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

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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 analyze LFP of older men, in order to determine whether changes in the rules governing Social Security benefits can explain these trends. The increasing generosity of Social Security from the 1960s through the 1970s explains only a small portion of the decline in LFP during this period. Recent increases in the Social Security Full Retirement Age and the Delayed Retirement Credit explain one quarter to one half of the recent increase in LFP. Increases in LFP of older married women and changes in the education composition of the older male population also contributed to the increase.

1. Introduction

The Labor Force Participation Rate (LFPR) of older men in the United States declined for much of the twentieth century (Costa, 1998). However, this long downward trend ended in the 1980s and in recent years the LFPR has increased for some age groups. 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 (see Figure 1). The participation rate for men aged 60 to 64 increased from 55% in 1985 to 58% in 2005. 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 assess the impact of changes in the rules governing Social Security benefits on trends in older male LFP. We also examine the role of changes in lifetime earnings, wage rates, the LFP of older married women, 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 a 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 in LFP 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 significantly for men who turned 65 beginning in 1982. The 1983 amendments increased the full retirement age from 65 to 66 in two month increments per year for birth cohorts 1938-1943, effectively reducing lifetime benefits[1]. The 1983 amendments also increased the reward for delaying entitlement past the full retirement age (the Delayed Retirement Credit) 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 full retirement age. 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 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. And we control for 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 these controls, unobserved differences across birth cohorts could give rise to spurious correlation between cohort-specific Social Security rule changes and employment trends. Therefore, to gauge the

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[1] A person born in 1943, whose full retirement age is 66, receives a benefit that is 6.67% lower if he retires at 65 than an identical individual born in 1937, whose full retirement age is 65. A further phased increase in the full retirement age from 66 to 67 is scheduled to take place from 2017-2022.

sensitivity of our results we estimate specifications with alternative controls for birth year.[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 with just a linear birth year trend, which yields the biggest effects of Social Security, implies that changes in Social Security can explain less than one fifth of the observed decline in LFP. 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 7% of the observed decline. Another 8% of the decline is accounted for by the increased attractiveness of Social Security Disability Insurance. The latter finding is quite robust. The small role of Social Security in explaining the decline in LFP is consistent with the findings of most previous studies.

Our results indicate that increases in the Social Security Full Retirement Age and Delayed Retirement Credit can account for between 25% and 50% of the recent increase in labor force participation, depending on the specification of birth year effects. The rise in labor force participation of older married women can explain 16% of the increase, and the growth in educational attainment can explain another 18%. Our estimates of the impact of changes in Social Security rules on the rise in LFP are smaller than those of Pingle (2006) and Mastrobuoni (2008). We attempt to reconcile the results of these studies with our findings.

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

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[2] As discussed below, the source of identification changes as richer birth year controls are added. Birth year, age, and calendar year are of course collinear. We specify the model with nonlinear functions of these variables , 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 calendar year effects in this specification. This is not a problem, however, since they are included only to control for otherwise unobserved factors.

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 LFP 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. Hurd and Boskin (1984) find that increases in Social Security benefits between 1970 and 1972 can 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 is unexplained. Kreuger and Pischke (1992) find that the 1977 amendments had almost no impact on LFP of older men. 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 the

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rise in lifetime income resulting from growth in 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 lifetime earnings have continued to increase in the US, at least for better educated men. The growth of wages in the 1960s and 1970s followed by stagnation more recently could have affected LFP trends as well.

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 retiree health insurance and pension coverage and type in our analysis, but our data do not have the detailed information needed to measure health insurance and pension incentives carefully. Hence, we do not make any claims about whether changes in these factors can explain trends in LFP.[3] 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 (Hurd, 1990; Blau, 1998; Gustman and Steinmeier, 2000). Schirle (2008) 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 include the LFPR of the wives of the men in our sample as an explanatory variable in order to investigate this issue.[4]

3. Empirical Model

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[3] We also assume that any unmeasured changes in pension and health insurance incentives are independent of changes in Social Security incentives.

[4] 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.

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 chooses consumption and labor force participation to maximize the expected present discounted value of remaining lifetime utility, subject to a set of constraints.[5] 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 focus on Social Security, and discuss other variables more briefly.

1. *Social Security*. 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 Social Security rules: SSB_a, $a = 62, 65, 70$, the retirement benefit an individual would receive at age a if he were to work full time in every year from the age of labor force entry through age a-1 at the mean of his

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

age-specific wage offer distribution, leave employment at age a, and never work again.[6] These

variables differ 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. In

order to isolate the effects of changes in Social Security rules from changes in lifetime earnings,

we include in the model average lifetime earnings through age 65. SSB_{65} is intended to capture

the wealth effect of Social Security (Moffitt, 1987), so we expect it to have a negative effect on

LFP.[7] SSB_{62} 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_{65}. A higher value of the

variable implies a smaller early retirement penalty, so it should have a negative effect on labor

force participation. SSB_{70} 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_{65}.[8]

A higher value implies a larger incentive to delay retirement, so it should have a positive effect

on the LFPR.[9]

We also include a measure of the Social Security disability benefit, SSB_{td}, the individual

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[6] 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 liquidity constraints are the most 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.

[7] The full retirement age is 65 for individuals born in or before 1937; $65 + x/6$ for birth years 1937+x, x=1,...,5; 66 for birth years 1943-1954; $66 + x/6$ for birth years 1954+x, x=1,...,5; and 67 for birth years 1960+. Holding claiming age constant, benefits are lower for cohorts affected by the increase in the FRA.

[8] Since the 1983 Social Security amendments, there has been no increase in the benefit for delaying retirement past age 70.

[9] It is worth noting that a standard life cycle model implies that benefits available conditional on retirement at a given age could affect LFP at other ages. Thus, for example, the benefit available conditional on exit from the labor force at age 70 may 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 as part of a sensitivity analysis.

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 FRA onward, because the SSDI benefit is converted to an OASI benefit at the FRA. This variable is intended to capture the incentive effects of SSDI benefits, and is expected to have a negative effect on LFP.[10]

   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.[11] It does not account for several other channels through which Social Security might affect retirement decisions, including 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 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 the SSET here because there is no straightforward way to measure its effect in our framework.

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[10]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.

[11] 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.

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.[12]

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. Time series variation in the payroll tax is not cohort-specific, and is picked up by calendar year effects.

2. *Pensions and health insurance*. We have data on coverage by Defined Benefit and Defined Contribution pension plans, but we do not observe the plan rules or the variables that determine benefits (job tenure, average earnings at the pension job, the DC account balance). 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 rate*. We observe the wage rate for an individual only if he chooses to work. To circumvent this problem, we use the fitted value from a birth-year-education-specific log wage

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[12]Labor 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.

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 is included in the labor force participation model.

4. *Labor Force Participation of Spouses*. We include the LFP of the wives of the men in the sample, assuming that the rapid growth in LFP of married women is independent of unobserved factors that affect labor supply decisions of husbands (conditional on various fixed effects discussed below). See Schirle (2008) for discussion of the validity of this assumption. An alternative specification based on a household labor supply model would include the wage rate of the spouse. We investigated this specification and found very little explanatory power from the wife's wage rate, consistent with the view that non-economic factors are mainly responsible for rapid growth in LFP of married women.

5. *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 potentially significant limitation of our specification, but in practice most studies of retirement have found a very small effect of net worth on the timing of retirement (e.g. Blau, 1994; Diamond and Hausman, 1984; Goodstein, 2008). In any case, most wealth accumulation results from saving out of earnings, so average lifetime earnings may pick up the effect of net worth. We discuss below the robustness of the results to controls for wealth proxies. Evidence on the effects of recent asset market bubbles and crashes on retirement is mixed but generally suggests little lasting effect on retirement behavior of these relatively short run phenomena (e.g. Gustman and Steinmeier, 2002; Coile and Levine, 2006).

As noted above, the model includes average lifetime earnings as an explanatory variable. A perfect foresight life cycle labor supply model with no borrowing constraint implies that labor

supply in period t can be expressed as a function of the period t wage offer and the marginal utility of lifetime wealth. The marginal utility of lifetime wealth is a function of wage offers in all periods. Thus we interpret average lifetime earnings as a proxy for the unobserved marginal utility of lifetime wealth. This interpretation implies that lifetime earnings should have a negative effect on labor supply. However, lifetime earnings may be correlated with factors that affect labor supply preferences, such as motivation and energy. If these factors are not well captured by observed demographic variables and birth year controls, then the coefficient estimate on lifetime earnings will pick up the effects of these omitted factors. We include lifetime earnings primarily in order to ensure that the estimated effects of Social Security are identified by rule changes rather than earnings changes, so the specific interpretation of the lifetime earnings effect is not of central importance.[13]

The model includes fixed effects for calendar year, age, and education. Social Security retirement rules vary only across birth cohorts, but lifetime earnings used to compute benefits vary across birth cohorts and education groups. If we exclude controls for birth year, we have three sources of identification of the Social Security benefit effects: changes in rules governing the Social Security benefit function, nonlinearity of the Social Security benefit function, and the interaction of the rule changes with the changing distribution of lifetime earnings by education. Changes in the Social Security benefit function provide a clean source of identification because they are exogenous, but the identifying assumption is potentially quite strong: the absence of any channels other than Social Security through which birth cohort could affect LFP. The Social

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[13] The lifetime earnings measure is the mean of monthly real earnings from ages 30 to 64. Assuming that wages grow steadily with age, these are the highest 35 years of earnings used to compute Average Indexed Monthly Earnings (AIME), the basis for calculating SS benefits. The AIME uses earnings truncated at the SS maximum taxable earnings, but the lifetime earnings measure included in the model uses untruncated earnings. The SS maximum taxable earnings is part of the rules used to compute SSB benefits so we do not want to control for it in the lifetime earnings measure.

Security rules changed at irregular intervals and in discontinuous jumps, so we can include some functions of birth cohort in the model and still rely on rule changes for identification. We try alternative birth cohort functions, such as linear and quadratic trends and four-year and two-year fixed effects, in order to determine the sensitivity of the results. If we include a full set of single-year birth cohort effects, we no longer have identification from the rule changes themselves, but only from the nonlinearity and the interaction of rule changes and differential changes in lifetime earnings across cohorts by education group. Nonlinearity is not a desirable source of identification, and the interaction effect is difficult to interpret. Thus it is feasible in principle to identify Social Security effects even with a full set of birth cohort effects, but the change in the source of identification is not an innocuous difference, and certainly affects the interpretation of the estimates.

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 for other reforms. There were eight changes to Social Security rules between 1961 and 1975, each increasing the generosity of benefits. Myopia implies that each change was expected to be the last one, which is not very plausible. The 1983 Social Security amendments mandated changes to be implemented in the distant future (beginning in the year 2000), so the assumption of myopia in this case is in practice equivalent to the assumption of perfect foresight for the affected birth cohorts. We conduct our analysis under two alternative extreme assumptions: perfect foresight and complete myopia. We cannot defend

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either assumption as appropriate for the entire period of our analysis, but we can determine how sensitive the results are to these alternative assumptions.[14]

4. Data

We estimate the econometric model on a synthetic panel data set constructed from micro data from the March 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 four education groups (high school dropout, high school graduate, some college, and college graduate). 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. The estimation sample contains observations on 2,453 cells with at least 30 observations per cell. Cells with fewer than 30 observations are dropped.[15]

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

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[14] 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. Assuming myopic expectations, we compute the SSB for a given claiming 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.

[15] 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.

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 1990s. Figure 3 shows that the downward trend through the mid 1980s was common to all of the age groups, but sharpest at ages 62 and above. And there was no increase in LFP since the 1990s for the 55-61 age group. 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.

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 6 illustrates trends in the real SSB for entitlement at ages 62, 65, and 70. SSB_{65} 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_{65} 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 FRA from 65 to 66. SSB_{70} rises relative to SSB_{65} 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 6 also shows the trend in the SSDI benefit, averaged over ages 55-64 (SSDI eligibility ceases at the FRA). The trend is generally upward, but is more irregular than the retirement benefit trend

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because benefits are age-specific, and the rules used to compute the potential benefit are the same for all awardees in a given year regardless of birth year. The "notch" induced by the 1977 benefit cut is clearly visible in this case. Figure 7 shows trends in average lifetime earnings per month by education group, and highlights the rapid growth in lifetime earnings disparity by education.

Figure 8 shows education-specific trends in the predicted log real wage rate. Real wages of older men have been stagnant or declining since the 1970s, and dispersion across education groups has increased. Note that wage stagnation in the 1970s and 1980s is consistent with rising average lifetime earnings in this period, since average lifetime earnings through the 1980s is dominated by earnings from the pre-1970 period.[16] Figure 9 shows trends in the LFPR of the wives of older men, by the husband's educational attainment. For men with at least a high school degree, LFPR of spouses has risen sharply, especially in the second half of the sample period. In contrast, there has been no increase in the LFPR among the wives of high school dropouts. See the Appendix for description of the other explanatory variables.

5. Results

A. Estimates

Coefficient estimates on the Social Security variables are shown in Table 1 for several specifications of the LFPR model (coefficient estimates on other variables are reported below). 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

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[16] Earnings inequality has increased within as well as across education groups. Our analysis relates mean LFP to mean earnings across education groups, and will not capture changes in LFP caused by changes in the earnings distribution within education groups. Defining the cells by earnings quantiles instead of education might provide a clearer view of the relationship between earnings and LFP trends. However, this approach is not feasible here because the published SSA data do not provide the full distribution of earnings.

years, and education groups. The columns differ by how birth year effects are specified.[17] The

first two specifications include linear and quadratic birth year trends, and the last three include

dummies for four-year, two-year, and one-year birth-cohort effects, respectively. All

specifications provide an excellent fit to the data, both overall and by age group. The test

statistics at the bottom of the columns indicate that the linear specification is not rejected against

the quadratic, and the four-year-birth-cohort specification in column 3 is not rejected against the

specifications with two-year and one-year birth cohort fixed effects.

As discussed above, the Social Security benefit for retirement at age 65 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 -0.166 in column 1 implies an elasticity of LFP with respect to SSB_{65}

of -0.32 at the means, compared to an elasticity of -0.15 based on the estimate in column 4. The

gain in SSB from claiming at 62 rather than at the 65 is expected to have a negative effect on

LFP. However, the coefficient estimate is positive, but very small and not significantly different

from zero in all specifications except the last. The gain in SSB from claiming at 70 rather than

65 is predicted to have a positive effect on the LFPR, and the results in all specifications except

the last confirm this. The implied elasticity based on the first column is .03. 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

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[17] We also estimated specifications in which the calendar year fixed effects were replaced by a linear or quadratic trend, or two-year and four-year fixed effects. Every less parsimonious specification of calendar year effects was rejected against the full set of single calendar year effects, for all specifications of birth year effects. However, the simulation results were very similar to those reported below, indicating that the results are not highly sensitive to the specification of time effects.

identification. The implied elasticity of the LFPR with respect to the SSDI benefit is -.12 at the means, based on the results in column 1. Average lifetime earnings (ALE) is estimated to have a positive impact on LFP in the first four columns. As discussed above, ALE could capture a wealth effect, in which case we would expect a negative sign, but it could also be correlated with omitted factors such as motivation, in which case a positive effect is possible. The estimate in column 1 implies an elasticity of LFP of 0.14.[18]

It is clear from the comparisons in Table 1 that the effects of Social Security retirement benefits are somewhat sensitive to the specification of birth cohort effects. Identification of Social Security effects in the specification with a full set of birth year fixed effects relies on variation in lifetime earnings growth across cohorts and on the non-linearity of Social Security benefit rules. The very different and counterintuitive results in this specification compared to the other four suggests that relying on variation in Social Security benefits other than from exogenous rule changes is problematic.[19] 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 specifications in the third and fourth columns, with four-year and two-year birth cohort effects, cannot be rejected against the specification in the last column provides further justification for discounting the results in the last column.

An alternative approach to identification of the Social Security effects is to drop the assumption of perfect foresight. We report results based on an extreme alternative to perfect foresight, namely complete myopia. The advantage of this assumption is that in some cases

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[18] If ALE is dropped from the model, the parameter estimates on the Social Security variables are smaller in absolute value. In this alternative specification, the Social Security variables pick up the effect of changes in both ALE and Social Security rules. Thus as noted above, it is important to control for ALE.

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

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 specifications as in Table 1, using the assumption of complete myopia to calculate benefits. The results are surprising: all three SSB retirement variables have effects that are the opposite of our expectations and very different from the results in Table 1. It is also surprising that these effects *increase* in magnitude with richer controls for birth year. These results are difficult to interpret, and may reflect the implausibility of myopic expectations for the entire sample period, as discussed above. 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.[20]

The effects of other variables are much less sensitive to the specification of birth year effects and assumptions about expectations, so in Table 3 we report coefficient estimates only for the specification from column 1 in Table 1. The wage effect is positive, as expected, and significantly different from zero. The estimate implies an elasticity of LFP with respect to the wage rate of about 0.19 at the mean LFPR of 0.608. The LFPR of spouses has a strong positive effect on LFP of their husbands, with an implied elasticity of 0.13 based on the estimate in column 1. The coefficient estimate of 0.226 is very similar to Schirle's (2008, Table 4) estimate. 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. Bad health has a large negative impact on LFP. 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 education gap in LFP is

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[20] We re-estimated the myopic specification for different subperiods, and found the same type of counterintuitive results in all subperiods.

"accounted for" in the regression mainly by the wage rate and the spouse's LFPR. Despite a 10 percentage point gap in the raw data, there is no difference in the LFPR of black and white men after controlling for the other variables in the regression.

B. Counterfactual Simulations

The main issue of interest is what explains 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. In the first counterfactual scenario, Social Security retirement rules are fixed at their 1978 values while other variables take on their actual values.[21] 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 10 shows the results of simulations based on the first and fourth specifications in Table 1. The simulated counterfactual trajectory based on the linear birth year trend is in fact somewhat flatter than the baseline trajectory, but the trajectory based on the two-year-birth-cohort-effects specification is similar to the baseline case using the actual changes in SS rules. According to these results, the decline in LFP from the early 1960s through the end of the 1980s would have been substantial 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 4, 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 4 shows that changes in Social Security retirement benefit rules can account for 16% of the decline using the

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[21] Benefits are computed for each cohort as if they turn 62 in 1978 (birth year 1916), but using their actual earnings history. This captures the effect of rule changes while holding earnings constant.

linear-birth-year-trend specification, but only 6-7% using the two-year or four-year birth-cohort-effects specification. Changes in average lifetime earnings did not contribute at all to the downward trend in LFP: the simulations indicate that if average lifetime earnings had remained constant at the 1966-1970 level, the downward trend would have been even stronger than the observed trend.[22] 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.[23]

Counterfactual simulation results for the other explanatory variables were very similar across all of the different specifications, so results are shown in Table 5 only for the specification with a linear control for birth cohort. Changes in other variables can account for only 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 6 shows that the LFPR increased by 4.7 percentage points between 1988-92 and 2001-05, and our models predict almost exactly this increase given the observed changes in the explanatory variables. Here, we find evidence that Social Security rules matter: the increases in the DRC and NRA account for between a quarter and a half of the increase in LFP. Other factors matter as well: increases in ALE can explain 7-13% of the increase. Rising labor force participation of married women can explain 15-18% of the increase. And the changing educational composition of the labor force can explain 16-18% of the increase. Changes in other variables either go in the wrong direction (health, year and birth year) or have no explanatory power (not shown). Overall, these results suggest that Social Security is the single most important factor in accounting for rising LFP of older men, but not the only factor. The Social Security results are somewhat sensitive to the specification of birth cohort effects, but they

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[22] The results for Social Security are qualitatively similar if average lifetime earnings are dropped from the model.
[23] 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.

account for at least one quarter of the rise in LFP in all specifications.

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 substantially larger for men with high school education or less compared to men with at least some college. The effect of rising LFP of married women is also stronger for the less educated group. The most notable difference in the estimates across age groups (55-61, 62-64, 65-66, and 67-69) is a much larger negative effect of SSB_{65} at ages 65-66 than at other ages. Simulations show that increases in the DRC and FRA can explain much more of the rise in LFP at ages 65-66 than at 62-64 or 67-69.[24] Estimating models separately for different periods (1962-1990, 1991-2005) did not change any of the main simulation results.

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 from the 1960s to the 1980s. This finding is consistent with the results of Moffitt (1987), using time series data; Krueger and Pischke (1992) and Peracchi and Welch (1994), using synthetic panel data like ours; and Burtless (1986) and Blau (1994), 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 point 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

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[24] LFPR at ages 55-61 did not increase over this period.

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 can explain only 10% of the decrease in LFP during this

period. This result is robust to alternative controls for birth year.[25] Thus the results are closer to

the findings of Krueger and Pischke than to those 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 12% 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

25% of the decline in the specification with four-year birth cohort effects, and 17% in the other

specifications.[26] These are smaller than the effects found by Stewart.

Pingle (2006) uses micro data from the SIPP for 1983-2003 to estimate the effect 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 caused the employment rate of men aged 65-

70 to increase 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 results in his paper to infer

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[25] Estimates with two-year and one-year birth year effects yielded implausible results, indicating lack of
identification. This is not surprising given that there are only 90 observations (9 years and 10 birth cohorts). The
specification of our model is different from the specification in 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.
[26] There are only 84 observations at ages 62-64 and 140 at ages 65-69 in this analysis, so two-year birth cohort
effects were not well-determined.

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 shown in Table 6 above, our estimates indicate that the increase in the

DRC can explain 15-28% of the observed increase in LFP depending on the specification of 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 indicate that the increase in the DRC can explain 31-38% of the observed increase in

LFP of men aged 65-69. This is slightly larger than the 15-28% effect reported in Table 6 above,

but is still considerably smaller than Pingle's estimate. The specifications estimated here and in

Pingle's analysis are not nested, so the evidence is not definitive on the effects of the DRC.

Mastrobuoni (2008) uses micro data from the CPS to estimate the impact on LFP of the

increase in the FRA mandated by the 1983 Social Security Amendments.[27] He finds that for

each two month increase in the FRA, the age of retirement (derived from a LFP model) rose by

one month. Our results discussed above indicated that the increase in the FRA can explain 11-

23% of the observed increase in LFP at older ages. Direct comparisons of these results are

difficult because of the differences in specifications. Mastrobuoni's estimate is derived from a

non-parametric "treatment effects" specification in which single-year age dummies are interacted

with single-year birth cohort dummies. He infers the effect of changes in the FRA by comparing

LFP at each age for "treated" cohorts (birth years 1938-1941) and "control" cohorts (birth years

1937 and earlier, with FRA of 65). In contrast, our estimate is derived from a parameterized

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[27] Duggan et al. (2007) investigate the effect of the increase in the FRA on SSDI enrollment, but they do not
examine the effect on LFP. Pingle (2006) finds a positive effect of the increase in the FRA 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
FRA is estimated to increase employment by 6-13 percentage points (see his Table 6).

model in which the impact of changes in the FRA is restricted to operate through the implied changes in Social Security benefits at given claiming ages. Mastrobuoni suggests that changes in the FRA may operate through non-economic channels, such as changes in social norms or the "focal" retirement age. His specification would pick up such effects while ours would not.[28]

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 results were very similar using all approaches. We used an after-tax measure of the wage rate, described in the Appendix, and again found very similar 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, 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.[29] To address this possibility, we included interactions between the

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[28] 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.

[29] We thank Mark Duggan for pointing this out to us.

education dummies and a linear time trend. The simulated effects of Social Security changes were similar to the main results, but the effect of the change in the educational composition is much larger. The results imply that the changing educational composition of the older male labor force can account for 100% of the increase in LFP. This suggests that our results attributing a relatively small role to changing education composition may not be robust. Similarly, the assumption that the age effects are fixed may be suspect, given that the relative magnitudes of the age-62 and age-65 spikes have changed over time. To examine this, we estimated an alternative specification in which the full set of age dummies was interacted with a quadratic time trend. The simulated effects of Social Security on both the downward and upward trends were slightly smaller in this specification.

Finally, our model omits household net worth. In order to determine whether the results are sensitive to this omission, we added two additional variables to the model: the capitalized value of income from interest, dividends, and rent (using a real return of 4**%**), and 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. And Social Security effects were not sensitive to the inclusion of these proxies for wealth.

6. Conclusions

The evidence reported here indicates that changes in Social Security were not a major cause of the decline in older male LFP from the 1960s through the 1980s. The estimates imply

that these changes can explain at most 16% of the observed decline. Changes in rules that

determine SSDI benefits can explain another 7% of the decline. Our findings are consistent with

those of most previous studies of the role of Social Security in explaining the decline in LFP.

Since the early 1980s, Social Security rule changes have favored increased LFP. The rise

in the DRC and the FRA are estimated to account for one quarter to one half of the increase in

LFP of older men since the 1980s. This amounts to a 1.2 to 2.4 percentage point increase in the

LFPR of men aged 55-69.   Rising LFP of married women and changes in the educational

composition of the older male population contributed to the increase as well.

An important question is whether we can expect further increases in LFP at older ages. A

definitive prediction is not possible, but we speculate that the main driving forces behind the

recent increase in older male LFP will soon be played out. The DRC reaches its maximum

scheduled rate of 8% per year for the 1943 birth cohort, and the FRA reaches its maximum of 66

for this cohort as well (although further increases are scheduled beginning with the 1954 birth

cohort). The 1943 birth cohort reached age 65 in 2008, suggesting that the effects of the

increases in the DRC and FRA will peak soon. The rapid replacement of low-participating high

school dropouts by higher-participating college attendees and college graduates has been

responsible for some of the recent increase in LFP. However the proportion of high school

dropouts in the male population aged 55-69 was 0.16 in 2005, very similar to the proportion of

0.15 in the male population aged 18-58, suggesting little scope for further increases from this

source. On the other hand, the LFPR of the wives of men aged 55-69 was about 20 percentage

points lower than the LFPR of their husbands in the first half of the 2000s (compare Figures 2

and 9), suggesting considerable scope for further increases from this source. On balance, these

trends suggest a slowdown in the rate of growth of LFP of older men in the absence of policy

changes such as advancing implementation of the scheduled increase in the FRA from 66 to 67.

Two key points remain unresolved by the findings reported here: what caused the long decline in LFP of older men, and why is Social Security more important in accounting for the recent LFP increases than in explaining the previous decline? The first question has been studied for many years without much success, and unfortunately our results do not suggest any new avenues of research. The second question raises the possibility that the form in which Social Security benefits are changed may be important beyond the magnitude of the change. Changes in the FRA may signal to workers that the government is advising them to change their behavior. Social Security statements sent to workers use the FRA as a reference point and explicitly refer to retiring early (before the FRA) and late (after the FRA). Until the 1980s there was a sharp kink in the benefit function at the FRA, but the increase in the DRC has eliminated the kink. Nevertheless, the FRA continues to be a focal age for claiming benefits (Song and Manchester, 2008) and for leaving the labor force (Mastrobuoni, 2008) as it shifts in two month increments. This suggests that further investigation of the role of the FRA in retirement decisions could be worthwhile.

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.[30] 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

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

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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. [31]

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

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[31] 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.[32]

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.[33] 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

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[32] For cohorts who reach ages 58 to 70 in future years we assume that nominal earnings grow at 3% annually.
[33] 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 $ER_{be} = \sum_{j=0}^{3} \lambda_{ej} b^j + \mu_{be}$.[34] 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 then 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 65, or 70.

We construct benefits under two alternative assumptions about future rule changes:

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[34] We omit cells with sample size less than 30 from this regression.

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 case[35]. 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 full 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 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

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[35] The ANYPIA program is available on the Social Security Website at
http://www.ssa.gov/OACT/ANYPIA/anypia.html

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, 65, 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

FRA, we set SSDI to 0 at ages greater than or equal to the FRA.   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

The log hourly wage rate is constructed from CPS data. 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 the wage for those in our sample who choose to work. Therefore we replace the observed log wage with a predicted log wage 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 Research[36]. 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 – MFTR – (OASDI+HI)).

A4. Health

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.

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[36] See http://www.nber.org/~taxsim/taxsim-calc7/index.html

We also measured 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.

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Table 1: Selected Estimates from the Labor Force Participation Model, Assuming Perfect Foresight

| | Birth Year Specification | | | | |
	Linear	Quadratic	4 Year	2 Year	1 Year
SSB65	-0.166 ***	-0.155 ***	-0.080 **	-0.078	0.171 *
	(0.022)	(0.025)	(0.041)	(0.053)	(0.098)
SSB62 - SSB65	0.001	0.010	0.012	0.017	0.346 *
	(0.018)	(0.021)	(0.039)	(0.058)	(0.181)
SSB70 - SSB65	0.058 ***	0.048 ***	0.052 *	0.032	-0.063
	(0.015)	(0.019)	(0.027)	(0.034)	(0.062)
Disability Benefit	-0.063 ***	-0.066 ***	-0.071 ***	-0.070 ***	-0.071 ***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
ALE	0.032 ***	0.032 ***	0.017 *	0.021 *	-0.019
	(0.008)	(0.008)	(0.010)	(0.011)	(0.017)
Birth Year	-0.006 ***	-0.070			
	(0.000)	(0.069)			
Birth Year Squared		0.002			
		(0.002)			
R Squared	0.963	0.963	0.964	0.965	0.965
P-Values					
SS Benefits	0.000	0.000	0.078	0.177	0.350
Specification	-	0.346	-	0.081	0.442

Notes: OLS estimates, weighted by cell size, 2453 observations. All specifications include age and calendar year dummies. * indicates that the estimate is significantly different from zero at the 10% level (** at the 5% level; *** at the 1% level). SSB_{65} is the monthly Social Security benefit available at age 65, and similarly for SSB_{62} and SSB_{70}. ALE is average lifetime earnings per month. Social Security benefits and ALE 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 ALE are all equal to zero. "P-Value: Specification" is from an F test against the specification in the previous column.

Table 2: Selected Estimates from the Labor Force Participation Model, Assuming Myopic Expectations

| | Birth Year Specification | | | | |
	Linear	Quadratic	4 Year	2 Year	1 Year
SSB65	0.154 ***	0.150 ***	0.241 ***	0.254 ***	0.278 ***
	(0.017)	(0.017)	(0.020)	(0.020)	(0.020)
SSB62 - SSB65	0.102 ***	0.110 ***	0.258 ***	0.292 ***	0.315 ***
	(0.018)	(0.018)	(0.026)	(0.029)	(0.031)
SSB70 - SSB65	-0.060 ***	-0.069 ***	-0.063 ***	-0.075 ***	-0.104 ***
	(0.011)	(0.012)	(0.015)	(0.019)	(0.023)
Disability Benefit	-0.044 ***	-0.048 ***	-0.065 ***	-0.062 ***	-0.060 ***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
ALE	-0.043 ***	-0.038 ***	-0.038 ***	-0.034 ***	-0.040 ***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Birth Year	-0.008 ***	-0.121 *			
	(0.000)	(0.071)			
Birth Year Squared		0.003			
		(0.002)			
R Squared	0.963	0.963	0.966	0.967	0.968
P-Values					
SS Benefits	0.000	0.000	0.000	0.000	0.000
Specification	-	0.111	-	0.001	0.004

Notes: See Table 1.

Table 3: Additional Estimates from the Labor Force Participation Model, Assuming Perfect Foresight

	Linear Birth Year Specification
Ln(Predicted Wage)	0.113 ***
	(0.020)
Spouse's LFP	0.226 ***
	(0.023)
DB Pension	-0.002
	(0.023)
DC Pension	0.028
	(0.041)
EPRHI	0.007
	(0.021)
Bad Health	-0.297 ***
	(0.029)
Married	0.101 **
	(0.042)
Previously Married	0.151 ***
	(0.047)
High School	0.023 ***
	(0.007)
Some College	0.035 ***
	(0.009)
College	0.057 ***
	(0.014)
Black	-0.005
	(0.037)
Constant	11.773 ***
	(0.737)

Notes: The estimates are from the specification in the first column of Table 1. DB = Defined Benefit, DC = Defined Contribution, EPRHI = Employer-Provided Retiree Health Insurance.

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

(a) Linear 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	70.2	71.4	72.9	72.5
1988 to 1992	54.6	54.6	54.7	54.4	50.5	71.4
Decrease	-18.4	-18.4	-15.5	-17.0	-22.4	-1.1
Difference			-2.8	-1.3	4.1	-17.3
% of Decrease			16%	7%	-22%	94%

(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	70.2	71.4	72.9	72.4
1988 to 1992	54.6	54.6	54.5	54.4	50.5	71.6
Decrease	-18.4	-18.4	-15.7	-17.0	-22.5	-0.9
Difference			-2.7	-1.4	4.1	-17.5
% of Decrease			14%	8%	-22%	95%

(c) 4 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	72.9	72.1	71.2	72.9	72.3
1988 to 1992	54.6	54.6	54.9	54.4	52.4	71.7
Decrease	-18.4	-18.4	-17.3	-16.9	-20.5	-0.6
Difference			-1.1	-1.5	2.1	-17.7
% of Decrease			6%	8%	-12%	97%

(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	72.9	71.7	71.3	72.9	72.2
1988 to 1992	54.6	54.6	54.6	54.4	51.8	72.0
Decrease	-18.4	-18.4	-17.2	-16.9	-21.1	-0.2
Difference			-1.2	-1.5	2.7	-18.2
% of Decrease			7%	8%	-15%	99%

Notes: Counterfactual values for SSB_{65}, 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 ALE between 1966 and 1970 to every cohort. The calendar year counterfactual value is 1968; the birth year counterfactual value is 1906.

The simulations in the last four columns show the predicted LFPR generated by replacing the observed value of the indicated variable with its counterfactual value, while other variables take on their observed values.

Table 5: Additional Counterfactual Simulations to Explain the Decline in Labor Force Participation, 1966-70 to 1988-92

	Actual LFPR	Predicted LFPR	Own Wage	Spouse's LFP	Pensions	EPRHI	Health	Marital Status	Education	Race
1966 to 1970	73.0	72.9	72.9	72.9	72.9	72.9	72.9	72.9	72.9	72.9
1988 to 1992	54.6	54.6	55.5	53.8	54.4	54.5	54.3	54.3	53.4	54.6
Decrease	-18.4	-18.4	-17.4	-19.1	-18.6	-18.4	-18.6	-18.7	-19.5	-18.3
Difference			-0.9	0.7	0.2	0.0	0.2	0.3	1.2	0.0
% of Decrease			5%	-4%	-1%	0%	-1%	-2%	-6%	0%

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 a linear control for birth cohort) where the variable of interest is replaced by its counterfactual value, and all other values take on their actual values.

Table 6: Selected Counterfactual Simulations to Explain the Increase in Labor Force Participation, 1988-92 to 2001-05

(a) Linear Birth Cohort Effects

	Actual LFP	Predicted LFP	SS Rules: DRC	SS Rules: FRA	Average Lifetime Earnings	Spouse's LFP	Education
1988 to 1992	54.6	54.6	54.2	54.6	54.6	54.5	54.6
2001 to 2005	59.2	59.2	57.6	58.2	58.6	58.4	58.4
Increase	4.7	4.6	3.3	3.6	4.1	3.8	3.9
Difference			1.3	1.1	0.6	0.8	0.8
% of Increase			28%	23%	13%	18%	17%

(b) Quadratic Birth Cohort Effects

	Actual LFP	Predicted LFP	SS Rules: DRC	SS Rules: FRA	Average Lifetime Earnings	Spouse's LFP	Education
1988 to 1992	54.6	54.6	54.3	54.6	54.6	54.5	54.6
2001 to 2005	59.2	59.2	57.8	58.2	58.6	58.4	58.5
Increase	4.7	4.6	3.6	3.6	4.1	3.8	3.9
Difference			1.1	1.0	0.6	0.8	0.7
% of Increase			23%	22%	13%	17%	16%

(c) 4 Year Birth Cohort Effects

	Actual LFP	Predicted LFP	SS Rules: DRC	SS Rules: FRA	Average Lifetime Earnings	Spouse's LFP	Education
1988 to 1992	54.6	54.6	54.2	54.6	54.6	54.5	54.6
2001 to 2005	59.2	59.2	57.8	58.8	58.9	58.5	58.4
Increase	4.7	4.7	3.6	4.2	4.4	3.9	3.9
Difference			1.1	0.5	0.3	0.7	0.8
% of Increase			24%	11%	7%	16%	18%

(d) 2 Year Birth Cohort Effects

	Actual LFP	Predicted LFP	SS Rules: DRC	SS Rules: FRA	Average Lifetime Earnings	Spouse's LFP	Education
1988 to 1992	54.6	54.6	54.4	54.6	54.6	54.5	54.6
2001 to 2005	59.2	59.2	58.3	58.7	58.8	58.5	58.5
Increase	4.7	4.7	4.0	4.1	4.3	4.0	3.9
Difference			0.7	0.5	0.4	0.7	0.8
% of Increase			15%	11%	8%	15%	17%

Notes: For the Delayed Retirement Credit (DRC) counterfactual, SSB_{65} and SSB_{62} are computed for each cohort using their actual earnings history and actual Social Security rules. SSB_{70} is computed using SSB_{65} and the DRC rules in place prior to the 1983 amendments. For the Full Retirement Age (FRA) counterfactual, SSB_{65}, SSB_{62}, and SSB_{70} are computed using actual earnings histories and counterfactual Social Security rules in which the FRA 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. Simulation results for other variables are available upon request.

Figure 1: Labor Force Participation Rate Trends by Age Group
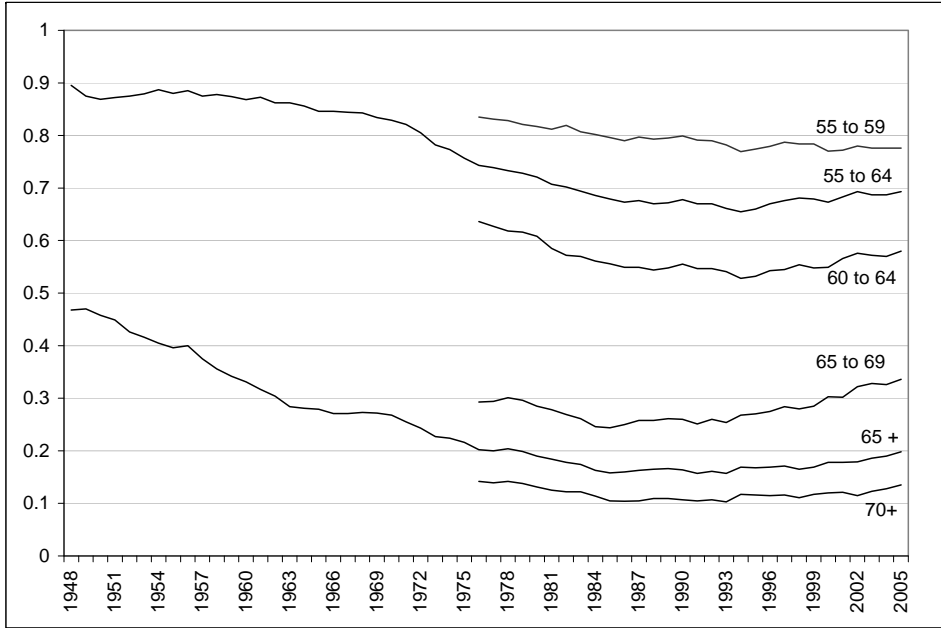(Source: Bureau of Labor Statistics)

Figure 2: Labor Force Participation Rate of Men Aged 55 to 69
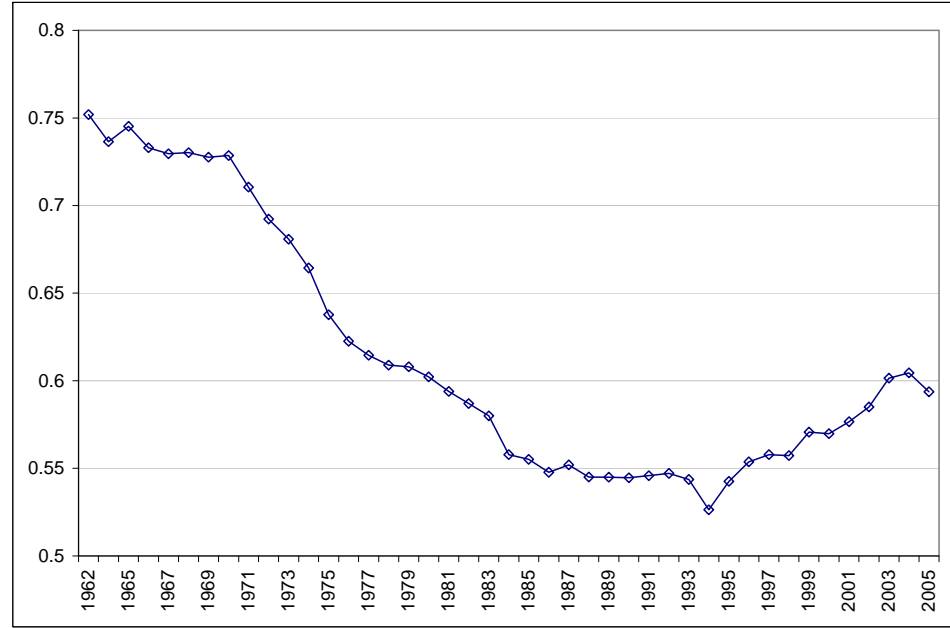

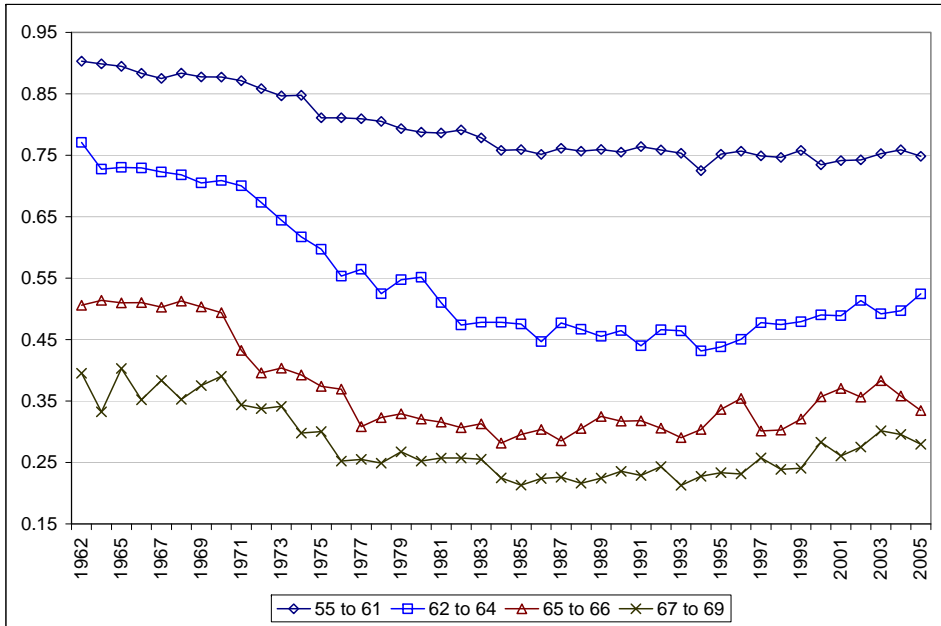Figure 3: Labor Force Participation Rate By Age Group

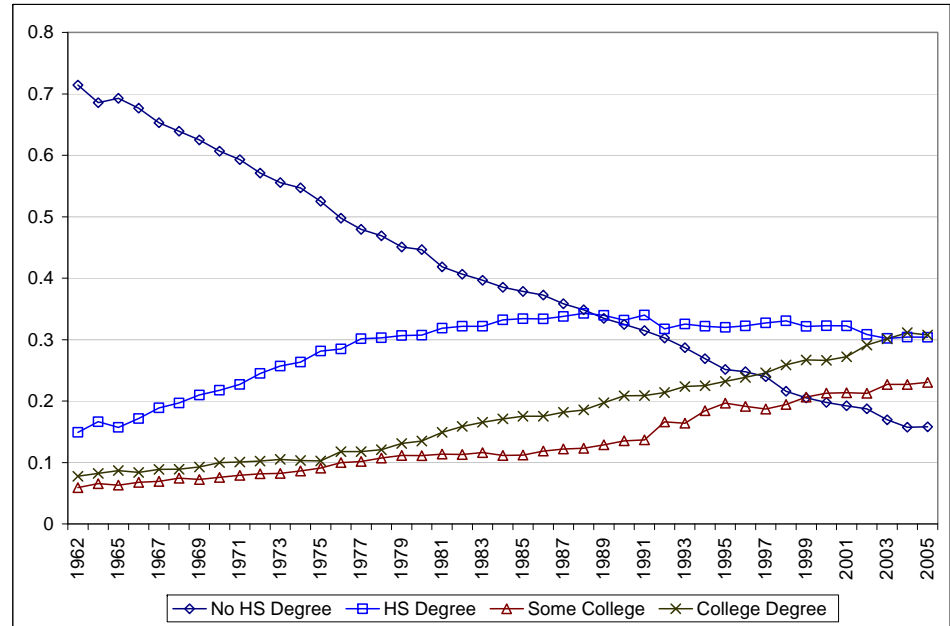Figure 4: Educational Distribution
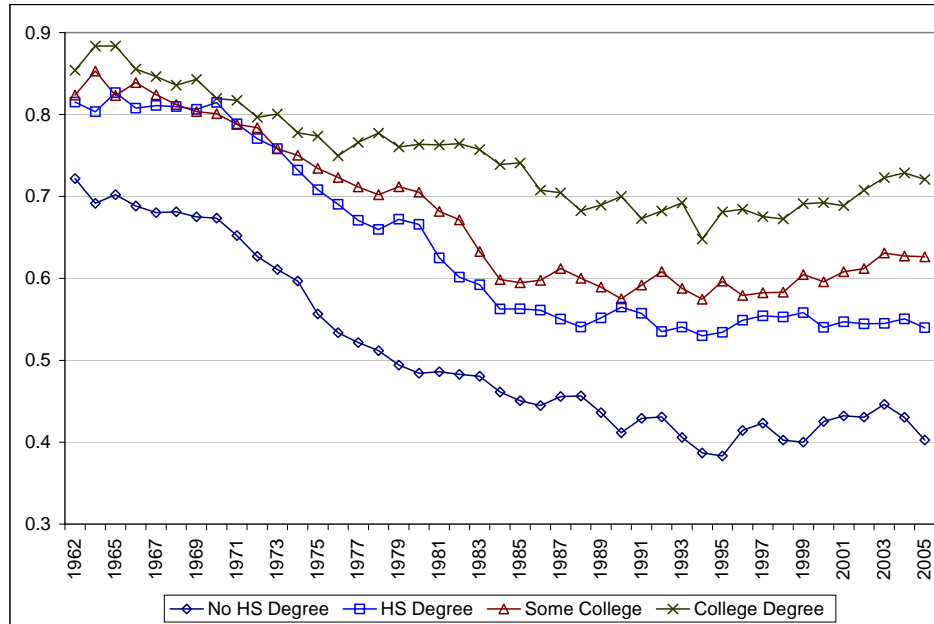
Figure 5: Labor Force Participation Rate by Education
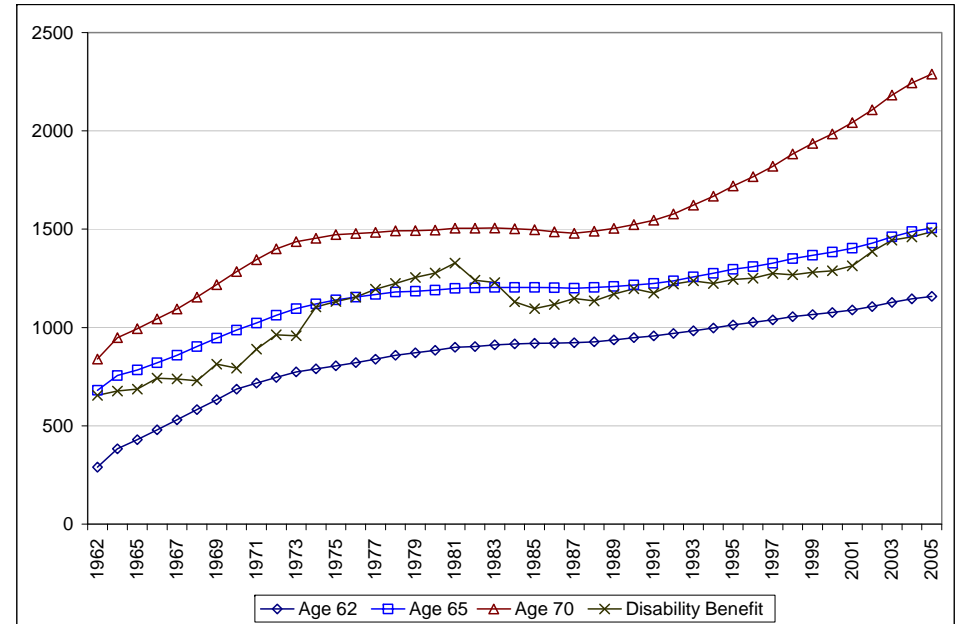

Figure 6: Monthly Social Security Benefit (2005 $)

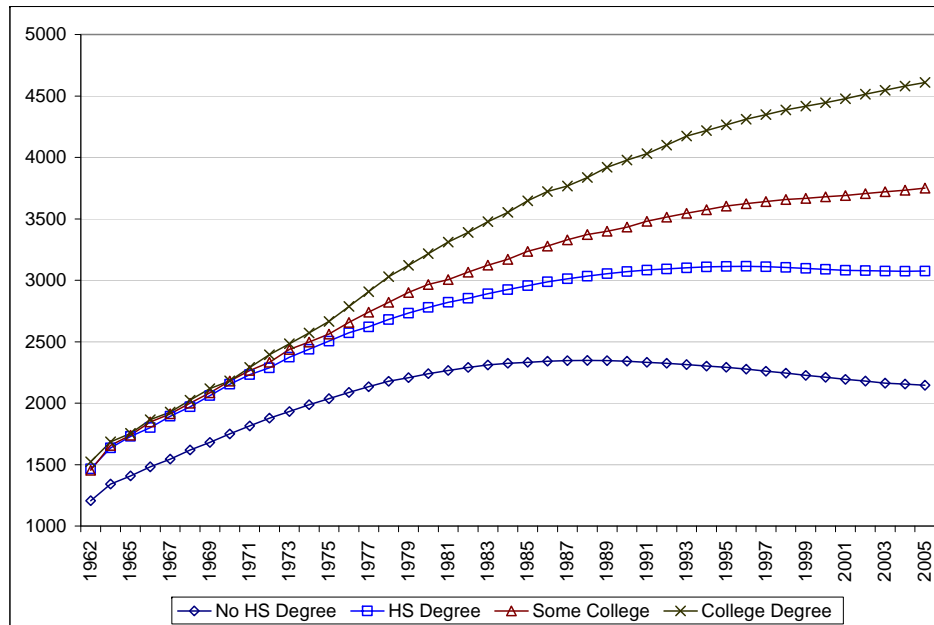Figure 7: Average Lifetime Earnings per Month by Education (2005 $)

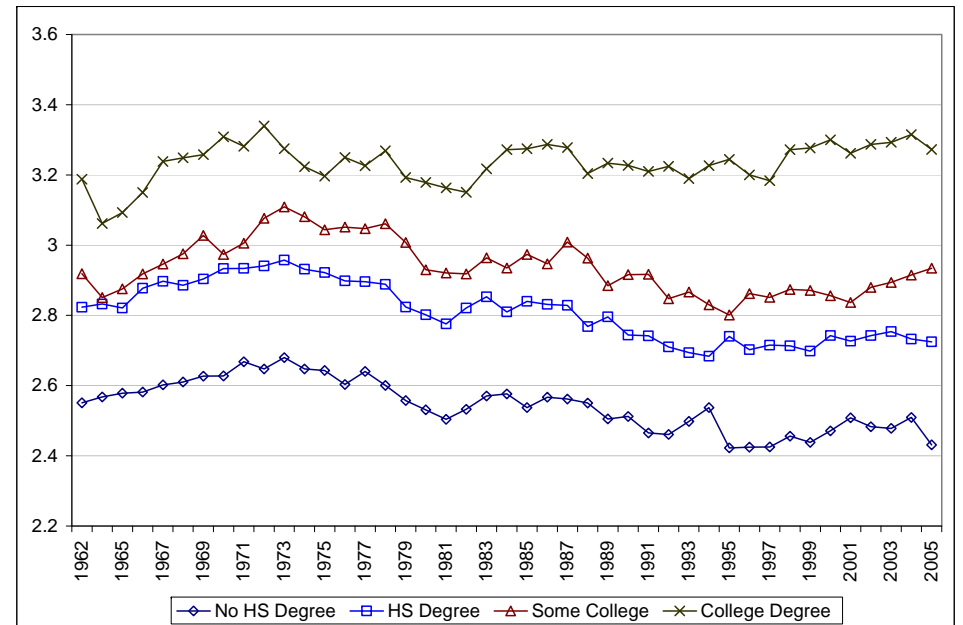Figure 8: Predicted Wage Rate by Education (in logs, 2005 $)

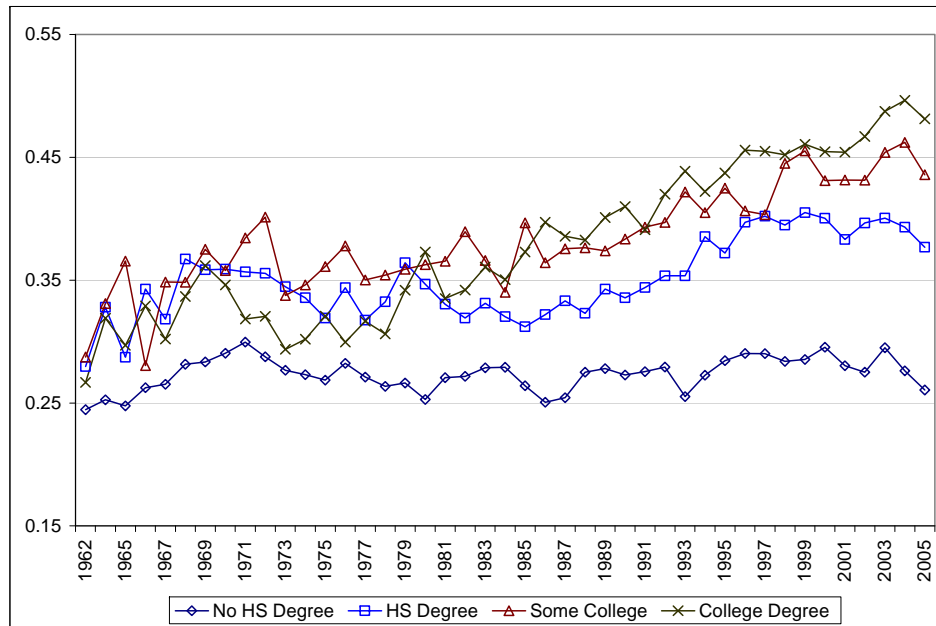Figure 9: Spouse's Labor Force Participation Rate by Education

Figure 10: Counterfactual Simulation of the Effect of Social Security on the Decline in Labor Force Participation, 1966-70 to1988-2