

Can Social Security Explain Trends in Labor Force Participation of Older Men in the United States?

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Revised March 2008

We are grateful for helpful comments from Mark Duggan, Giovanni Mastrobuoni, Jonathan Pingle, Tim Smeeding, and participants at the 2006 IZA/SOLE Transatlantic Meeting of Labor Economists, the 2007 Annual Meeting of the Population Association of America, and the 2007 research workshop of the Michigan Retirement Research Center. Financial support from the National Institute on Aging (Grant P30 AG024376) is gratefully acknowledged. None of the above is in any way responsible for the content.

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Abstract

After nearly a full century of decline, the Labor Force Participation Rate (LFPR) of older men in the United States leveled off in the 1980s, and began to increase in the late 1990s. We use a time series of cross sections from 1962 to 2005 to model the LFPR of men aged 55-69, with the aim of determining whether changes in the rules governing Social Security benefits can explain these trends. Our results indicate that the decline in the LFPR from the 1960s through the 1980s cannot be explained by the increasing generosity of Social Security during this period. The recent increase in the LFPR of older men can be explained by changes in the composition of the older male population away from high school dropouts and toward college attendees and graduates. Changes in Social Security may have contributed to the recent increase as well, but this result is sensitive to specification.

1. Introduction

The Labor Force Participation Rate (LFPR) of older men in the United States declined for much of the twentieth century. The magnitude and duration of this trend is remarkable. The LFPR of men aged 65 and older fell from 68% in 1900 to 19% in 1980 (Moen, 1987). However, this long downward trend ended in the 1980s. More recently, the LFPR of men in some age groups began to rise. For example, after falling to a 20th century low of 24% in 1985, the LFPR of men aged 65 to 69 increased to over 33% in 2005. The participation rate for men aged 60 to 64 increased from 55% in 1985 to 58% in 2005 (see Figure 1). The U.S. population will be aging rapidly in the next two decades and beyond, so it is important to understand why the downward trend in the LFPR of older men ended, and whether the recent increases are likely to persist.

The goal of this paper is to quantitatively assess the impact of changes in the rules governing Social Security retirement and disability benefits on trends in older male LFP. We also examine the role of changes in lifetime earnings, wage rates, and the demographic composition of the older male population, particularly the dramatic increase in educational attainment. We combine data from the Current Population Survey (CPS), the Survey of Income and Program Participation (SIPP), and the Social Security Administration (SSA) to generate a synthetic panel data set spanning the period 1962 to 2005. Individual-level data from the CPS and SIPP are aggregated into cells defined by defined by calendar year, age, and education, and merged, along with aggregate data from the SSA.

A priori, changing Social Security rules is the most plausible explanation for the observed trends in LFPR. The generosity of benefits was increased often from the inception of Social Security in 1935 through the early 1970s, coinciding with declining LFP of older men. Further,

the decline accelerated as the rate of growth in Social Security benefit generosity increased from the mid 1960s through the mid 1970s. The downward trend in LFP ended and reversed after several Social Security reforms increased the incentive to work at older ages. Amendments in 1977 reduced benefits for men who turned 65 beginning in 1982. The 1983 amendments increased the normal retirement age in two month increments per year from 65 in 1999 to 66 in 2005, effectively reducing lifetime benefits¹. The 1983 amendments also stipulated increases in the reward for delaying entitlement past the normal retirement age over the period 1987 to 2005. Finally, amendments in 1983 (effective in 1990) and in 2000 modified the Social Security Earnings Test, first reducing and then eliminating the implicit tax on earnings for men at and above the normal retirement age.

A number of studies have analyzed the impact of changes in Social Security retirement and disability benefits on the older male LFPR in the 1960s and 1970s (Anderson, Gustman, and Steinmeier, 1999, Hurd and Boskin, 1984; Parsons, 1980; Moffitt, 1987; Bound, 1989; Krueger and Pischke, 1992; Stewart, 1995). More recently, Pingle (2006), Mastrobuoni (2006), Gustman and Steinmeier (2006), and others have analyzed the effects of the 1983 reforms. An important contribution of our study is to assess the explanatory power of Social Security over a long period of time that encompasses many of the major changes to Social Security rules and in which there was a major reversal of the secular decline in the older male LFPR. This setting provides a challenge to any mono-causal explanation: such an explanation will have to account for many years of decline, and the recent increase.

We specify an econometric model that can be interpreted as a linear approximation to the

¹ A person who retires at the normal retirement age of 66 in 2005 collects benefits for a full year less than an equivalent individual who retired at the normal retirement age of 65 in 1999, holding constant life expectancy. The reduction in lifetime benefits is also reflected in an increased penalty associated with claiming benefits before the normal retirement age. A further phased increase in the normal retirement age from 66 to 67 is scheduled to take place from 2017-2022.

labor force participation decision rule implied by a life cycle economic model. We include calendar-year fixed effects in the model to control for secular trends and cyclical patterns in employment that might give rise to spurious correlation between trends in the explanatory factors and trends in LFP. We include education group fixed effects in order to account for permanent unobserved differences across education groups in the relative attractiveness of employment at older ages. And we use age fixed effects to account for features of Social Security and Medicare rules that provide strong age-specific employment incentives and that have remained mostly unchanged during the period covered by our data. Despite all of these controls, unobserved differences across birth cohorts could give rise to spurious correlation between Social Security rule changes and employment trends. Many of our explanatory variables vary by the *interaction* between birth cohort and education. This makes it possible to control for birth cohort fixed effects that account for the influence of any unobserved birth-cohort-specific factors. However, in practice identification is rather tenuous with a full set of single-year-of-birth fixed effects, so we present results for several alternative specifications of birth year effects. We assume that all two-way and higher-order interactions among calendar year, education, birth year, and age can be excluded from the model, and this assumption provides identification.²

Our empirical results indicate that changes in Social Security can account for only a small proportion of the observed decline in LFP from the 1960s through the 1980s. The results for Social Security are somewhat sensitive to the specification of birth year effects. But even the specification without any birth year controls, which yields the biggest effects of Social Security, implies that changes in Social Security can explain only one fifth of the observed decline in LFP.

² Birth year, age, and calendar year are of course collinear. We specify the model with nonlinear functions of these variables (fixed effects), so it is possible to include all three in the model. It would be inappropriate to give any particular interpretation to the effects of birth year, age, and birth year effects in this specification. This is not a problem, however, since they are included only to control for otherwise unobserved factors.

The specification with the richest controls for birth year in which Social Security effects are well determined (two-year birth cohort fixed effects) implies that Social Security changes can explain only 4% of the observed decline. Another 5-7% of the decline is accounted for by the increased attractiveness of Social Security Disability Insurance. The latter finding is quite robust.

Changes in lifetime earnings and wages contribute very little to explaining either the LFP decline or the later increase.

Thus, changes in Social Security policy fail to account for the bulk of the decline in LFP from the 1960s through the 1980s. By providing evidence against the most plausible explanation for the downward trend, our results imply that unobserved changes in preferences, constraints, and institutions are the driving forces. This of course leaves open the question of what those unobserved changes were.

Our results do provide a more specific explanation for the recent increase in labor force participation: changes in the education composition of the older male population. Low-participating high school dropouts have been rapidly replaced in recent years by higher-participating college attendees, and college graduates. This trend can explain the entire increase in LFP of older men in recent years, and the results are quite robust on this score. However, this compositional effect is not a fundamental explanatory factor, and it will eventually end as the transition to a more educated labor force is completed. Pingle (2006) finds that increases in the Delayed Retirement Credit (DRC) have played an important role in the recent increase in LFP of older men. Our findings indicate that the increase in the DRC can explain 64% of the recent increase in older male LFP in a specification without birth cohort effects, but only 10% in a specification with two-year birth cohort effects. Mastrobuoni (2006) finds that changes in the Normal Retirement Age (NRA) have also been an important cause of the rise in LFP. Our

estimates suggest that the increased NRA played little or no role in explaining the rise in LFP. Pingle and Mastrobuoni estimate the “treatment effects” of these changes, while our approach forces the effects of Social Security rules to operate through their impact on the Social Security benefit. Mastrobuoni suggests that changes in the NRA may affect behavior through non-economic channels, by altering social norms or implicit advice from the government on when to retire. Our approach does not pick up effects of Social Security that operate through such mechanisms.

Section 2 provides information on the context of our study, and discusses the contributions of previous studies. Section 3 discusses the conceptual framework for the analysis and the empirical specification implied by the framework. Section 4 describes the data, section 5 discusses the results, and section 6 concludes.

2. Background

As noted above, circumstantial evidence suggests that changes in the generosity and structure of Social Security may have affected labor force behavior of older men. However, estimates of the effect of changes to Social Security on LFP of older men vary widely. Moffitt (1987) uses time-series data to assess the impact of increases in benefits from the 1950s through the 1970s. He concludes that unanticipated Social Security policy changes can explain no more than 20% of the observed decline in the 1970s. In a similar analysis using a longer time-series, Stewart (1995) finds that up to 40% of the change in the LFPR of older men between 1965 and 1990 can be attributed to changes in Social Security benefits. Researchers have also used individual-level panel data to assess the impact of particular SS amendments. Hurd and Boskin (1984) find that increases in Social Security benefits between 1970 and 1972 account for nearly

the entire decline in the LFPR of older men between 1969 and 1973. Blau (1994) finds that changes in Social Security benefits can explain part of the decline in older male LFP in the 1970s, but the majority of the decline is unexplained. Kreuger and Pischke (1992) use synthetic panel data and find that the 1977 amendments had almost no impact on LFP rates of older men in the 1970s and 1980s. There is also disagreement over the role of Social Security Disability Insurance (SSDI) in explaining the decline in LFP at ages before eligibility for retirement benefits (Parsons, 1980; Bound, 1989).

The LFPR of older men was declining for many years before the inception of Social Security (Costa, 1998). This decline is not unique to the United States. Similar patterns are found in other industrialized countries, suggesting that the principal explanations for the trend toward earlier retirement may be common across developed nations. One such explanation is that lifetime income has been rising as a result of growing real wages (Costa, 1998; Burtless and Quinn, 2000). Other things equal, wealthier men have a higher lifetime demand for leisure, and can more readily “afford” to retire early. However, the increase in the LFPR of older men since the late 1990s has occurred during a period when real earnings have continued to increase in the US, at least for some groups.

Other proposed explanations for changing patterns of LFP at older ages include changes in the availability and structure of private pension plans (Friedberg and Webb, 2005) and employer provided retiree health insurance (Blau and Gilleskie, 2001; Madrian, 1994). We control for pension type in our analysis, but our data do not have the detailed information needed to measure pension incentives carefully. Hence, we do not make any claims about whether or not changes in pension coverage can explain trends in LFP. Similarly, we control for retiree health insurance, but our measure is quite crude and we cannot credibly analyze the causal effect of

changes in the availability of health insurance on employment.

The LFPR of married women has nearly tripled since 1950 (Costa, 2000) and several studies have found that working husbands and wives tend to retire at the same time (e.g. Hurd, 1990; Blau, 1998; Gustman and Steinmeier, 2000). If a husband values leisure more highly when it is shared with his spouse he may delay retirement until his wife, who is typically younger, becomes eligible for Social Security or pension benefits. The connection between increasing LFP of older married women and the recent increase for older men was analyzed by Schirle (2007), who found that about one quarter of the recent increase in older male LFP in the U.S. could be accounted for by growth in participation by older wives. We account for increases in the LFPR of married women by including the wife's wage offer in our model.

Trends in the health of older men have been discounted as a potential explanation for the long run decline in the LFP rate of older men. Health has a major impact on labor force behavior, but trends in health have been positive rather than negative in recent decades (Burtless and Quinn, 2000). Nonetheless we control for health status in our analysis.

3. Empirical Model

We specify an empirical model that can be interpreted as an approximation to the decision rule for employment at older ages implied by a life cycle model. Each period a man, and his wife if he is married, chooses consumption and labor force participation to maximize the expected present discounted value of remaining lifetime utility, subject to a set of constraints³.

³ We focus on behavior at older ages, rather than attempting to model the full life cycle, as in French (2005) and Moffitt (1987). Hours of work of men are clustered around full-time hours (approximately 2000 per year) and to a lesser extent part-time or part-year hours (approximately 1000 per year) (Rust, 1990). At younger ages there is very little non-participation by men. Withdrawal from the labor force at older ages typically involves an abrupt transition from full time or part time to zero hours of work, and understanding this behavior is unlikely to be aided by analysis of hours of work choices at younger ages. Moffitt's (1987) evidence suggests that younger men do not take account

Utility is derived from leisure and consumption, and preferences may depend on individual characteristics such as age, health, race, marital status, and education. The constraints include Social Security and pension rules, wage offer functions, net worth, and the rate of return on assets. The labor force participation decision is made by comparing the maximized value of discounted utility from working and not working, given expectations about future realizations of random variables. Now consider how to derive a useful empirical approximation to the decision rule for labor force participation. We discuss Social Security, pensions and health insurance, wage rates, and assets, in turn.

1. *Social Security*. The parameters of the Social Security system, together with an individual's average lifetime earnings and beliefs about future benefit rules, wages, health, and mortality fully characterize Social Security benefits in the model. We approximate the effects of Social Security rules with a small number of variables measuring the benefit that an individual would receive as a result of following a specified sequence of labor supply choices and exiting the labor force at a specified age, conditional on experiencing a specified earnings sequence. There is an infinite number of such Social Security benefit variables, depending on the labor supply and wage sequences specified, but they are all highly correlated since they depend on the same underlying rules. We use the following variables as “approximately sufficient statistics” for the effect of Social Security on LFP⁴:

of Social Security and pension incentives that will affect their standard of living far in the future when they are retired.

⁴ Many studies of the effect of Social Security on retirement convert the monthly benefit into a stock of “Social Security wealth” using an assumed interest rate and mortality schedule. This approach is based on the assumption of a perfect capital market. This is not a very appealing assumption in the context of Social Security, given that a liquidity constraint is the only plausible reason for the large spike in labor force exit at the earliest entitlement age. Using the benefit instead of a wealth measure means that the coefficient estimate captures the effects of liquidity constraints, discounting, and mortality expectations, as well as retirement incentive effects. This should be kept in mind when interpreting the estimates. We discuss below alternative specifications using Social Security wealth. Other studies use the replacement rate (the benefit divided by earnings) as an explanatory variable to capture the effect of Social Security. We include the wage offer, thus implicitly accounting for the replacement rate.

(a) SSB_{NRA} , the retirement benefit an individual would receive at the normal retirement age (NRA) if he were to work full time in every year from the age of labor force entry through age $NRA-1$ at the mean of his age-specific wage offer distribution, and were to leave employment at age NRA and never work again.⁵ SSB_{NRA} varies across individuals only as a result of differences in the rules in effect for different birth cohorts and differences in lifetime earnings across birth cohorts and education groups. This variable is intended to capture the wealth effect of Social Security (Moffitt, 1987), so we expect it to have a negative effect on LFP. In order to isolate the effects of rule changes from lifetime earnings changes, we include in the model the average lifetime earnings implied by the assumed age-specific earnings sequence.

(b) SSB_{62} , the retirement benefit the individual would receive at age 62 (the earliest age at which the Social Security retirement benefit can be claimed) if he were to work full time from the age of labor force entry through age 61 at the mean of his age-specific wage offer distribution, leave employment at age 62, and never work again. This variable is intended to capture the effect of the early retirement penalty. In order to facilitate this interpretation, it is specified in differenced form as $SSB_{62}-SSB_{NRA}$. A higher value of the variable implies a smaller early retirement penalty, so it should have a negative effect on labor force participation.

(c) SSB_{70} , the retirement benefit the individual would receive at age 70 if he were to work full time through age 69 at the mean of his age-specific wage offer distribution, leave employment at age 70, and never work again. Since the 1983 Social Security amendments, there has been no increase in the benefit for delaying retirement past age 70. This variable picks up the effect of the Delayed Retirement Credit (DRC), which rewards later claiming with higher benefits. It is specified in differenced form as $SSB_{70}-SSB_{NRA}$. A higher value implies a larger

⁵ The normal retirement age is 65 for individuals born in or before 1937; $65 + x/6$ for birth years $1937+x$, $x=1, \dots, 5$; 66 for birth years 1943-1954; $66 + x/6$ for birth years $1954+x$, $x=1, \dots, 5$; and 67 for birth years 1960+.

incentive to delay retirement, so it should have a positive effect on the LFPR.⁶

(d) SSB_{td} , the Social Security disability benefit the individual would receive in period t if he were to work full time through age $t-2$ at the mean of his age-specific wage offer distribution, withdraw from the labor force at age $t-1$, and become eligible for SSDI at age t . The requirement of not working at age $t-1$ is intended to capture the waiting period, which in reality is five months. SSB_{td} is zero from the NRA onward, because the SSDI benefit is converted to an OASI benefit at the NRA. This variable is intended to capture the incentive effects of SSDI benefits, and is expected to have a negative effect on LFP.⁷

This specification captures the main labor force participation incentives of Social Security: the wealth effect, the early retirement penalty, the delayed retirement credit, and the SSDI incentive effect.⁸ It does not account for several other channels through which Social Security might affect retirement decisions. The most important omitted channels are the Social Security Earnings Test (SSET), spouse benefits, and the payroll tax. The SSET imposes a tax on benefits for each dollar of earnings above a specified threshold, but repays the benefits lost due to the earnings test when the individual's earnings subsequently drop below the threshold. The

⁶ It is worth noting that a standard life cycle model implies that benefits available conditional on retirement at alternative ages should affect LFP at a given age. Thus, for example, the benefit available conditional on exit from the labor force at age 70 will affect the retirement decision at age 55. The model does not condition on past labor force participation, nor does it assume that exit from the labor force is irreversible. However, the life cycle model also implies that the effect of the benefit available at a given age will differ depending on the individual's current age. We do not allow for this in our main specification, but we discuss results from such a specification below.

⁷ A higher SSDI benefit increases the incentive to apply for SSDI and withdraw from the labor force, conditional on health. Many SSDI applications are denied, so the coefficient on SSB_{td} picks up both the incentive effect and the cost of applying for SSDI given that the application may be unsuccessful. See Autor and Duggan (2003), Chen and van der Klaauw (2008), and Benitez-Silva et al. (2004) for recent analyses of SSDI.

⁸ We investigated whether the Social Security variables described above are "approximately sufficient statistics" for the effects of Social Security by computing other Social Security benefit variables, assuming different earnings paths and different ages of entitlement. We regressed each of these other variables on the three retirement benefit variables described above and the associated average lifetime earnings. For benefits available at alternative claiming ages using the same earnings history, the R^2 exceeded 0.99 in every case. For benefits based on alternative earnings histories with a similar lifetime average value but a different slope, the R^2 was in the range 0.91 to 0.95. For benefits based on alternative earnings histories with lower or higher lifetime average value, the R^2 was in the range 0.80 to 0.95. Thus, the Social Security variables included in the specification capture most of the variation in Social Security rules.

SSET has been found to have moderate labor supply effects on affected individuals (those who would work in the absence of the SSET), but affected individuals are in practice a small share of the older population (Friedberg, 2000; Burtless and Moffitt, 1985). We ignore it here because there is no straightforward way to measure its effect in our framework.

A married man's wife is eligible for a Social Security benefit based either on her own earnings record or her husband's earnings record, depending on which provides the larger benefit. While it is reasonable to specify Social Security benefits for men based on the assumption of continuous full time employment for many years, this assumption would not be reasonable for married women. In the absence of longitudinal data on the earnings histories of wives, there is no straightforward way to compute a reasonable approximation to the benefit for which a spouse would be eligible, so we omit spouse benefits.⁹

Finally, we do not model the Social Security payroll tax, which is a proportional levy on covered earnings up to a maximum taxable amount. The only variation in the tax rate in a given year is that the marginal rate is zero for men whose earnings are above the taxable maximum. Given our focus on Social Security benefits computed at mean earnings, this source of variation is irrelevant because mean earnings are below the taxable maximum for nearly every cell in our data¹⁰. Time series variation in the payroll tax is not cohort-specific, and is picked up by calendar year effects. We discuss below the sensitivity of our results to ignoring taxes.

2. *Pensions and health insurance.* We have data on coverage by Defined Benefit and Defined Contribution pension plans, but we do not observe the rules or the variables that determine benefits (job tenure, average earnings at the pension job, the DC account balance).

⁹Labor force participation of married women increased substantially during the period covered by our analysis, so the wives of more recent cohorts of married men are more likely to qualify for a benefit based on their own earnings history rather than the husband's earnings history. Thus it would be quite misleading to assume that all wives receive a spouse benefit rather than a benefit based on their own earnings record.

¹⁰ Mean annual earnings are greater than the maximum taxable earnings for some cohorts from 1960-1972.

Similarly, we observe whether an individual is covered by an employer-provided health insurance plan with retiree benefits, but we do not observe the associated rules or state variables. We include the coverage variables as crude controls for trends in pensions and health insurance, but we do not claim to capture the incentive effects of these potentially important factors.

3. *Wage rates.* We observe the wage rate for an individual only if he or she chooses to work. To circumvent this problem, we use the fitted value from a birth-year-sex-education-specific log wage regression on age, race, marital status, region, and metropolitan status. These regressions are not corrected for selection on unobservables, since there is no plausible source of identification. The Appendix describes the regression specification in more detail. The predicted value of the man's log wage offer, and, if he is married, the spouse's predicted log wage offer, are included in the labor force participation model.

4. *Net worth.* We lack data on net worth for most of our sample, so it is not feasible to include net worth in the analysis. This is a significant limitation of our specification, although in practice most studies of retirement have found a very small effect of net worth (e.g. Blau, 1994; Diamond and Hausman, 1984). However, if most wealth accumulation results from saving out of earnings, average lifetime earnings may pick up the effect of net worth. We discuss below the robustness of the results to controls for wealth proxies.

In order to facilitate aggregation, we specify a linear model for individual labor force participation. We aggregate the data within cells defined by calendar year, single year of age, and four categories of educational attainment (high school dropout, high school graduate, some college, and college graduate). The dependent variable is the labor force participation rate, and the explanatory variables are Social Security benefits, lifetime average earnings, pension and EPRHI coverage, wage rates, health status, marital status, and race.

As noted above, we also include fixed effects for calendar year, age, education, and alternative controls for birth year. Social Security rules generally vary only across birth cohorts for a given age at claiming, but lifetime earnings used to compute benefits vary across education groups. This makes it feasible to identify Social Security effects even with controls for birth year. However, including birth cohort controls changes the source of identification from cross-birth-cohort variation in rules to within-birth-cohort variation in lifetime earnings by education. This is not an innocuous difference, and certainly affects the interpretation of the estimates. The purpose of controlling for birth cohort is to guard against the possibility of unobserved changes in preferences, constraints, or institutions across birth cohort that might have influenced retirement behavior. Nevertheless, we recognize that this may be problematic in practice, so we present estimates both with and without birth year controls.

An important issue for identification and interpretation is how to model expectations about Social Security rule changes. Krueger and Pischke (1992) assume myopic expectations in their analysis of the 1977 reform, arguing that because this reform unexpectedly reduced benefits after a long series of previous benefit increases, it is unlikely that the benefit reduction was foreseen by individuals. This may be a reasonable assumption for the 1977 reform, but the assumption of myopia is less tenable in years prior to 1977. There were eight changes to Social Security rules between 1961 and 1975, each increasing the generosity of benefits. We conduct our analysis under two alternative extreme assumptions: perfect foresight and complete myopia. We cannot defend either assumption as appropriate for the entire period of our analysis, but we can determine how sensitive the results are to these alternative assumptions.¹¹

¹¹ Moffitt (1987) specified a time series forecasting model of benefit changes in his analysis of the 1950s and 1960s. We tried the same approach for our period, but the results yielded implausible forecasts, so we did not pursue this approach.

4. Data

We estimate the econometric model on a synthetic panel data set constructed from micro data from the Current Population Survey (CPS) and the Survey of Income and Program Participation (SIPP), combined with aggregate data from the Social Security Administration (SSA). Individual records on men aged 55-69 from the CPS and SIPP are aggregated into cells defined by single year of birth, single year of age, and the four education groups defined above. The aggregated data from the CPS and SIPP are merged at the cell level. The result is a synthetic panel data set covering 58 birth years (1892 to 1949) between 1962 and 2005, although no cohort has data for all of these years, and some cohorts are dropped due to small sample sizes. Data from 1963 are dropped because there is no information on education in the 1963 CPS. Because we focus on LFP behavior at older ages, we include only cohorts that can be observed at ages 55 to 69 in our sample. The estimation sample contains observations on 2,453 cells with at least 30 observations per cell. Cells with fewer than 30 observations are dropped.¹²

Most of the data are from the March supplement to the CPS from 1962 to 2005. These data are used to construct measures of demographic characteristics, labor force participation, and wage rates of older men and their spouses. Figure 2 shows the trend in the male LFPR at ages 55-69 averaged over all education groups for the period 1962-2005. A man is treated as a labor force participant if he worked or was actively searching for work (unemployed) in the week prior to the March survey. The LFPR in this age range declined slowly in the 1960s, and then fell from over 70% in the early 1970s to 55% in the mid 1980s. The LFPR was essentially flat from the mid 1980s to the mid 1990s, and then rose by about five percentage points after the mid

¹² The CPS reports age at the survey date, but not birth year. The majority of individuals interviewed in March will have their birthday later in the year. For simplicity, we assume that all men have their birthday after the March survey date, implying that birth year = survey year minus age minus one. Below, we discuss the robustness of our results to alternative assumptions about birth year. Birth year is available in the SIPP.

1990s. Figure 3 shows the trends for four age groups separately. The downward trend through the mid 1980s was common to all of the age groups, but sharpest at the older ages. And the rebound in LFP since the 1990s occurred only for the older groups (65-66 and 67-69). Trends in the education distribution of the older male population during this period are shown in Figure 4, which illustrates the rapid shift from a large majority of high school dropouts in 1962 to mainly high school graduates and college attendees today. Figure 5 shows that the LFPR is on average about 10 points lower for high school dropouts than for high school graduates. Thus, educational composition effects may be important.

Figure 6 shows the trend in bad health, based on CPS data. We follow Peracchi and Welch (1994) in defining a man to be in bad health if he did not work full time in the survey reference week or in the previous year and he attributes that choice to disability. The CPS measure shows a decline in the incidence of poor health from 18-20% in the early 1970s to around 12% in the 1990s. Because this measure depends on labor force status in previous periods it is likely to be endogenous with respect to LFP choice in the current period. Figure 6 also shows the incidence of poor health for the same cohorts of men based on data from the National Health Interview Survey (NHIS). The NHIS measure is derived from a question on general health status, with responses of fair and poor treated as “bad” and responses of good, very good, and excellent treated as “good.” Although the levels of the two measures differ, they follow the same trend over time. The NHIS measure is available only from the 1970s, so we use the CPS measure because it is available for the 1960s as well.

We use data from the *Annual Statistical Supplement to the Social Security Bulletin* on average taxable earnings by cohort and age to construct measures of benefits. The published SSA data are combined with CPS earnings data to form earnings histories that are input to the

ANYPIA benefit calculator provided by SSA to compute benefits. Details on the construction of the benefit measures are provided in the data appendix. Figure 7 illustrates trends in the real SSB for entitlement at ages 62, the NRA, and 70. SSB_{NRA} follows an upward trend during the entire period, but with much slower growth in the 1980s than in other periods. The SSB_{62} trend is parallel to the SSB_{NRA} trend until the late 1990s, when it begins to diverge. The divergence is due to the increase in the penalty for early retirement resulting from the increase in the NRA from 65 to 66. SSB_{70} rises relative to SSB_{NRA} for most of the period, but the increase is especially notable in the 1990s and 2000s as the increased DRC legislated in the 1983 reform is phased in.

Figure 8 shows the trend in the SSDI benefit, averaged over ages 55-64 (SSDI eligibility ceases at the NRA). The trend is generally upward, but is more irregular than the retirement benefit trend because benefits are age-specific, and the rules used to compute the potential benefit are the same for all awardees in each year regardless of birth year. The “notch” induced by the 1977 amendment is clearly visible in this case. Figure 9 shows trends in lifetime average monthly earnings by education group, and highlights the rapid growth in lifetime earnings disparity by education.

Figures 10 and 11 show education-specific trends in predicted log real wage rates for men aged 55-69 and for wives of married men in this age range. Real wages of older men and women have been stagnant or declining since the 1970s, and dispersion across education groups has increased.

We use data from topical modules of various SIPP panels to measure participation in DB and DC pensions, and availability of EPRHI. Respondents are asked if they are covered by EPRHI only if they are receiving income from a private pension at the time of the survey. To

deal with small sample sizes for early birth cohorts, our measures of pension participation and availability of EPRHI are averaged across the earliest birth years separately by education group. Data for the earliest birth years likely suffer from mortality bias. There may be additional biases for our measure of EPRHI, as individuals covered by EPRHI are more likely to be retired and receiving retirement income than those not covered by EPRHI. Details on how DB, DC, and EPRHI indicators are constructed are included in the appendix. Figure 12 shows that for men aged 55-69 DB pension coverage trended upward until the 1980s and began to decline in the 1990s. DC pension coverage increased slowly but steadily during the entire period. EPRHI coverage rose through the 1980s and has been roughly constant since then.

5. Results

A. Estimates

Regression results are shown in Table 1 for several specifications of the LFPR model for men aged 55-69, using data from 1962 through 2005. All specifications shown in Table 1 are based on the assumption of perfect foresight with respect to Social Security rules, and all include fixed effects for single years of age, single calendar years, and education groups. The columns differ by how birth year effects are specified.¹³ The first specification has no controls for birth year, the second includes a quadratic birth year trend, the third includes dummies for five-year birth-cohort group effects, the fourth includes dummies for two-year birth-cohort group effects, and the last includes a full set of single-year-of-birth effects. Figure 13 shows the actual and fitted trends in LFP for all of the specifications. All specifications provide a good fit to the data, both overall, and by age group (not shown). In fact, the alternative birth year specifications are

¹³ We also estimated specifications in which the calendar year fixed effects were replaced by a quadratic time trend. The main results were similar to those reported here, indicating that the results are not highly sensitive to the specification of time effects.

virtually indistinguishable in terms of model fit. The test statistics at the bottom of the columns of Table 1 indicate that the no-birth-year-effects model in column 1 is strongly rejected against the quadratic birth year specification (and against all of the other specifications; results not shown), while the two-year-birth-cohort specification in column 4 is not rejected against the specification with a full set of birth year fixed effects in column 5.

As discussed above, the Social Security benefit for retirement at the normal retirement age (SSB_{NRA}) should capture the wealth effect of Social Security, so we expect it to have a negative effect on LFP. The results confirm this expectation across all of the specifications except the last. The magnitude of the effect declines across the columns of Table 1, as richer controls for birth year effects are added. The coefficient estimate of -0.21 in column 1 implies an elasticity of LFP with respect to SSB_{NRA} of -0.41 at the means, compared to an elasticity of -0.10 for the estimate in column 4. The gain in SSB from claiming at 62 rather than at the NRA is expected to have a negative effect on LFP. The coefficient estimate is negative in columns 1 and 3, but positive in columns 2, 4, and 5. The effects are small and insignificantly different from zero in the first four columns. The gain in SSB from claiming at 70 rather than at the NRA is predicted to have a positive effect on the LFPR, and the results in the first four columns confirm this. The implied elasticity ranges from .07 in the first column to .02 in column 4, and the coefficient estimate is significantly different from zero in the first three columns. The coefficient estimate on the SSDI benefit is negative, as expected, and is robust in magnitude and significantly different from zero in all specifications. This variable varies by age as well as by birth year and education, and this additional variation seems to provide a robust source of identification. The implied elasticity of the LFPR with respect to the SSDI benefit is -.10 at the means, based on the results in column 4. Average lifetime monthly earnings (AME) is estimated

to have a positive impact on LFP in the first four columns, significantly different from zero. The sign of the AME effect on LFP is ambiguous in the context of our approximate decision rule specification; AME could capture a wealth effect, in which case we would expect a negative sign, but it could also be correlated with higher future wages, implying a positive effect if the value of working in the future is positively associated with the value of working today. The magnitude of the effect is robust across the first four specifications, and implies an elasticity of LFP in the range of 0.10 to 0.21.

It is clear from the comparisons in Table 1 that identification of the effects of Social Security retirement benefits depends on the specification of birth cohort effects. Although all of the Social Security effects are identified in principle even with the inclusion of a full set of birth year fixed effects, there is insufficient variation in practice to permit reliable estimates with a full set of single-year birth cohort effects.¹⁴ We discount the results in the last column as implausible due to lack of identification, and in simulations discussed below we compare the results from the first four columns. The fact that the specification in the fourth column, with two-year birth cohort effects, cannot be rejected against the specification in the last column provides justification for ignoring the results in the last column.

An alternative approach to identification of the Social Security effects is to drop the assumption of perfect foresight. As discussed above, it is difficult to determine an alternative to perfect foresight that would be a reasonable assumption over the entire 1962-2005 period. We report results based on an extreme alternative to perfect foresight, namely complete myopia. The advantage of this assumption is that in some cases Social Security benefits vary by calendar year as well as birth year and education. Table 2 shows the coefficient estimates on the Social Security variables for the same five specifications as in Table 1, using the assumption of

¹⁴ This pattern of findings persists in more parsimonious specifications that include only one SSB variable.

complete myopia to calculate benefits. The results are surprising: all three SSB retirement variables have effects that are the opposite of our expectations based on the model described above. The effect of SSB_{NRA} is positive and significantly different from zero in all five columns, with effect sizes that *increase* with richer controls for birth year. The gain in SSB from retiring at age 70 instead of the NRA has a negative effect on LFP that increases in magnitude and becomes significantly different from zero as richer controls for birth year are added. A higher age-62 benefit (which implies a smaller early retirement penalty) is associated with higher LFP, and the magnitude of the effect again increases across the columns. The effect of AME is also negative and robust across the columns. The results for the SSB variables are difficult to interpret. The assumption of myopia introduces an additional source of variation in the SSB variables: within-birth-cohort variation over time due to unanticipated benefit changes. This yields more precise estimates, but the pattern of the effects is inconsistent with our expectations. It is also difficult to understand why the effect sizes are larger with richer controls for birth year effects. One possible explanation for these results is that there were many rule changes in the 1960s and early 1970s that increased the generosity of benefits. Moffitt (1987) points out that the assumption of myopia during this period is rather implausible since it implies that each of the many changes is assumed to be the last one that will ever occur. The same argument could be made for the 1980s as well. Krueger and Pischke (1992) assume myopic expectations about Social Security rule changes, and they also find results that are quite sensitive to specification and often counterintuitive.

The effects of other variables are less sensitive to the specification of birth year effects. The own log-wage effects in Table 1 are positive, as expected, and significantly different from zero. The estimates imply an elasticity of LFP with respect to the wage rate of about 0.20 at the

observed mean LFPR of 0.608. The wife's log-wage effect is also positive but quite small and insignificantly different from zero. Thus there is no evidence that rising wages of women have induced older men to remain in the labor force longer. DB pension coverage is estimated to have very small and insignificant effects on LFP of older men. DC pension coverage has a small positive effect on LFP, insignificantly different from zero. EPRHI coverage has a very small effect on LFP in all specifications. Bad health has a large and precisely estimated negative impact on LFP in all specifications, and the magnitude of the effect is robust across the columns. Married and previously married men are much more likely to be in the labor force than their never-married counterparts. Education has positive but surprisingly small effects on LFP, compared to the large raw differences shown in Figure 5. The large raw education gap in LFP is "accounted for" in the regression mainly by the wage rate. There is no difference in the LFPR of black and white men after controlling for the other variables in the regression. There is a 10 percentage point gap in the raw data in favor of whites, which is mainly accounted for by education.¹⁵

B. Counterfactual Simulations

The main issue of interest is whether the results can explain the observed LFP trends. We simulate several counterfactual experiments, in order to determine which, if any, of the explanatory variables can account for the trends. Figure 14 shows the result of an experiment in which Social Security retirement rules are fixed at their 1978 values while other variables take

¹⁵In other specifications not reported here, we included the proportion of men working in manual occupations and the proportion self-employed, in order to determine whether physical demands of work at older ages and the greater control over working conditions provided by self-employment affect the LFPR. Manual workers are estimated to have a higher LFPR than their white collar counterparts, contrary to our expectation, but the effect is quite small after controlling for birth year effects. The fraction of men self-employed is associated with a higher LFPR, but virtually the entire effect is eliminated with controls for birth year effects.

on their actual values.¹⁶ We picked the 1978 rules because these were among the most generous rules in the history of Social Security for men claiming benefits at the normal retirement age or earlier. Benefit amounts were increasing prior to 1978, and subsequent reforms have reduced the generosity of Social Security benefits. If changes to Social Security benefits are an important contributor to the downward LFP trend, then fixing benefits at their 1978 level should result in a much flatter LFP trajectory. Figure 14 shows the results of simulations based on the first and fourth specifications in Table 1. The simulated counterfactual trajectory based on the no-birth-year-effects specification (Table 1, column 1) is in fact somewhat flatter than the baseline trajectory, but the trajectory based on the two-year-birth-cohort-effects specification is virtually identical to the baseline case using the observed changes in SS rules. According to these results, the decline in LFP from the early 1960s through the end of the 1980s would have occurred even if there had been no changes in Social Security retirement rules. This result is not an artifact of the specific choice of SS rules; using the rules for other years yields the same finding.

The simulation results are quantified in Table 3, which shows that the mean LFPR at ages 55-69 declined by 18.4 percentage points between 1966-70 and 1988-92. The predicted decline in the LFPR generated by our model, given the observed changes in the explanatory variables during this period, is also 18.4 percentage points for all four specifications. Table 3 shows that changes in Social Security retirement benefit rules can account for 19% of the observed decline in the LFPR using the no-birth-year-effects specification, but only 4% using the two-year-birth-cohort-effects specification. Changes in the rules governing the SSDI benefit can explain 5-7% of the decline in older male LFP during this period. Changes in average lifetime earnings since 1970 did not contribute at all to the downward trend in LFP: the simulations indicate that if

¹⁶ Benefits are computed for each cohort as if they turn 62 in 1978 (birth year 1916), but using their actual earnings history. This allows us to capture the effect of rule changes while holding earnings constant.

average lifetime earnings had remained constant at the 1970 level, the downward trend would have been even stronger than the observed trend. Finally, the table shows that the calendar year and birth year effects can “explain” virtually all the downward trend in the LFPR from 1966-70 to 1988-92.¹⁷

Counterfactual simulation results for the other explanatory variables were very similar across all of the different specifications, so results are shown in Table 4 only for the specification with two-year birth year dummies. None of these other variables can account for more than a tiny fraction of the decline in LFP.

We now use our estimates to analyze the increase in the older male LFPR in recent years. Table 5 shows that the LFPR increased by 4.7 percentage points between 1988-92 and 2001-05, and our models predict exactly this increase given the observed changes in the explanatory variables. We find that the observed increase in LFP can be entirely explained by changes in the education distribution, and this finding is robust across the alternative regression specifications. More educated men participate in the labor force at higher rates (see Figure 5), and the proportion of older men with some college or a four year college degree has increased significantly since the mid 1980s (see Figure 4). Figure 15 shows the implications of our estimates: if the education distribution of the older male labor force was fixed at its 1985 level, LFP would have continued to decline in the 1990s and 2000s rather than increase. This finding is robust to dropping the assumption of perfect foresight with respect to Social Security rules and replacing it with the assumption of myopic expectations (not shown).

Another potential explanation for the rise in male LFP is the increase in the DRC. The first panel of Table 5 indicates that without controlling for birth cohort the rise in the DRC can

¹⁷ Simulation results based on the assumption of myopic expectations concerning Social Security benefits have very similar implications: changes in Social Security benefits cannot explain the decline in male LFP.

explain 64% of the rise in the male LFPR. However, the magnitude of this effect is quite sensitive to the specification of birth cohort effects. The rise in the DRC can explain only 10% of the increase in LFP in the richest birth cohort specification. The other main change in Social Security policy during recent years, the rise in the NRA, can explain at most 12% of the increase in LFP. Including any birth year effects in the model eliminates the NRA effect completely.

Table 5 shows that changes in other explanatory variables either go in the wrong direction or account for only a modest proportion of the increase in LFP. Changing patterns of pension coverage can account for 2-11% of the increase depending on the specification. Calendar year and birth year effects appear to be moderately important in some specifications, but the results are not robust. Changes in own wages cannot explain any of the increase in male LFP, and changes in the wife's wage can explain 4-5% of the increase.¹⁸

The specifications reported above restrict the effects of Social Security and other variables to be the same for different education groups, age groups, and time periods. However, there is no particular reason to expect homogeneous effects across these groups. Therefore, we estimated additional models disaggregated by education, age, and calendar year, in order to determine whether there are any important differences in the effects of Social Security and other variables by education, age, and period. The estimated Social Security effects were quite similar across the four education groups. Counterfactual simulations based on these disaggregated

¹⁸ We estimated models with the same specification as in Table 1 but using the LFPR of the wives of the married men as the dependent variable. These models show consistently positive and significant effects of wives' wages on their own LFPR. This suggests that factors that caused increased LFP of older married women in recent years may have contributed to the increase in older male LFP, but the simulation results in Table 5 suggest that the magnitude of such effects is very small. In addition, we estimated a specification in which the LFP status of the wife is included as an explanatory variable, similar to the approach used in Schirle (2007). The results show a strong positive association between the labor force participation of older men and their wives. A counterfactual simulation based on the estimate indicated that growth in labor force participation of older married women could explain about one quarter of the recent increase in older male labor force participation. This is virtually identical to Schirle's result.

estimates revealed consistent patterns across education groups: as in the pooled estimates, changes in Social Security can account for at most one fifth of the decline in LFP, and adding richer controls for birth year effects reduced the estimated impact of Social Security substantially. The effects were also similar across age groups (55-61, 62-64, 65-66, and 67-69) in the perfect foresight specification, but the myopic specification yielded substantially different effects of Social Security by age. Specifically, the effects for ages 55-61 were similar to those reported in Table 2, while the effects for the three older age groups were more consistent with our prior expectations. However, the magnitudes of these effects are highly sensitive to specification, and many are implausibly large. Finally, estimates by period (1962-1988, 1989-2005) did not reveal any important differences in the effects of the key variables across periods.

C. Reconciling results with the literature

Our results imply that changes in Social Security benefits were not a major cause of the decline in LFP of older men up to the 1980s. This finding is consistent with the results of Moffitt (1987), using time series data, and Krueger and Pischke (1992), who use synthetic panel data like ours. Blau (1994) used longitudinal data on individuals and found that Social Security is important in accounting for variation across individuals in the timing of labor force exit, but that trends over time in Social Security benefits could not explain much of the secular trend in the exit rate from the labor force over the period 1961-1979. Peracchi and Welch (1994) reached a similar conclusion, as did Burtless (1986), using longitudinal data. However, Hurd and Boskin (1984) used longitudinal data on individuals for the period 1968-1973, and found that changes in Social Security benefits could explain the entire 8.2 percentage decrease in participation of men aged 59-67 during this period. Krueger and Pischke (1992) found different results for this period, using synthetic panel data: after accounting for calendar year fixed effects, Social

Security benefits were found to have a small effect on LFP. We attempted to replicate the Krueger-Pischke analysis, by aggregating our data over education groups and re-estimating our model for the period 1967-1975 for ages 58-67. Including age and calendar year fixed effects and excluding birth year controls, like Krueger and Pischke, we find that changes in Social Security benefits cannot explain any of the decrease in LFP during this period. Controlling for a quadratic birth year trend, Social Security can explain 6% of the decline; and controlling for five year birth cohort effects, Social Security can explain 33% of the decline.¹⁹ Thus the results are somewhat sensitive to specification, but they are closer to the findings of Krueger and Pischke than to the findings of Hurd and Boskin.

Stewart (1995) used time series data through 1990 to update Moffitt's (1987) analysis, and found that Social Security benefits could account for about 40% of the decline in LFP of men aged 62-64 and 65-69 from the early 1960s to 1990. We re-estimated our model using data through 1990, aggregated over education. Social Security could explain as much as 19% of the decline in LFP for men aged 65-69 in the specification with a quadratic birth year trend, but the other specifications yielded much smaller effects. At ages 62-64, Social Security could explain as much as 39% of the decline in the specification with two-year birth cohort effects, but again the effects were much smaller in the other specifications.

Pingle (2006) uses micro data from the SIPP for 1983-2003 to estimate the effect on the employment of older men of the increase in the DRC mandated by the 1983 Social Security Amendments. His findings indicate that each one percentage point increase in the DRC raised the employment rate of men aged 65-70 by 1.5 percentage points and of men aged 60-70 by 1.8 percentage points. He does not present counterfactual simulation results, but one can use the

¹⁹ Estimates with two-year and one-year birth year effects yielded implausible results, indicating lack of identification. Our specification is not the same as Krueger and Pischke, so we do not claim that this is a true replication effort, but the specifications are similar enough to make comparisons useful.

results in his paper to infer that the increase in the DRC from 3% in 1983 to 6.5% in 2003 would be predicted by his model to have caused an increase in the employment rate of .0525 for men aged 65-69. The observed increase was .0641, so his model can explain 82% of the observed increase as resulting from the increase in the DRC. As noted above, with no control for birth year, our estimates indicate that the DRC could explain 64% of the observed increase in LFP, but the effect falls monotonically with richer birth year controls, and is only 10% with two-year birth year effects. In order to provide a closer comparison to Pingle's approach, we aggregated our data over education groups and re-estimated our model using the same years and ages as Pingle. The results were again sensitive to birth year controls, but in this case the two-year birth year effects specification (like the specification in Pingle's Table 4, column 4) were closer to Pingle's: the increase in the DRC can explain 139% of the observed increase in LFP of men aged 65-69 and 74% for men aged 62-64. Our specification and Pingle's specifications are not nested, so the evidence is not definitive on the effects of the DRC.

Mastrobuoni (2006) uses micro data from the CPS to estimate the impact on LFP of older men and women of the increase in the NRA mandated by the 1983 Social Security Amendments.²⁰ He finds that for each two month increase in the NRA from 65 to 66, the age of retirement (derived from a model of non-participation in the labor force) rose by one month. Our results discussed above indicated that without any controls for birth year the increase in the NRA can explain only 10% of the observed increase in LFP at older ages; with birth year controls, the effect falls to zero. Mastrobuoni's specification includes age and birth cohort effects but not calendar year effects. Mastrobuoni's model estimates the "treatment effect" of raising the NRA,

²⁰ Duggan et al. (2005) investigate the effect of the increase in the NRA on SSDI enrollment, but they do not examine the effect on LFP. Pingle (2006) finds a positive effect of the increase in the NRA on employment of men aged 60 to 70, but not for men aged 65 to 70. However, his estimate is implausibly large: a one year increase in the NRA is estimated to increase employment by 6-13 percentage points (see his Table 6).

while we force the effect of changes in the NRA to operate through benefits. Mastrobuoni suggests that changes in the NRA may operate through non-economic channels, such as changing social norms or the “focal” retirement age. His specification would pick up such effects while ours would not, thus potentially reconciling our estimate of a small effect of the NRA with Mastrobuoni’s large estimated effect.²¹

D. Robustness

We briefly summarize here the findings from a number of alternative specifications. We repeated the analysis using an alternative measure of labor market attachment: weeks worked in the previous calendar year. The results were qualitatively very similar. We replaced Social Security benefits with Social Security wealth, computed using a standard approach, described in the Appendix. The simulated effects of counterfactual Social Security scenarios were very similar using this approach. We used alternative assumptions about year of birth in order to determine whether the results are sensitive to our assumption that all individuals have their birthdays after the March survey date. The alternatives were to assume that everyone had their birthday before the survey date, and to randomly assign 80% of the observations to have their birthday after the survey date and 20% before the survey date. The results were very similar using all three approaches. We used an after-tax measure of the wage rate, described in the Appendix, and found virtually identical results.

One potential problem with our education fixed effects specification is that the average skill of a given education group may decline as educational attainment increases. For example,

²¹ Baker and Benjamin (1999) estimated the impact of a reduction in the age of eligibility for public pension benefits in Canada. Their approach is similar to Mastrobuoni’s in the sense that they estimated the “treatment effect” of the change in age of eligibility. Their estimates indicate that reducing the age of eligibility increased the rate of benefit receipt but did not reduce labor force participation. They suggest that the marginal beneficiaries would have been out of the labor force anyway.

the marginal college attendee today may have lower ability than in previous periods in which college attendance was less common. This would imply that education effects on LFP should not be treated as fixed.²² To address this possibility, we included interactions between the education dummies and a linear time trend. The implied effects of education were quite sensitive to birth year effects in this alternative specification, in contrast to the results reported in Table 5. This suggests that our results attributing the recent increase in LFP to the changing education composition of the older male population may not be robust.

Finally, our model omits wealth. In order to determine whether the results are sensitive to this omission, we added two additional variables to the model. One is the capitalized value of income from interest, dividends, and rent (using a real return of 4%). The other is a binary indicator of home ownership. These measures are available in the CPS on a consistent basis only from 1977 forward. The estimates show a very small effect of capitalized asset income on LFP, and a rather large negative effect of home ownership. Counterfactual simulation results indicate that changes in home ownership rates and in the capitalized value of asset income have had very little impact on the LFPR of older men.

6. Conclusions

Despite trends in benefit rules that suggest a potentially important role for Social Security, we find no evidence that changes in Social Security were a major cause of the decline in older male LFP from the 1960s through the 1980s. Our estimates imply that changes in the rules that govern Social Security retirement benefits can explain at most 19% of the observed decline in LFP from the late 1960s through the late 1980s. Changes in rules governing SSDI benefits can explain another 7% of the decline. Our results imply that the long run trend has

²² We thank Mark Duggan for pointing this out to us.

occurred mainly as a result of unobserved changes in preferences, constraints, or institutions over time and across birth cohorts. This interpretation is consistent with the fact that LFP of older men was declining for several decades before implementation of Social Security in the U.S. And it is consistent with the fact that LFP trends in many European countries have been similar to those in the U.S. despite differences in the timing of Social Security reforms.

We find that the recent increase in the LFPR of older men can be accounted for by the changing educational composition of the older male labor force. Low-participating high school dropouts have been rapidly replaced in the older male population by higher-participating college attendees and college graduates in recent years. This result is robust across most but not all of the specifications we estimated. Education is likely correlated with unobserved factors that influence preference for work, such as motivation. Men with higher levels of education also enter the labor force at later ages, so it is possible that these men work longer to reach retirement incentive milestones (e.g. tenure rules associated with DB plans) or may simply prefer to exit the labor force at later ages. We attribute a small role to the increases in the DRC, although in some specifications we find a larger effect.

Our findings are consistent with those of several previous studies that suggest a small role for Social Security in explaining LFP trends. Social Security incentives have been found to be powerful in explaining cross-country variation in retirement patterns (Gruber and Wise, 2004) and in explaining variation in labor force behavior within cohorts (e.g. Hurd and Boskin, 1984; Blau, 1994; Rust and Phelan, 1997). In principle, these apparently contradictory results could be reconciled, depending on the relative magnitude of variation in Social Security benefits across countries, across individuals within a cohort, and across cohorts. Alternatively, some sources of variation in Social Security benefits may be more plausibly exogenous than others. Cross

country differences in Social Security benefits and incentives may be confounded by unobserved differences across countries. Cross-individual variation in Social Security benefits can identify behavioral responses only because of the non-linearity of the Social Security benefit rules. Variation over cohorts in Social Security rules within a country may be the most plausibly exogenous source of variation available. If this is the case, then our estimates suggest that the true effect of Social Security on labor force participation at older ages is small. Of course, our results cannot answer the obvious next question of interest: if it is not changes in Social Security (or pensions, EPRHI, or lifetime earnings) that have caused such striking trends in labor force behavior at older ages, then what is responsible?

Appendix

A1. Social Security Benefits

Our analysis requires measures of mean Social Security benefits by cohort. Cohorts are defined by birth year (1892 to 1949) and education group (less than high school; high school graduate; some college; college graduate). Cohort Social Security benefits are a function of Social Security regulations (which vary by birth year) and mean earnings history of each cohort (which varies by birth year and education group). Section A.1.1 details the methods used to construct earnings histories; Section A.1.2 describes how these earnings histories are used to compute cohort specific measures of monthly Social Security benefits; and section A.1.3 describes how monthly benefits are converted to measures of Social Security Wealth.

A1.1 Cohort Specific Earnings Histories

We construct mean earnings at ages 27 through 70 for each cohort using data from the March Current Population Survey (CPS) between 1962 and 2005 and from editions of the *Annual Statistical Supplement* published by the Social Security Administration (SSA) between 1973 and 2005.

The SSA data contain median earnings of male workers by age group (25-29, 30-34, etc.) and calendar year (1937, 1940, 1945, 1950, 1955, and 1960 through 2005). We first convert *median* earnings to *mean* earnings for each age group and calendar year cell using the CPS. In each year of the CPS, respondents report their earnings from the previous year. We use these data to calculate mean earnings, median earnings, and their ratio by age group for years 1961 through 2004.²³ We then estimate the model $MM_{ay} = \alpha_{a0} + \alpha_{a1}y + \varepsilon_{ay}$ separately by age group, where MM_{ay} is the mean-median ratio for age group a in calendar year y . The estimates are used

²³ We capped reported earnings in the CPS at the maximum taxable earnings for that year before computing means and medians by cell.

to generate a predicted value of MM_{ay} for each age group-calendar year cell. Each value for median earnings reported in the SSA is then multiplied by the predicted value of the mean-median ratio for the corresponding age cell to create measures of mean earnings by calendar year and age group.

Next, we assign our measure of mean earnings by calendar year and age group to the midpoint age of that cell. The midpoint age and calendar year is then used to compute birth year. For example, mean earnings of men ages 25-29 in 1972 was \$6,870 (based on median earnings of \$7,405 published by SSA and the ratio of mean to median earnings for this cell in the CPS); this value is assigned to age 27 for birth cohort 1945. To limit selection bias due to non-participation, we ignore SSA earnings data for age groups below 25 and above 59. This process results in values for mean earnings at ages 27, 32, 37, 42, 47, 52, and 57 for each age and birth cohort covered by the SSA data.²⁴

Because the *Annual Statistical Supplement* does not include earnings data in years prior to 1937 (and only in select years between 1937 and 1960), we are unable to assign a value for mean earnings to some age and birth cohort cells. For example, earnings at age 27 (based on age group 25 to 29) are unavailable for birth cohorts prior to 1910; earnings at age 32 are unavailable for birth cohorts prior to 1905; and so on. We impute the missing values for each age by interpolating and extrapolating earnings from observed birth years. We regress log mean earnings on a sixth order polynomial in birth year b separately for each age a using the

model $\ln(E_{ab}) = \sum_{j=0}^6 \beta_{aj} b^j + \varepsilon_{ab}$. Estimates of this model allow us to generate predicted values of

²⁴ Assigning earnings by age group to the midpoint age of each cell is arbitrary, but is preferable to the alternative of assigning the earnings value to every age in the cell. The latter approach would impose the restriction that earnings are equal across some successive birth cohorts and ages. For instance, from the example described above, mean earnings of \$6,870 would be assigned to age 25 for birth year 1947; age 26 for birth year 1946; age 27 for birth year 1945; age 28 for birth year 1944; and age 29 for birth year 1943.

earnings at ages 27, 32, 37, 42, 47, 52, and 57 for all birth years in our sample. The predicted values from our estimates closely match the actual values, and the models generate reasonable predicted values for the age groups and birth years not included in the SSA tables.

Our next step is to “fill in” earnings data at ages 28 to 31, 33 to 36, 38 to 41, 42 to 46, 48 to 51, and 53 to 56 for each birth cohort by linearly interpolating from observed earnings at ages 27, 32, 37, 42, 47, 52, and 57. We regress predicted mean earnings (from the previous step) on a fourth order polynomial in age separately for each birth year using the model

$$\hat{E}_{ba} = \sum_{j=0}^4 \gamma_{bj} a^j + \tau_{ba} .$$

Estimates of this model are used to generate predicted values of mean

earnings at ages 27 through 57 for each birth year cell in our sample. We then compute earnings at ages 58 through 70 by assuming that nominal earnings at ages 57 and later grow at the rate of the average annual wage as published in the SSA.²⁵

Finally, we use CPS data to disaggregate earnings histories by education group. We compute the ratio of mean earnings for each education group to mean population earnings in the CPS separately by birth year.²⁶ We denote this measure the “earnings-ratio” ER_{be} , where b is birth year and e is education group. Because we don’t observe earnings prior to age 57 for birth years before 1906 in the CPS, we assign the 1906 earnings-ratio to these birth years. We then compute predicted values of the earnings-ratio from estimates of a third order polynomial

²⁵ For cohorts who reach ages 58 to 70 in future years we assume that nominal earnings grow at 3% annually.

²⁶ Ideally we could compute earnings-ratios separately for each education group by birth-year and age. However, because the CPS only goes back to 1962 we lack data on earnings at younger ages for earlier birth years. Thus our measure of the earnings ratio “averages out” life-cycle earnings patterns for each birth year. This creates biases for at least two reasons. First, the returns to schooling are higher at older ages, so for higher levels of education we overstate mean earnings at younger ages and understate mean earnings at older ages. The opposite is true for lower levels of education. Second, because we do not observe younger ages for earlier birth years in the CPS, and earnings for better educated men are relatively higher at later ages, we are overstating (understating) the ratio of mean earnings to population earnings for higher (lower) levels of education at earlier birth years.

regression of ER_{be} on birth year using the model $ER_{be} = \sum_{j=0}^3 \lambda_{ej} b^j + \mu_{be}$.²⁷ These predicted earnings ratios are used to construct education group-specific measures of mean earnings (E) by age and birth cohort according to the formula $\hat{E}_{eba} = \hat{E}_{ba} * \hat{ER}_{eb}$. For example, for the 1945 birth cohort, predicted mean earnings at age 27 are \$7,136 (“actual” mean earnings for this cell are \$6,870 as noted above). The predicted ratio of mean earnings of college educated men to all men born in 1945 is 1.30. Thus, mean earnings of the college educated 1945 birth cohort at age 27 are \$9,277. This computation is done for each birth year, age, and education group cell; these are the final measures of mean earnings used to compute Social Security Benefits.

There are some limitations of using CPS data to calculate Social Security earnings histories. The CPS includes some workers who may not have been covered by Social Security, particularly in earlier years. In addition, the CPS data (generated from surveys) is likely subject to a higher degree of measurement error than the SSA data (generated from administrative records). To address this, we removed observations with suspect earnings data from our sample. We dropped all records where the real weekly wage (total earnings in the previous calendar year / number of weeks worked last year) was below \$50 and above \$40,000. This reduced the number of observations with positive earnings in our CPS sample by 2.8% (18,881 records).

A1.2 Computing Cohort Specific Social Security Benefits

We use the earnings history generated for each birth year and education group cell to compute the monthly Social Security Benefit (SSB) conditional on claiming at age t , using cohort mean earnings at each age between 27 and age $t-1$, where t is 62, the NRA, or 70. Although our data are annualized, we compute the SSB conditional on claiming benefits

²⁷ We omit cells with sample size less than 30 from this regression.

precisely at the age (in months) of the cohort-specific NRA. For cohorts born in 1942 and earlier, we include earnings from ages 27 through 64, and for cohorts born in 1943 and later we include earnings between ages 27 and 65 (because the NRA for the latter cohorts is 66)²⁸.

We construct benefits under two alternative assumptions about future rule changes: perfect foresight and myopia. In both cases we assume that future earnings, future average annual wages used to index wages, and future inflation rates are known with perfect foresight. Computing benefits under the assumption of perfect foresight is equivalent to using the benefit rules in place at the assumed claim age. We use the ANYPIA benefit calculator provided by the Social Security Administration to compute SSB in this case²⁹. Based on birth year and retirement age, the ANYPIA program computes the appropriate Primary Insurance Amount (PIA) and monthly benefit for a given earnings history. Under the assumption of perfect foresight there is no variation in the SSB for a given claim age within birth cohort.

Alternatively, under myopia we compute the SSB for a given claim age in year t using the rules that, as of year t , are scheduled to be in place at the assumed claiming age. This assumption results in variation in the SSB by age for a given birth-year-education group cell if there were rule changes between year t and the year in which the individual reaches the assumed claiming age. This variation is most significant in years up to and including the 1977 amendments when substantial rule changes were implemented with relatively little lead time. However, legislation passed in 1983 announced changes in the normal retirement age and the delayed retirement credit many years in advance. As a result these changes were known at earlier ages even under the assumption of myopia, resulting in very little variation in SSB by age

²⁸ Per Social Security rules, earnings in the year that Social Security benefits are claimed are ignored in the computation of PIA.

²⁹ The ANYPIA program is available on the Social Security Website at <http://www.ssa.gov/OACT/ANYPIA/anypia.html>

in the later years of our sample. The ANYPIA program is unable to calculate expected SSB under myopia, so we wrote our own SAS code to do this.

A1.3 Computing Social Security Wealth

In an alternative empirical specification we replace monthly Social Security Benefits with Social Security Wealth, the expected present discounted value of lifetime Social Security

retirement benefits. Social Security Wealth is defined as $SSW_{ia} = \sum_{j=a}^T SSB_{ia} (1+r)^{a-j}$, where

SSB_{ia} is the monthly Social Security benefit awarded to cohort i conditional on claim age a ($a = 62, NRA, 70$); T is life expectancy (in months) from age a , based on life tables published by the Social Security Administration; and r is the monthly interest rate, here set at .167, or roughly 2% annually. We assume the individual survives with certainty to his expected age at death, T , in order to simplify the calculations.

A1.4 Computing Cohort Specific Social Security Disability Insurance Benefits

We use the ANYPIA program and the earnings history generated for each birth year and education group cell to compute the monthly Social Security Disability Insurance (SSDI) benefit. For each potential claim age t we assume full time work through period $t-2$ and no work in period $t-1$ and t . Because SSDI benefits are converted to retirement benefits after reaching the NRA, we set SSDI to 0 at ages greater than or equal to the NRA. The Social Security rules used to compute SSDI depends only on the year in which disability benefits are claimed, and not on the birth year.

A2. Pensions and Employer-Provided Retiree health Insurance (EPRHI)

Pension measures were derived from SIPP topical modules in the 1984 panel (wave 4), 1986 panel (wave 4), 1990 panel (wave 4), 1991 panel (wave 7), 1992 panel (wave 4), 1996 panel (wave 7), and 2001 panel (wave 7). Other panels were excluded due to incompleteness of

data or changes in questionnaire design. These data have small sample sizes for earlier birth years: those who were born in 1900 are 84 at the time of the first survey so there is likely to be significant mortality bias.

Different questions on pensions are asked depending on whether the respondent is currently working, has had a job in the past, has received a lump sum payment from a retirement plan, or is currently receiving retirement benefits (other than Social Security). We compute binary pension coverage indicators as follows:

- Defined Benefit (DB): DB coverage is assumed if (a) the pension from the current job is a DB plan, or (b) the respondent expects to receive pension benefits from a past job, or (c) the retirement benefits he is currently receiving are from a DB plan. Otherwise the respondent does not have a DB plan.
- Defined Contribution (DC): DC coverage is assumed if (a) the pension from his current job is a DC plan, or (b) he owns a business that has a pension plan he participates in, or (c) he is receiving retirement benefits from a DC plan, or (d) he received a lump sum payment from a pension plan in the past. Otherwise the respondent does not have a DC plan.

A SIPP respondent is asked about EPHRI coverage only if he is currently receiving retirement benefits. The binary EPRHI coverage indicator is set to 1 if the SIPP respondent affirms that he has health coverage provided by a former employer, and 0 otherwise.

A3. Wages

Wage rates are constructed from CPS data for the men in our sample and their spouses. The wage rate is defined as total earnings from wages and salary in the previous year divided by annual hours worked in the previous year. Annual hours is the product of weeks worked in the

previous year and “hours usually worked per week in the previous year” for survey years 1976 and later. In years prior to 1976 annual hours is defined as the product of weeks worked in the previous year and hours worked in the week prior to the survey. We follow Blau and Kahn (2007) in the handling of top-coded values for earnings. Generally top-coded values are multiplied by a factor of 1.45 and included in the sample used to estimate the regression equation. Again following Blau and Kahn (2007), we convert all wages to real 2005 dollars and drop observations with hourly wages below \$2 and above \$200.

We only observe wages for those in our sample who choose to work. Therefore we replace observed wages with predicted log wages from regression equations estimated separately by birth year, sex, and education group. The wage equations include as regressors a quadratic in age and indicators for race (white, black, or other), marital status (married, once-married, never-married), geographic region (Northeast, Midwest, South, West), and metropolitan status.

The marginal tax rate is calculated for each individual in the CPS sample using the TAXSIM program provided by the National Bureau of Economic Research³⁰. The marginal federal tax rate (MFTR) is computed based on labor earnings assuming full-time work (2000 hours annually) at the predicted wage rate, observed earnings of the wife (if married), and income from interest, dividends, and net rentals. Payroll taxes for Old-Age, Survivors, and Disability Insurance (OASDI) and Hospital Insurance (HI) are applied to labor earnings (assuming full time work at the predicted wage rate) up to the maximum taxable earnings amount. The after-tax wage rate is the product of the predicted wage rate and $(1 - \text{MFTR} - (\text{OASDI} + \text{HI}))$.

³⁰ See <http://www.nber.org/~taxsim/taxsim-calc7/index.html>

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Table 1: Labor Force Participation Model Estimates Assuming Perfect Foresight

	Birth Year Specification				
	None	Quadratic	5 Year	2 Year	1 Year
SSBnra	-0.206 (0.024)	-0.168 (0.025)	-0.090 (0.033)	-0.052 (0.051)	0.201 (0.099)
SSB62 - SSBnra	-0.046 (0.023)	0.015 (0.027)	-0.001 (0.032)	0.017 (0.057)	0.321 (0.159)
SSB70 - SSBnra	0.122 (0.018)	0.082 (0.020)	0.061 (0.024)	0.027 (0.037)	-0.064 (0.067)
Disability Benefit	-0.045 (0.010)	-0.065 (0.013)	-0.058 (0.012)	-0.065 (0.013)	-0.067 (0.014)
AME	0.037 (0.008)	0.046 (0.008)	0.022 (0.009)	0.024 (0.011)	-0.020 (0.016)
ln(own wage)	0.116 (0.020)	0.116 (0.020)	0.125 (0.021)	0.120 (0.021)	0.129 (0.022)
ln(spouse's wage)	0.016 (0.018)	0.015 (0.018)	0.014 (0.018)	0.018 (0.019)	0.020 (0.019)
DB Pension	-0.035 (0.023)	0.005 (0.024)	-0.014 (0.025)	-0.017 (0.026)	-0.022 (0.027)
DC Pension	0.061 (0.042)	0.026 (0.042)	0.050 (0.043)	0.020 (0.045)	0.076 (0.049)
EPRHI	0.015 (0.021)	0.004 (0.021)	0.009 (0.021)	0.002 (0.023)	-0.003 (0.026)
Bad Health	-0.294 (0.030)	-0.319 (0.030)	-0.299 (0.030)	-0.306 (0.030)	-0.294 (0.031)
Married	0.183 (0.042)	0.213 (0.042)	0.195 (0.041)	0.205 (0.042)	0.218 (0.042)
Previously Married	0.167 (0.049)	0.175 (0.048)	0.163 (0.048)	0.169 (0.049)	0.182 (0.050)
High School	0.026 (0.008)	0.017 (0.008)	0.016 (0.008)	0.015 (0.008)	0.012 (0.009)
Some College	0.041 (0.011)	0.028 (0.011)	0.028 (0.011)	0.025 (0.012)	0.018 (0.013)
College	0.053 (0.017)	0.036 (0.018)	0.040 (0.019)	0.039 (0.019)	0.038 (0.021)
Black	0.003 (0.039)	0.013 (0.039)	-0.006 (0.039)	0.004 (0.040)	-0.013 (0.040)
Constant	0.590 (0.069)	357.137 (67.175)	0.522 (0.075)	0.506 (0.079)	0.510 (0.079)
Birth Year		-0.366 (0.070)			
Birth Year Squared		0.009 (0.002)			
R Squared	0.962	0.963	0.963	0.964	0.964
P-Values					
SS Benefits	0.0000	0.0000	0.0000	0.0000	0.0001
Specification	-	0.0000	-	-	0.1752

Notes: OLS estimates with robust standard errors, 2453 observations.

All specifications include age and calendar year dummies.

Estimates in bold are significantly different than zero at the 5% level.

Social Security retirement benefits are computed under the assumption of perfect foresight.

Monthly Social Security benefits and AME are measured in year 2005 dollars and divided by 1000.

"P-Value: SS Benefits" is from a test of the hypothesis that the coefficients on the four

Social Security variables and AME are all equal to zero.

"P-Value: Specification" is from F test against the specification in the previous column.

Table 2: Labor Force Participation Model Estimates Assuming Myopia

	Birth Year Specification				
	None	Quadratic	5 Year	2 Year	1 Year
SSBnra	0.140 (0.017)	0.131 (0.017)	0.168 (0.020)	0.246 (0.020)	0.274 (0.021)
SSB62 - SSBnra	0.089 (0.021)	0.101 (0.022)	0.123 (0.028)	0.268 (0.031)	0.301 (0.032)
SSB70 - SSBnra	-0.009 (0.013)	-0.018 (0.014)	-0.037 (0.016)	-0.085 (0.019)	-0.120 (0.022)
AME	-0.037 (0.006)	-0.029 (0.007)	-0.026 (0.007)	-0.025 (0.007)	-0.034 (0.008)
R Squared	0.962	0.962	0.964	0.966	0.967
P-Values					
SS Benefits	0.0000	0.0000	0.0000	0.0000	0.0000
Specification	-	0.0125	-	-	0.0001

Notes: OLS estimates with robust standard errors, 2453 observations.

All specifications include age and calendar year dummies.

Estimates in bold are significantly different than zero at the 5% level.

Social Security retirement benefits are computed under the assumption of myopia.

Monthly Social Security benefits and AME are measured in year 2005 dollars and divided by 1000.

"P-Value: SS Benefits" is from a test of the hypothesis that the coefficients on the four

Social Security variables and AME are all equal to zero.

"P-Value: Specification" is from F test against the specification in the previous column.

Table 3: Selected Counterfactual Simulations to Explain the Decline in Labor Force Participation, 1966-70 to 1988-92

(a) No Birth Cohort Effects

	Actual LFPR	Predicted LFPR	Social Security Retirement	Social Security Disability	Average Lifetime Earnings	Calendar and Birth Year
1966 to 1970	73.0	72.9	70.6	71.9	72.9	72.8
1988 to 1992	54.6	54.6	55.7	54.4	49.8	70.9
Decrease	18.4	18.4	15.0	17.4	23.1	1.9
Difference			3.4	0.9	-4.7	16.5
% of Decrease			19%	5%	-26%	90%

(b) Quadratic Birth Cohort Effects

	Actual LFPR	Predicted LFPR	Social Security Retirement	Social Security Disability	Average Lifetime Earnings	Calendar and Birth Year
1966 to 1970	73.0	72.9	70.7	71.4	72.9	72.3
1988 to 1992	54.6	54.6	54.9	54.4	48.8	71.7
Decrease	18.4	18.4	15.8	17.0	24.2	0.6
Difference			2.6	1.4	-5.8	17.8
% of Decrease			14%	7%	-31%	97%

(c) 5 Year Birth Cohort Effects

	Actual LFPR	Predicted LFPR	Social Security Retirement	Social Security Disability	Average Lifetime Earnings	Calendar and Birth Year
1966 to 1970	73.0	73.0	72.1	71.6	73.0	71.5
1988 to 1992	54.6	54.6	55.0	54.4	51.7	70.6
Decrease	18.4	18.4	17.1	17.2	21.2	0.9
Difference			1.3	1.2	-2.8	17.5
% of Decrease			7%	7%	-15%	95%

(d) 2 Year Birth Cohort Effects

	Actual LFPR	Predicted LFPR	Social Security Retirement	Social Security Disability	Average Lifetime Earnings	Calendar and Birth Year
1966 to 1970	73.0	73.0	72.3	71.4	73.0	72.0
1988 to 1992	54.6	54.6	54.6	54.4	51.6	72.0
Decrease	18.4	18.4	17.7	17.0	21.4	0.0
Difference			0.7	1.4	-3.0	18.4
% of Decrease			4%	7%	-16%	100%

Notes: Counterfactual values for SSB_{NRA} , SSB_{62} , and SSB_{70} are computed for each cohort using their actual earnings history and the Social Security rules in effect as if they turn 62 in 1978 (birth year 1916). Counterfactual Social Security Disability Insurance benefits are generated by computing SSDI for each cohort and age under the rules in effect in 1970. Counterfactual Average Lifetime Earnings are generated by assigning the average of AME between 1966 and 1970 to every cohort. The calendar year counterfactual value is 1968; the birth year counterfactual value is 1906.

The "Social Security Retirement" counterfactual is the predicted LFPR where actual values for SSB_{NRA} , SSB_{62} , and SSB_{70} are replaced by their counterfactual values, and all other variables take on their actual values. Similarly, the "Social Security Disability", "Average Lifetime Earnings", "Calendar Year", and "Birth Year" counterfactual is the predicted LFPR where the variable of interest is replaced by its counterfactual value, and all other variables take on their actual values.

Table 4: Additional Counterfactual Simulations to Explain the Decline in Labor Force Participation, 1966-70 to 1988-92

	Actual LFPR	Predicted LFPR	Black	Marital Status	Health	Pensions	EPRHI	Own Wage	Spouse's Wage	Education
1966 to 1970	73.0	73.0	73.0	73.0	72.9	73.0	73.0	73.0	73.0	69.3
1988 to 1992	54.6	54.6	54.5	54.6	54.3	54.6	54.6	55.5	54.7	49.2
Decrease	18.4	18.4	18.4	18.3	18.6	18.4	18.4	17.4	18.3	20.2
Difference			0.0	0.1	-0.2	0.0	0.0	1.0	0.1	-1.8
% of Decrease			0%	0%	-1%	0%	0%	5%	1%	-10%

Notes: The counterfactual value for each variable of interest is its average value between 1966 and 1970. Counterfactual LFPR is the predicted value (based on coefficient estimates from the specification with 2 year birth cohort effects) where the variable of interest is replaced by its counterfactual value, and all other values take on their actual values.

Table 5: Selected Counterfactual Simulations to Explain the Increase in Labor Force Participation, 1988-92 to 2001-05 Change decrease to increase in each panel

(a) No Birth Cohort Effects

	Actual LFP	Predicted LFP	Black	Marital Status	Health	Pensions	EPRHI	Own Wage	Spouse's Wage	Average Lifetime Earnings	SS Rules: DRC	SS Rules: NRA	Calendar and Birth Year	Education
1988 to 1992	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	53.9	54.8	54.6	53.4
2001 to 2005	59.2	59.2	59.2	59.5	59.7	58.7	59.3	59.2	59.0	58.5	55.6	58.9	59.5	52.4
Increase	4.7	4.7	4.7	4.9	5.1	4.1	4.7	4.7	4.5	4.0	1.7	4.1	4.8	-0.9
Difference			0.0	-0.2	-0.5	0.6	0.0	0.0	0.2	0.7	3.0	0.6	-0.2	5.6
% of Decrease			0%	-5%	-10%	13%	-1%	0%	4%	15%	64%	12%	-3%	120%

(b) Quadratic Birth Cohort Effects

	Actual LFP	Predicted LFP	Black	Marital Status	Health	Pensions	EPRHI	Own Wage	Spouse's Wage	Average Lifetime Earnings	SS Rules: DRC	SS Rules: NRA	Calendar and Birth Year	Education
1988 to 1992	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.2	54.7	54.2	53.4
2001 to 2005	59.2	59.2	59.2	59.6	59.7	59.1	59.2	59.2	59.0	58.4	57.2	59.5	57.5	52.3
Increase	4.7	4.7	4.6	5.0	5.2	4.6	4.7	4.7	4.5	3.8	3.1	4.7	3.3	-1.1
Difference			0.0	-0.3	-0.5	0.1	0.0	0.0	0.2	0.8	1.6	-0.1	1.4	5.7
% of Decrease			0%	-7%	-11%	2%	0%	0%	4%	18%	34%	-2%	29%	123%

(c) 5 Year Birth Cohort Effects

	Actual LFP	Predicted LFP	Black	Marital Status	Health	Pensions	EPRHI	Own Wage	Spouse's Wage	Average Lifetime Earnings	SS Rules: DRC	SS Rules: NRA	Calendar and Birth Year	Education
1988 to 1992	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.2	54.7	55.0	53.3
2001 to 2005	59.2	59.2	59.2	59.5	59.7	58.9	59.2	59.2	59.1	58.8	57.5	59.2	59.5	52.3
Increase	4.7	4.7	4.7	5.0	5.1	4.3	4.7	4.7	4.5	4.3	3.2	4.5	4.5	-1.0
Difference			0.0	-0.3	-0.5	0.4	0.0	0.0	0.2	0.4	1.4	0.2	0.2	5.7
% of Decrease			0%	-7%	-10%	8%	0%	0%	4%	9%	31%	3%	4%	122%

(d) 2 Year Birth Cohort Effects

	Actual LFP	Predicted LFP	Black	Marital Status	Health	Pensions	EPRHI	Own Wage	Spouse's Wage	Average Lifetime Earnings	SS Rules: DRC	SS Rules: NRA	Calendar and Birth Year	Education
1988 to 1992	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.6	54.4	54.6	55.6	53.3
2001 to 2005	59.2	59.2	59.2	59.6	59.7	59.0	59.2	59.2	59.0	58.8	58.6	59.4	59.3	52.3
Increase	4.7	4.7	4.7	5.0	5.2	4.4	4.7	4.7	4.4	4.2	4.2	4.8	3.8	-1.0
Difference			0.0	-0.3	-0.5	0.2	0.0	0.0	0.2	0.4	0.5	-0.1	0.9	5.7
% of Decrease			0%	-7%	-10%	5%	0%	0%	5%	9%	10%	-2%	19%	122%

Notes: For the Delayed Retirement Credit (DRC) counterfactual, SSB_{NRA} and SSB_{62} are computed for each cohort using their actual earnings history and actual Social Security rules. SSB_{70} is computed using SSB_{nra} and the DRC rules in place prior to the 1983 amendments. For the Normal Retirement Age (NRA) counterfactual, SSB_{nra} , SSB_{62} , and SSB_{70} are computed using actual earnings histories and counterfactual Social Security rules where the NRA is equal to 65 for all birth cohorts. The calendar year counterfactual value is 1990; the birth year counterfactual value is 1928. For each other variable the counterfactual value is its average between 1988 and 1992. The counterfactual LFPR is the predicted value (based on relevant coefficient estimates) where the variable of interest is replaced by its counterfactual value, and all other values take on their actual values.

Figure 1: LFPR Trends by Age Group (Source: Bureau of Labor Statistics)

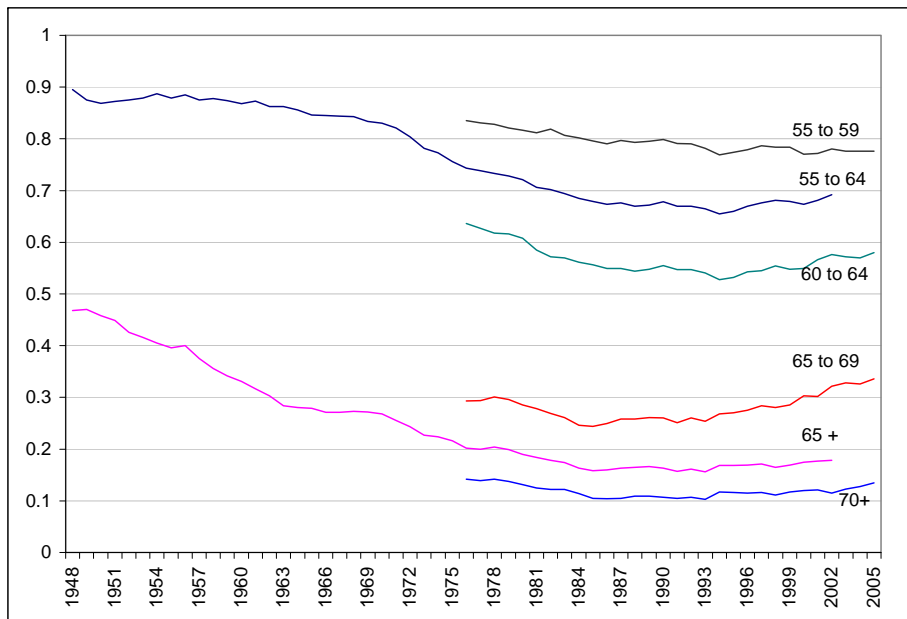


Figure 2: LFPR of Men Aged 55 to 69

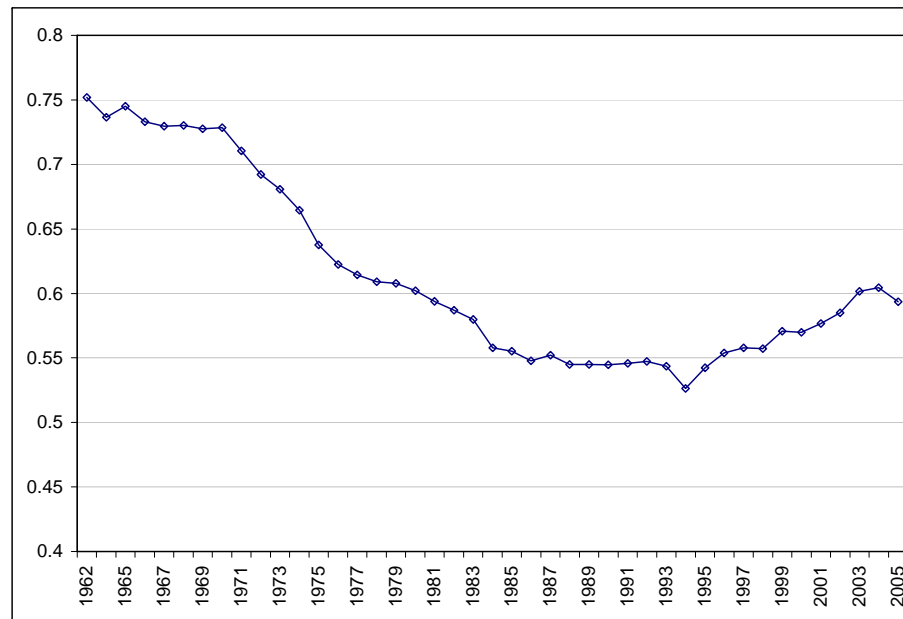


Figure 3: LFPR By Age Group

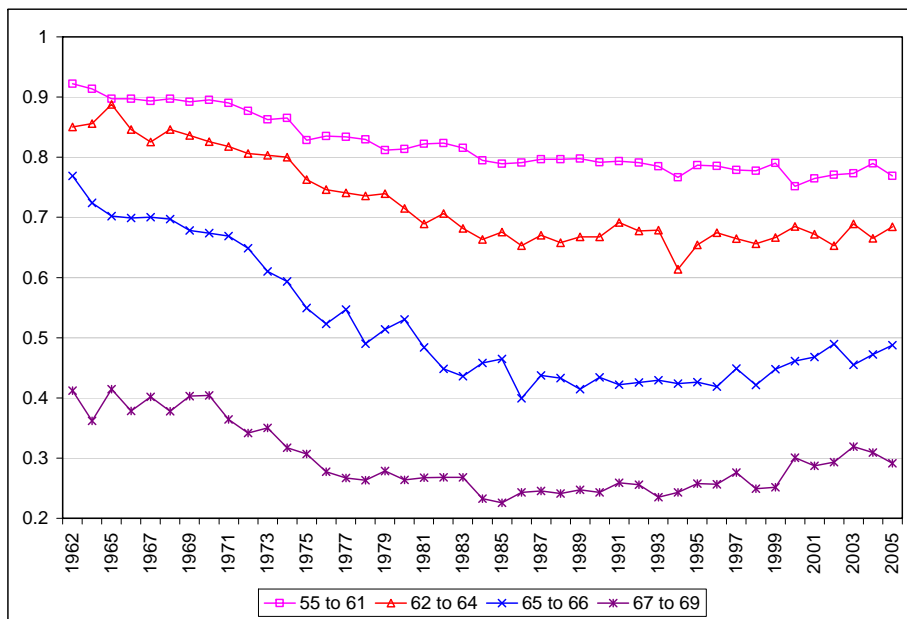


Figure 4: Educational Distribution

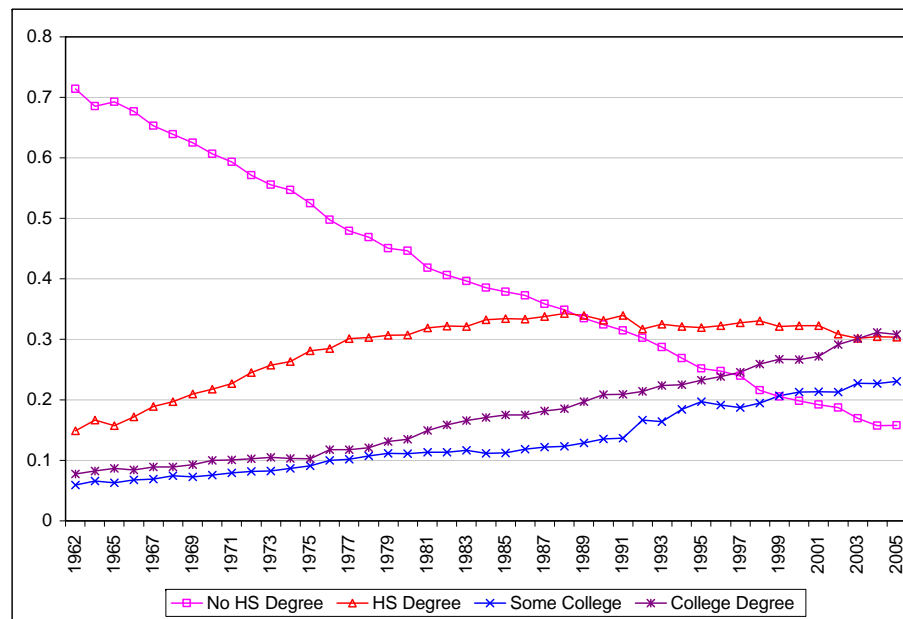


Figure 5: LFPR by Education

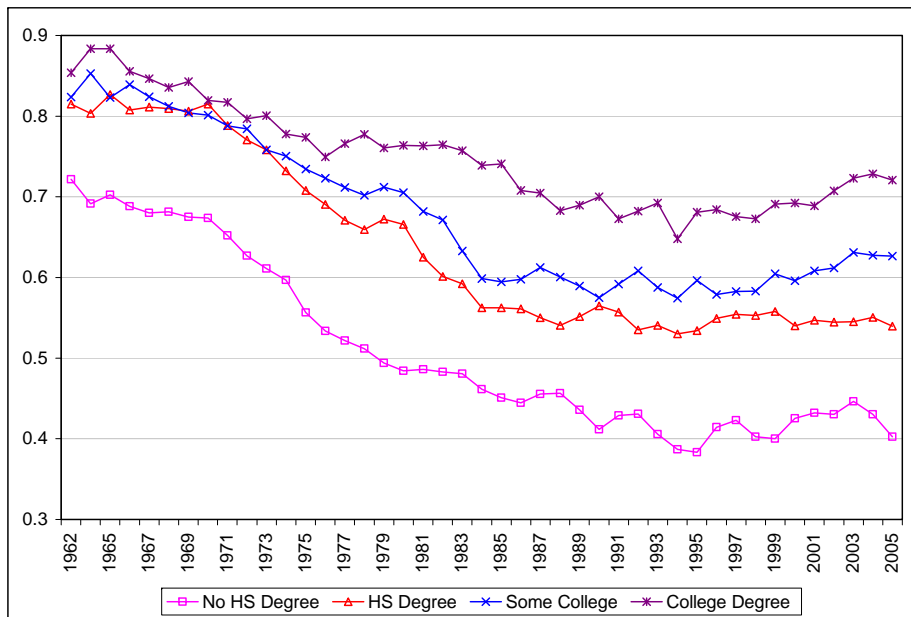


Figure 6: Incidence of Bad Health (by Data Source)

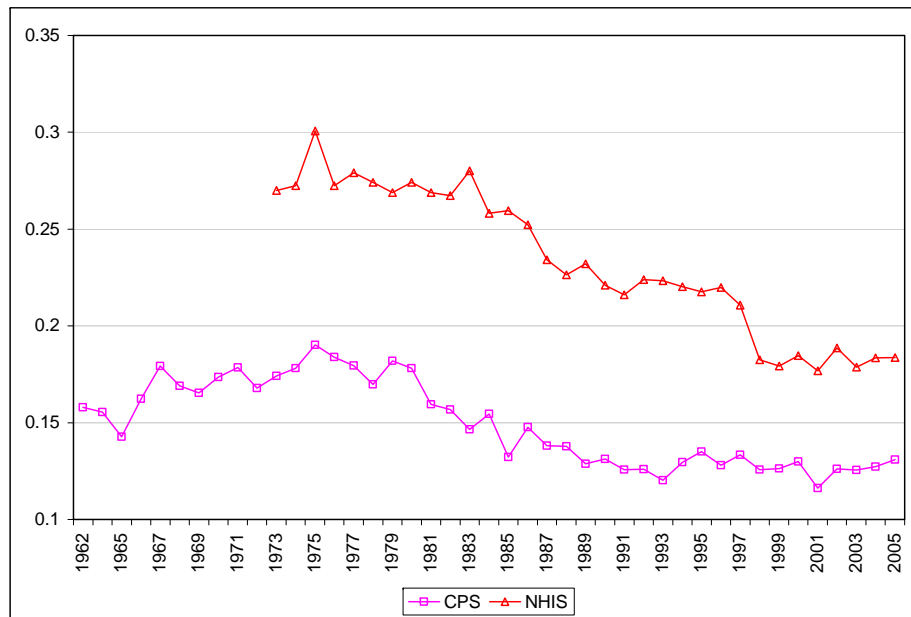


Figure 7: Monthly Social Security Benefit by Claim Age (2005 \$)

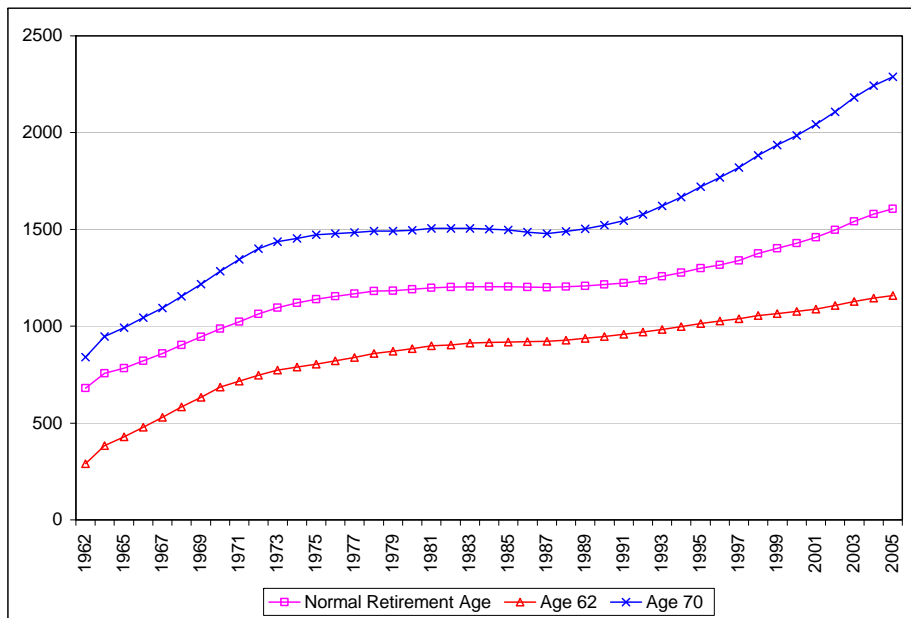


Figure 8: Monthly Social Security Disability Insurance Benefit (2005 \$)

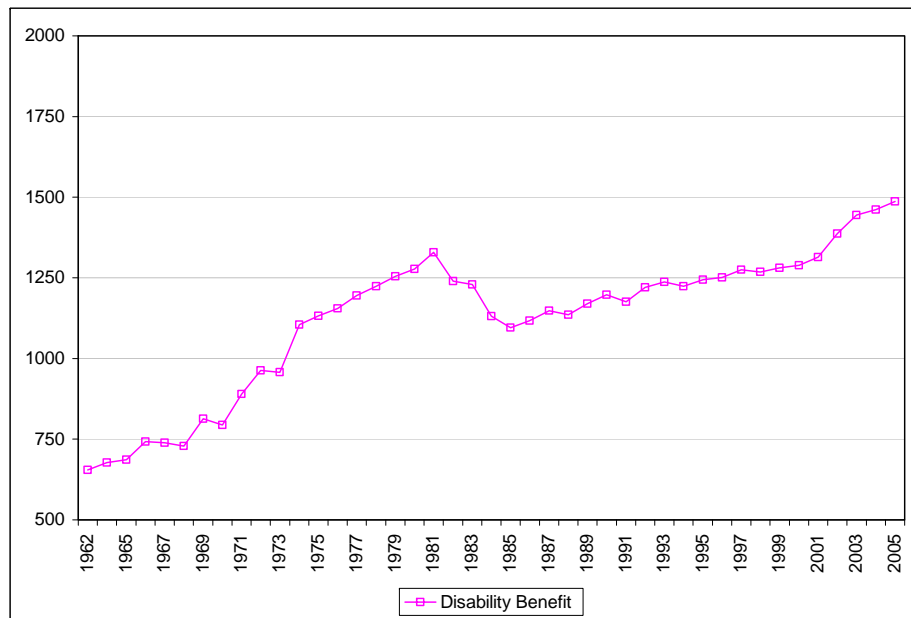


Figure 9: Average Monthly Earnings by Education (2005 \$)

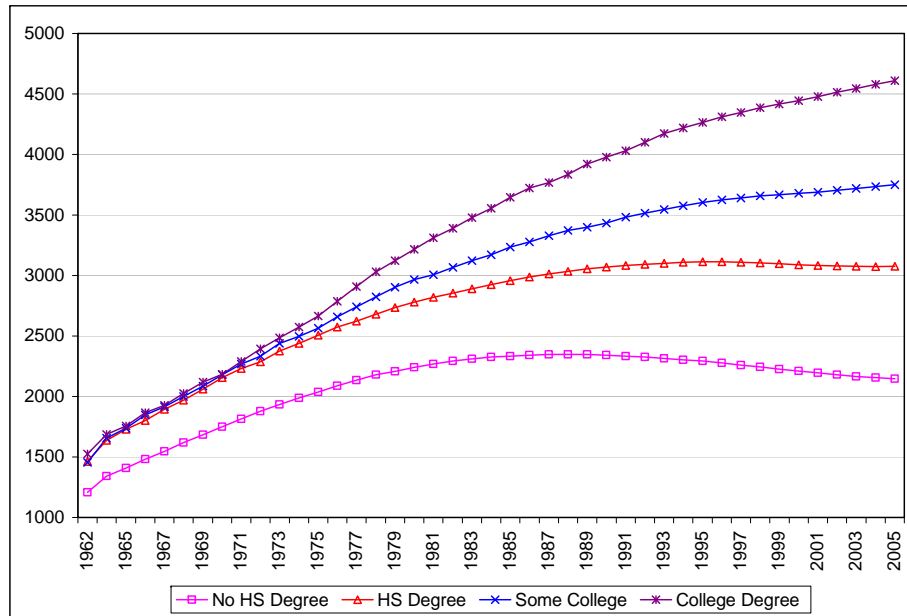


Figure 10: Male Wage by Education (in logs, 2005 \$)

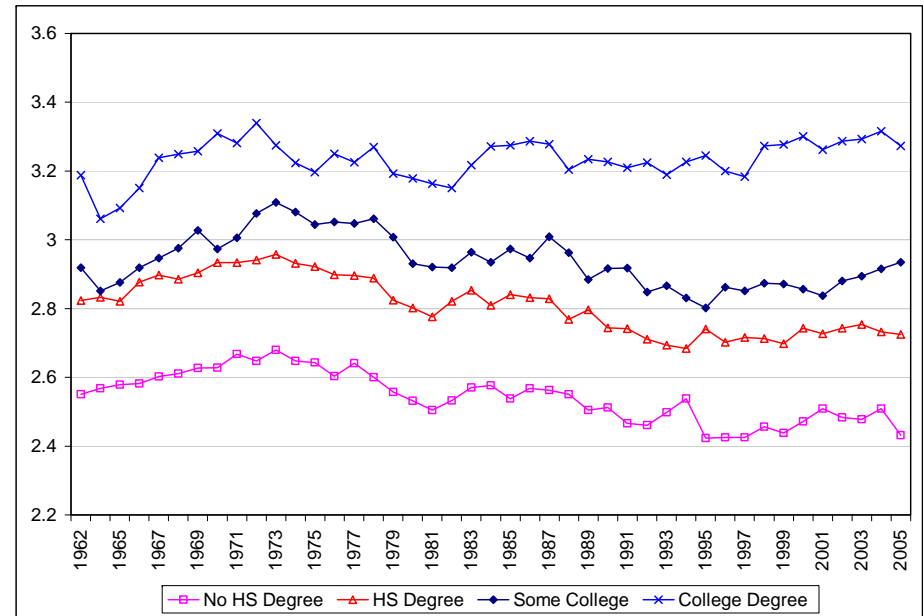


Figure 11: Spouse's Wage by Education of Husband (in logs, 2005 \$)

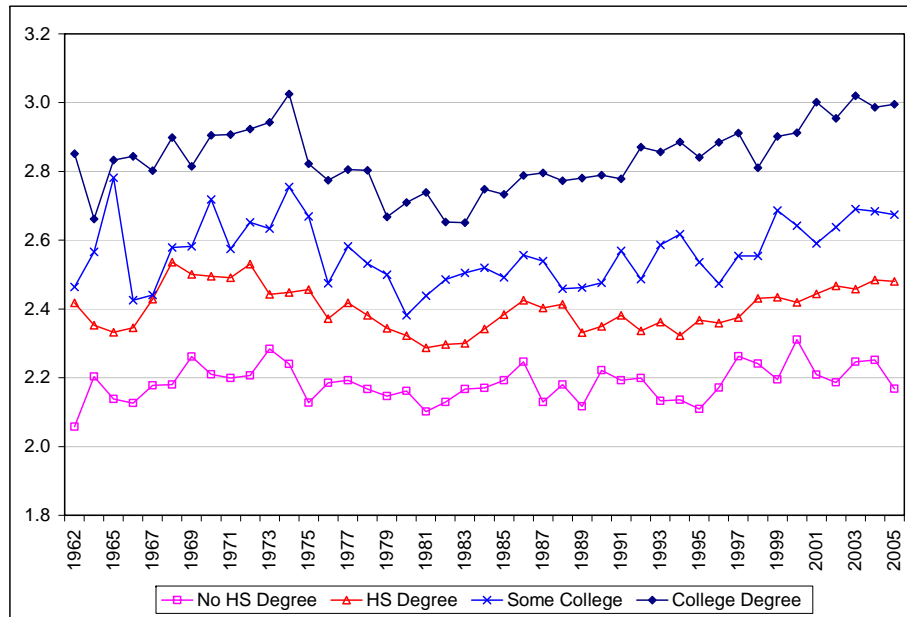


Figure 12: Pension and Retiree Health Insurance Coverage

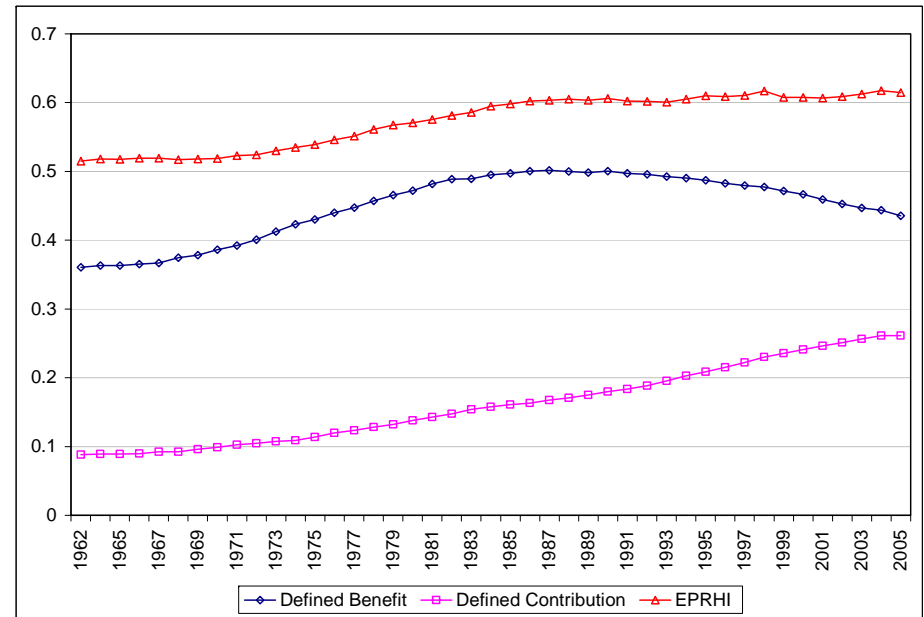


Figure 13: Actual vs Predicted LFPR by Age Group

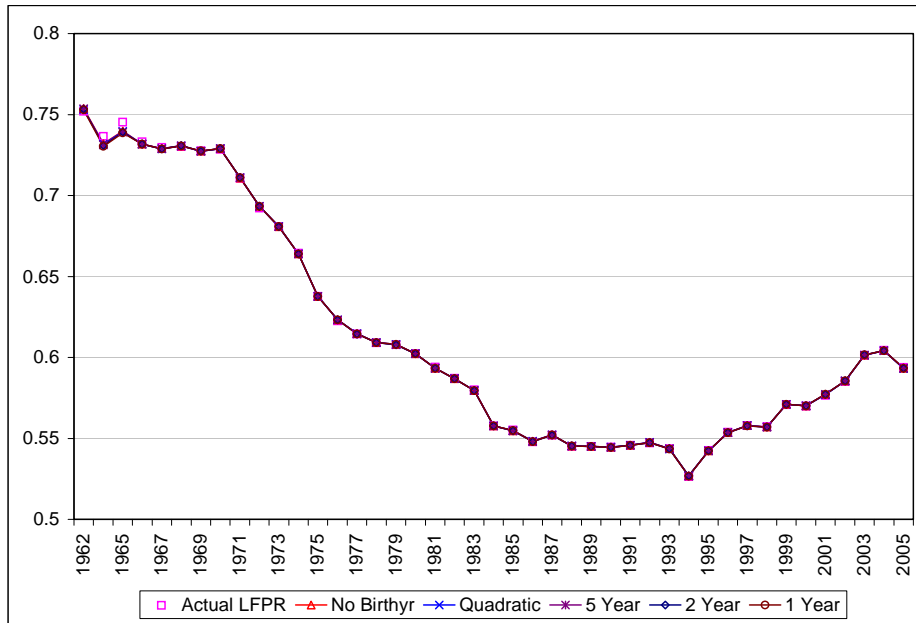
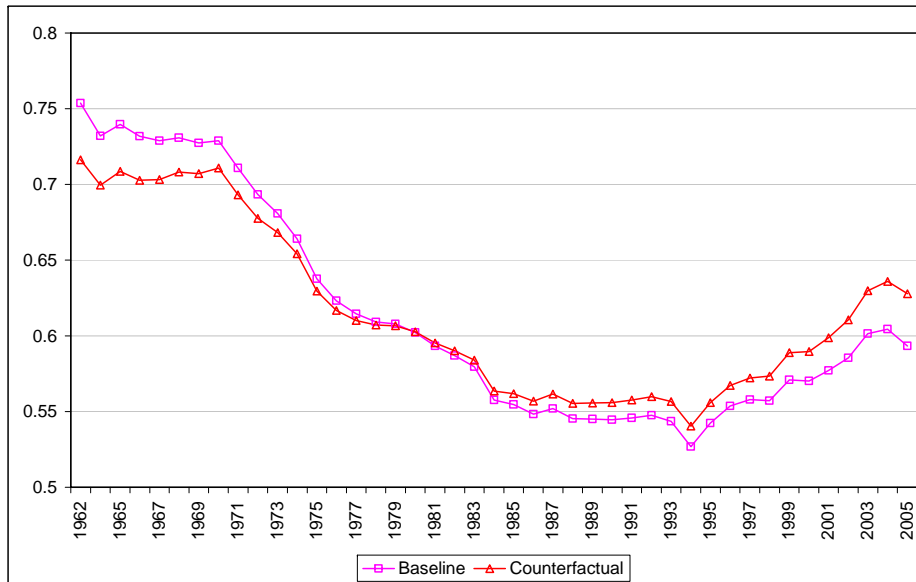


Figure 14: Downtrend Counterfactual: Social Security

(a) No Birth Cohort Effects



(b) 2 Year Birth Cohort Effects

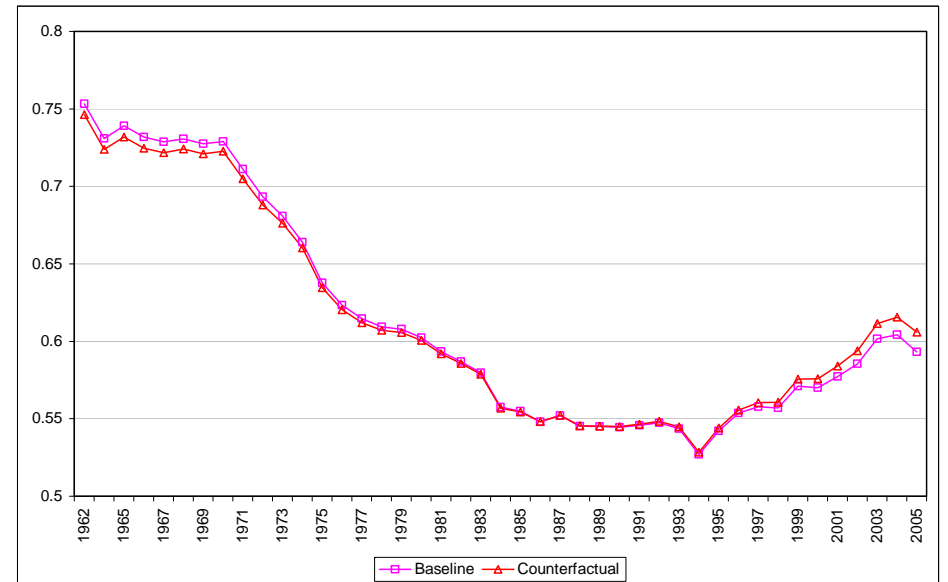
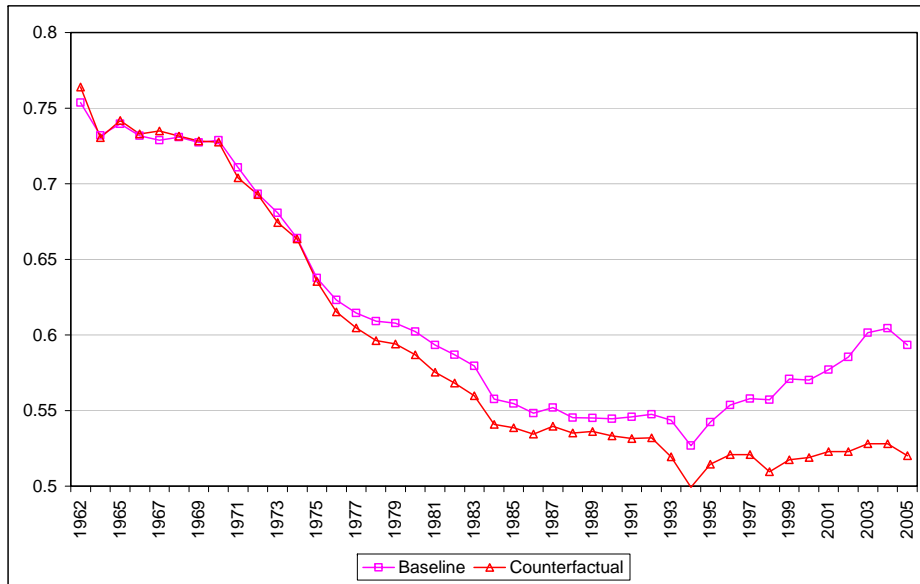


Figure 15: Uptrend Counterfactual: Educational Distribution

(a) No Birth Cohort Effects



(b) 2 Year Birth Cohort Effects

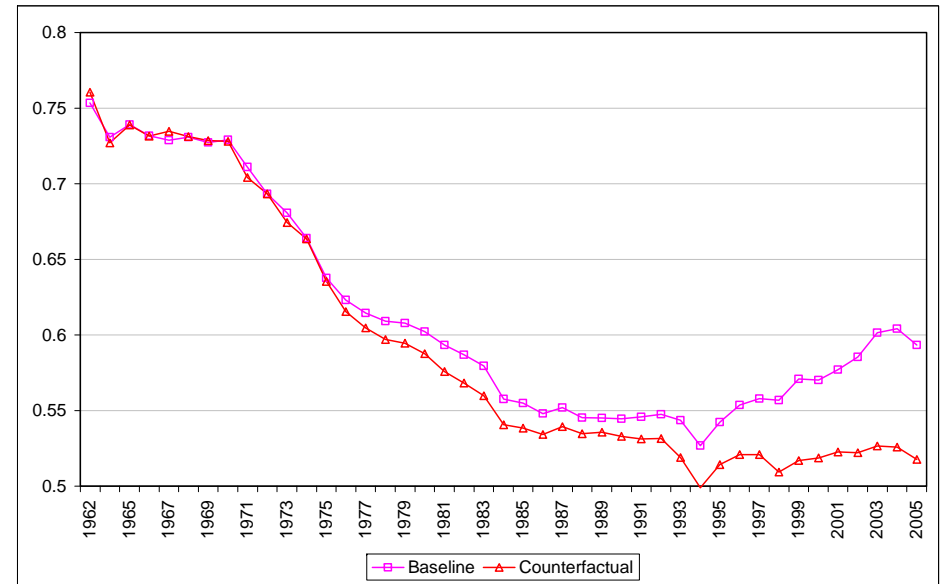
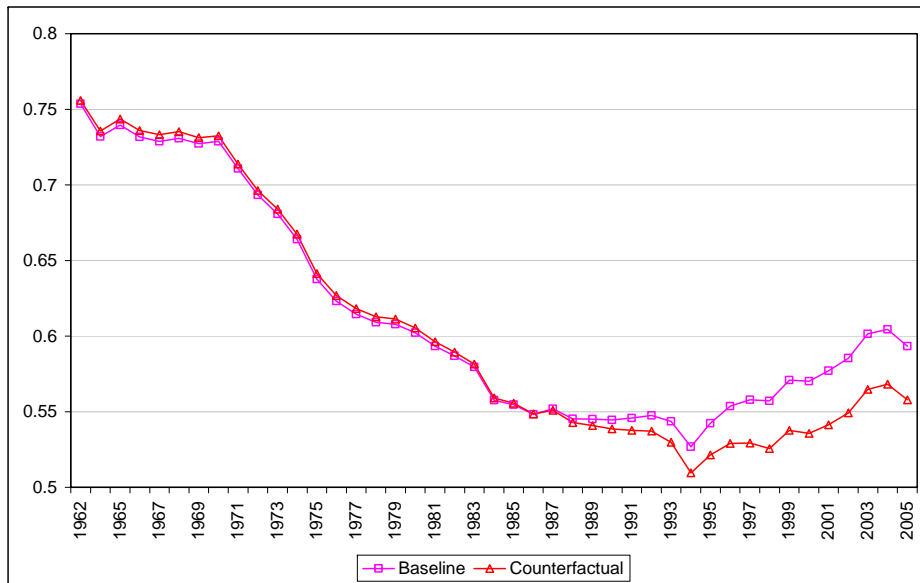


Figure 16: Uptrend Counterfactual: Social Security Delayed Retirement Credit

(a) No Birth Cohort Effects



(b) 2 Year Birth Cohort Effects

