Employment Trends by Age in the United States

Why Are Older Workers Different?

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ABSTRACT

In the 1960s, 1970s, and 1980s, male employment rates were declining or flat at all ages, and female employment rates were rising or flat at all ages. But employment trends diverged more recently, with employment rising at older ages and falling at younger ages. We estimate labor supply models for men and women, allowing differences in behavior across age groups. The results indicate that changes in the educational composition of the population, the increase in age at first marriage, and Social Security reforms can account for a modest proportion of the divergence. However, much of the divergence remains unexplained.

I. Introduction

Perrachi and Welch (1994) summarize empirical findings from their analysis of U.S. labor force behavior from the 1960s through the 1980s as indicating that ". . . the forces shaping employment for younger men do not appear to be fundamentally different from the forces determining the participation behavior of the oldest." (p. 238). On the basis of this interpretation of the evidence they argue that ". . . [T]he search for explanations of trends in the labor force behavior of older people should primarily emphasize the larger question surrounding participation in general, and only secondarily should the peculiarities of advancing age be addressed. . . . [W]e

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believe that the retirement literature is too specialized. Obviously, old age has its distinguishing aspects, but it seems that the major trends in the data cannot be attributed to them." (p. 212).

These recommendations may seem strange to researchers who study labor force behavior at older ages. Younger workers rarely withdraw permanently from the labor force, but the great majority of workers do exactly this at older ages.[1] Older workers often leave the labor force around the time at which they become eligible for Old Age and Survivors Insurance (OASI) benefits from Social Security, benefits from an employer-provided pension, or health insurance coverage from Medicare. Obviously, these institutions were created precisely to deal with the "distinguishing aspects" of old age, and there is abundant evidence that labor force behavior is influenced by these programs.

Nevertheless, the trends in employment documented by Perrachi and Welch did in fact show many similarities across age groups. Figures 1a (for men) and 1b (for women) present our replication of a graph in their paper (Figure 7) illustrating trends in full-time equivalent weeks worked per year (divided by 52), using data from the 1966–90 March supplements to the Current Population Survey. The trends are easily summarized: Male employment generally declined until the 1980s, with larger drops at older ages. Female employment generally increased, with larger increases at younger ages. Female employment at older ages began to increase in the 1980s. The levels and rates of change differ by age group, but the trends are mainly in the same direction: down (or flat) for men, up (or flat) for women. These data do not prove that common forces have shaped employment trends across the life cycle, but they demonstrate that there were common trends in employment by age that in principle *could* be explained by broad economic, demographic, and social forces without resorting to age-specific explanations.

There are at least two specific arguments as to why a common-forces explanation is plausible. First, while OASI and Medicare are intended deal with the distinguishing aspects of old age, there is another social insurance program intended to deal with the distinguishing aspects of younger ages: Social Security Disability Insurance (SSDI). SSDI is an increasingly important source of support for individuals who are deemed unable to work and are not yet eligible for retirement benefits. Furthermore, SSDI beneficiaries become eligible for Medicare coverage after two years of benefit receipt. This suggests that the institutional environment facing older and younger workers may not be as different as one might think at first glance.

Second, PW and others have pointed out that the characteristics of older workers who tend to retire relatively early are similar to those of younger workers who withdraw from the labor force: poor health, low education, black, and, for men, unmarried. Not coincidentally, these characteristics are associated with low wage rates. The opportunity cost of withdrawing from the labor force is relatively small for low-wage workers, regardless of age (Juhn 1992). Older and disabled low-wage workers face an especially low opportunity cost of labor force exit because the progressivity of the Social Security benefit schedule results in a relatively high replacement rate of earnings for low-wage workers.

The last year of data used by Perrachi and Welch was 1990. Since 1990 there have been major changes in labor force behavior at both older and younger ages. These

1. There is some variation in the classification of younger and older age groups across studies. We define our categorization below.

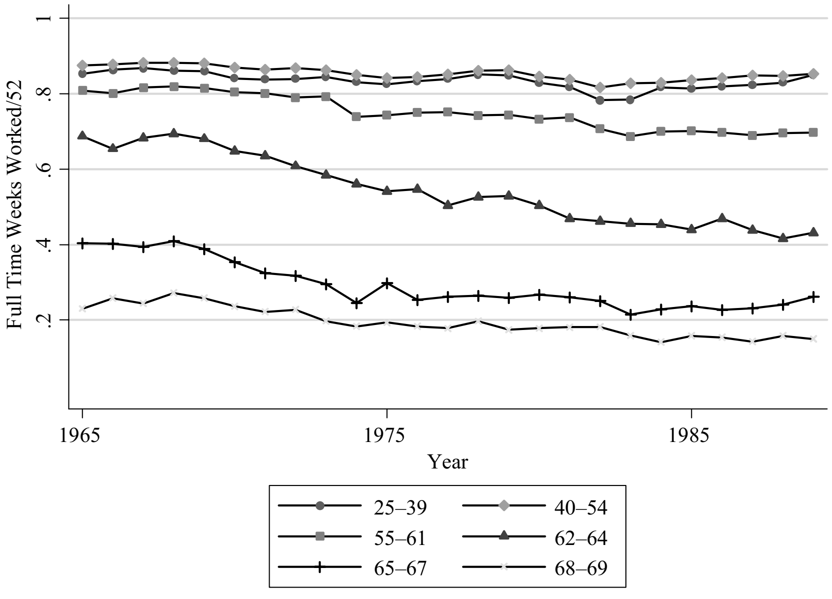


Figure 1a
Trends in Full-Time Equivalent Weeks Worked by Age Group, Men, 1965–89

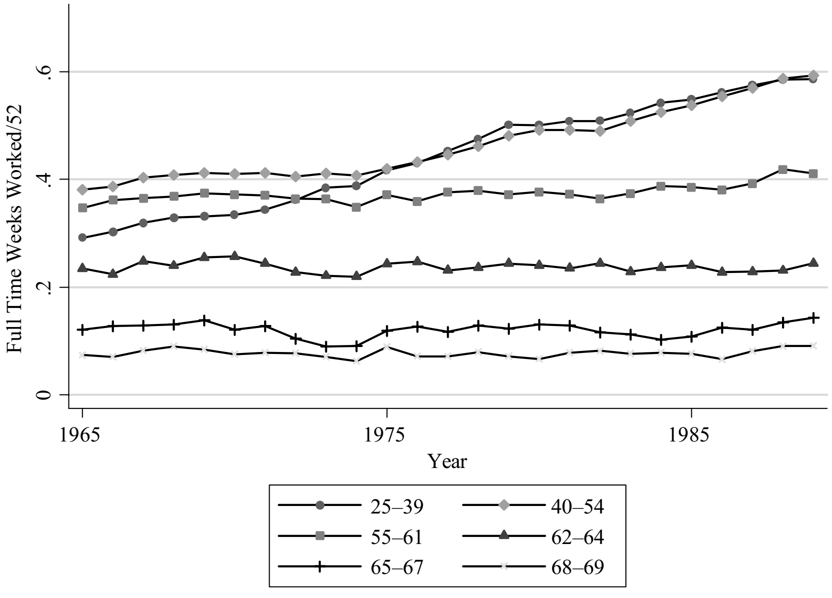


Figure 1b
Trends in Full-Time Equivalent Weeks Worked by Age Group, Women, 1965–89

changes are illustrated in Figures 2a and 2b, which update Figures 1a and 1b, respectively, through 2010. Between 1990 and 2010, the employment rate has *increased* substantially for older men and *decreased* for younger men. Aggregating the three older groups shown in Figure 2a (62–64, 65–67, 68–69), the male employment rate rose from around 30 percent in 1990 to 36.2 percent in the prerecession years of 2005–2007, and to 37–38 percent in 2008–2010. Aggregating over ages 25–39 and 40–54, the employment rate of younger men declined from around 84 percent in 1990 to 82.9 percent in 2005–2007 and 77.3 percent in 2008–2010.[2] The middle group, ages 55–61, experienced little change. The changes for women were also quite striking. The long trend of rising female employment at ages 25–54 ended around 2000, and has been declining since then. But, at older ages, female employment has been increasing at a rate very similar to that of older men. If common forces were driving employment trends across the age distribution pre-1990, those forces have either ended or been swamped by age-specific factors in the last two decades.

Changes in the institutional environment facing older workers have been proposed as explanations for the rise in employment at older ages. Social Security reforms that affected cohorts reaching their 60s during the 1990s and 2000s raised the Full Retirement Age (FRA) from 65 to 66, eliminated the Social Security Earnings Test for workers at or above the FRA, and increased the actuarial adjustment in benefits for delayed claiming past the FRA (the Delayed Retirement Credit, or DRC). These reforms all encourage employment at older ages (Blau and Goodstein 2010). Defined Benefit pension plans have become increasingly scarce in the private sector, largely replaced by Defined Contribution plans such as the 401k (Poterba, Venti, and Wise 2007). Defined Benefit plans typically encourage early retirement, while Defined Contribution plans have no specific retirement incentives. In addition, the increase in employment of older married women has resulted in a large increase in the proportion of older married couples in which both spouses have had significant attachment to the labor force. This makes joint labor force decisions of greater importance and may encourage employment of older men (Schirle 2008).

Potential explanations for employment declines at younger ages are less obvious. Moffitt (2012) shows that part of the drop for men aged 16–64 in recent years can be explained by falling wages and part by demographic changes. The end of the upward trend for women and the beginning of the recent decline have been more difficult to explain (Goldin 2006, Macunovich 2010). Blau and Kahn (2007) and Heim (2007) have shown that the elasticity of women's labor supply with respect to the wage rate declined substantially in the 1980s and 1990s, and Moffitt (2012) reports a negligible elasticity for women in the 2000s. But this does not explain a reversal of the age-specific employment trends for women.

Understanding trends in employment by age is important because these trends determine the future size and age composition of the U.S. labor force, and have important implications for the Social Security system. The United States experienced an unprecedented decline in the employment-population ratio beginning in 2000 (Moffitt 2012), even before the deep recession of the last few years. This was largely due to the decline in employment of younger workers noted above, as well as less educated

2. The low employment rate in 2009 and 2010 is obviously a result in large part of the Great Recession, but it is likely that some portion of the decline will persist.

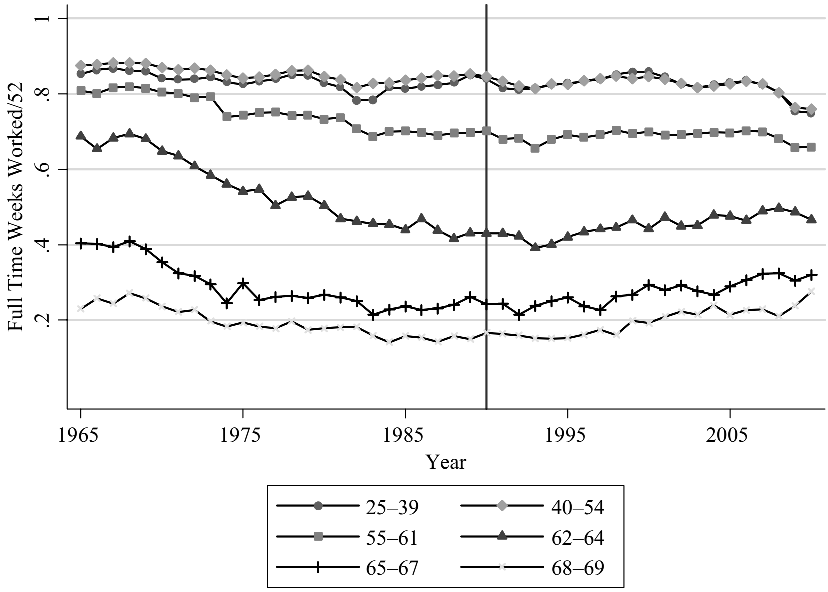


Figure 2a
Trends in Full-Time Equivalent Weeks Worked by Age Group, Men, 1965–2010

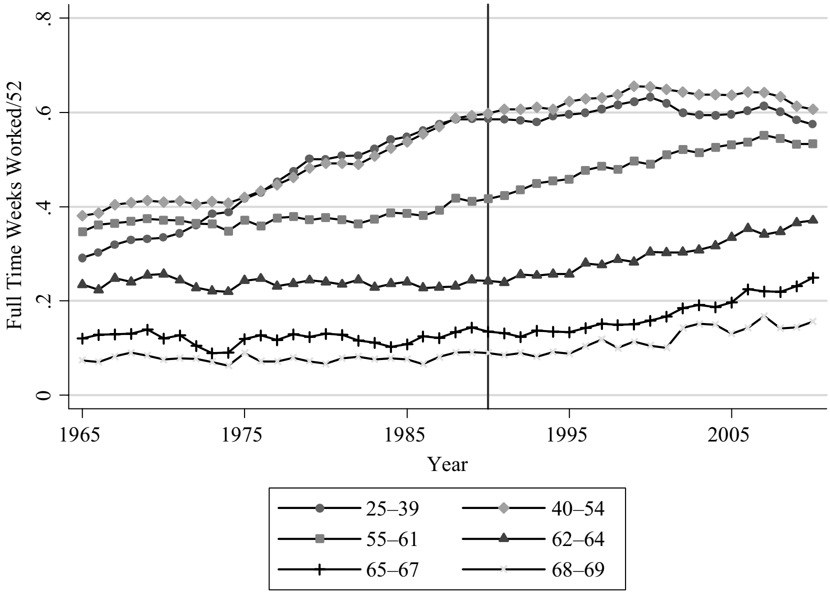


Figure 2b
Trends in Full-Time Equivalent Weeks Worked by Age Group, Women, 1965–2010

workers. This decline has been widely discussed, but analysis has been fairly limited (Aaronson et al. 2006, Moffitt 2012). The literature has focused mainly on labor supply of younger workers, but the rising employment trend at older ages will help offset the decline among younger workers. There will be a large increase in the share of the elderly population in the next two decades, increasing the importance of understanding labor supply at both younger and older ages.

In this paper, we evaluate potential explanations for the divergence in employment trends between younger and older workers in recent years. Like Moffitt (2012) and others, we use a labor supply framework to motivate the empirical specification, but, unlike other papers, we focus on differences in labor supply behavior across age groups.[3] We analyze the effects on labor supply by age group of two broad sets of driving forces: economic factors, including the wage rate, Social Security policy, pension coverage, and the income tax rate; and demographic factors, including education, marital status, race and ethnicity, number of children, and health.[4] The effects of these variables are allowed to differ by age group, and we use the results to analyze the contribution of age-specific trends in the explanatory variables to explaining differences in employment trends across age groups. We use data from 1965 through 2010 to estimate the labor supply models. The models are specified and estimated in levels, and the results are used to explain trends — that is, growth and decline, as well as levels. For most of the paper we follow a dichotomous age classification. For men, ages 25–61 and 62–69 are referred to as the younger and older age groups, respectively. For women, the respective age groups are 25–54 and 55–69. The reason for the difference by gender is discussed below.

We have three main findings. First, changes in demographic composition can explain most of the decline in employment of younger men. The two main drivers are changes in marital status and race/ethnicity. We estimate that never-married and divorced, separated, and widowed men are 19–27 percentage points less likely to work than their married counterparts, other things equal. The share of younger men who were never married increased by ten percentage points from 1965–88 to 1989–2010, and the share widowed, divorced, or separated rose by 4 percentage points. The increase in the share never-married is the result of a delay in first marriage; there was no increase in the share never-married at older ages. So this difference is an important age-specific demographic trend that clearly contributed to the divergence in employment trends across younger and older men. Black, other race, and Hispanic men have lower employment rates than white men, and the share of these groups in the population increased substantially, contributing to the decline in younger male employment. Changes in marital status and racial and ethnic composition each can explain about half of the decline in the employment growth rate of men aged 25–61 from 1965–88 to 1989–2010.

Second, Social Security reforms can explain a modest portion, 9 percent, of the increase in employment of men at older ages, and can explain 27 percent of the decline in employment of younger men and 6 percent of the decline in employment of younger

3. Aaronson et al. (2006) estimate employment models by age group using aggregate data. They have an extensive discussion of trends in employment by age group, but their analysis focuses on how age-specific trends affect the aggregate employment rate, rather than on explaining age-specific trends.

4. We also analyze the effects of the minimum wage, life expectancy, the SSDI award rate, and net imports. However, these variables vary only in the aggregate, so we do not have much confidence in our ability to identify their effects.

women. For given lifetime earnings, Social Security benefits have declined for recent cohorts as a result of the increase in the FRA, and the incentive structure has tilted to favor later claiming. Our estimates indicate that these changes resulted in increased male employment at older ages, confirming results of other recent studies (Blau and Goodstein 2010, Mastrobuoni 2009). A novel contribution of our paper is to show that Social Security reforms also contributed to the *decline* in employment of younger men, and to a lesser extent younger women. The decline in benefits is predicted to cause an increase in LFP at all ages, but in the presence of a borrowing constraint less generous benefits could induce lower work effort at younger ages in anticipation of greater work effort at older ages. In the presence of a liquidity constraint, the inability to borrow against future benefits may prevent a worker from retiring as early as he would like. This will result in a constrained optimum in which retirement occurs at the earliest age at which benefits are available. Depending on preferences (intertemporal complementarity in hours worked), this could lead to adjustments in labor supply at younger ages as well. A cut in benefits reduces the likelihood that the liquidity constraint binds, causing later retirement and reinforcing the wealth effect at older ages. But the effect on labor supply at younger ages depends on preferences and could be positive or negative. If the effect is negative and large enough to offset the wealth effect, the net effect of the benefit cut on labor supply at younger ages could be negative. We do not have any direct evidence on this mechanism, but the reduced form results suggest that recent benefit cuts help explain divergence in employment trends across age groups.

Third, changes in the educational composition of the labor force account for a small share of the increase in employment at older ages: 11 percent for older men and 14 percent for older women. Cohorts that experienced large increases in high school graduation and college attendance reached their 50s and 60s in recent years, while more recent cohorts have had a much slower rate of increase in educational attainment.

The effect of Social Security reforms on the divergence in labor supply by age will very likely persist and perhaps increase in magnitude as cohorts with an FRA of 67 reach their 60s in the 2020s. In contrast, the effects of rising educational attainment will be transitory, as future cohorts of older workers will be as well-educated as their younger counterparts. We think it is unlikely that the proportions never-married and divorced, widowed, or separated at older ages will ever approach the proportions observed at younger ages. If this is correct, the effects of the increase in age at first marriage on the divergence in employment trends by age likely will be persistent.

The next section briefly reviews related literature and highlights our contributions. Section III describes the employment data, and Section IV discusses model specification and measurement of the explanatory variables. Section V presents and discusses the results, and Section VI concludes. We discuss implications of the findings for Social Security policy reforms in the conclusion.

II. Related Literature

Our analysis is related to three main areas of the labor supply literature: the effects on labor supply of (a) the wage rate, (b) OASI, and (c) SSDI. We discuss these in turn, followed by a brief discussion of other employment determinants that do not fit neatly into the labor supply framework.

A. Wages

The labor market returns to skill have increased substantially over the last four decades (Acemoglu and Autor 2011). Low-skill workers have faced declining relative wages and in many cases declining *absolute* real wages as well. Changes in the wage structure affect workers of all ages, but the effects may differ by age, as suggested by the life cycle model. We analyze the effect of the wage rate on labor supply at different ages, but we find that wage effects on labor supply are small and differences in wage trends across age groups cannot account for divergence in employment trends. These findings are similar to those of Moffitt (2012) for women, but Moffitt finds somewhat larger explanatory power of wages for men, most likely because he includes men aged 16–24 in the population analyzed while we do not. We verify previous findings indicating that changes in wages are not a major factor behind changes in employment trends.

B. OASI

Social Security retirement benefits became increasingly generous from the beginning of the program in the 1930s through the mid-1970s. However, the evidence suggests that increased generosity of OASI was not the main cause of the decline in employment of older men during this period (Blau and Goodstein 2010, Krueger and Pischke 1992). Social Security reforms in 1977 and 1983 reduced the benefit available at a given age of claiming, increasing the incentive to work at older ages, as discussed above. Blau and Goodstein (2010) estimate that changes in Social Security can explain between one quarter and one half of the increase in employment of older men since the 1980s. Our contributions are to analyze how OASI benefits affect employment behavior at younger ages as well as at older ages, and for women as well as men.

C. SSDI

If an individual successfully applies for disability benefits, the SSDI benefit is equal to his Primary Insurance Amount (PIA), which is determined by average indexed earnings. Unlike in the case of OASI, the benefit does not depend on the age of claiming. The OASI benefit for a retired worker is equal to his PIA if he claims the benefit at his FRA but is reduced if he claims early. The earliest claiming age is 62, and the reduction for claiming at 62 is 20–30 percent depending on the FRA, which is determined by year of birth. For example, an individual with an FRA of 67 can receive an OASI benefit equal to his PIA if he claims it at age 67, a benefit equal to 0.7*PIA if he claims at age 62, and a benefit equal to his PIA if he is awarded disability benefits, *regardless of age* at the time of SSDI eligibility.[5] Thus a decline in the OASI benefit as a result of an increase in the FRA increases the relative attractiveness of SSDI (Duggan et al. 2007). The increase in the FRA effectively cut the OASI benefit for claiming at a given age, but did not affect the SSDI benefit. Von Wachter et al. (2011) found that 30 percent of new awards and over half of rejected applications in 2007 were from

5. The benefit will differ in each scenario to the extent that average indexed earnings at the time of claiming differ. For example, if earnings rise with age then the PIA will be larger for claiming at a later age, for both OASI and SSDI.

individuals aged 30–44. There is no evidence of a decline in health of younger men, suggesting that a growing share of applications is "induced" by the program.

A large literature analyzes the effect of SSDI on labor supply, but few studies analyze the effect of the SSDI benefit. This is because the SSDI benefit varies only in the time series, conditional on average lifetime earnings, so there is no cross-cohort variation available for identification. For this reason, we are not able to analyze the impact of the SSDI benefit level, but we explore the impact of the SSDI award rate. Almost all studies find a negative effect of being awarded SSDI benefits on labor supply but most conclude that the effect is relatively small.[6] However, Autor and Duggan (2003) and Black, Daniels, and Sanders (2002) argue that labor supply of low-skill workers is more sensitive to the relative value of SSDI benefits, likely because the benefit schedule is progressive, replacing a higher proportion of earnings at low levels of earnings.

D. Other Determinants of Employment

Employment is affected by demand-side factors that are not fully captured by the wage rate, and labor-demand effects on employment may differ by age. For example, older workers are less likely to experience loss of a job due to layoff or business closing, but the consequences of job loss are more severe for older workers. Older workers are much less likely to be reemployed within one to three years, and experience much larger wage losses upon reemployment (Johnson and Mommaerts 2010, Farber 2005). Age discrimination in employment is another example of a demand-side factor that has differential effects by age. Employment protection aimed at older workers may reduce age discrimination in firing but also alters hiring incentives (Lahey 2008, Neumark and Song 2012). We do not directly analyze the effects of labor demand and related policies except for the minimum wage, so it is important to bear in mind that what we interpret as labor supply effects could be partially a consequence of labor demand factors. This of course affects the interpretation of our estimates but not their consistency.

III. Employment Data

The main source of data for the analysis is the March supplement to the CPS. We use data from the 1966 through 2011 surveys on individuals aged 25–69.[7] To facilitate computation and merging with data from other sources, we aggregate the

6. Recent studies that take a structural approach to estimation have estimated the effect of the SSDI benefit on labor supply: Bound et al. (2010), Kim (2014), and Low and Pistaferri (2015). Other recent studies estimate the treatment effect of being awarded SSDI benefits without identifying the impact of the amount of the benefit: Chen and van der Klaauw (2005); French and Song (2014); and Maestas, Mullen, and Strand (2013).

7. Alexander, Davern, and Stevenson (2010) report that the Census Bureau inadvertently introduced errors in age and sex in the CPS public use files in several years in the 2000s as part of their procedures to avoid disclosure. These errors apply to the population aged 65 and above, and the authors report that there may have been a significant effect on studies of the older population. Fisher (2010) uses Social Security administrative records matched to the CPS for 2001–2006 to explore the extent of age misclassification. She finds that the probability of misclassification of age by more than one year increases linearly with age beginning at 65, reaching about 15 percent for men at ages 68–69 and 10 percent for women. She also reports that there are

individual data into cells defined by gender, single year of age, single calendar year, and education group. Calendar year refers to the year prior to the March survey, and age is measured as age at the March survey minus one.[8] The four education groups are high school dropout, high school graduate, college attendee, and four year college graduate.[9]

The dependent variable is the number of full-time-equivalent weeks worked in the calendar year, divided by 52, with weeks worked part-time (35 or fewer hours) treated as half of full-time weeks. This measure, which we refer to as Full Time Weeks worked (FTW), combines the intensive and extensive margins of labor supply behavior. [10]

A useful way to characterize changes in trends across age groups is in the form of growth rates. We compute the average annual growth rate in FTW for two subperiods, 1965–88 and 1988–2010, for three age groups: 25–54, 55–61, and 62–69. Figures 3a (for men) and 3b (for women) show the results. For men, there is little difference in growth across the periods for the two younger groups but at older ages the contrast is stark: a 2.2 percent average annual rate of decline in FTW in the earlier period and an increase of 1.2 percent in the more recent period. The contrast is sharp for women as well, in this case for both the youngest and oldest age groups. In the earlier period growth was most rapid for the youngest group at 2.4 percent per year and declined with age, while in the more recent period the age pattern was reversed, with essentially no growth for the youngest group, 1.1 percent for the middle group, and 2.6 percent at ages 62–69.

In the analysis that follows we further aggregate the age groups in order to simplify the analysis. Based on Figures 3a and 3b, we define younger men as ages 25–61 and older men as ages 62–69. For women, we use ages 25–54 as the younger age group and ages 55–69 as the older group. We estimate models of FTW in levels, and then use the results to derive implications for the growth rate.

IV. Model Specification and Measurement of Explanatory Variables

We specify an empirical model of employment behavior based loosely on the static labor supply framework. As noted above, we aggregate the individual data to cell means in order to combine data from different sources (described below), with cells defined by single year of age, education group, and single calendar year. The analysis is carried out separately for men and women, so we omit gender from the

errors in years that were not subject to inadvertent Census Bureau errors, so the net effect of the misclassification introduced by the change in disclosure practices was 12 and 7 percent for men and women, respectively.

8. Birth year is an important variable in our analysis because it determines the applicable Social Security rules. We assume that individuals were born after the March survey date, which implies that birth year equals calendar year minus age minus one. This introduces some measurement error. Blau and Goodstein (2010) indicate that their results are not very sensitive to alternative assumptions. See Mastrobuoni (2009) for an alternative approach to inferring birth year in the CPS, using monthly data.

9. There is a great deal of variation over time and across age groups in educational wage differentials and other explanatory variables, so it is useful to incorporate education in the definition of the cells in order to exploit this variation in the analysis.

10. The trends in alternative employment measures such as labor force participation in the survey week are very similar to those reported here for FTW. Parameter estimates and simulation results are also very similar.

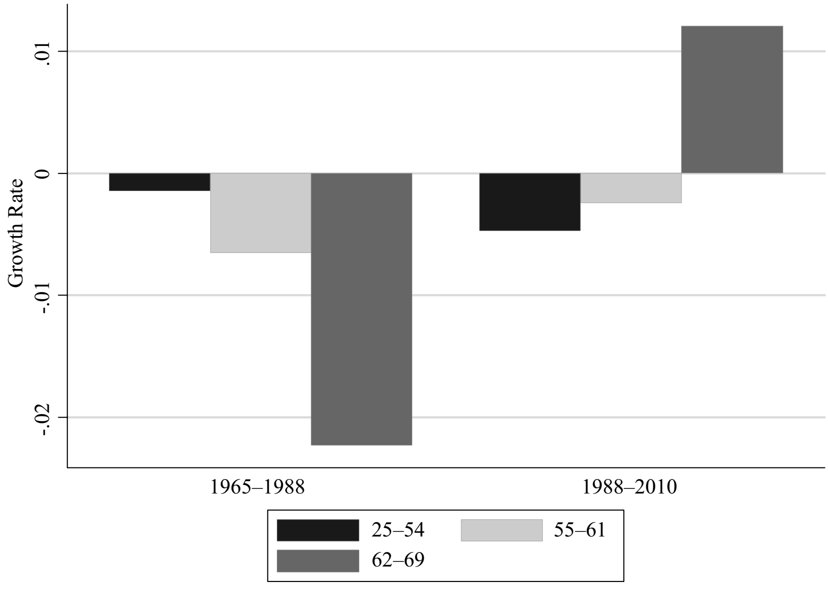


Figure 3a
Average Annual Rate of Change in Full-Time Equivalent Weeks Worked by Period and Age Group, Men

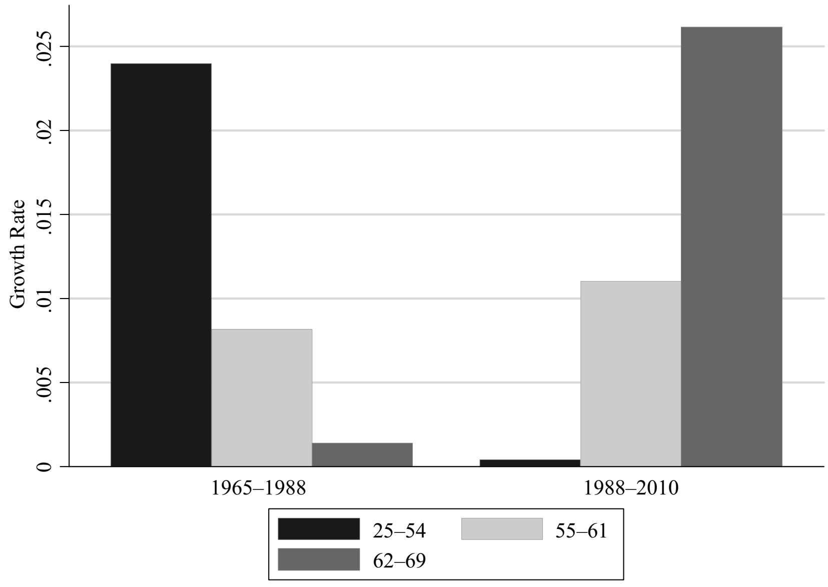


Figure 3b
Average Annual Rate of Change in Full-Time Equivalent Weeks Worked by Period and Age Group, Women

definition of the cells. The model is linear in order to facilitate aggregation. Using the cell as the unit of observation, the dependent variable is E_{jat}, the weighted mean value of FTW for the population in education group j observed at age a in year t.[11] Define $c = t - a$ as birth year, and let g denote an age group. The model is

$$(1)\quad E_{jat} = \beta^g X_{jat} + \gamma^g Z_{jc} + \alpha^g Y_t + \delta_a + f^g(c) + h^g(t) + \theta_j + \varepsilon_{jat},$$

where X_{jat} is a vector of education-age-and-time-varying variables (for example, the wage rate), Z_{jc} is a vector of variables that varies across birth cohorts and education groups but not by age within a cohort (for example, the OASI benefit, for a given claiming age), Y_t is a set of variables that vary only in the time series, δ_a is an age fixed effect, $f^g(c)$ is a function of birth cohort, $h^g(t)$ is a function of calendar year, θ_j is an education-group fixed effect, and ε_{jat} is a disturbance. All coefficients are allowed to differ by age group but not by period.

The coefficients of most interest are β^g and γ^g. The specification of cohort, age, and time effects is crucial for identification and interpretation. Unrestricted age fixed effects are included in order to account for persistent life cycle patterns of labor supply. Unrestricted education fixed effects are included because there may be differences across education groups in unmeasured factors such as preferences that would cause bias in estimates of the effects of wages and other variables that vary with education. Birth cohort effects are included in order to avoid confounding the effects of cohort-specific variables such as OASI benefits with unobserved cohort trends. We seek to explain time trends in employment, so one might think that time trends should not be included and that time-varying variables should be forced to explain the trends in employment. But there are undoubtedly omitted time-trending variables associated with the included time-varying variables such as wages, health, and others. Hence, some controls for such unobserved trends should be included. However, we cannot be completely flexible in specifying cohort and time effects, given the well-known age-cohort-period relationship.

First, consider identification of γ^g, the effects of cohort-education specific variables such as OASI benefits that are independent of age. OASI rules differ only by cohort, but benefits vary within birth cohorts as a result of differences across education groups in lifetime earnings. However, this source of variation does not identify the effects of changes in the OASI rules, which do not vary across education groups. We include lifetime earnings in Z_{jc} as a control variable, so the effect of OASI benefits is identified by changes in the benefit formula and nonlinearity of the formula with respect to lifetime earnings. An unrestricted set of cohort fixed effects would eliminate changes in OASI rules as a source of identification, leaving only nonlinearity of the benefit formula as a source of identification. As long as cohort effects are not completely unrestricted, the effects of OASI benefits (an element of γ^g) are identified by discontinuous changes in cohort-specific OASI rules such as changes in the FRA and DRC mentioned above. These changes are described more fully below. Our main specification of the birth cohort function f is a third order polynomial. Alternative specifications, including two-year and four-year fixed effects and no cohort effects, are discussed in the next section. The key identifying assumption is that unobserved cohort effects

11. We use the CPS March supplement weight to construct cell means. In the regression analysis we weight each cell by the number of individual-level observations used to construct the cell mean.

can be adequately captured by less-than-fully-nonparametric specifications, such as a smooth polynomial function or a set of two-year or four-year dummies.

Now consider identification of β^g, the effects of age-time-and education-specific variables such as the wage rate, marital status, number of children, and health. Given the specification of age and cohort effects described above, identification of β^g depends crucially on the specification of calendar time effects. The most flexible specification of time effects is age-group-specific individual year fixed effects, say $h^g(t) = \pi_{gt}$. In this case identification of β^g is mainly from differences in time trends in the explanatory variables by age group within education group (or equivalently, by education group within age group). For example, wage trends differ considerably by education within age groups. This is illustrated for men in Figure 4 and women in Figure 5. For each of the four education groups, there was a large increase in the wage gap between younger and older men, reflecting the well-known increase in returns to work experience. The increase in the wage gap for women was smaller but still evident.

A more restrictive specification limits year fixed effects to be common across age groups: $h^g(t) = \pi_t$. Some variables are independent of education, such as the SSDI acceptance rate. The acceptance rate varies by age and time, but the age trends are very similar over time, so the effects of the acceptance rate are not identified even with a restricted set of time effects. Also, this specification does not permit identification of the effects of aggregate variables such as the minimum wage and life expectancy.[12] The most restrictive specification excludes time effects altogether and includes a set of variables that vary only by time. We estimate all three specifications and compare results.

The key explanatory variables of interest are the hourly wage rate, the average income tax rate, OASI benefits, the SSDI award rate, pension coverage, and demographic variables. The specification is motivated by the standard static labor supply model, but we do not adhere to the model rigidly because our aim is to explain changes in trends rather than estimate behavioral parameters. For example, given the focus on older workers, we incorporate Social Security benefits rather than nonwage income or household wealth. We discuss measurement of the key variables, followed by a brief discussion of other variables and some limitations of the specification.[13]

A. Wage Rate and Tax Rate

The hourly wage rate net of taxes is a key variable in any labor supply model.[14] We compute average hourly earnings of full time year round workers (at least 45 weeks and 35 hours per week) from CPS data.[15] The sample is limited to ages 25–59 in order to reduce the potential for selection bias from participation decisions at older ages.[16]

12. Life expectancy differs by age, but the trends are virtually identical across age groups. Life expectancy by age and education is not available.

13. The specification ignores joint labor supply issues. In one of the extensions discussed below, we include spouse characteristics and spouse earnings or employment as explanatory variables.

14. The results were very similar using the weekly wage rate in place of the hourly wage rate.

15. Cases were dropped if average hourly earnings were less than \$5 or greater \$500 in 2010 dollars.

16. Wages at ages 60 plus are assumed to be equal to the age-59 wage. Selection bias could be important at younger ages, especially for women. We attempted to generate a correction for selection into the wage sample following the linear probability model approach of Moffitt (2012), but we were unable to find exclu-

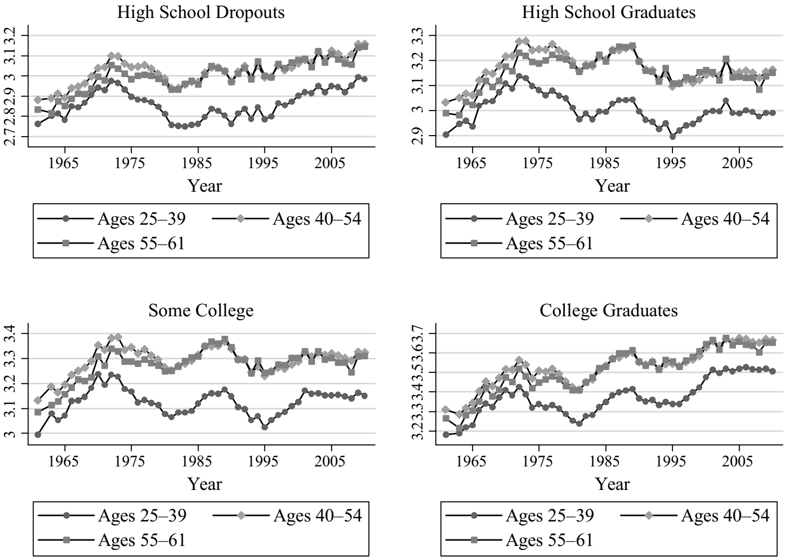


Figure 4
Trends in the Log Wage for Men by Education and Age Groups

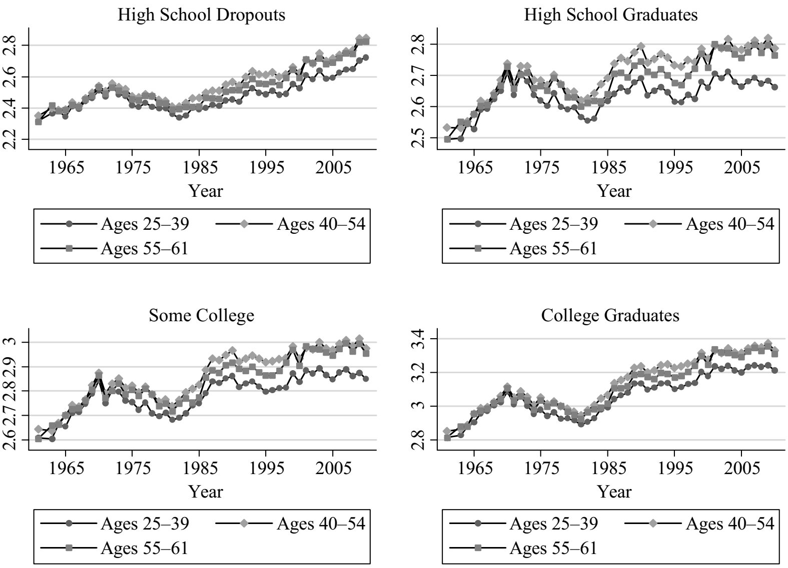


Figure 5
Trends in the Log Wage for Women by Education and Age Groups

In order to eliminate composition effects on wage rates, the log wage is regressed on a quadratic in age, and dummies for race, ethnicity, marital status, and census division, separately by the combination of education group, gender and single calendar year. The explanatory variables are a subset of the variables in the labor supply equation, but each variable is (implicitly) interacted with a full set of year dummies. Thus, allowing all coefficients of the wage equation to differ by year while restricting year effects to the intercept in the labor supply equation provides identification of the wage effects. The estimates are used to compute the fitted value of the log wage, holding the explanatory variables other than age and education constant (the baseline values are white, non-Hispanic, married, and Census geographic division 1). This approach preserves variation in the wage rate by education, age, and year, the three main dimensions of interest. We also constructed a measure of the fitted log wage that incorporated variation in race, ethnicity, marital status, and region, and found very similar results.

We compute the income tax rate facing each individual based on marital status, number of children, and the predicted wage rate. The combined federal income, state income (beginning in 1977), and payroll average tax rate (ATR) is computed using the NBER TAXSIM program under the following assumptions: (a) income from sources other than earnings, interest, dividends, and rent is ignored, (b) hours of work are assumed to be 2,000 per year, and (c) married couples file jointly and single individuals file as singles or head of household depending on whether they have dependent children. Tax rates for married individuals are computed under two alternative assumptions: The spouse works 2,000 hours and the spouse does not work. The coefficient estimates differed quite a bit, but the simulation results were very similar for the two alternative measures, so we report results only for the former case. There is a noticeable drop in the average tax rate on earnings at younger ages beginning in the mid-1980s (not shown here), around the time of the Tax Reform Act of 1986. We include the wage rate and tax rate as separate explanatory variables because preliminary results showed a better fit than in a specification in which they are restricted to have the same effect.

B. Social Security Retirement Benefits

Social Security benefits are computed using birth-cohort-specific Social Security rules, and birth-cohort-and-education-specific mean age-earnings profiles derived from the CPS, supplemented by published Social Security Administration (SSA) data for years before CPS data are available. The appendix describes the computation of these earnings profiles in detail. The earnings profiles are input to the anypia program provided by the SSA to compute benefits for several alternative scenarios: work continuously (from an assumed age of labor force entry that depends on education) through age 61 and claim at 62; work through 64 and claim at 65; and work through 69 and claim at 70. In each case, it is assumed that exit from employment is permanent. We compute

sion restrictions that could produce stable and plausible selectivity-corrected wage equation estimates. As an alternative, we employed a more standard Heckman selectivity correction approach. The selection correction is identified only by functional form because there were no plausible exclusion restrictions. For example, the Social Security variables discussed below might have served as exclusion restrictions, but they are not available at the individual level. The results using this approach were very similar to the main results with the exception of one case, discussed in the results section.

the present discounted value (PDV) of benefits as of age 55, using life table mortality and a real interest rate of 3.0 percent. The PDVs of benefits for claiming ages 62 and 70 are included in the model in the form of differences from the PDV of the benefit for claiming at age 65. In this specification, the age-65 benefit captures the wealth effect of benefit generosity for a given payroll tax, while the differences between the age-62 and age-65 and age-70 and age-65 PDV of benefits capture incentives to claim and retire early and late, respectively (Blau and Goodstein 2010).[17] The earliest age of eligibility for OASI benefits is 62, but behavior at younger ages may be influenced by expectations of future benefits, so we allow the benefit to affect employment decisions at younger ages. Social Security has wealth and substitution effects on labor supply. We expect the wealth effect on employment to be negative, while the sign of the substitution effect depends on age. In order to ensure that the SS variables are capturing the effects of changes in benefit rules rather than changes in lifetime earnings, we control for a sixth order polynomial function of the average earnings variable used to compute the benefit.

We assume perfect foresight about future rule changes. For example, a 1983 Social Security reform increased the FRA for cohorts reaching age 62 in 2000 or later, effectively cutting benefits. The first cohort affected by the rule change was born in 1938 and was age 45 in 1983. This cohort had at least 17 years to respond to the new rules; we assume that they behave as if they knew about the rule change from the time of labor force entry. Blau and Goodstein (2010) explored this and alternative assumptions and found that the perfect foresight assumption yielded the most sensible results.[18]

Figure 6 illustrates the trend in the present discounted value of real benefits for claiming at age 65. In order to focus on rule changes, the figure shows benefits calculated holding the lifetime earnings profile fixed while applying the rules for each birth cohort.[19] There were many ad hoc benefit increases in the early years of Social Security, but there was no automatic adjustment for inflation. Benefits rose irregularly until the famous "notch" that reduced benefits beginning with the 1917 cohort (Krueger and Pischke 1992). There were additional ad hoc benefit cuts that affected cohorts born in the 1920s, and a major reform in 1983 cut benefits for cohorts born after 1937 by increasing the FRA. The increases in the FRA were phased in irregularly, beginning with the 1938 cohort and ending with the 1960 cohort.[20] The benefit is adjusted automatically for inflation, but slow projected wage growth leads to a decline in benefits for cohorts born after 1960.

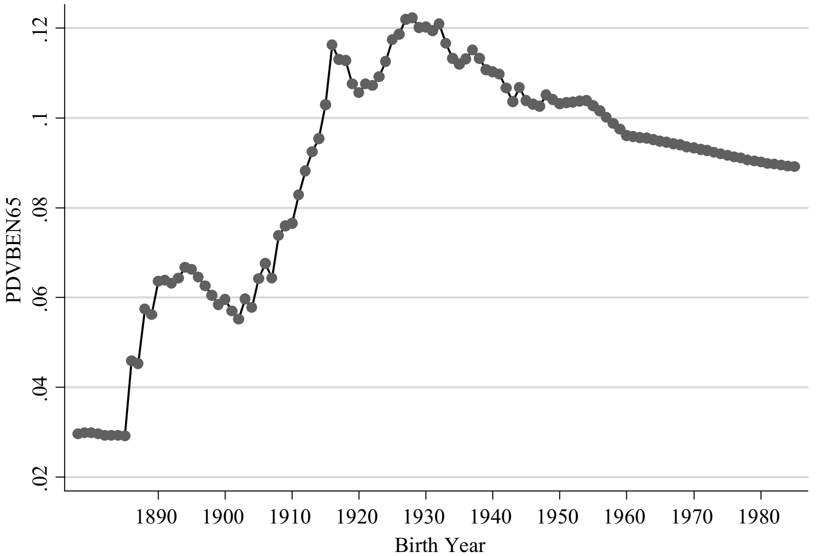
Figure 7 illustrates changes in claiming-age incentives, calculated as the gain in PDV from claiming at 62 relative to 65, and from claiming at 70 relative to 65.

17. This approach to measuring benefits is arbitrary, but Blau and Goodstein (2010) show that benefit measures computed under a variety of alternative assumptions are highly correlated with the benefit measures used here.

18. The elimination of the Social Security Earnings Test for workers who have reached their FRA was enacted unexpectedly in 2000. We do not analyze the impact of this policy change. See Haider and Loughran (2008) for a recent analysis and summary of the evidence.

19. The figure uses the earnings profile of male high school dropouts born in 1937, but the results are very similar using profiles of other groups. The regression analysis uses the actual (predicted) profile for each cohort.

20. The full retirement age is 65 for individuals born in or before 1937; $65 + x/6$ for birth years $1937+x$, $x=1, \ldots, 5$; 66 for birth years 1943–54; $66 + x/6$ for birth years $1954+x$, $x=1, \ldots, 5$; and 67 for birth years 1960+. Each one year increase in the FRA is equivalent to a 6.67 percent benefit cut for a given claiming age.



Based on lifetime earnings for high school droput men born in 1937. Millions of year 2010 $

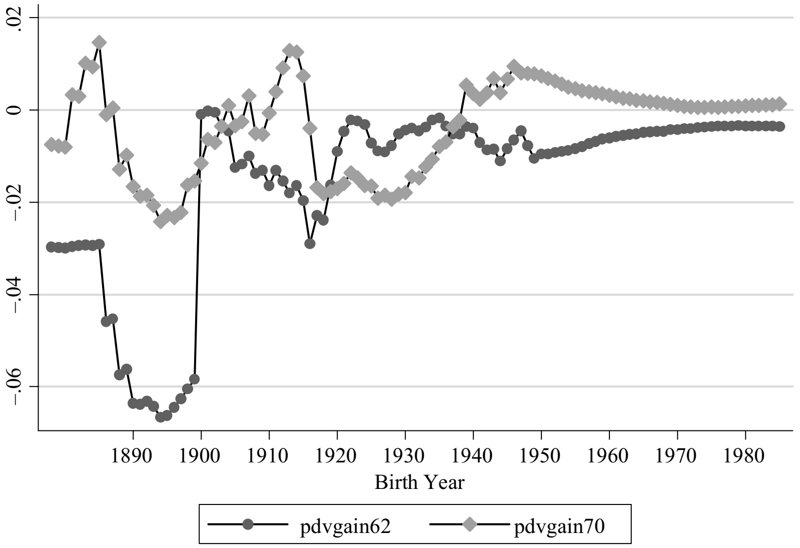
Figure 6
Expected Present Discounted Value of Lifetime Old Age and Survivors Insurance Benefits for Claiming at Age 65, by Birth year

Until 1972, there was no increase in the benefit from claiming after age 65, so the gain for many of the earliest cohorts was negative in PDV terms. A Delayed Retirement Credit (DRC) was instituted in 1972, providing a permanent 1 percent increase in the benefit per year of claiming past age 65 (up to age 70). This was increased to 3 percent in 1977, and then increased by 0.5 percent per two-year birth cohort, from 3.5 percent for the 1925–26 cohorts to 8.0 percent per year for the 1943 and subsequent cohorts. The possibility to claim before age 65 was instituted in 1956 for women and 1961 for men. This explains the large negative values for the gain from claiming at 62 until the 1901 cohort, which was the first (male) cohort to have this opportunity. The gain from claiming at age 62 was not affected by most subsequent reforms, which tended to change benefits between ages 62 and 65 by roughly equal amounts.

C. Social Security Disability Insurance

The incentive to apply for SSDI benefits is likely to be influenced by the probability of a successful application, known as the award rate (Low and Pistaferri, 2015). We have aggregate time series data on the award rate for the entire period, and age-group-specific data beginning in 1992.[21] Figure 8 shows that the award rate increases

21. We are grateful to the Social Security Administration for providing unpublished tabulations on age-specific award rates.



Based on lifetime earnings for high school droput men born in 1937. Millions of year 2010 $

Figure 7
Gain in Expected Present Discounted Value of Lifetime Old Age and Survivors Insurance Benefits for Claiming at 62 and 70 Relative to 65, by Birth year

with age, but the time trends in the award rate are very similar across age groups. Thus, in practice we have only time series variation, so as discussed previously we can analyze the effect of the award rate only in specifications without time effects. The award rate may be endogenous to labor supply if the composition of the applicant population with respect to severity of disability is influenced by the award rate. We cannot account for this directly, but we include the fraction of the insured population that applied for SSDI in a given year to control for the composition of the applicant population.[22]

D. Pension Coverage

Employer-sponsored pension plans are quite heterogeneous, and it is difficult to compute benefits without knowing the details of each plan. We use the CPS to compute a binary indicator of pension coverage that varies by birth cohort and education but not by age, as described in the appendix. This is a crude proxy for the influence of pensions. Unfortunately, data on pension type are not available in the CPS.

22. In a specification without time effects we could include the SSDI benefit as well as the award rate. However, in practice the benefit effect is poorly identified even in this specification as a result of the high-order polynomial control for the average earnings used to compute the benefit and the absence of benefit rule variation affecting SSDI since the late 1970s.

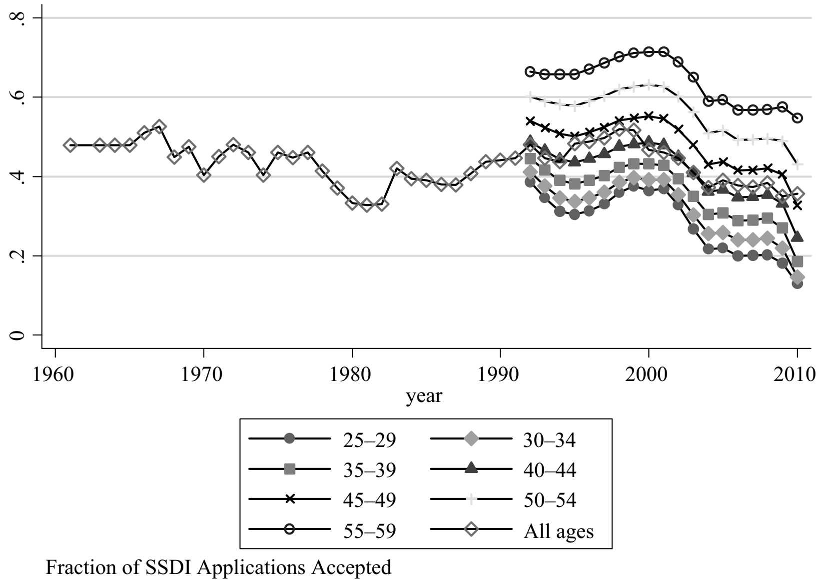


Figure 8
Trend in Social Security Disability Insurance Award Rate by Age Group

E. Other Variables

The specification includes race (black, other), marital status (widowed, divorced or separated, and never married), Hispanic ethnicity, and number of children younger than 6 and younger than 18, all derived from the CPS. We also include measures of self-reported health and work days lost as a result of illness, derived from the National Health Interview Survey. These data are aggregated to the sex-age-education-year cell level and merged with the CPS data. The appendix provides further details.

V. Results

A. Coefficient Estimates

Table 1 shows coefficient estimates from models of FTW estimated for age groups 25–61 and 62–69 for men, and 25–54 and 55–69 for women. In addition to the variables shown in the table, the specification includes a sixth order polynomial in average indexed earnings used to compute OASI benefits, age and year fixed effects, a cubic polynomial in birth year, and geographic division dummies (full results are available on request). The upper panel shows results for the economic variables. The log wage coefficient estimate is 0.09 for younger men and 0.001 for older men. The implied elasticities at the sample mean values of FTW (see the bottom of the table) are 0.11 for younger men and zero for older men. The coefficient estimates for women are

Table 1
Coefficient Estimates from regressions of Full-Time Equivalent Weeks Worked/52 (FTW)

	Men		Women	
	25–61	62–69	25–54	55–69
Economic variables				
Log wage	0.09	0.001	0.05	–0.07
	(0.01)	(0.04)	(0.02)	(0.04)
Average tax rate	–0.56	0.03	–0.14	0.22
	(0.06)	(0.22)	(0.09)	(0.17)
PDVBEN65	0.64	–0.72	1.98	–0.29
	(0.16)	(0.59)	(0.49)	(0.38)
Gain from early claiming	0.24	–1.17	0.43	–0.31
	(0.16)	(0.41)	(0.26)	(0.29)
Gain from later claiming	–0.11	0.54	0.89	0.65
	(0.13)	(0.30)	(0.22)	(0.19)
Pension coverage	0.07	–0.01	0.20	0.16
	(0.02)	(0.07)	(0.02)	(0.04)
Demographic variables				
Divorced, widowed, or separated	–0.19	–0.05	0.13	0.17
	(0.01)	(0.03)	(0.02)	(0.02)
Never married	–0.27	–0.10	0.16	0.12
	(0.01)	(0.05)	(0.02)	(0.04)
Black	–0.05	–0.02	0.07	–0.06
	(0.02)	(0.05)	(0.02)	(0.03)
Other race	–0.18	0.09	–0.15	0.01
	(0.03)	(0.08)	(0.03)	(0.06)
Hispanic	–0.17	–0.02	–0.13	–0.07
	(0.01)	(0.06)	(0.02)	(0.04)
Number of kids < 6	–0.02	–0.06	–0.08	0.02
	(0.004)	(0.06)	(0.01)	(0.04)
Number of kids < 18	0.01	–0.03	–0.03	–0.10
	(0.002)	(0.02)	(0.002)	(0.02)
Health very good	0.04	–0.01	0.04	0.03
	(0.01)	(0.03)	(0.01)	(0.02)
Health good	–0.07	–0.01	–0.03	0.01
	(0.01)	(0.03)	(0.01)	(0.02)
Health fair	–0.12	–0.06	–0.11	0.04
	(0.01)	(0.03)	(0.02)	(0.02)
Health poor	–0.19	–0.02	–0.26	0.02
	(0.02)	(0.05)	(0.04)	(0.04)
Fraction of year unable to work due to illness	–0.07	–0.05	0.01	–0.02
	(0.01)	(0.03)	(0.02)	(0.02)

(*continued*)

Table 1 (*continued*)

	Men		Women	
	25–61	62–69	25–54	55–69
High school dropout	–0.04	–0.20	–0.02	–0.12
	(0.01)	(0.03)	(0.02)	(0.03)
High school graduate	–0.01	–0.13	0.03	–0.06
	(0.004)	(0.02)	(0.01)	(0.02)
Some college	–0.008	–0.09	0.02	–0.02
	(0.003)	(0.01)	(0.01)	(0.01)
Mean of dependent variable	0.815	0.349	0.546	0.315
R^2	0.89	0.94	0.94	0.95
(number of cells)	(6808)	(1472)	(5520)	(2760)

Notes: PDVBEN65 = Present Discounted Value of OASI benefit if claimed at age 65 (discounted to age 55). Gain from early claiming = PDVBEN62 – PDVBEN65. Gain from later claiming = PDVBEN70 – PDVBEN65. Reference groups for categorical variables are white, married, health excellent, and college graduate. All monetary amounts except the log wage are measured in millions of year-2010 dollars. Coefficients on the sixth order polynomial in the average earnings variable used to compute OASI benefits, age- and time-fixed effects, the cubic in birth year, and geographic dummies are not shown.

0.05 and –0.07. The negative effect for older women is anomalous. The wage rate was predicted assuming full-time year-round employment, which could produce misleading results for women. As noted above (Footnote 16), we reestimated the wage models correcting for selection on employment (specifically, observing a wage rate for year-round full-time employment). The effects for men were similar to those shown in Table 1. For younger women, the positive effect becomes essentially zero, and for older women the effect changes from –0.074 to –0.035. This is still anomalous but smaller quantitatively.

The average tax rate has negative effects for younger men and women, but the estimates are positive for older individuals. The implied elasticities at the means are –0.26 and –0.09 for younger men and women, and 0.04 and 0.25 for older men and women (see Appendix Table A2 for the sample means of the explanatory variables).

The estimated effects of the PDV of Social Security benefits at age 65 are positive at younger ages and negative for the older groups. The elasticities at the sample means for younger men and women are 0.11 and 0.47, and for older men and women are –0.20 and –0.08. At older ages we would expect a negative wealth effect, while at younger ages the sign of the effect is theoretically ambiguous. There is a negative wealth effect, but in the presence of a borrowing constraint more generous benefits could induce greater work effort at younger ages in anticipation of less work effort at older ages. We expect the gain from claiming at 62 relative to 65 to have a negative effect on labor supply, and the results show this for older ages but not for younger ages. We expect the gain from claiming at 70 relative to 65 to have a positive effect on labor supply, and the results show this except for younger men. In both cases the effects are quite small, with elasticities of 0.04 or less in absolute value.

Pension coverage has a positive effect on labor supply for all groups except older men, with small elasticities for men (0.05 and –0.02), and modest elasticities for women (0.15 and 0.23). These pension effects are difficult to interpret in economic terms because of the absence of measures of benefits or even pension type.

The results for demographic and health variables shown in the lower panel of Table 1 are similar to those reported in many other studies: Unmarried men work less and unmarried women work more than their married counterparts; blacks, members of other racial groups, and Hispanics generally work less than whites, with the notable exception of younger black women; men and women with children present in the household work less; individuals who report fair or poor health and more days lost due to illness generally work less; and less-educated individuals work less, especially at older ages.

B. Counterfactual Simulations

Table 2 shows the results of counterfactual simulations of the change in the average annual growth rate of FTW from 1965–88 to 1989–2010. We use the regression results to simulate the level of FTW under alternative assumptions, and then compute the implied growth rates. We focus on growth rates because they are more directly informative about changes in trends (results for levels, reported in Appendix Table A1, are briefly discussed below). The first three rows of Table 2 show the observed average annual growth rate of FTW in each period, and the change in the growth rate from the earlier to the later period. For example, the column for older men indicates that in the earlier period FTW declined by 2.23 percent per year on average, while in the later period it rose by 1.21 percent. The change in the average annual growth rate from the earlier to the later period was 3.44 percent. The fourth row shows that the model predicts the difference in growth rates perfectly (thanks to the year fixed effects), using the observed values of the explanatory variables.

The subsequent rows show the predicted change in the growth rate holding constant the value of each variable or group of variables at their 1965–88 means, one at a time. The percent of the observed change that can be accounted for by changes in the explanatory variables is shown in parentheses for cases in which the explanatory power is non-negligible (at least 5 percent) *and* is in the right direction. For example, the row labeled "education" at the bottom of the table indicates that if the educational composition of the older male population had remained at its average 1965–88 value during 1989–2010, the change in the employment growth rate would have been 0.0305 instead of the observed increase of 0.0344. So the change in education can account for 11 percent ([0.0344 – 0.0305)]/0.0344) of the decline in the employment growth rate of older men and 14 percent for older women.[23]

Table 3 shows the changes in the mean values of the explanatory variables from 1965–88 to 1989–2010. The share of men aged 62–69 who were high school dropouts decreased by 29 percentage points, and Table 1 indicates that high school dropouts work substantially less than their more educated counterparts. As a result, the increase

23. The entries in Table 2 are rounded, so the percent change in the table, which is based on unrounded figures, is slightly different in some cases from the percent change calculated from the rounded entries.

Table 2
Counterfactual Simulations of the Average Annual Growth Rate of FTW

	Men		Women	
	25–61	62–69	25–54	55–69
1965–88 annual growth rate	–0.0020	–0.0223	0.0240	0.0040
1989–2010 annual growth rate	–0.0047	0.0121	0.0004	0.0184
Observed change	–0.0027	0.0344	–0.0236	0.0144
Predicted change	–0.0027	0.0344	–0.0236	0.0144
Counterfactual predicted change, replacing 1989–2010 values of explanatory variables with 1965–88 values				
Economic variables				
Wage rate	–0.0032	0.0341	–0.0240	0.0178
Average tax rate	–0.0056	0.0367	–0.0243	0.0149
OASI	–0.0016	0.0357	–0.0205	0.0154
(percent of total change explained)	(27)	(9)	(6)	
[base]	[–0.0022]	[0.0390]	[–0.0218]	[0.0154]
PDVBEN65	–0.0016	0.0355	–0.0199	0.0145
(percent of total change explained)	(27)		(8)	
[base]	[–0.0022]	[0.0361]	[–0.0216]	[0.0144]
Gain from early claiming	–0.0027	0.0352	–0.0236	0.0156
[base]	[–0.0027]	[0.0351]	[–0.0236]	[0.0155]
Gain from later claiming	–0.0026	0.0338	–0.0242	0.0142
(percent of total change explained)		(8)		
[base]	[–0.0026]	[0.0365]	[–0.0238]	[0.0144]
Pension coverage	–0.0025	0.0345	–0.0234	0.0137
(percent of total change explained)	(8)			
Demographic variables				
Marital status	–0.0012	0.0347	–0.0249	0.0143
(percent of total change explained)	(54)			
Race/ethnicity	–0.0014	0.0343	–0.0223	0.0144
(percent of total change explained)	(48)		(5)	
Number of children	–0.0028	0.0363	–0.0221	0.0180
(percent of total change explained)			(6)	
Health	–0.0027	0.0337	–0.0235	0.0142
Education	–0.0029	0.0305	–0.0234	0.0123
(percent of total change explained)		(11)		(14)

Notes: FTW = (Full-time equivalent weeks worked)/52. The counterfactual change for all variables except OASI and its components replaces the observed value of each variable or group of variables (one at a time) in 1989–2010 with its 1965–88 mean value. The OASI simulations replace the 1989–2010 values with the values for the 1937 birth cohort but using observed lifetime earnings of each cohort to compute the benefit. The predicted change is used as the base to compute percent of the total explained. The OASI counterfactual cannot be computed using the anypia program, so we used our own approximation of the Social Security benefit formula. In order to ensure that the baseline and counterfactual simulations are comparable, we also used our code to simulate the baseline values for OASI. These values are shown in brackets for OASI and its components. The text provides more discussion. Health is not measured before 1972, so the means for the earlier period use 1972–88 values (the very good category was not introduced until 1982, so the mean is measured from 1982–88). Hispanic ethnicity is not available until 1970, so the mean is measured for 1970–88. State tax rates are not available until 1977, so the mean tax rate is measured for 1977–88. OASI = Old Age and Survivors Insurance. PDVBEN65 = Present Discounted Value of OASI benefit if claimed at age 65 (discounted to age 55). Gain from early claiming = PDVBEN62 – PDVBEN65. Gain from later claiming = PDVBEN70 – PDVBEN65. The OASI simulation changes PDVBEN65 and the gains from early and late claiming jointly. All monetary amounts except the log wage are measured in millions of year-2010 dollars.

Table 3
Change in means of the explanatory variables from 1965–88 to 1989–2010

	Men		Women	
	25–61	62–69	25–54	55–69
Log wage	0.093	0.162	0.243	0.269
Average tax rate	–0.088	–0.079	–0.092	–0.075
PDVBEN65	–0.0163	–0.0030	–0.0184	–0.0043
Gain from early claiming	0.0015	0.0003	0.0023	0.0006
Gain from later claiming	0.0112	0.0044	0.0113	0.0057
Pension coverage	–0.063	–0.009	0.014	0.042
Divorced, widowed, or separated	0.043	0.039	0.025	0.0001
Never married	0.090	–0.008	0.083	0.002
Black	0.015	0.004	0.020	0.015
Other race	0.038	0.026	0.039	0.034
Hispanic	0.068	0.033	0.061	0.049
Number of kids<6	0.036	0.053	0.067	0.064
Number of kids<18	0.006	0.156	0.030	0.192
Health very good	0.044	0.055	0.036	0.057
Health good	–0.064	–0.036	–0.099	–0.070
Health fair	–0.013	–0.043	–0.021	–0.047
Health poor	–0.009	–0.032	–0.003	–0.014
Fraction of year unable to work due to illness	–0.027	–0.105	–0.008	–0.035
High school dropout	–0.147	–0.295	–0.136	–0.268
High school graduate	–0.021	0.066	–0.121	0.034
Some college	0.095	0.097	0.128	0.122

Notes: Each entry is the mean value of the indicated variable in 1989–2010 minus the mean value in 1965–88. See Table 2 for additional notes. Monetary amounts except the log wage are measured in millions of year-2010 dollars.

in educational attainment can account for a modest part of the increase in employment of older men.[24]

Table 3 indicates that the mean real wage rate increased by 9–16 log points for men across periods, and by 24–27 log points for women. However, these wage increases cannot explain changes in employment growth for any of the groups. The positive wage coefficients are too small for the wage changes to make much difference, and the negative coefficient for older women implies that their wage increase should have

24. The large changes in educational attainment over this period were probably accompanied by changes in the average unobserved skill of the education groups. For example, as high school completion approaches 90 percent, the remaining dropouts may be more negatively selected than when the high school graduation rate was only 75 percent. We reestimated the models allowing education effects to differ across the two periods. The explanatory power of education increased from 14 percent to 47 percent for older women, and was unchanged for the other groups.

caused a decline in their labor supply rather than the observed increase. The decline in the average tax rate of 0.07–0.09 shown in Table 3 also cannot account for the observed changes in employment growth between periods. Overall, changes in the net reward to working in a given year cannot help explain the observed changes in employment.

We use a different approach for the counterfactual simulations for OASI benefits. The anypia program cannot be used to compute benefits for a given earnings history and a counterfactual OASI formula. Instead, we use the PIA produced by anypia as input into our own program that computes benefits for alternative policy regimes. This introduces some measurement error since our program does not produce the same benefits as anypia using our approximation of the actual rules for each cohort. We do not have the code for the anypia program and cannot determine the source of the error. Nevertheless, our calculations yield benefits that are very highly correlated with the benefits produced by anypia (0.98). We use the benefits from anypia in estimation, and we use our program to generate both counterfactual and baseline benefits (shown in brackets) for the simulations, to ensure that any errors in calculations cancel out when we take the difference.

The OASI counterfactual simulation assigns the benefit computation rules for the 1937 cohort to everyone but uses each cohort's observed (predicted) lifetime earnings. This approach isolates the effect of rule changes, holding average indexed earnings constant for each cohort. The 1937 cohort was the last to have an FRA of 65.[25] As indicated in Table 3, the PDV of lifetime OASI benefits if claimed at age 65 (discounted to age 55) would have been higher by 16–18K in 1989–2010 for the younger cohorts if the 1937 SS rules had remained in effect for subsequent cohorts, and by 3–4K for the older groups.[26] Table 2 shows that the decline in benefits can account for 27 percent of the decline in employment growth of younger men and 8 percent of the decline for younger women (shown in the row labeled PDVBEN65). The small changes in the gains from earlier and later claiming had little impact on employment except for older men. The OASI reforms as a group cannot explain any of the increase in employment growth of older men and women, and can account for 27 percent and 6 percent of the declines for younger men and women, respectively (shown in the row labeled OASI, which sums the effects of PDVBEN65 and the gains to early and late claiming).[27]

Concerning the demographic characteristics in Table 2, the row labeled "marital status" indicates that if the marital status composition of the younger male population had remained unchanged from 1965–88 to 1989–2010, the decline in the employment growth rate would have been only –0.0012 per year instead of the actual decline of –0.0027. So changes in marital status can account for 54 percent ([–0.0027 –

25. Alternative counterfactuals based on the rules for other cohorts yielded very similar results. Results using benefit levels in place of the present discounted value of benefits were qualitatively similar, but the explanatory power of OASI rule changes was smaller.

26. These are declines of 11 percent, 3 percent, 14 percent, and 5 percent for the four groups, using the overall sample means reported in the Appendix as the base. The magnitude of the decline depends on the mix of birth years in the period two samples.

27. This differs from the results for older men in Blau and Goodstein (2010), who found that changes in OASI could explain one quarter to one half of the rise in LFP of older men. Our results were closer to theirs when we estimated a specification that included only a linear term in lifetime earnings, as in their specification.

(–0.0012)]/–0.0027) of the decline in the employment growth rate of younger men over this period. This was a consequence of substantial increases in the proportion of the younger male population that was never-married, divorced, widowed, or separated. There were increases of similar magnitudes for women, but they cannot explain changes in employment since never-married women work more at younger ages than their married counterparts, while female employment growth declined at younger ages. The only other change in the demographic characteristics that can help account for changes in employment growth (aside from education, which was discussed above) is the change in the racial and ethnic composition of the younger male population. The black, other race, and Hispanic shares of the population increased for all groups (see Table 3), but the effects of these variables on labor supply are largest at younger ages. These composition changes can explain about half of the slowdown in employment growth for younger men. For younger women, blacks work more than whites, so the increased share of blacks offset the effects of the increased shares of the other groups.

C. Alternative Specifications and Simulations

(1) As discussed above, we simulate the growth rate of employment, because this is more informative about trends than are employment levels. Nevertheless, it is worth examining simulation results for employment levels briefly. These are reported in Appendix Table A1, using the estimation results from Table 1. Qualitatively, the results are very similar. The explanatory power of several of the variables is much larger for younger men but similar in magnitude for the other groups.

(2) A key issue discussed in the previous section is how to control for time-trending unobservables that could be correlated with the explanatory variables. The results reported in Table 1 are from a specification that includes age-group-specific year fixed effects, which control for such unobserved factors in a very flexible way. In fact, this specification might be too flexible for our purposes, since we are interested in common trends. We estimated two other specifications to gauge the importance of this issue. The first incorporates a full set of year fixed effects with coefficients constrained to be equal across age groups, by sex. The simulation results from this specification are shown in Table 4 (coefficient estimates are not shown). An important point to note is that the model does not predict the observed changes as well as in the specification with unrestricted year fixed effects. The predicted change reported in row four of Table 4 is used as the baseline for the counterfactual simulations, to ensure that the counterfactual and base simulations are comparable. Most of the simulation results are quite similar qualitatively to the results in Table 2. The main exception is that the explanatory power of OASI is smaller for younger men and women, while it is larger for older women. There are also modest increases in the explanatory power of health for older men and women, and a moderate increase in the explanatory power of pensions for older women.

The second alternative specification omits all calendar year effects, replacing them with a set of observed aggregate variables. These include the minimum wage, SSDI award and application rates,[28] net imports, life expectancy, and GDP growth. The

28. The award rate is determined in part by the composition of the applicant population, so other things may not be equal as the award rate varies. We use the application rate (the share of the insured population that applies in a given year) as a rough proxy to control for changes in the composition of the applicant population.

Table 4
Counterfactual Simulations of Annual Average Growth Rate of Full-Time Equivalent Weeks Worked/52 (FTW), Restricted Year Fixed Effects

	Men		Women	
	25–61	62–69	25–54	55–69
1965–88 annual growth rate	–0.0020	–0.0223	0.0240	0.0040
1989–2010 annual growth rate	–0.0047	0.0121	0.0004	0.0184
Observed change	–0.0027	0.0344	–0.0236	0.0144
Predicted change	–0.0022	0.0251	–0.0221	0.0073
Counterfactual change, replacing 1989–2010 values with 1965–88 values of explanatory variables				
Economic variables				
Wage rate	–0.0027	0.0237	–0.0221	0.0073
(percent of total change explained)		(5)		
Average tax rate	–0.0043	0.0214	–0.0218	0.0075
(percent of total change explained)		(15)		
OASI	–0.0016	0.0241	–0.0215	0.0061
(percent of total change explained)	(15)			(14)
[base]	[–0.0019]	[0.0028]	[–0.0217]	[0.0071]
PDVBEN65	–0.0017	0.0244	–0.0211	0.0076
(percent of total change explained)	(13)			
[base]	[–0.0020]	[0.0238]	[0.0215]	[0.0071]
Gain from early claiming	–0.0022	0.0246	–0.0221	0.0073
[base]	[–0.0022]	[0.0246]	[–0.0221]	[0.0073]
Gain from later claiming	–0.0021	0.0253	–0.0225	0.0057
(percent of total change explained)				(21)
[base]	[–0.0021]	[0.0245]	[–0.0222]	[0.0072]
Pension coverage	–0.0020	0.0255	–0.0219	0.0165
(percent of total change explained)	(10)			(6)
Demographic variables				
Marital status	–0.0008	0.0266	–0.0235	0.0068
(percent of total change explained)	(64)			(7)
Race/ethnicity	–0.0010	0.0262	–0.0210	0.0082
(percent of total change explained)	(56)			
Number of children	–0.0022	0.0249	–0.0207	0.0092
(percent of total change explained)			(6)	
Health	–0.0022	0.0222	–0.0219	0.0065
(percent of total change explained)		(11)		(11)
Education	–0.0024	0.0237	–0.0223	0.0062
(percent of total change explained)		(6)		(14)

Notes: Simulations are based on a specification with year effects restricted to be the same across age groups for each sex. The coefficient estimates from this specification are not shown, and are available upon request. All monetary amounts except the log wage are measured in millions of year-2010 dollars. See Table 2 for additional notes.

Table 5
Counterfactual Simulations of Annual Average Growth Rate of Full-Time Equivalent Weeks Worked/52 (FTW), No Controls for Calendar Time

	Men		Women	
	25–61	62–69	25–54	55–69
1965–1988 annual growth rate	–0.0020	–0.0223	0.0240	0.0040
1989–2010 annual growth rate	–0.0047	0.0121	0.0004	0.0184
Observed change	–0.0027	0.0344	–0.0236	0.0144
Predicted change	–0.0021	0.0306	–0.0191	0.0176
Counterfactual change, replacing 1989–2010 values with 1965–88 values of explanatory variables (percent of total change explained)				
Economic variables				
Wage rate	–0.0023	0.0305	–0.0193	0.0186
Average tax rate	–0.0017	0.0281	–0.0171	0.0177
(percent of total change explained)	(20)	(8)	(11)	
OASI	–0.0017	0.0329	–0.0126	0.0184
(percent of total change explained)	(10)	(13)	(19)	
[base]	[–0.0019]	[0.0377]	[–0.0155]	[0.0185]
PDVBEN65	–0.0017	0.0327	–0.0117	0.0176
(percent of total change explained)	(12)		(22)	
[base]	[–0.0019]	[0.0340]	[–0.0151]	[0.0175]
Gain from early claiming	–0.0022	0.0316	–0.0192	0.0188
[base]	[–0.0021]	[0.0315]	[–0.0192]	[0.0186]
Gain from later claiming	–0.0021	0.0298	–0.0202	0.0171
(percent of total change explained)		(10)		
[base]	[–0.0021]	[0.0335]	[–0.0196]	[0.0175]
Pension coverage	–0.0021	0.0306	–0.0191	0.0165
(percent of total change explained)				(6)
Demographic variables				
Marital status	–0.0008	0.0311	–0.0211	0.0174
(percent of total change explained)	(61)			
Race/ethnicity	–0.0007	0.0303	–0.0178	0.0179
(percent of total change explained)	(66)		(7)	
Number of children	–0.0023	0.0303	–0.0182	0.0193
(percent of total change explained)			(5)	
Health	–0.0021	0.0297	–0.0190	0.0177
Education	–0.0025	0.0245	–0.0184	0.0168
(percent of total change explained)		(20)		

(*continued*)

Table 5 (*continued*)

	Men		Women	
	25–61	62–69	25–54	55–69
Aggregate variables				
Minimum wage	–0.0014	0.0275	–0.0182	0.0182
(percent of total change explained)	(32)	(10)	(5)	
SSDI award and application rates	0.0011	0.0258	–0.0153	0.0174
(percent of total change explained)	(154)	(16)	(20)	
Net imports	–0.0025	0.0295	–0.0199	0.0174
Life expectancy	–0.0029	0.0467	–0.0195	0.0200

Notes: Simulations based on a specification with no year effects. The coefficient estimates from this specification are not shown. See Table 2 for additional notes.

counterfactual simulation results based on these estimates are shown in Table 5, using the predicted values as the baseline. Qualitatively, the results are quite similar to those in Table 4. The simulated effects of the aggregate variables are shown at the bottom of Table 5. The results suggest that changes in the minimum wage and the SSDI award rate can account for part of the changes in employment growth. However, these results should be interpreted cautiously given that they are identified by the very strong assumption of the absence of unobserved aggregate trends correlated with the included variables.[29]

(3) Another issue of interest is the sensitivity of the results to the sample period used in estimation. We use all available years (1965–2010), but it is possible that behavior has changed over time, and as a result the restriction that the coefficients do not vary over time could be incorrect. This seems plausible given the relative lack of explanatory power of many of the explanatory variables. We reestimated the models for two subperiods: 1965–91 and 1992–2010. This choice of periods is motivated by the availability of some additional data on the SSDI award rate beginning in 1992. Many of the coefficient estimates (not shown) are qualitatively and quantitatively similar in the two periods, but there are some notable differences as well. The simulation results (not shown) are quite different in some cases. This is not surprising: If changes in the values of the explanatory variables cannot account for much of the employment trends, changes in the effects of the explanatory variables are likely to have played a role.

(4) As noted above, the specification of cohort effects is important for identification. The results reported so far use a cubic polynomial in birth cohort. We tried several other specifications, including two-year fixed effects, four-year fixed effects, and

29. Beginning in 1992, data on SSDI applications and awards are available by age group. This provides an additional source of variation beyond the pure time series available back to 1965. As noted above, we expect a negative effect of the award rate on employment, other things equal. The results (not shown here) reveal small positive effects of the award rate on employment for men, a negative effect for younger women, and no effect for older women. Unfortunately, a counterfactual simulation is not possible because we lack age-specific award rate data before 1992.

no cohort effects. The results (not shown here) indicate that the explanatory power of OASI is smaller in the fixed effects specification for men, but larger for younger women.

(5) To check if the results are sensitive to the choice of periods for the simulations, we recomputed the simulations for an alternative pair of periods: 1980–88 as Period 1 and 1998–2006 as Period 2. These periods correspond to the turning point for labor supply at older ages (Period 1) and a recent period before the Great Recession (Period 2). The results (not shown) are qualitatively very similar to the original analysis. For older men, OASI and education are still the only factors that can explain a significant part of the change in the annual average growth rate between periods. For young women, race/ethnicity and number of kids continue to be the only factors that can explain the change in growth rates. But for this set of periods, the percentage changes in growth explained by these factors are much higher. For older women also, the same factors as in the original analysis continue to have the most explanatory power. For younger men, marital status, race, and pensions have increased explanatory power.

(6) Finally, we estimated several specifications that included spouse variables for married individuals.[30] A family or collective labor supply model implies that the spouse's wage rate should be included in the specification. The spouse's predicted wage rate had a statistically significant coefficient estimate for two of the four groups, but changes in the spouse's wage rate had no explanatory power in simulations (results not shown here). In another specification, the spouse's age, education, employment. and/or earnings were included. Counterfactual simulations (not shown here) indicated that changes in the spouse's education could explain 6–12 percent of observed employment changes for all four groups, and changes in spouse's employment status could explain 9 percent of the increase in employment for older men. The explanatory power of the other variables remained unchanged. These are interesting findings, but it is quite likely that spouse earnings and employment are endogenous, so these results are mainly of descriptive interest.

D. Discussion

The main goal of this paper is to explain the divergence in employment trends by age group in recent years. Our results suggest three partial explanations for men. The first is demographic change (other than educational composition, discussed below), specifically the delay in first marriage and increases in the population share of nonwhite and Hispanic men. Most men eventually marry, and despite the large increase in the share of younger men who have never married, there has been no increase among older men (see Table 3). Never-married men are much less likely to work at any age, so the delay in marriage can explain reduced employment growth of younger men but had no impact on older men. In addition, while the increase in the share of divorced, widowed, and separated men was about the same for both age groups (four percentage points), there is a negative effect of this marital status on employment only for younger men.

30. The spouse's age, education, predicted wage rate, observed employment (FTW), and observed annual earnings (including spouses outside the 25–69 age range) were added to each married individual's record before collapsing the data to the cell level, with means taken over the married subsamples. The spouse variables are included in the regression interacted with the fraction married in the cell.

In quantitative terms, the change in the rate of employment growth is much larger for older men. The increase across periods in the annual rate of male employment growth at older ages was 0.0344 and the decline at younger ages was –0.0027, so the difference across age groups in the rate of change was 0.0363. The results in Table 2 indicate that the change in marital status can explain a large share of the small decline in growth at younger ages but only a very small share (3 percent) of the much larger *difference* in the change in growth rates by age. The same logic implies that the change in racial and ethnic composition can explain only a small share of the divergence in employment growth for older and younger men. Thus, demographic change was not a major factor in the divergence in employment growth by age.

The second explanation is the increase in educational attainment. This can account for 10 percent of the observed divergence in employment growth by age for men, also a rather small share of the change.[31]

The third explanation is Social Security reform. Our results add to a growing body of evidence indicating that the decline in benefits and the increased incentive to delay claiming have contributed to the increase in employment at older ages. Our study is the first to investigate the impact of these reforms at younger ages. The results show that OASI benefits have a positive impact on labor supply at younger ages, and the decline in benefits contributed to the reduction in employment at younger ages. As noted above, a positive effect of benefits at younger ages is consistent with the lifecycle framework, although our reduced form approach does not reveal whether a lifecycle explanation for the finding is warranted. The contribution of OASI reform to the 0.0412 difference in the change in the average annual growth rate is 9 percent.[32] Thus, the combination of changes in marital status, educational attainment, and Social Security policy can explain only about one-fifth of the observed age difference in the change in employment growth for men.

For women, OASI reform contributed to the divergence in growth across age groups, but only via increasing employment at older ages. The estimates indicate that OASI benefits have a positive impact on labor supply at younger ages, but the effect is too small to matter. Using the OASI baselines, the increase in employment growth for women at older ages was 0.0154 and the decline at younger ages was –0.0218, so the difference in the change across age groups was 0.0372. The results in Table 2 indicate that the change in OASI reform can explain 0.0013 of this difference, or 3.5 percent.[33] Education can explain 0.0023, or 6 percent of the observed change. So we can explain only about 10 percent of the observed change for women.

VI. Conclusions

Social Security reforms, the delay in first marriage, and changes in the education distribution can account for about 20 percent of the recent divergence in employment growth by age. As discussed in the introduction, the impact of Social

31. This is calculated as (0.0344 – 0.0305) – (–0.0027 –[–0.0029]) = 0.0037, which is 10 percent of 0.0363.
32. (0.0390 – 0.0357) – (–0.0022 – [–0.0016]) = 0.0039 out of the observed 0.0412. See the OASI row in Table 2.
33. (0.0154 – 0.0154) – (–0.0218 – [–0.0205]) = 0.0013 out of the observed 0.0372. See the OASI row in Table 2.

Security reforms should persist, because all future cohorts are affected. The impact of increases in educational attainment is unlikely to persist because the major changes of recent decades have ended, and in the absence of unforeseen changes future retiring cohorts will have an educational composition similar to today's retirement-age cohorts.

The future effects of delayed marriage are more difficult to predict. Median age at first marriage increased by two full years from 2000 to 2010 for men, and increased by four and half years from its low point in the 1950s and 1960s (Elliott et al. 2012). The share of the male population that was never married by age 45 increased by three percentage points from 1990 to 2010. Even if these trends have run their course, they will have persistent effects as long as the share never married remains low at older ages.

OASI reforms have contributed modestly to the age divergence, but they are clearly not the main factor. We have been unable to convincingly analyze the impact of SSDI policy, but we speculate that it might have played a significant role in reducing labor supply at younger ages, as suggested by Autor and Duggan (2003), Duggan et al. (2007), and others. If this is correct, the main implication is that SSDI policy reforms to tighten screening criteria may be of more importance than OASI reforms. However, Low and Pistaferri (2015) have argued that tighter screening criteria would reduce social welfare. Further research on the role of SSDI should be a priority.

Another important area for future research is the impact of labor demand and institutional factors on the divergence in employment growth by age. Age discrimination and policies intended to counteract it is one example of such a factor. These factors are more difficult to measure than the determinants studied here, but the payoff to such an effort could be high.

Appendix

Data

A. Dependent Variables

The main outcome analyzed in this paper is full-time-equivalent weeks worked per year (FTW), defined as weeks worked in the previous calendar year if usual hours worked were at least 36, and weeks worked divided by two if usual hours worked were between one and 35. The measure is divided by 52 to restrict it to the unit interval for ease of interpretation. An alternative outcome analyzed is a categorical measure of labor force status in the week prior to the survey date. An individual is defined as employed if the employment status recode indicates that he was employed or searching for work.

B. Social Security

We use CPS earnings from ages 25–59 to compute OASI benefits, assuming continuous employment at cell-specific average annual earnings (truncated at the maximum taxable amount), as in Blau and Goodstein (2010). The CPS data are augmented with published Social Security Administration data on median covered earnings by age prior to the availability of CPS data. Cells are defined by gender, age, education, and year. We use ages 25–59 because most individuals are finished with schooling by age 25 and have not yet retired by age 59. Thus we do not have to deal with issues of selection on entry to and exit from employment, at least for men. This provides the 35 years of earnings used in the computation of Average Indexed Monthly Earnings (AIME), the basis for determining the Social Security benefit. This is an arbitrary approach, but the resulting benefit is highly correlated with benefits computed using alternative assumptions about the earnings history (see Blau and Goodstein 2010). We do the same for women, despite the fact that many women do not work continuously. For women the assumption of no selection bias is implausible, but there is no straightforward way to deal with this.

Benefits are computed under three alternative assumptions about the age of claiming: 62, 65, and 70. We use the batch version of the SSA computer program "anypia" to compute the OASI benefit for each of the three OASI claiming ages. We compute the Expected Present Discounted Value (EPDV) of benefits using standard mortality schedules and an assumed real interest rate of 3 percent. Benefits are assumed to be constant in real terms (as they have been since the automatic COLA was introduced). Benefits are discounted to the year in which the individual turns age 55. This is arbitrary but has no impact on the results.

The details of the earnings and benefit calculations are as follows. We use data on wage-salary income, with the bottom and top 1 percent within each cell trimmed. Earnings are capped at the taxable maximum earnings applicable in each year. Earnings data from the CPS for calendar years1961–2010 (from March 1962–2011 files) are used to compute the cell mean of positive values of capped earnings. We use published SSA median earnings data for various years and ages from 1937–60, prior to the availability of CPS data. The medians are transformed to means using mean/median ratios from the CPS. The means are then capped, and data are filled in for missing years and ages using regression imputations. Combining CPS and SSA data, we have information for birth years 1878–1985 at ages 25–59. The specific steps involved in combining CPS and SSA data are as follows:

1. Compute the CPS mean/median ratio, and run sex-age-group-specific regressions to project backward.
2. Compute the ratio of education-specific mean earnings to overall mean earnings using the CPS 1961–2010, for use in adjusting 1937–60 SSA data, which are not available by education. Regress the ratio on year by sex and age group.
3. Apply the adjustments from steps a and b to the SSA data on median earnings, which are available only for selected ages and years. Interpolate missing years and ages.
4. Run sex-education-group-specific log earnings regressions for ages 25–59 on a cubic in age, a cubic in birth year, and interactions, in order to smooth earnings profiles.
5. Use the regression coefficients to generate predicted earnings paths for each of the alternative retirement age scenarios, assuming constant real earnings at ages 60 and above (using the age-59 value). We also use average earnings

Table A1
Counterfactual Simulations of the Level of Full-Time Equivalent Weeks Worked/52 (FTW)

	Men		Women	
	25–61	62–69	25–54	55–69
1965–88 mean FTW	0.830	0.379	0.450	0.266
1989–2010 mean FTW	0.807	0.327	0.613	0.355
Observed change	–0.023	–0.053	0.163	0.089
Predicted change	–0.023	–0.053	0.163	0.089
Counterfactual change, replacing 1989–2010 values with 1965–88 values of explanatory variables (percent of total change explained)				
Economic variables				
Wage rate	–0.032	–0.059	0.155	0.128
(percent of total change explained)			(5)	
Average tax rate	–0.071	–0.018	0.146	0.090
(percent of total change explained)		(66)	(10)	
OASI	–0.014	–0.044	0.180	0.087
(percent of total change explained)	(52)			
[base]	[–.029]	[–0.039]	[0.155]	[0.087]
PDVBEN65	–0.014	–0.046	0.194	0.088
(percent of total change explained)	(52)			
[base]	[–.029]	[–.045]	[.0156]	[.088]
Gain from early claiming	–0.024	–0.054	0.163	0.089
[base]	[–0.023]	[–0.054]	[0.163]	[0.088]
Gain from later claiming	–0.022	–0.048	0.149	0.090
(percent of total change explained)	(6)		(8)	
[base]	[–0.023]	[–0.045]	[0.162]	[0.090]
Pension coverage	–0.019	–0.052	0.159	0.085
(percent of total change explained)	(18)			(5)
Demographic variables				
Marital status	0.009	–0.052	0.147	0.089
(percent of total change explained)	(137)		(10)	
Race/ethnicity	–0.006	–0.053	0.175	0.087
(percent of total change explained)	(74)			
Number of children	–0.022	–0.047	0.167	0.103
(percent of total change explained)		(11)		
Health	–0.034	–0.061	0.156	0.091
Education	–0.028	–0.084	0.166	0.069
(percent of total change explained)				(23)

Notes: Simulations are based on the coefficient estimates shown in Table 1. See Table 2 for additional notes.

Table A2
Sample Means for Estimation Samples

	Men		Women	
	25–61	62–69	25–54	55–69
FTW	0.815	0.345	0.546	0.315
Log wage	3.195	3.179	2.803	2.732
Average tax rate	0.370	0.380	0.350	0.360
PDVBEN65	0.136	0.099	0.129	0.087
Gain from early claiming	–0.0053	–0.0082	–0.0087	–0.0084
Gain from later claiming	–0.0053	–0.0071	0.0041	–0.0006
Pension coverage	0.547	0.603	0.468	0.456
Divorce, widowed, or separated	0.116	0.149	0.172	0.318
Never married	0.175	0.054	0.138	0.052
Black	0.103	0.086	0.124	0.099
Other race	0.041	0.028	0.045	0.029
Hispanic	0.097	0.050	0.098	0.057
Number of kids<6	0.339	0.058	0.391	0.069
Number of kids<18	1.101	0.235	1.363	0.263
Health very good	0.313	0.256	0.322	0.268
Health good	0.258	0.330	0.282	0.345
Health fair	0.072	0.173	0.0788	0.164
Health poor	0.026	0.079	0.0216	0.063
Fraction of year unable to work due to illness	0.125	0.334	0.110	0.272
High school dropout	0.190	0.375	0.169	0.325
High school graduate	0.344	0.297	0.378	0.374
Some college	0.210	0.142	0.228	0.163
Age	41.4	65.3	38.8	61.5
Sample size	6,808	1,472	5,520	2,760

Notes: FTW = (Full-time equivalent weeks worked)/52. PDVBEN65 = Present Discounted Value of OASI benefit if claimed at age 65 (discounted to age 55). Gain from early claiming = PDVBEN62 – PDVBEN65. Gain from later claiming = PDVBEN70 – PDVBEN65.

growth by year for future years implied by the predicted earnings paths to generate wage index and price index values in future years (2011+).

C. Other Variables

Data on pensions and health insurance are available beginning with the 1980 CPS survey. However, we do not use the health insurance data because the trends show unexplained breaks related to changes in the survey. Pension coverage is measured by enrollment, and we limit the universe for measuring coverage to nonagricultural private sector workers. Pensions are important only if an individual is covered for a long period of time and expects to receive a benefit. We approximate this by measuring pension coverage at ages 45–55 and assigning coverage at those ages as a permanent characteristic. The type of pension is not recorded.[34]

We use data on self-reported health and days lost due to illness from the National Health Interview Survey, downloaded from the Minnesota Population Center's IHIS web site. Data on work days lost due to illness are available beginning in 1969. The reference period changed from the previous two weeks to the previous calendar year in 1997. The self-reported health measure is available as a four-point scale (poor, fair, good, excellent) from 1972–81 and as a five- point scale (poor, fair, good, very good, excellent) beginning in 1982.

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34. Cohorts that are never observed at these ages are assigned the mean value of the 1925–29 cohort if they were born before 1925, or the mean value of the 1963–65 cohort if they were born after 1965. We explored another source of aggregate data on pensions from EBRI, but while this source provides a time series of total coverage by DB and DC plans, it does not provide a measure of the eligible population, so a coverage rate cannot be computed. It is also an aggregate time series, with no variation across groups. Other sources of pension data such as the National Compensation Survey provide only a very limited time series.

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