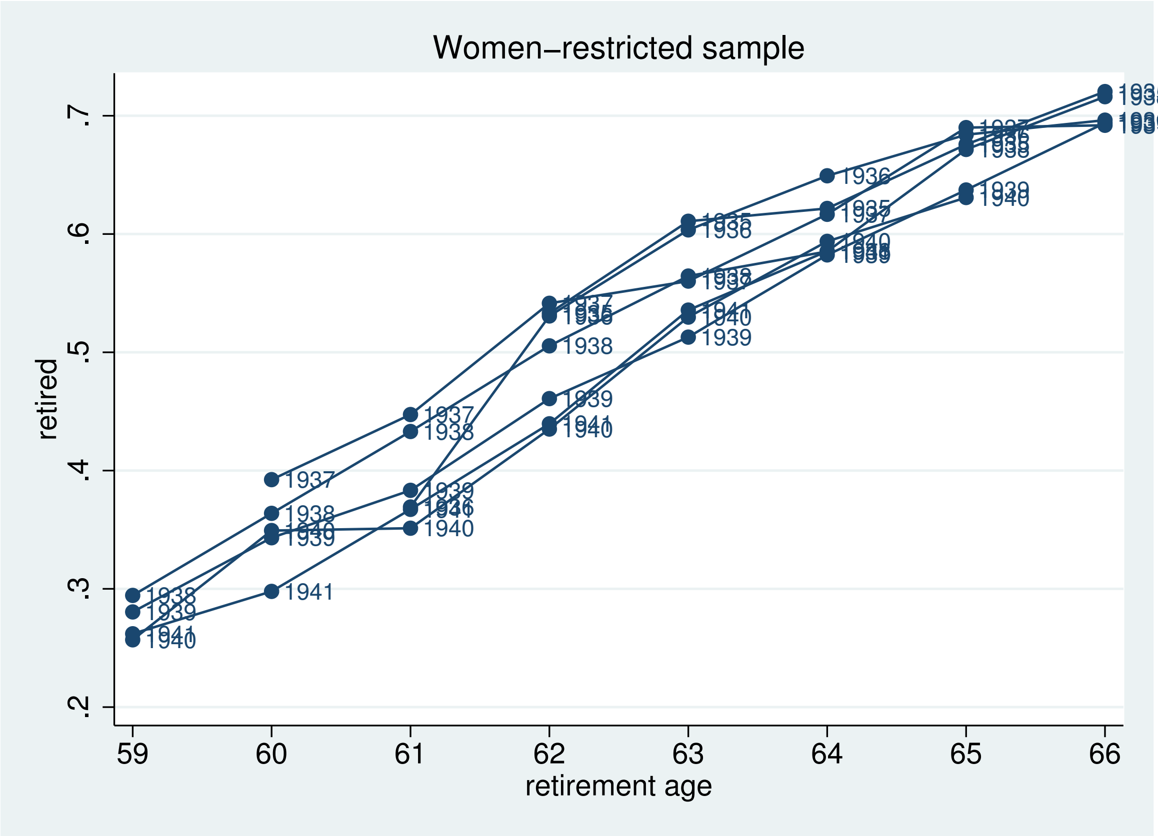

Figure 2.5: Cumulative distribution function of retirement age by year of birth. Restricted female sample.

Table 2.1: Summary statistics (mean and standard deviation) of the sample aged 61-65. Family income is reported by classes. Full sample

	1936		1937		1938		1939		1940	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	63.53	1.13	63.09	1.44	63.10	1.42	63.12	1.41	63.05	1.41
Year	1999.5	1.13	2000	1.44	2001	1.43	2002	1.42	2003	1.41
Male	0.49	0.50	0.49	0.50	0.49	0.50	0.49	0.50	0.49	0.50
Retired	55.58	49.69	51.67	49.97	50.15	50.00	49.47	50.00	47.37	49.93
Not Married	0.28	0.45	0.27	0.44	0.27	0.45	0.28	0.45	0.29	0.45
<High Sc.	0.19	0.39	0.18	0.38	0.17	0.37	0.16	0.36	0.15	0.36
Some college	0.15	0.36	0.16	0.37	0.16	0.37	0.17	0.37	0.16	0.37
College	0.18	0.38	0.19	0.39	0.19	0.39	0.20	0.40	0.20	0.40
Post coll.	0.09	0.29	0.09	0.29	0.10	0.30	0.10	0.31	0.12	0.32
Black	0.09	0.29	0.08	0.27	0.08	0.27	0.08	0.27	0.08	0.27
Asian	0.03	0.17	0.03	0.17	0.03	0.18	0.03	0.17	0.03	0.17
Other race	0.01	0.09	0.01	0.09	0.01	0.10	0.01	0.11	0.02	0.13
Children	0.01	0.09	0.01	0.11	0.02	0.13	0.02	0.15	0.02	0.15
#HH=1	0.17	0.38	0.16	0.37	0.17	0.37	0.16	0.37	0.18	0.38
#HH>2	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40	0.20	0.40
Midwest	0.24	0.42	0.24	0.43	0.24	0.43	0.25	0.43	0.25	0.43
South	0.32	0.47	0.31	0.46	0.30	0.46	0.29	0.46	0.30	0.46
West	0.22	0.41	0.22	0.42	0.23	0.42	0.24	0.42	0.24	0.42
Veteran	0.25	0.43	0.24	0.42	0.23	0.42	0.22	0.41	0.21	0.41
Metro. area	0.73	0.44	0.73	0.45	0.72	0.45	0.72	0.45	0.72	0.45
Fam.inc<20k	0.31	0.46	0.27	0.45	0.27	0.44	0.26	0.44	0.25	0.43
Fam.inc<40k	0.36	0.48	0.36	0.48	0.35	0.48	0.35	0.48	0.35	0.48
Fam.inc<60k	0.17	0.37	0.18	0.39	0.19	0.39	0.20	0.40	0.22	0.41
Fam.inc>75k	0.17	0.37	0.18	0.38	0.20	0.40	0.19	0.39	0.18	0.38

Table 2.2: Summary statistics (mean and standard deviation) of the sample aged 61-65. Family income is reported by classes. Restricted sample

	1936		1937		1938		1939		1940	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age	63.54	1.13	63.12	1.42	63.07	1.43	63.09	1.41	63.04	1.40
Year	1999.5	1.13	2000	1.42	2001	1.46	2002	1.44	2003	1.43
Male	0.49	0.50	0.49	0.50	0.49	0.50	0.48	0.50	0.49	0.50
Retired	56.15	49.62	52.00	49.96	50.00	50.00	49.05	49.99	45.55	49.80
Not Married	0.28	0.45	0.28	0.45	0.28	0.45	0.29	0.45	0.31	0.46
<High Sc.	0.19	0.39	0.18	0.38	0.16	0.37	0.16	0.37	0.15	0.36
Some college	0.16	0.36	0.15	0.36	0.16	0.36	0.17	0.37	0.17	0.37
College	0.18	0.38	0.19	0.39	0.19	0.39	0.20	0.40	0.20	0.40
Post coll.	0.09	0.29	0.10	0.29	0.10	0.31	0.10	0.30	0.11	0.32
Black	0.08	0.28	0.08	0.28	0.08	0.27	0.09	0.28	0.08	0.27
Asian	0.03	0.17	0.03	0.17	0.03	0.17	0.03	0.16	0.03	0.17
Other race	0.01	0.09	0.01	0.09	0.01	0.10	0.01	0.11	0.02	0.12
Children	0.01	0.10	0.01	0.11	0.02	0.14	0.03	0.16	0.02	0.15
#HH=1	0.18	0.38	0.17	0.38	0.17	0.37	0.17	0.38	0.19	0.39
#HH>2	0.20	0.40	0.19	0.40	0.20	0.40	0.20	0.40	0.19	0.39
Midwest	0.23	0.42	0.24	0.43	0.24	0.42	0.26	0.44	0.25	0.43
South	0.32	0.47	0.31	0.46	0.31	0.46	0.29	0.45	0.30	0.46
West	0.23	0.42	0.23	0.42	0.22	0.42	0.24	0.43	0.24	0.43
Veteran	0.24	0.43	0.23	0.42	0.23	0.42	0.22	0.42	0.21	0.41
Metro. area	0.73	0.44	0.72	0.45	0.73	0.44	0.72	0.45	0.72	0.45
Fam.inc<20k	0.31	0.46	0.29	0.45	0.27	0.45	0.27	0.44	0.25	0.44
Fam.inc<40k	0.37	0.48	0.35	0.48	0.35	0.48	0.35	0.48	0.37	0.48
Fam.inc<60k	0.16	0.36	0.18	0.38	0.19	0.39	0.20	0.40	0.21	0.41
Fam.inc>75k	0.17	0.37	0.17	0.38	0.19	0.39	0.18	0.39	0.16	0.37

Table 2.3: Empirical and uniform distribution of months of birth.

Month	Emprical	Empirical CDF	Uniform	Uniform CDF
1	9.28	9.28	8.33	8.33
2	8.17	17.45	8.33	16.67
3	8.72	26.16	8.33	25.00
4	8.51	34.68	8.33	33.33
5	7.97	42.65	8.33	41.67
6	8.28	50.93	8.33	50.00
7	9.14	60.07	8.33	58.33
8	9.79	69.86	8.33	66.67
9	8.26	78.12	8.33	75.00
10	7.56	85.68	8.33	83.33
11	8.27	93.95	8.33	91.67
12	6.05	100	8.33	100.00

Notes: The empirical distribution is based on 7801 certain matches born between 1937 and 1939 and aged 61 to 65.

Table 2.4: Estimated differences (in percent) in the CDFs of retirement age for the female sample.

	(1)	(2)	(3)	(4)	(5)	(6)
Model	Sophisticated		Naive		Restricted	
Age 61&Coh.36	-6.9	-7.6	-4.0	-4.6	-7.8	-8.7
	(2.8)*	(2.7)**	(1.8)*	(1.7)**	(3.0)**	(3.0)**
Age 62&Coh.36	-1.4	-2.3	-1.2	-1.9	-1.1	-1.8
	(1.9)	(1.8)	(1.5)	(1.5)	(2.6)	(2.6)
Age 63&Coh.36	4.6	3.8	2.6	2.1	4.3	3.4
	(1.8)*	(1.8)*	(1.5)	(1.5)	(2.6)	(2.6)
Age 64&Coh.36	1.5	1.0	1.2	0.7	3.3	2.4
	(1.7)	(1.7)	(1.4)	(1.4)	(2.5)	(2.4)
Age 65&Coh.36	-1.9	-1.9	-1.2	-1.1	-0.6	-1.0
	(1.6)	(1.5)	(1.3)	(1.2)	(2.3)	(2.2)
Age 61&Coh.38	-2.5	-2.3	-1.5	-1.1	-1.4	-0.9
	(2.2)	(2.2)	(1.4)	(1.4)	(2.5)	(2.5)
Age 62&Coh.38	-2.9	-2.5	-2.2	-1.7	-3.6	-3.7
	(2.2)	(2.1)	(1.5)	(1.4)	(2.6)	(2.5)
Age 63&Coh.38	-0.7	-0.5	-1.8	-1.3	0.4	-0.2
	(2.1)	(2.1)	(1.4)	(1.4)	(2.6)	(2.5)
Age 64&Coh.38	-4.6	-4.3	-4.0	-3.7	-3.1	-3.4
	(2.0)*	(2.0)*	(1.3)**	(1.3)**	(2.4)	(2.4)
Age 65&Coh.38	-3.1	-3.1	-2.3	-2.2	-1.8	-2.1
	(1.9)	(1.8)	(1.2)	(1.2)	(2.3)	(2.2)
Age 61&Coh.39	-5.0	-4.2	-3.7	-3.1	-6.4	-5.4
	(2.0)*	(2.0)*	(1.5)*	(1.5)*	(2.6)*	(2.6)*
Age 62&Coh.39	-5.2	-4.0	-4.8	-3.7	-8.1	-7.7
	(1.9)**	(1.8)*	(1.5)**	(1.4)*	(2.5)**	(2.5)**
Age 63&Coh.39	-2.9	-1.7	-3.5	-2.3	-4.7	-3.9
	(1.9)	(1.8)	(1.5)*	(1.4)	(2.5)	(2.5)
Age 64&Coh.39	-2.4	-2.0	-3.2	-2.7	-3.4	-3.1
	(1.7)	(1.7)	(1.3)*	(1.3)*	(2.4)	(2.4)
Age 65&Coh.39	-4.3	-3.7	-3.5	-2.9	-5.3	-4.8
	(1.7)*	(1.6)*	(1.3)**	(1.3)*	(2.3)*	(2.2)*
Age 61&Coh.40	-9.3	-8.4	-7.9	-6.9	-11.0	-9.7
	(2.2)**	(2.2)**	(1.7)**	(1.7)**	(2.7)**	(2.7)**
Age 62&Coh.40	-6.2	-5.7	-5.7	-5.0	-11.0	-10.3
	(2.3)**	(2.2)**	(1.7)**	(1.7)**	(2.8)**	(2.7)**
Age 63&Coh.40	-2.4	-2.3	-2.8	-2.2	-5.5	-5.3
	(2.2)	(2.2)	(1.7)	(1.7)	(2.8)*	(2.8)
Age 64&Coh.40	-5.2	-4.4	-4.2	-3.6	-5.1	-4.1
	(2.2)*	(2.1)*	(1.7)*	(1.6)*	(2.7)	(2.6)
Age 65&Coh.40	-4.2	-3.0	-3.4	-2.3	-5.9	-4.5
	(2.0)*	(2.0)	(1.5)*	(1.5)	(2.5)*	(2.5)
Other Xs	no	yes	no	yes	no	yes
Observations	133963	133963	124102	124102	24905	24905
R-squared	0.58	0.59	0.57	0.59	0.57	0.59

Notes: Standard errors clustered by individuals in parentheses, * significant at 5 percent, ** significant at 1 percent. Other Xs include marital status, education, race, children in the household, total members of the household, geographic region, veteran status and whether the household resides in a metropolitan area.

Table 2.5: Estimated differences (in percent) in the CDFs of retirement age for the male sample.

	(1)	(2)	(3)	(4)	(5)	(6)
Model	Sophisticated		Naive		Restricted	
Age 61&Coh.36	-1.0	-0.9	1.5	1.5	-0.9	-0.4
	(2.5)	(2.5)	(1.6)	(1.6)	(2.8)	(2.8)
Age 62&Coh.36	-1.7	-2.3	0.8	0.3	2.1	1.4
	(1.7)	(1.7)	(1.4)	(1.4)	(2.5)	(2.5)
Age 63&Coh.36	-1.8	-2.6	-1.7	-2.2	-0.4	-0.7
	(1.9)	(1.9)	(1.5)	(1.5)	(2.7)	(2.6)
Age 64&Coh.36	0.3	-0.2	0.7	0.2	1.5	0.7
	(1.8)	(1.8)	(1.5)	(1.5)	(2.7)	(2.6)
Age 65&Coh.36	-0.4	-0.8	-0.8	-1.1	-0.3	-1.0
	(1.7)	(1.6)	(1.3)	(1.3)	(2.4)	(2.4)
Age 61&Coh.38	-1.7	-1.0	-0.6	0.0	-4.4	-4.1
	(1.9)	(1.9)	(1.2)	(1.2)	(2.2)	(2.2)
Age 62&Coh.38	-3.0	-2.6	-1.1	-0.8	-4.5	-4.6
	(2.1)	(2.1)	(1.5)	(1.4)	(2.5)	(2.5)
Age 63&Coh.38	-6.4	-6.4	-4.7	-4.5	-3.3	-2.9
	(2.2)**	(2.2)**	(1.5)**	(1.5)**	(2.6)	(2.6)
Age 64&Coh.38	-2.7	-2.8	-2.3	-2.3	-4.8	-4.9
	(2.1)	(2.0)	(1.4)	(1.4)	(2.5)	(2.5)
Age 65&Coh.38	-2.0	-2.0	-0.9	-0.8	-3.8	-3.7
	(1.9)	(1.9)	(1.3)	(1.2)	(2.4)	(2.3)
Age 61&Coh.39	-0.9	-0.2	-0.3	0.4	-4.6	-3.9
	(1.7)	(1.7)	(1.3)	(1.3)	(2.3)*	(2.3)
Age 62&Coh.39	-3.8	-3.5	-3.0	-2.6	-3.0	-2.9
	(1.8)*	(1.8)*	(1.4)*	(1.4)	(2.5)	(2.4)
Age 63&Coh.39	-4.2	-4.4	-4.2	-4.2	-4.7	-4.4
	(1.9)*	(1.9)*	(1.5)**	(1.5)**	(2.7)	(2.6)
Age 64&Coh.39	-2.4	-2.8	-2.4	-2.6	-5.6	-5.8
	(1.9)	(1.8)	(1.4)	(1.4)	(2.6)*	(2.5)*
Age 65&Coh.39	-1.5	-1.5	-2.2	-2.1	-1.8	-2.2
	(1.7)	(1.7)	(1.4)	(1.3)	(2.4)	(2.3)
Age 61&Coh.40	-3.7	-2.2	-2.3	-1.0	-4.2	-3.2
	(1.9)	(1.9)	(1.4)	(1.4)	(2.4)	(2.4)
Age 62&Coh.40	-8.4	-7.9	-6.1	-5.6	-6.1	-6.2
	(2.1)**	(2.0)**	(1.6)**	(1.6)**	(2.6)*	(2.5)*
Age 63&Coh.40	-6.2	-6.2	-5.1	-5.0	-8.0	-7.5
	(2.2)**	(2.2)**	(1.7)**	(1.7)**	(2.8)**	(2.7)**
Age 64&Coh.40	-4.5	-4.0	-4.0	-3.7	-6.7	-5.8
	(2.2)*	(2.2)	(1.7)*	(1.7)*	(2.8)*	(2.7)*
Age 65&Coh.40	-5.3	-4.6	-4.2	-3.6	-4.0	-3.8
	(2.1)*	(2.1)*	(1.6)**	(1.6)*	(2.7)	(2.6)
Other Xs	no	yes	no	yes	no	yes
Observations	133963	133963	124102	124102	24905	24905
R-squared	0.58	0.59	0.57	0.59	0.57	0.59

Notes: Standard errors clustered by individuals in parentheses, * significant at 5 percent, ** significant at 1 percent. Other Xs include marital status, education, race, children in the household, total members of the household, geographic region, veteran status and whether the household resides in a metropolitan area.

Table 2.6: Estimated effect on the average retirement age (in months).

Model	(1) Sophisticated	(2)	(3) Naive	(4)	(5) Restricted	(6)
	Panel A: Female Sample					
1937-1936	-0.34	-0.09	-0.17	0.02	-0.71	-0.36
	(0.50)	(0.49)	(0.44)	(0.4)	(0.71)	(0.69)
1937-1938	1.34	1.24	1.23	1.07	0.98	1.14
	(0.59)*	(0.55)*	(0.39)**	(0.38)**	(0.71)	(0.70)
	[0.67]	[0.62]	[0.62]	[0.54]	[0.49]	[0.57]
1937-1939	1.15	0.89	1.22	0.94	1.61	1.43
	(0.42)**	(0.41)*	(0.33)**	(0.43)*	(0.70)*	(0.56)*
	[0.29]	[0.22]	[0.31]	[0.24]	[0.40]	[0.36]
1937-1940	1.83	1.55	1.81	1.46	3.09	2.79
	(0.56)**	(0.57)**	(0.41)**	(0.32)**	(0.58)**	(0.68)**
	[0.31]	[0.26]	[0.30]	[0.24]	[0.52]	[0.47]
	Panel B: Male Sample					
1937-1936	0.42	0.72	0.13	0.33	-0.35	-0.05
	(0.51)	(0.43)	(0.41)	(0.39)	(0.6)	(0.59)
1937-1938	1.70	1.65	1.07	1.00	1.97	1.93
	(0.6)**	(0.58)**	(0.34)**	(0.34)**	(0.71)**	(0.7)**
	[0.85]	[0.83]	[0.54]	[0.5]	[0.99]	[0.97]
1937-1939	0.97	1.04	1.05	1.06	1.45	1.49
	(0.44)*	(0.54)	(0.43)**	(0.42)*	(0.72)*	(0.69)*
	[0.24]	[0.26]	[0.26]	[0.27]	[0.36]	[0.37]
1937-1940	2.67	2.58	2.13	2.01	2.85	2.79
	(0.55)**	(0.5)**	(0.40)**	(0.40)**	(0.71)**	(0.71)**
	[0.45]	[0.43]	[0.36]	[0.34]	[0.48]	[0.47]
Other Xs	no	yes	no	yes	no	yes

Notes: Sum of the coefficients (times 12/100) of a given cohort excluding age 61. Other Xs include marital status, education, race, children in the household, total members of the household, geographic region, veteran status and whether the household resides in a metropolitan area. Standard errors clustered by individuals in parentheses, * significant at 5 percent, ** significant at 1 percent. The values in squared brackets represent the change in the average retirement age divided by the change in the NRA.

Chapter 3

The Social Security Earnings Test Removal. Money Saved or Money Spent by the Trust Fund?

3.1 Introduction

Beneficiaries of Social Security face restrictions on how much they can earn without incurring the earnings test (ET). Before year 2000, the benefits above the annual exempt amount were subject to a 50 percent tax for those below age 65 and were subject to a 33 percent tax for those between age 65 and 70. On April 7, 2000, President Clinton signed the "Senior Citizens Freedom to Work Act of 2000," which eliminated the 33 percent earnings test.[1] Although benefits that are taxed away are actuarially adjusted and later returned to the beneficiary as soon as she either reaches age 70 or her earnings fall below the earnings test, empirical evidence seems to suggest that workers perceive the tax to be permanent (Gruber and Orszag, 2003).

[1]The legislation, effective retroactively to January 1, 2000, still requires that the test's higher exempt amount be applied to beneficiaries' earnings in the year they attain their normal retirement age.

The earnings test removal (ETR) was seen as an opportunity to increase the number of retired people going back to work. Since the Trust Fund is projected to become insolvent in about forty years, policy makers' main concern was that the ETR might worsen the long–term finances of the fund. Fifteen years ago, Honig and Reimers (1989) estimated the cost of a complete removal to be close to 2 billion dollars or a 2.3 percent increase in the present discounted value of the stream of benefits, the so called Social Security Wealth (SSW). A few years later, Gustman and Steinmeier 1991 estimated the budgetary cost of an ETR for beneficiaries above age 65 considering different behavioral assumptions. The largest estimated cost is equal to 92 billion dollars when workers and retirees time their application to maximize the SSW. The cost drops to 43 billion dollars if liquidity constrains force workers to claim benefits as soon as they retire, and to -12 billion dollars, in which case the administration actually saves money, if workers claim at age 65, meaning as soon as they are not subject to the ET.

Following the ETR, economists have shown that it has positive labor supply effects (Tran 2004, Song 2004, Loughran and Haider 2005, Song and Manchester 2005) but, despite the difficult financial situation of the Trust Fund, its long–term impact on the budget has not been investigated yet. The aim of this chapter is to estimate this impact.

3.2 The impact of the ETR on the Trust Fund

Since other papers have already estimated the effect of the ETR on labor supply, I focus on how the ETR has changed workers' SSW. The ETR can only affect workers' SSW if it induces them to change their claiming behavior.

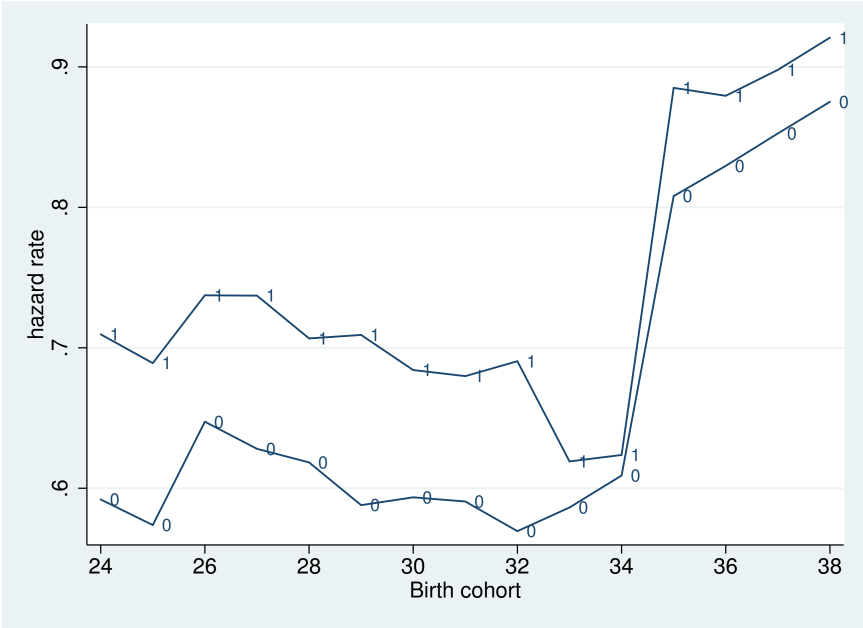

Figure 3.1: Hazard rates at the normal retirement age for cohort 1924 to 1938. Men (1) and women (0). Based on 1% of the SSA's Master Beneficiary Data.

3.2.1 Changes in claiming behavior

Figure 3.1 shows the dramatic change in the probability of claiming within a month of reaching the normal retirement age (NRA) conditional on not having claimed before (the hazard rate).[2] Workers born in 1935, the first cohort not subject to the ET, are 25 percentage points more likely (65% to 90%) to claim their benefits at the NRA than workers born just one year earlier.

Men born in 1924 have a hazard rate of 70 percent, while men born ten years later have a hazard rate that is almost 10 percentage points smaller. This decrease is probably due to the increase in the actuarial adjustment for claiming after the NRA. This adjustment, called the delayed retirement credit (DRC), increased during this period from 3 percent to 5.5 percent and is scheduled to reach 8 percent for the 1943 cohort. A higher DRC gives incentives for late claiming, and generates a reduction

[2]The NRA is increasing over time, and what was known as the 65-spike should now be renamed the NRA-spike. See Appendix .2.

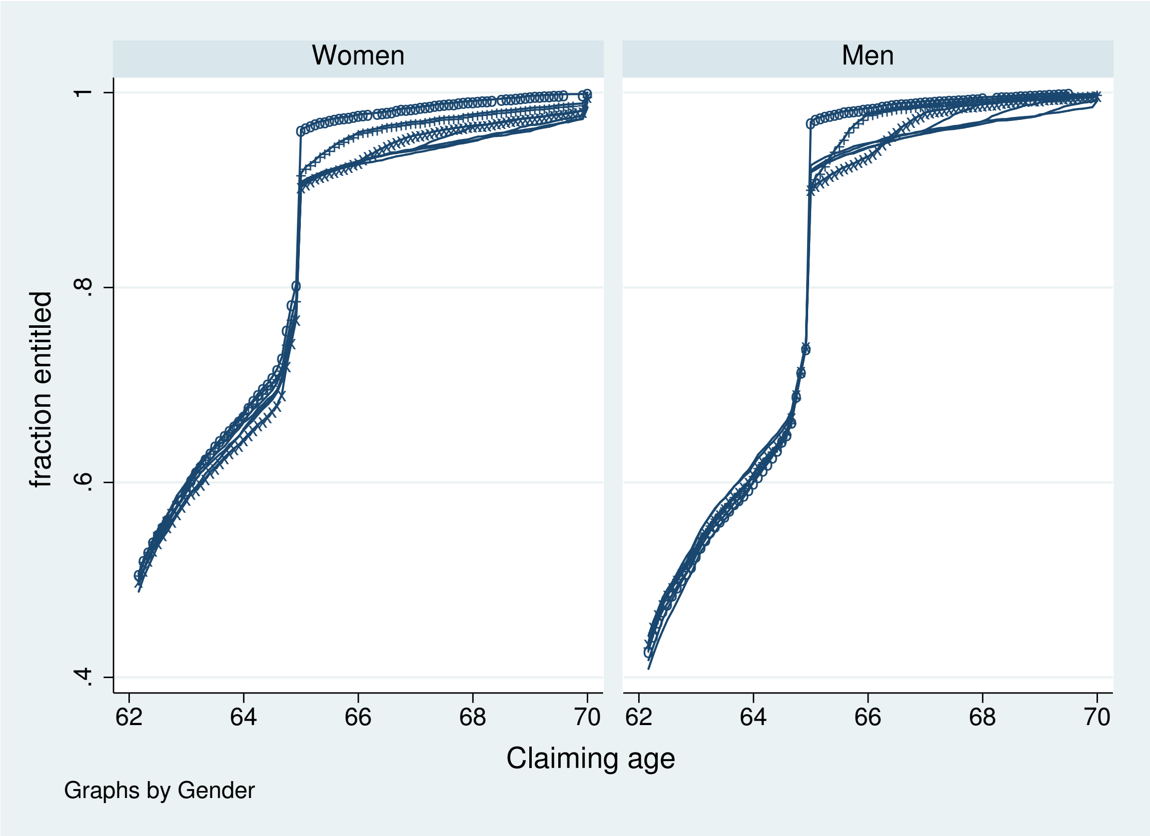

Figure 3.2: CDF of entitlement age. pre-1932 (-), 1933 (x), 1934 (+), 1935 (o).

in the hazard at the NRA. For women, who generally face longer life–expectancy, the reduction in the hazard rate seems less pronounced.

The increase in the hazard rate due to the ETR generates a gap between the cumulative distribution functions of entitlement age (Figures 3.2 and 3.3). Notice that the CDFs for workers born in 1934 (1935, etc.) converge towards the 1935 CDF with a 1 year (2 years, etc.) lag, which corresponds to the year of the ETR. Most workers who would have otherwise claimed after their NRA respond to the ETR by claiming at the NRA. On the other side, there do not seem to be changes in distribution of claiming ages before age 65.

Despite the rising DRC, these adjustments are not yet actuarially fair, and the observed changes in claiming behavior are likely to produce changes in workers' SSW. For a worker born in year c who claims at age x, $NRA_c < x \leq 70 \times 12$, the SSW

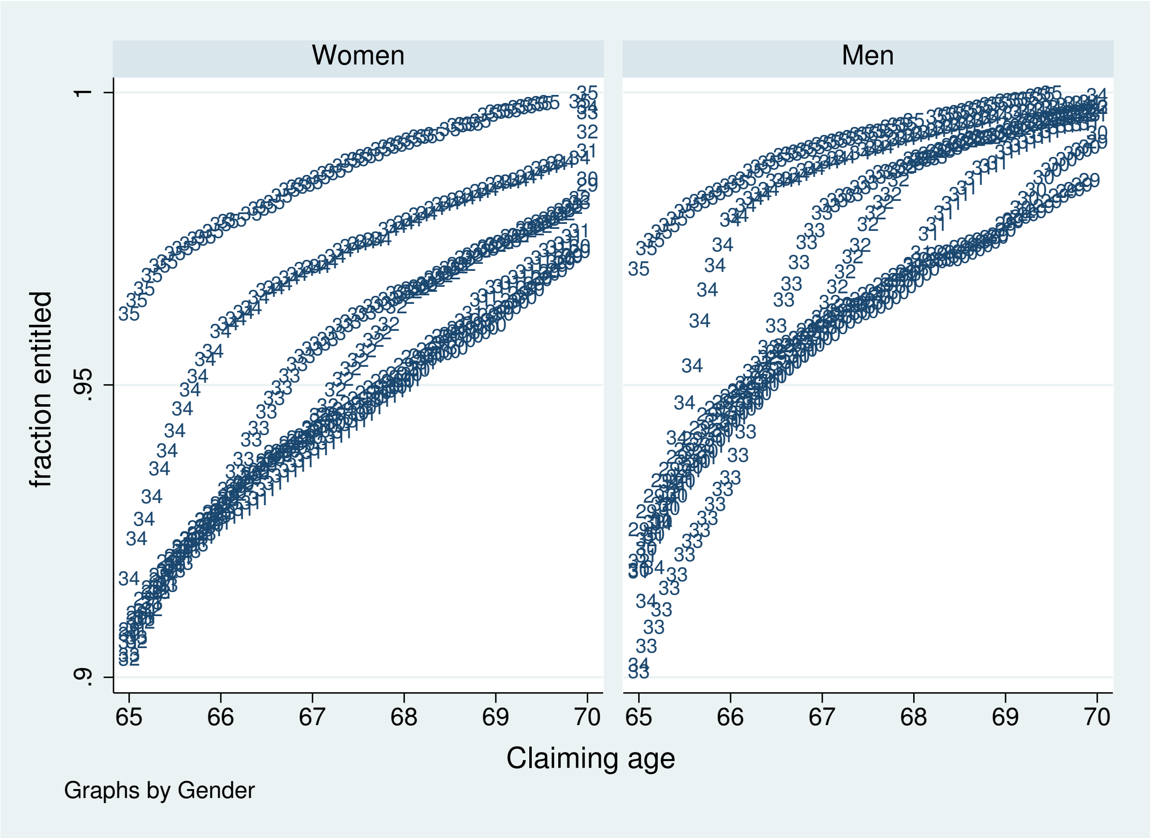

Figure 3.3: CDF of the claiming age. Based on 1% of SSA's Continuous Work History Sample.

evaluated at the NRA depends on the cohort–specific probability of survival until age (in months) t, $p_{c,NRA_c}(t)$, the benefits claimed at age x, $B_c(x)$, and the real interest rate i,

$$SSW(x,c) = \sum_{t=x}^{112\times 12} \frac{p_{c,NRA_c}(t)B_c(x)}{(1+i)^{t-NRA_c}} \quad . \tag{3.1}$$

The SSW for a worker who claims at the NRA, but would have otherwise claimed at age x, is equal to

$$SSW(NRA_c,x,c) = \sum_{t=NRA_c}^{112\times 12} \frac{p_{c,NRA_c}(t)B_c(NRA_c)(1-\delta_t\tau)}{(1+i)^{t-NRA_c}}, \tag{3.2}$$

where $\delta_t = \delta \times 1(t < x)$ represents the change in the federal income tax rate, τ, that is due to the earlier claiming, a concept that will be clarified in section 3.2.2.

The percentage change in the SSW that is due to the ETR is

$$\frac{\Delta SSW(x,c)}{SSW(x,c)} = \frac{SSW(NRA_c,x,c) - SSW(x,c)}{SSW(x,c)} \quad . \tag{3.3}$$

In order to evaluate the cohort–specific percentage change, I weight the relative importance of each claiming age x. Defining $\beta(x)$ as the difference between the CDFs that is due to the ET, the weighted average effect for cohort c is simply

$$\overline{\frac{\Delta SSW(c)}{SSW(c)}} = \sum_{x=NRA\times 12}^{70\times 12-1} \frac{\beta(x)}{\sum_{y=NRA_c\times 12}^{70\times 12-1}\beta(y)} \frac{\Delta SSW(x,c)}{SSW(x,c)} \,. \tag{3.4}$$

To construct the βs I use the youngest cohort that has not been affected by the ETR (1929) and the oldest cohort that has been entirely affected by the ETR (1935).

3.2.2 The Social Security Wealth

These changes in the SSW, evaluated at the NRA, differ across cohorts mainly because of different DRCs, different NRAs, and different cohort-specific probabilities of survival. The DRC is equal to 6 percent for the 1935 and 1936 cohort and increases by 0.5 percentage points every two years. For workers born in 1943 or later, the DRC is 8 percent. Higher DRCs make it more attractive for worker to claim their benefits later. The NRA is 65 for workers born before 1938. Staring with the 1938 cohort, the NRA increases by 2 months every year.[3] This increase squeezes the age interval affected by the ETR, reducing the changes in SSW.

Mortality tables are probably the most important factor when calculating the SSW, and adverse selection has to be taken into account in order to make sound assumptions.

Mortality estimates for late claimers

Since SSA's actuarial adjustments are based on population wide mortality tables, workers with higher life expectancy have an incentive to claim later much in the same way annuitants with higher life expectancy have an incentive to buy more annuities. Because of this selection, it is certainly problematic to use SSA life tables for late claimers. Using these tables I would certainly overstate the long–term cost of the ETR. Waldron (2001, 2004) uses the 1973 CPS data linked to Social Security death records to show that even after controlling for education, male workers who claim at age 65 or later have considerably lower mortality log-odds, and that these differences are widening with age. Men born between 1906 and 1931 who claim at 65 or later have log-odds that are about 20 to 30 percentage points lower than average. Unfortunately she did not carry out the same analysis for women.

[3] After a 12 year break at age 66 (between cohort 1943 and cohort 1954), the NRA is scheduled to reach age 67 for workers born in 1960 and later.

Table 3.1: Differences between late claimers' $(65, 70]$ and early claimers' $[62, 65]$ summary statistics

	Average for early claimers	T-stat	Difference wrt late claimers	T-stat	Obs.
Panel A: Female sample					
Married	0.75	63.71	-0.24	-7.48	1638
Widowed	0.10	11.51	0.15	6.41	1638
Black	0.07	10.11	0.05	2.67	1638
Hispanic	0.05	8.24	0.00	0.29	1637
High School	0.47	35.46	-0.10	-2.87	1638
College	0.35	27.15	0.12	3.54	1638
Household Wealth (10,000)	33.58	20.02	4.19	0.92	1638
Panel B: Male sample					
Married	0.82	76.53	0.01	0.54	1571
Widowed	0.05	7.60	0.01	0.84	1571
Black	0.07	9.58	0.01	0.71	1565
Hispanic	0.05	8.13	0.01	0.66	1565
High School	0.38	28.57	-0.15	-4.72	1565
College	0.42	30.71	0.18	5.68	1565
Household Wealth (10,000)	34.10	18.61	15.64	3.69	1571

If using the SSA's mortality tables generate an upper bound for the budgetary cost, life insurance annuity tables (Johansen, 1997) are likely to generate a lower bound. People who buy life insurance tend to live longer, and their mortality tables might better reflect late claimers' mortality tables. Since annuity tables are periodic, meaning that they measure the probability of survival at a given point in time, I need to convert them into cohort–specific tables. Mortality log-odds after age 60 tend to be linear with respect to age; therefore, I first measure the distance between the SSA's and the annuitants' periodic mortality log–odds and then impute this same distance to SSA's cohort–specific log–odds to generate annuity cohort–specific log–odds (see Appendix .3).

For men, the difference between the "annuity" and the "SSA" log–odds is approximately constant across ages and equal to 0.55 (see Equation 7 in Appendix .3). For women, the gap is 1.1, but is decreasing with age. The predictions will be carried out using the SSA's mortality estimates (upper bound) and the annuitants' mortality estimates (lower bound). The average of the two is very close to Waldron's estimate, though it is likely that, since she controls for education and I am not, even the average is likely to understate the late claimers' probability of survival.

Earnings, Income and the ET

Social Security benefits are not always tax exempt, and the ETR might have influenced the amount of Social Security benefits subject to the federal income tax (FIT). Since 1983, if beneficiaries file a federal tax return as "an individual," ("a couple") and the combined adjusted gross income plus tax–exempt interest is between \$25,000 and \$34,000 (\$32,000 and \$44,000), they pay taxes on up to 50 percent of their Social Security benefits. Moreover, since 1993, if the combined income is more than \$34,000 (\$44,000), up to 85 percent of the Social Security benefits are subject to income tax. Because these thresholds are not being adjusted for inflation over time, more and more beneficiaries pay income taxes on their benefits (Orszag, 2002).

The IRS collects the tax, but the revenues due to the 1983 reform go to the Social Security Trust Fund and those due to the 1993 reform go to the Medicare Trust Fund. Table 3.2.2 shows that between 1990 and 2002 the number of tax returns that contained taxable Social Security benefits doubled from 5 to 10 million, and the fraction of taxable benefits increased from 8.8 percent to 24.1 percent. The amount of taxes collected increased from 8 billion dollars (in 2004 dollars) to more than 21 billion dollars. The per–return tax increased from \$1,500 to \$2,000. At the time of the ETR, the corresponding average tax rate (τ in equation 3.2), calculated dividing the average tax by the average total benefit of those workers who filed a return, is close to 9 percent.

Before the ETR, the main reason people claimed after their NRA was to avoid the ET. In other words, would–be late claimers have earnings above the ET thresholds

Table 3.2: Federal income tax of Social Security benefits (FIT). Values are expressed in $2004.

Source: *Statistic:* Tax Year	SSA benefits billions	IRS returns millions	IRS taxable benefits billions	SSA fraction taxed in %	IRS total tax billions	Medicare total tax billions	ALL total tax per return	ALL tax rate in %
1990	333.5	5.1	28.5	8.8	8.0	0.0	1,575	6.8
1991	343.2	5.3	28.3	8.5	8.1	0.0	1,529	6.6
1992	350.0	5.5	29.1	8.5	7.7	0.0	1,401	6.0
1993	355.7	5.7	29.0	8.3	6.8	0.0	1,197	5.2
1994	361.5	5.9	49.2	13.8	6.3	4.8	1,894	8.2
1995	364.6	6.6	56.6	15.7	7.0	4.9	1,803	7.8
1996	372.2	7.4	64.1	17.6	7.6	4.2	1,600	6.9
1997	378.7	8.3	72.5	19.5	10.0	5.9	1,912	8.3
1998	379.1	8.9	79.6	21.0	11.5	7.5	2,137	9.2
1999	386.9	9.5	85.2	22.5	13.7	9.7	2,457	10.6
2000	397.1	10.6	98.6	25.5	12.6	8.0	1,939	8.4
2001	407.5	10.7	99.6	25.1	13.2	8.7	2,051	8.9
2002	410.5	10.8	98.2	24.1	12.7	8.5	1,962	8.5

(denoted in the table $> ET$). But, these workers now claim and collect their benefits as soon as they reach their NRA, and this makes them more likely to have part of their benefits be subject to the federal income tax. Table 3.3 shows that after 2000, half of the workers who claim their benefits at the NRA would be subject to the ET had the ET not been eliminated.[4] Overall, the probability of being subject to the 50 (85) percent FIT, (denoted in the table $> FIT$ and $> FIT2$), conditional on being subject to the ET ($| > ET$), is close to 90 (75) percent. How much lower would these probabilities be if workers decided to retire and had no earnings? Keeping everything else constant the answer can be found by adding earnings W to the FIT thresholds, $> FIT + W| > ET$. The probability for workers who claim after the NRA when the ET was still in place drops from 93 percent to 39 percent. The numbers are very similar for workers who claim at the NRA after the ETR. When I compute the SSW I assume that between the NRA and the age at which workers would have claimed in the absence of the ETR (x) the probability of being subject to the FIT increases by

[4]After the 2000 ETR, I assume that the ET threshold would have been in real terms equal to the 1999 ET threshold.

50 percent. In terms of equation 3.2, this means that I set $\delta = 0.5$.

Table 3.3: Probability of having benefits subject to the ET and the FIT.

	Claim before 2000				Claim after 2000			
	Claim at NRA		Claim after the NRA		Claim at NRA		Claim after the NRA	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
$> FIT \mid > ET$	0.77	0.43	0.93	0.26	0.95	0.22	0.94	0.24
$> FIT + W \mid > ET$	0.30	0.46	0.39	0.49	0.38	0.49	0.53	0.50
$> FIT2 \mid > ET$	0.60	0.50	0.78	0.42	0.85	0.36	0.91	0.28
$> FIT2 + W \mid > ET$	0.24	0.43	0.28	0.45	0.24	0.43	0.40	0.49
	N=198		N=376		N=169		N=148	

Notes: Based on the HRS (1992–2002). $> ET$ ($> FIT$) represents the probability of being subject to the ET (FIT). $FIT2$ represents the second threshold, above which 85 percent of the benefits become taxable. W stands for earnings. After the 2000 ETR, I assume that the ET threshold would have been in real terms equal to the 1999 ET threshold. For example, $> FIT2 + W \mid > ET$ represents the probability that conditional on being subject to the ET income without earnings is above the second federal income tax threshold and up to 85 percent of the benefits are taxable.

3.2.3 The long–term budgetary impact

The last two elements needed to compute the SSW are the real interest rate and the average monthly benefit. I estimate the budgetary impact using three different interest rates: 2.1 percent, 2.9 percent, and 3.6 percent, which correspond to the high cost, intermediate cost, and low cost assumptions used in the 2006 SSA Trustees Report. Table 3.4 shows the percentage changes in SSW for men without dependent spouse (independent), women whose benefits are based on their own earnings history (independent), and couples with dependent benefits using the two different assumptions about mortality and the three different assumptions about the real interest rates. I restrict the analysis to workers born before 1944, but the results based on these nine cohorts that I analyze seem sufficient to grasp the trends in the data.

When I use the SSA's mortality assumptions, independent men show large percentage changes in SSW (*Panel A*). Using the intermediate interest rate, the change is equal to 7.93 percentage points for the 1935 cohort. However, because of the downward trend in mortality it drops to 4.05 percentage points for the 1943 cohort. When I use the annuitants' mortality table instead (*Panel B*), the change is only 4.16 per-

Table 3.4: Changes in SSW (in percent) using cohort-specific SSA and annuitants' mortality.

Group	Single women			Single men			Married couples		
Real int. rate (in%)	2.1	2.9	3.6	2.1	2.9	3.6	2.1	2.9	3.6
	Panel A: SSA's Mortality Assumptions								
1935	3.74	5.01	6.16	6.66	7.93	9.09	2.73	3.90	4.96
1936	3.71	4.98	6.13	6.60	7.88	9.03	2.72	3.89	4.95
1937	2.70	3.95	5.08	5.57	6.83	7.97	1.78	2.93	3.98
1938	2.79	4.03	5.15	5.67	6.92	8.04	1.88	3.02	4.05
1939	1.88	3.06	4.14	4.73	5.93	7.01	1.04	2.14	3.14
1940	1.90	3.06	4.10	4.73	5.89	6.94	1.10	2.17	3.14
1941	1.06	2.16	3.17	3.84	4.96	5.96	0.33	1.36	2.29
1942	1.10	2.17	3.13	3.83	4.91	5.88	0.38	1.37	2.27
1943	0.33	1.35	2.27	3.02	4.05	4.98	-0.33	0.62	1.47
	Panel B: Annuitants' Mortality Assumptions								
1935	1.40	2.60	3.69	2.97	4.16	5.24	0.52	1.63	2.64
1936	1.38	2.58	3.67	2.94	4.12	5.20	0.51	1.62	2.63
1937	0.40	1.58	2.66	1.97	3.14	4.21	-0.39	0.70	1.69
1938	0.50	1.66	2.72	2.06	3.22	4.27	-0.31	0.77	1.76
1939	-0.35	0.77	1.79	1.21	2.32	3.34	-1.09	-0.05	0.90
1940	-0.28	0.80	1.80	1.25	2.33	3.32	-1.01	0.01	0.93
1941	-1.06	-0.02	0.93	0.46	1.51	2.45	-1.72	-0.75	0.14
1942	-0.97	0.04	0.95	0.51	1.52	2.43	-1.63	-0.69	0.17
1943	-1.67	-0.71	0.17	-0.20	0.77	1.64	-2.28	-1.38	-0.56

centage points for the 1935 cohort and 0.77 percentage points for the 1943 one. For this cohort, using the lower real interest rate (2.1 percent), the percentage change becomes negative, meaning that SSA's benefit payments decrease.

However, based on SSA's 2004 Benefits and Earnings Public-Use File men without a dependent spouse represent only around 25 percent of the population. Most men and women are either married or widowed, and their SSW changes are, due to their joint probability of survival, significantly lower.[5] Using a real interest rate of 2.9 percent, and the SSA's mortality assumptions, the changes drop from 3.90 percentage points

[5]In my simulations, I assume that for married couples the wife is two years younger than her husband and receives dependent spouse benefits as soon as her husband claims his benefits. Both assumptions are close to the sample averages, and small perturbations of these assumptions generate negligible changes in the results.

for the 1935 cohort to 0.62 percentage points for the 1943 one. With the annuity tables, the changes starting with the 1939 cohort are already close to zero. Changes for independent women tend to lie between those for independent men and dependent couples.

Finally, I use these results to estimate the budgetary impact on the Trust Fund. According to the HRS, women who claim late and are subject to the ET receive on average almost the same monthly benefit amount as men do (approximately $1,100 in 2004 dollars). Multiplying the individual SSW changes by the number of workers who claim their benefits after the NRA and before age 70 (for each cohort there are approximately 120,000 late claimers), and summing the effect over the three different types of workers gives the cohort–specific budgetary effect, assuming that the removal has no effects on earnings, and therefore on contributions.[6] Table 3.5 shows these effects for cohorts 1935 to 1943 using the SSA mortality tables (*Panel A*) and using the annuity tables (*Panel B*). In *Panel A*, depending on the interest rate used, for the 1935 cohort the change in SSW that is due to the ETR varies between 0.86 and 1.15 billion dollars ($2004). These changes drop over time, and the last cohort we consider has changes that range between 0.07 and 0.41 billion dollars. Using the annuity tables, for each cohort these changes drop by between 0.5 and 0.7 billion dollars, resulting in negative changes for workers born at or after 1938 when the 2.1 percent interest rate is used and workers born at or after 1940 when the 2.9 percent interest rate is used.

Each of these effects is evaluated at the workers' NRA. In order to compute the total effect evaluated in year 2000, we need to take the discounted sum of the cohort–specific budgetary effects. The total effect ranges between 4.12 and 6.46 billion dollars for SSA's mortality assumptions and between -0.64 and 3.36 billion dollars for the annuitant's mortality assumptions. Notice, that since the effects for the 1943 cohort

[6]For each cohort approximately 20 percent are independent women, 25 percent are independent women and 55 percent are dependent couples.

are close to zero or negative, extending the analysis to workers born after 1943 would only lower the total budgetary effect.

Recent studies have shown that the ETR increased labor supply. Loughran and Haider (2005) use CPS data to estimate the change in earnings due to the ETR. Their identifying assumption is that people aged 70-71 are not affected by the ETR, and that in the absence of the ETR, their earnings would have followed the same trend as the earnings of workers aged 65 (the NRA in their sample) to 69. They estimate a change in earnings of \$2,100 for men and \$500 for women. They do not find significant differences in their estimates across ages.

Multiplying these estimates by the payroll tax rate (12.4 percent) and by the size of the population in the Social Security area (around 4.5 million men and 5.2 million women, *SSA's 2005 Annual Statistical Supplement*) gives the total yearly change in contributions. Since the increase in earnings due to the ETR happens only between the NRA and age 70, younger cohorts have lower increases in the present discounted value of contributions. In order to estimate a cohort–specific effect on the contributions, the increase in the NRA has to be taken into account. These changes are approximately equal to 1.4 billion dollars for the 1935 cohort and 1.2 billion dollars for the 1943 cohort.

Whenever these additional yearly earnings enter the benefit formal (the total real earnings are larger then the lowest 35 years of earnings), SSA recomputes the benefits. In order to take this additional change in the SSW into account, I use SSA's 2004 Benefits and Earnings Public-Use File. Unfortunately the data does not allow me to estimate the effect of the ETR on earnings. What I can do is to simulate the effect on the benefits of an increase of \$500 (\$2100) in earnings for women (men) between the age of 65 and 69. The average increase in monthly benefits is \$1.87 for independent women and \$4.13 for independent men and dependent couples (the data does not contain information on marital status).[7] Then I calculate the present discounted value of this average increase in benefits using SSA's mortality assumptions (this time the estimate is based on the entire population of beneficiaries) and sum it across beneficiary types (independent women, independent men, and dependent couples) weighting the sum by the relative size within a cohort.

Looking at *Panel C* it is immediately clear that the recomputation tends to neutralize the increase in contributions. The net gain lies between 0.07 and 0.25 billion dollars for the 1935 cohort and between 0.16 and 0.31 billion dollars for the 1943 cohort. Using Loughran and Haider's estimates, the benefits from the increased contributions outweigh the costs among younger cohorts. Without using any estimates of labor supply effects, it is still possible to calculate the change in earnings that would be necessary to keep the Trust Fund financial situation unchanged. Using intermediate assumptions, both in terms of mortality and interest rates (the average between *Panel A*'s and *Panel B*'s estimate that uses a 2.9 percent interest rate), the break–even change in earnings is equal \$5.618 for the 1935 cohort and only \$61 for the 1943 cohort.

Subtracting the total change in contribution evaluated in year 2000 from the final change in SSW gives the total budget effect,

$$BUDGET_{2000}(i) = \sum_{c=1935}^{1943} N_c \frac{\Delta\overline{SSW}(c,i)}{(1+i)^{c-1935}} - \sum_{c=1935}^{1943} \sum_{t=NRA_c}^{69} \frac{N_{c,t}\Delta\overline{W}_t \times 0.124}{(1+i)^{t-NRA_c+c-1935}} \quad (3.5)$$

Using SSA's mortality assumptions, the Social Security Trust Fund is going to spend between 3.27 and 4.32 billion dollars on the first nine cohorts that were subject to the ETR. Using the annuitants' assumptions reduces the cost by around 4 billion dollars. But based on reasonable estimates of a change in earnings, and assuming that

[7] Kestenbaum et al. (1999) estimate that beneficiaries aged 65 to 69 whose benefits are recomputed the average increase in monthly benefits is \$13 for men and \$11 for women. According to Loughran and Haider the ETR increased earnings by about 30 percent for men and 20 percent for women, so that a naive estimate equal to 30 percent of \$13 for men and 20 percent of \$11 is reassuringly quite close to my estimate.

Table 3.5: Total budgetary impact of the ETR in billions of 2004 dollars.

	Panel A: SSA Mortality Assumptions			*Panel B: Annuitants' Mortality Assumptions*			*Panel C: Contributions & Recomputation*			*Panel D: Break–even change in earnings*
i(in%)	2.1	2.9	3.6	2.1	2.9	3.6	2.1	2.9	3.6	2.9
1935	0.86	1.03	1.15	0.32	0.59	0.78	0.07	0.17	0.25	5,618
1936	0.85	1.02	1.15	0.32	0.59	0.78	0.07	0.17	0.25	5,603
1937	0.63	0.82	0.96	0.04	0.34	0.55	0.07	0.17	0.25	4,043
1938	0.64	0.82	0.96	0.06	0.35	0.56	0.09	0.19	0.27	3,760
1939	0.42	0.62	0.77	-0.18	0.13	0.35	0.10	0.20	0.28	2,221
1940	0.43	0.62	0.75	-0.16	0.14	0.35	0.12	0.22	0.29	2,092
1941	0.23	0.43	0.58	-0.37	-0.07	0.15	0.14	0.23	0.30	964
1942	0.24	0.43	0.56	-0.34	-0.05	0.16	0.15	0.24	0.30	946
1943	0.07	0.26	0.40	-0.54	-0.24	-0.02	0.16	0.24	0.31	61
Total	4.12	5.53	6.46	-0.64	1.75	3.36	0.86	1.61	2.14	
-Panel C	3.27	3.92	4.32	-1.49	0.14	1.22				

Notes: The change that is due to an increase in earnings, and therefore contributions is based on estimates taken from Loughran and Haider 2005. Their estimated effect of the ETR on earnings for men (women) aged 65-69 is equal to \$2100 (\$500). The break–even change in earnings represents the average change in earnings needed to cover the average between *Panel A*'s and *Panel B*'s estimated cost.

"would–be" late claimers continue to claim at their NRA the Trust Fund is likely to start saving money starting with the 1941 cohort.

3.3 Conclusions

Following the 2000 ETR, several papers have analyzed its effect on labor supply, but despite the difficult financial situation of the Trust Fund, its effect on SSA's finances is still unknown. Using intermediate assumptions in terms of both real interest rates and mortality rates, I find that for the 1935 cohort the Trust Fund increased its spending by about 4 billion dollars as a result of the ETR. However, because of increasing life–expectancy, higher actuarial adjustments for late claiming, and increasing NRA, these effects are decreasing over time, and for workers born in 1943, the additional cost is probably close to zero. At the same time, the ETR is believed to have significantly increased earnings and therefore contributions between the NRA and age 69. Using estimates from Loughran and Haider (2005), I find that each cohort contributes additional 0.20 billion dollars as a result of the ETR. Nevertheless, the Trust Fund appears to have increased it's liabilities towards the first workers who were subject to the ETR. But for workers born after 1940 the Trust Fund seems to actually have saved money. If workers maximize their family utility function, by a revealed preference argument, the ETR has been for workers born after 1941 Pareto–improving. There are two reasons that suggest that removal of the remaining part of the earnings test (between age 62 and the NRA) is unlikely to produce larger costs. First, if we believe that after age 62 disutility from work is increasing with age, labor supply between age 62 and the NRA is going to respond even stronger to an ETR. Second, mortality between age 62 and the NRA is low, especially because the additional removal would affect much younger cohorts, and the actuarial adjustments are high. Thus, most workers are better off claiming around the NRA. For these workers, earlier claiming is likely to produce lower long–term spending for the Trust Fund. These results suggest benefits for repealing the remaining portion of the earnings test.

Bibliography

Berkeley Mortality Database. Life tables. Online at http://demog.berkeley.edu/wilmoth/mortality.

Jonathan Gruber and Peter Orszag. Does the social security earnings test affect labor supply and benefits receipt? *National Tax Journal*, 4(56):755, 773 2003.

Alan L. Gustman and Thomas L. Steinmeier. Changing the social security rules for workers over 65. *Industrial and Labor Relations Review*, 44(4):733–745, July 1991.

Marjorie Honig and Cordelia Reimers. Is it worth eliminating the retirement test? *American Economic Review*, 79(2):103–07, 1989.

Robert J. Johansen. Review of adequacy of 1983 individual annuity mortality table. *Insurance: Mathematics and Economics*, 19(2):145–145, 1997.

Bertram Kestenbaum, Michael Shackleford, and Chris Chaplain. Effect on benefits of earnings at ages 65 or older, 1995. *Social Security Bulletin*, 62(1):4–9, 1999.

David Loughran and Steven Haider. Do the Elderly Respond to Taxes on Earnings? Evidence from the Social Security Retirement Earnings Test. Labor and Population Program Working Papers WR-223, RAND, 2005.

Olivia S. Mitchell, James M. Poterba, Mark J. Warshawsky, and Jeffrey R. Brown. New evidence on the money's worth of individual annuities. *American Economic Review*, 89(5):1299–1318, 1999.

Peter R. Orszag. Tax on social security benefits providing more trust fund revenue. *Tax Notes, 9, 2002, p. 1359*, (9):1359, December 2002.

Jae G. Song. Evaluating the Initial Impact of Eliminating the Retirement Earnings Test. *Social Security Bulletin*, 65(1), 2004.

Jae G. Song and Joyce Manchester. New evidence on earnings and benefit claims following the removal of the retirement earnings test in 2000.

Bac V. Tran. *The Impact of the Repeal of the Retirement Earnings Test on the Labor Supply of Older Workers*. PhD thesis, University of Maryland, Baltimore, October 2004.

Hilary Waldron. Links Between Early Retirement and Mortality. Technical Report 93, SSA, Office of Research, Evaluation, and Statistics, 2001.

Hilary Waldron. Heterogeneity in Health and Moratlity Risk Among Early Retiree Men. Technical Report 105, SSA, Office of Research, Evaluation, and Statistics, 2004.

.1 Data

Health and Retirement Survey I use the 1992-2002 waves of the Health and Retirement Survey, a biyearly panel survey of around 13,000 individuals aged 51 to 61 in their first wave. I delete observations of those who get disability benefits. In order to obtain the exact date of claiming, I use the retrospective information. However, I restrict the sample to workers who claimed after 1992 and use only the first wave following the claiming date. Finally, I discard observations for which no exact measure of the monthly claiming age can be established.

SSA's Master Beneficiary File http://www.ssa.gov/policy/docs/microdata

Annuity tables Data extracted from Johansen (1997).

Cohort-specific life tables Office of the Actuary of the Social Security Administration Database

.2 How the "65 spike" became the "NRA spike"

Notice that the 1938 cohort's second spike is at 65 and 2 months and not 65 (Fig. 4). The reason for this is that for that cohort, the NRA increased by two months. The 1983 amendments scheduled a yearly increase in the NRA starting with the 1938 birth cohort. The NRA will reach age 66 for workers born in 1943. After a 10 year break, the rise will resume and stop at age 67 for workers born in 1960 or later. Given this evidence, when forecasting claiming behavior for future retirees, I will assume that the second spike coincides with the NRA.

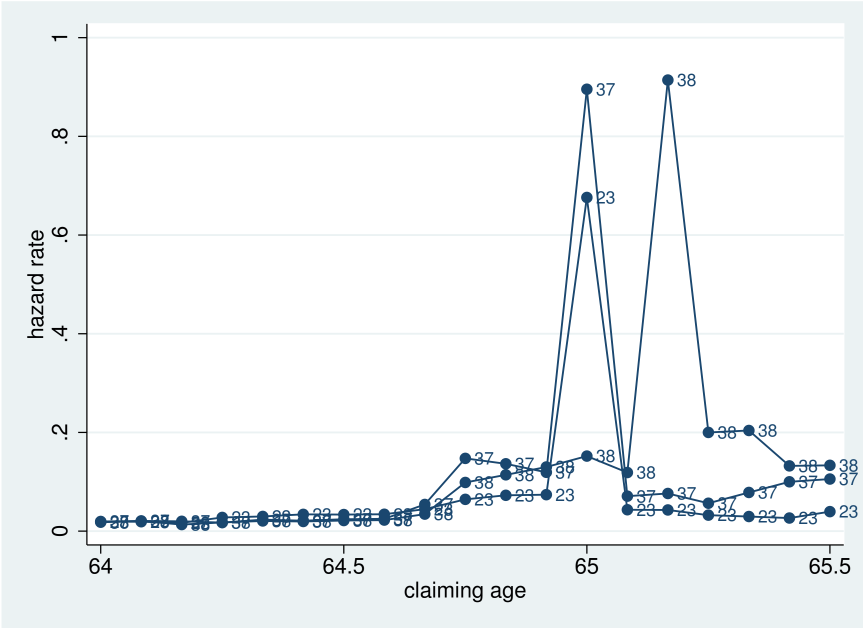

Figure 4: Hazard rates for cohort 1924, 1937 and 1938. Based on 1% of SSA's Master Beneficiary Data.

.3 Cohort-specific annuity tables

In order to generate cohort-specific annuity life tables, I follow an approach that is slightly different than the one used by Mitchell et al. Mitchell et al. (1999). Define $q^{p}_{x-1}(x, y)$ and $q^{c}_{x-1}(x, y)$ to be respectively the periodic and the cohort-specific mortality probabilities for the whole population and q' the basic (unloaded) ones for the annuitants. The authors first interpolate $q'^{p}_{x-1}(x, 1983)$ and $q'^{p}_{x-1}(x, 2000)$ to get $q'^{p}_{x-1}(x, 1995)$. Then they multiply this number by $\frac{q^{c}_{x-1}(x,1995)}{q^{p}_{x-1}(x,1995)}$ to get the cohort-specific mortality rates.

I use an alternative approach. Since log-odds, $LO = log(\frac{q}{1-q}) = \alpha + \beta x$ are linear in age (see Fig.5), the difference in the periodic mortality between annuitants and the population is equal to $\alpha' - \alpha + (\beta' - \beta)x$, which can be easily estimated using OLS.

Assuming that the same difference applies to cohort-specific mortalities,

$$q'^{c}_{x-1}(x, y) = \frac{exp(LO'^{c}(x, y))}{1 + exp(LO'^{c}(x, y))}\ , \tag{6}$$

where

$$
\begin{aligned}
LO'^{c}(x,y) &= LO^{c}(x,y) + \widehat{\alpha}' - \widehat{\alpha} + (\widehat{\beta}' - \widehat{\beta})x \\
&= LO^{c}(x,y) - 0.54679439 + 0.00028393x \text{ for men} \\
&= LO^{c}(x,y) - 1.0997845 + 0.00803148x \text{ for women .} \qquad (7)
\end{aligned}
$$

Finally, I interpolate the life tables using a spline to get monthly probabilities. Figure 5 shows the fit for the first step using periodic tables.

Figure 5: Periodic mortality probabilities, and log odds for annuitants, the general population and the implied “low mortality” population.

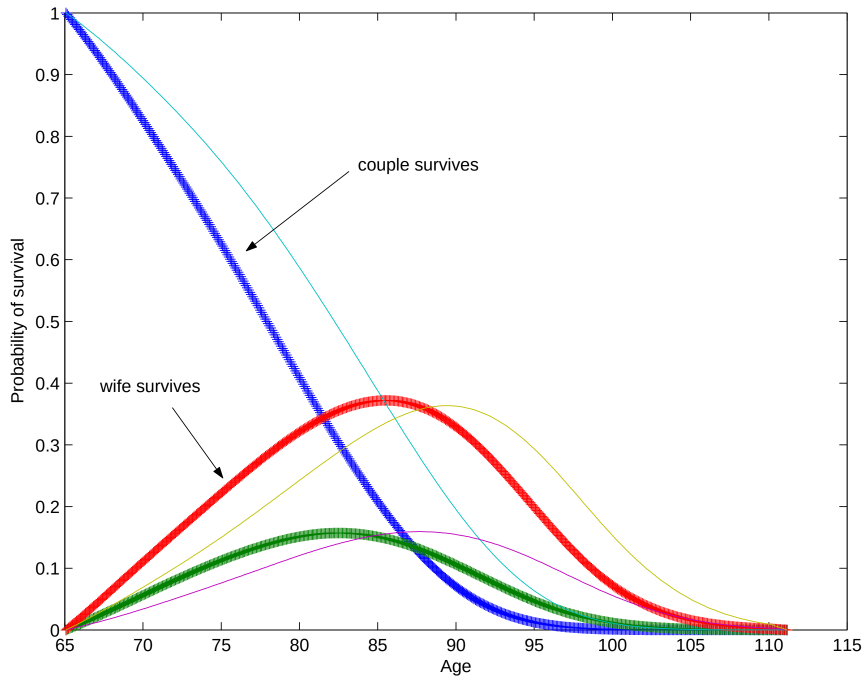

Figure 6: Survival probabilities conditional on age 65. SSA estimates have a solid line.