NBER WORKING PAPER SERIES

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Sita Slavov
Devon Gorry
Aspen Gorry
Frank N. Caliendo

Working Paper 23506
http://www.nber.org/papers/w23506

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2017

This research was supported by the Mercatus Center at George Mason University. The findings and conclusions expressed are solely those of the authors. We thank Muzhdah Karimi for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w23506.ack

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NBER Working Paper No. 23506
June 2017
JEL No. D14,H31,H55

ABSTRACT

Typical neoclassical life-cycle models predict that Social Security has a large and negative effect on private savings. We review this theoretical literature by constructing a model where individuals face uninsurable longevity risk and differ by wage earnings, while Social Security provides benefits as a life annuity with higher replacement rates for the poor. We use the model to generate numerical examples that confirm the standard result. Using several benefit and tax changes from the 1970s and 1980s as natural experiments, we investigate the empirical relationship between Social Security and private savings and find little to support the strong predictions from the theoretical model. We explore possible reasons for the divergence between theoretical predictions and empirical findings.

Sita Slavov
Schar School of Policy and Government
George Mason University
3351 Fairfax Drive, MS 3B1
Arlington, VA 22201
and NBER
sslavov@gmu.edu

Devon Gorry
Department of Economics and Finance
Utah State University
3565 Old Main Hill
Logan, UT 84322-3565
devon.gorry@usu.edu

Aspen Gorry
Department of Economics and Finance
Utah State University
Logan, UT 84322-3565
aspen.gorry@gmail.com

Frank N. Caliendo
Department of Economics and Finance
Utah State University
Logan, UT 84322-3565
frank.caliendo@usu.edu

I. Introduction

Projections by the Social Security Administration suggest that the Old Age and Survivors Insurance (OASI) trust fund will be depleted in 2035. To shore up the program's finances, some policy makers propose cutting benefits relative to current law. However, other policy makers express concern that benefit cuts will harm lower-income retirees and instead propose expanding the Social Security system by increasing benefits for large segments of the population, with these increases funded by raising payroll taxes. Because Social Security requires contributions from workers and provides benefits during retirement, theory predicts that expanding the program will crowd out private saving for individuals who are not liquidity constrained. Besides retirement income, Social Security also provides life insurance in the form of benefits paid to the dependent children of deceased workers. Theory predicts that expanding Social Security will also crowd out private life insurance purchases among those who are not liquidity constrained. In this paper, we review the existing theory and then explore the empirical evidence for its predictions.

Understanding the effect of Social Security on private saving and life insurance holdings is important for a few reasons. First, behavioral responses are an important factor in evaluating how well Social Security meets its objectives. Any potential welfare gains from mandatory saving and risk sharing through Social Security depend on the degree to which households attempt to unwind their Social Security contributions through reductions in private saving and insurance holdings. For instance, Hosseini (2015) shows that the large welfare gains from mandatory annuitization are mostly washed out by the distortions that it causes to the equilibrium price of annuity contracts, as adverse selection causes high mortality individuals to exit the private annuity market. Second, the impact of changes in Social Security rules to maintain fiscal solvency in the face of a demographic shock depend on how individuals respond (Kitao 2014; Bagchi 2016). Finally, if Social Security causes large reductions in personal savings, then growth in

the generosity of the Social Security system in recent decades may help to explain the precipitous decline in the aggregate personal saving rate in the U.S. from near 10% in the 1980s to close to zero in more recent years (Parker 1999; Gokhale, Kotlikoff, and Sabelhaus 1996).

To motivate our empirical work, we provide a brief review of the predictions of a basic, neoclassical life-cycle model. We use a variant of Yaari's (1965) classic model to review the theoretical effect of Social Security on private savings by income class. The model features rational individuals who face uninsurable longevity risk and differ by their level of earnings. Social Security provides longevity insurance because it pays benefits as a life annuity. Moreover, it redistributes wealth from high to low wage earners because the benefit-earning rule is progressive. We document that Social Security has a large effect on private asset holdings at retirement among all income groups below the tax cap.

Next, we empirically examine the impact of plausibly exogenous variation created by the 1977 and 1983 Social Security reforms, as well as a reduction in the life insurance value of Social Security that was phased in between August 1981 and April 1985. The 1977 reform reduced benefits for individuals born in 1917 and later, but not for individuals born in 1916 and earlier. The 1983 reform increased payroll taxes for self-employed individuals relative to wage earners; it also increased the full retirement age for individuals born in 1938 and later. An increase in the full retirement age is equivalent to a cut in Social Security wealth. The reduction in the life insurance value of Social Security comes from a 1981 reform that eliminated dependents benefits for college students aged 18-21 beginning in 1982 with a phase out period completed in 1985.

There is a large macroeconomic literature that assesses the impact of Social Security (and Social Security reform) on private savings. This literature uses calibrated, dynamic general equilibrium models (e.g., Blau 2016; Conesa and Garriga 2008; and Kitao 2014). Relative to the macroeconomic literature that focuses primarily on aggregate savings behavior, our review of theoretical predictions focuses on the differential impact of Social Security on private savings across different income groups.

Numerous microeconometric studies have also estimated the relationship between Social Security wealth (or pension wealth more generally) and private saving. Some early studies use aggregate time series data (e.g., Feldstein 1974, 1996; Leimer and Lesnoy 1982), while others use micro data (Feldstein and Pellechio 1979; Diamond and Hausman 1984; Bernheim 1987; Bernheim and Levin 1989; Gale 1998; Gustman and Steinmeier 1998; Kotlikoff 1979). These studies find mixed results. Some find that Social Security wealth crowds out private saving almost one for one; others suggest that Social Security wealth reduces private saving somewhat, but not one for one, so that total household saving (public plus private) increases in the presence of Social Security.

One shortcoming of this approach – which relies on examining the correlation between Social Security wealth and private saving, after controlling for observables – is that there are likely numerous unobservable factors that influence both Social Security wealth and saving preferences. Our contribution is to use plausibly exogenous policy-induced variation in Social Security wealth to estimate the impact on saving. Similar studies have been done for other countries (e.g., Attanasio and Brugiavini 2003; Aguila 2011; Feng, He, and Sato 2011; Attanasio and Rohwedder 2003), and these studies have found that public pensions tend to crowd out private savings in Italy, Mexico, China, and the United Kingdom. However, evidence for the U.S. is much more limited. Engelhardt and Kumar (2011) examine the impact of defined benefit pension wealth on non-pension saving for the U.S., using variation in the benefit accumulation rules for such plans. They find that increases in pension wealth reduce non-pension saving. However, they do not focus on Social Security.

In contrast to previous microeconometric studies based on policy-induced variation in public and private pension wealth, we find very little evidence that policy-induced variation in Social Security has affected private saving. There is some evidence that payroll tax increases in 1983 may have reduced saving, but there is not much evidence that the benefit changes enacted in 1977 and 1983 had an impact

on saving. We also find no evidence that the elimination of dependent benefits for college students reduced life insurance holdings.

We explore several possible explanations for our lack of a finding. First, our standard errors are large, so we cannot rule out the possibility that there is an economically meaningful effect. Second, it is possible that individuals chose to delay retirement, rather than save more, in response to the benefit cuts that we study. Third, it is possible that individuals simply lack knowledge about their Social Security benefits and were unaware of the policy changes. Fourth, the timing of information about the policy changes could play a role in determining when behavioral changes are observed in the data. If the policy changes were anticipated in advance, behavior may have adjusted well before the policy changes were implemented. Finally, expectations may also affect the timing of behavioral responses. For example, Ricardian equivalence suggests that individuals fully anticipate policy changes designed to restore actuarial balance to the Social Security program. Thus, adjustment to these policy changes may occur long before they are implemented or even discussed publicly.

The remainder of this paper is organized as follows. Section II reviews the predictions from a standard neoclassical life cycle model. Section III presents our empirical methods. Section IV presents our empirical results. Section V provides a discussion exploring why the empirical findings differ from the theoretical results. Section VI concludes.

II. Brief review of predictions from a standard, neoclassical life-cycle model

This section reviews a standard version of a life-cycle consumption savings model to understand the theoretical implications of Social Security on private savings. Before we begin, we emphasize that there is no such thing as *the* neoclassical life-cycle model; instead, it is a framework for analyzing the effects of public policies on decision-making and welfare. The framework assumes that individuals behave

rationally given the risks and policies that they face. Beyond this assumption, we do not attempt to include every feature that has been considered in the large literature using such models to study consumption and savings behavior. We focus on a stylized setting where private insurance is missing, capital markets are complete, and labor supply is inelastic.

Age is continuous and is indexed by t. At each moment in time an infinitely divisible cohort of unit mass is born. Individuals are born at $t = 0$ and die no later than $t = 1$. The probability of surviving to age t from the perspective of age 0 is $S(t)$, where $S(0) = 1$ and $S(1) = 0$. Individuals receive an exogenous and constant flow of wage income w up to the exogenous retirement date t_R, and they receive constant Social Security benefits $b(w)$ in the form of a life annuity after retirement. Wage income is taxed at the Social Security tax rate τ. Individuals differ only by wage income. Each individual draws their income from the p.d.f. $g(w)$ with support $[0,1]$.

An individual's consumption is $c(t)$ and flow utility from consumption is $u(c(t))$, where $u' > 0$ and $u'' < 0$. Annuity markets are completely closed and saving is done in a zero-interest storage technology $k(t)$. The assets of the deceased are bequeathed to survivors. Bequest income per new born B is collected at $t = 0$. Factor prices are fixed because we focus on a small, open economy.

We study stationary equilibria in which individuals behave rationally in an environment where they face longevity risk, the Social Security budget is balanced, and the transmission of wealth across generations is consistent in the sense that the aggregate assets of the deceased equal the inheritances of the living. We will compare equilibrium assets at retirement with and without Social Security.

a. Equilibrium

At $t = 0$ individuals learn their wage type w, and they take as given τ, $b(w)$, and B and choose $(c(t), k(t))_{t\in[0,1]}$ according to:

$$max \int_0^1 S(t)u(c(t))dt,$$

subject to

$$\dot{k}(t) = (1-\tau)w - c(t), for\ t \in [0, t_R],$$

$$\dot{k}(t) = b(w) - c(t), for\ t \in [t_R, 1],$$

$$k(0) = B, k(1) = 0.$$

The Social Security system runs a balanced budget and hence the tax rate τ must satisfy the government's budget constraint for a given benefit-earning rule $b(w)$

$$\tau R \int_0^1 g(w)wdw = \int_0^1 g(w)b(w)dw,$$

where R is the ratio of workers to retirees, $R \equiv \int_0^{t_R} S(t)dt \,/\, \int_{t_R}^1 S(t)dt$. There are no inefficiencies in financing Social Security. The government can store wealth at zero interest just like the private market. However, Social Security pays an above-market implicit rate of return because it pools the contributions of the deceased to pay an annuity to the living.

The inheritances of the living must equal the assets of the deceased. We follow the standard assumption that bequest income is spread evenly across the surviving population. Equilibrium bequest income B solves the following implicit function

$$B = \int_0^1 \int_0^1 g(w)\left(-\dot{S}(t)\right)k(t|w, B)dtdw.$$

For a given income distribution $g(w)$ and Social Security benefit rule $b(w)$, a Stationary Equilibrium consists of (i) individual consumption and saving decisions $\{c^*(t|w), k^*(t|w)\}$ for each wage type w that satisfy individuals' optimization problem, (ii) a Social Security tax rate τ^* that balances the government's aggregate budget constraint, and (iii) bequest income B^* that balances the inheritances of newborns with the assets of the deceased.

b. Numerical Example

The parameters that we need to select in order to generate numerical examples are the retirement age t_R, the survival function $S(t)$ which in turn pins down the ratio of workers to retirees R, the utility function $u(c)$, the wage density $g(w)$, and the Social Security benefit rule $b(w)$. Once these parameters are chosen, we can solve for the equilibrium quantities $\{c^*(t|w), k^*(t|w), \tau^*, B^*\}$.

We imagine an individual who works from age 25 to age 65, and dies no later than age 100. Hence, $t_R = 40/75$. We follow Caliendo, Gorry, and Slavov (2016) and set the unconditional survival probabilities to $S(t) = 1 - t^{3.28}$.[1] We use CRRA utility $u(c) = c^{1-\sigma}/(1-\sigma)$ with $\sigma = 3$.

The Social Security benefit-earning rule is a piecewise linear function of an individual's wage. There are three kinks or bend points which change each year based on average wage growth, but we follow Alonso-Ortiz (2014) and others and assume that the bend points are the following constant multiples of the economy-wide average wage, 0.2, 1.24, and 2.47. Social Security replaces 90% of the individual's wage up to the first bend point, 32% of wage income between the first and second bend points, and 15% of wage income between the second and third bend points. The third bend point, 2.47,

[1] This survival function is estimated from Social Security Administration cohort mortality tables for men born in 1990.

corresponds to the maximum taxable earnings (beyond which individuals do not pay payroll taxes or receive additional benefits). Therefore, Social Security replaces 0% of wages beyond the third bend point.

Finally, wages follow a beta distribution with density

$$g(w) = \frac{w^{\gamma-1}(1-w)^{\beta-1}}{\int_0^1 w^{\gamma-1}(1-w)^{\beta-1}dw}, for\ w \in [0,1].$$

There are two parameters, γ and β, to choose. To abstract from the portion of the income distribution above the third bend point (or maximum taxable earnings), we assume that the top wage earner with $w = 1$ earns 2.47 times that of the average earner. In addition, the 2015 US Census reports a Gini coefficient of 0.479 on the distribution of income. This gives us two targets with which to calibrate γ and β

$$E(w) = \frac{\gamma}{\gamma+\beta} = \frac{1}{2.47},$$

$$Gini = \frac{1}{2E(w)}\int_0^1\int_0^1 g(x)g(y)|x-y|dxdy = 0.479.$$

By setting $\gamma = \beta/1.47$ we match the desired mean. Given this restriction, $\beta = 1.8$ delivers a Gini coefficient of 0.3463, somewhat less than the desired target but about the best we can do given the (truncated) beta distribution that we have assumed. We would expect the model to somewhat understate the true degree of inequality given that we are ignoring individuals above the maximum taxable income.

Using this benefit-earning rule, together with the survival function $S(t)$ and the wage density $g(w)$, we find that the balanced-budget tax rate is $\tau^* = 19.463\%$. This is somewhat larger than the

current tax in the U.S. (10.6%), but of course the current rate is not sufficient to balance the expected future budget under current life expectancies. According to the 2016 Social Security Trustees Report, eliminating the infinite horizon actuarial shortfall in the retirement and disability programs combined would require a 4.2 percentage point increase in the payroll tax. That increase – which overestimates the additional payroll tax needed to eliminate the shortfall in the retirement program alone – would still result in a tax rate that is below the one in our model. However, our analysis also ignores the mass of individuals who max out their Social Security contributions each year and would increase the overall revenue. To test for sensitivity, we compute results by setting the tax to 10.6% and ignoring the budget balance and find that the qualitative results that we emphasize do not change.

We compare two equilibria. The first equilibrium features a Social Security program as described above. The second equilibrium has no Social Security with $\tau = b(w) = 0$ for all w. Everything else is the same across the two equilibria.

Table 1 reports equilibrium assets at retirement as a fraction of lifetime income (wage income plus bequest income) for individuals at each of the three bend points, as well as for average earners. Without Social Security, all individuals save about 41% of their lifetime income. The impact of Social Security on private savings is enormous in this theoretical model. The effect is especially large among the poor who face the highest replacement rates. Social Security wipes out as much as half of the private savings of those near the earnings cap, and it wipes out all of the private savings of those at the first bend point. Note that while the declines in private savings as a percent of lifetime income are especially large for the poor, the aggregate reduction in dollars saved still may be smaller as their wages are much lower than other income groups considered. Including other features will alter the precise quantitative effect of Social Security on private savings, but as long as individuals have perfect foresight, they tend to unwind a significant portion of mandatory saving with reductions in private saving. Indeed, in our model if we were

to abstract from longevity risk and wage heterogeneity, then Social Security taxation would crowd out private saving one for one.

In addition to private savings, Social Security may also affect insurance holdings. In theory, Social Security will reduce the demand for both life insurance and demand for annuity insurance. In our model, bequests are accidental rather than the result of an explicit bequest motive. With the addition of an explicit bequest motive, individuals would demand private life insurance. The magnitude of the demand for life insurance would depend on the strength of the bequest motive and on the level of life insurance provided publicly through Social Security. Hence, Social Security may crowd out private life insurance holdings in addition to its crowding out of private savings (Hong and Ríos-Rull 2007; Li 2016). Similarly, Social Security also reduces the demand for private annuities, because the program offers significant annuitization through its payment of benefits as a life annuity. This effect is particularly large for those with high mortality. Since Social Security disproportionately causes those with the highest mortality to exit the private annuity market, it worsens adverse selection problems in the private annuity market (Hosseini 2015).

III. Empirical Methods

Our empirical strategy relies on plausibly exogenous variation from three policy experiments. Our first policy experiment, the 1977 reform, resulted in reduced benefits for individuals born in 1917 and later. The purpose of the reform was to correct a mistake in a 1972 law that had introduced automatic cost-of-living adjustments for Social Security benefits. The indexation formula in the 1972 law resulted in cost-of-living adjustments that far exceeded inflation for individuals who had not yet claimed. The correction made in 1977 was phased in for individuals born between 1917 and 1921. The lower benefits

paid to those born between 1917 and 1921, relative to those born in 1916 and earlier, are sometimes referred to as the "notch," with individuals in the 1917-1921 birth cohorts referred to as "notch babies."[2]

Our second policy experiment, the 1983 reform, increased payroll taxes for self-employed individuals relative to wage earners. Figure 1 shows the total statutory Old Age, Survivor, and Disability Insurance (OASDI) payroll tax rate faced by employees (including employer and the employee shares) as well as the self-employed. Starting in 1984, it is clear that the payroll tax rate for the self-employed rose sharply relative to that of employees. The 1983 reform also increased the full retirement age for individuals born in 1938 and later.[3] While benefits could still be claimed as early as 62 with an actuarial reduction, the increase in the full retirement age effectively reduced the monthly benefit payable at each possible claiming age.[4] Thus, holding taxes and benefit levels at full retirement age constant, the increase in the full retirement age was equivalent to cutting the present value of lifetime benefits.

Our third policy experiment comes from a 1981 reform that reduced dependents' benefits paid to children. Prior to August 1981, the children of deceased, retired, or disabled workers could receive Social Security dependent benefits if they were either under 18, or 18-21 and a full-time high school or college student. Legislation passed in August 1981 started the process of phasing out benefits for college students aged 18-21, with the phase-out complete by April 1985. Benefits for secondary school students

[2] The error in the indexation formula effectively double-indexed benefits for individuals who had not yet claimed in 1972. Thus, double indexation primarily affected individuals born between 1910 and 1916, who turned 62 in 1972 or later but were not affected by the 1977 reform. We do not use the 1972 error as a policy experiment as individuals born before 1910 received ad hoc cost-of-living adjustments. See Social Security Administration (2004) and Krueger and Pischke (1992) for additional details.

[3] See https://www.ssa.gov/planners/retire/retirechart.html.

[4] The 1983 reform also gradually increased the delayed retirement credit, or the actuarial increase in the monthly benefit for each year of delayed claiming beyond full retirement age. The increases in the delayed retirement credit were phased in for individuals born in 1925 and later. However, very few individuals delay benefits beyond full retirement age (see, e.g., Goda et al. 2017). Also, the combination of the increases in the delayed retirement credit and the full retirement age in most cases reduce the benefit that individuals born in 1938 and later can receive regardless of the age at which they claim. For additional details, see https://www.ssa.gov/oact/ProgData/ar_drc.html.

older than 18 were eliminated by August 1982.[5] This reform reduced the expected present value of the life insurance benefits available from Social Security for individuals with dependent children.

We use the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey (NLS) to evaluate changes in savings due to decreases in benefits resulting from the 1977 reforms, and we use the Consumer Expenditure (CE) quarterly interview surveys to evaluate changes in savings due to the 1983 reform and the 1981 elimination of student benefits.

a. Panel Study of Income Dynamics

The PSID is an ongoing panel survey of U.S. households that began in 1968. The survey has been conducted each year until 1997 and every other year thereafter. We use data from 1968-1980. There are two family wealth variables available in 1968-1972 and again in 1975, 1979, and 1980.[6] First, families report whether they currently have savings equal to two months of income or more. Second, they report whether they have, at any point in the past five years, had savings equal to two months of income or more. The PSID also includes information on family money income and the ages of the head and spouse. We deflate income, expressing it in 1980 dollars, using the average Consumer Price Index for All Urban Consumers (CPI-U) over the survey year. In all of our analysis, we use the PSID longitudinal family weights.

We begin by estimating the following difference-in-differences model:

$$y_{it} = \beta_1 post_t + \beta_2 post_t T_i + \beta_3 inco_{\ \ it} + \beta_4 age_{it} + \gamma_t + \alpha_i + \epsilon_{it} \quad (1)$$

Here, y_{it} is one of the indicators that family i has savings equal to two months or more of income in year t; $post_t$ is a dummy equal to 0 for years before 1977 and 1 for 1977 and later; T_i is a treatment dummy equal to 1 if the household head was born in 1917 or later and zero otherwise; $incom_{\ it}$ is family money

[5] See DeWitt (2001) for additional details.
[6] These wealth variables are not available beyond 1980, although more detailed wealth variables are consistently available starting in the 1990s.

income; age_{it} is a set of dummies for the age of the head of household; γ_t is a year effect; α_i is a family fixed effect; and ϵ_{it} is a stochastic error term. The coefficient of interest is β_2, which is expected to be positive. That is, the probability of having savings equal to two or more months of income is higher for individuals whose benefits were cut following the 1977 reform. We would expect a higher income to cause higher savings. The age dummies control for the age profile of assets. The family fixed effects control for unobservable family heterogeneity, such as differences in the rate of time preference. The year dummies control for changes in macroeconomic conditions, differences in survey conditions, and other common factors that affect reported savings for all families in specific years.

Equation (1) does not take into account the degree of reduction in benefits that each post-1917 cohort experienced. Thus, we also estimate

$$y_{it} = \beta_1 post_t + \beta_2 post_t r_i + \beta_3 income_{it} + \beta_4 age_{it} + \gamma_t + \alpha_i + \epsilon_{it} \qquad (2)$$

where r_i is the percentage reduction in monthly benefits payable at full retirement age experienced by individual i as a result of the 1977 amendments. The reduction in monthly benefits is zero percent for individuals born before 1917, 13 percent for individuals born in 1917, 19 percent for individuals born in 1918, 26 percent for individuals born in 1919, and 29 percent for individuals born in 1920 or later.[7]

b. National Longitudinal Survey

The NLS is a panel survey of men representative of the United States population. We use the cohort of older men who were 45-59 in 1966, the first year of the surveys. Additional rounds of the survey continued either annually or biannually until 1983 and a final round was conducted in 1990. To measure savings, we use respondents' reported values of savings (including checking or savings accounts, or accounts with savings and loan companies or credit unions), bonds, and investments (which include

[7] We obtain the percentage reduction in benefits from https://www.ssa.gov/history/notchfile3.html.

bonds, stocks and mutual funds). These values are reported in 6 different years, four years prior to the policy and 2 years after the policy. We also create a total savings variable which is the sum of savings and investments. As with the PSID data, we deflate these variables using the CPI-U, expressing them in 1980 dollars. In all the analyses, we use NLS custom longitudinal weights.

We estimate the following difference-in-differences model:

$$y_{it} = \beta_1 post_t + \beta_2 post_t T_i + \beta_3 incom_{it} + \beta_4 age_{it} + \gamma_t + \alpha_i + \epsilon_{it} \quad (3)$$

where y_{it} represents an individual *i*'s savings, bonds, investments, or total savings in year *t*; $post_t$ is a dummy equal to 0 for years before 1977 and 1 for 1977 and later; T_i is a treatment dummy equal to 1 if the individual was born in 1917 and later and zero otherwise; $inco_{it}$ is an individual's reported income; ag_{it} is a set of individual age dummies to control for differences in savings behavior common across ages; γ_t is a year effect to control for macroeconomic conditions, differences in surveys, or common factors that impact savings in a given year; α_i is an individual fixed effect to control for unobserved individual heterogeneity that is constant across years; and ϵ_{it} is a stochastic error term. The coefficient of interest is again β_2. This coefficient should be positive if individuals compensate for lower expected Social Security payments by saving more.

To account for the degree of reduction in benefits, we also estimate the following:

$$y_{it} = \beta_1 post_t + \beta_2 post_t r_i + \beta_3 incom_{it} + \beta_4 ag_{it} + \gamma_t + \alpha_i + \epsilon_{it} \quad (4)$$

where r_i is the percentage reduction in benefits as outlined above.

c. Consumer Expenditure Surveys

The CE quarterly interview surveys are a rotating panel in which households report their income and expenditures in different categories for up to four consecutive quarters. We use the National Bureau of Economic Research (NBER) extracts of the CE. The NBER extracts aggregate the quarterly information

provided by each household and member to create annual income and spending variables in a set of broad categories (see Harris and Sabelhaus 2000). Each survey includes a family-level file that includes aggregate consumption and income information for the family, as well as a member-level file that includes demographic and earnings information for each member. We use data from 1980 through 1995.[8] We restrict the sample to families who are designated as "complete income reporters" and families who are interviewed for a full four quarters. We drop student households. We merge the family-level files to the individual-level files, and for each family we retain the member with the highest earnings (defined as wages plus business and farm income) and define that individualas the household head.[9] We drop any families in which the household head has zero earnings, is not working, is working in the public sector, or is not the survey-designated head or spouse.[10]

As described by Harris and Sabelhaus (2000), we construct comprehensive measures of family consumption and before-tax family income by summing the various categories of consumption and income provided in the survey.[11] We also construct three measures of saving. The first defines saving as income minus taxes minus consumption. The second defines saving as the change in net worth.[12] The third

[8] Data are not available on families entering the survey during the third and fourth quarters of 1985 and 1995.

[9] If the high earner is not unique, we order the high earners according to their relationship to the survey-designated household head as follows: head, spouse, child, grandchild, in-law, brother/sister, mother/father, other relative, and unrelated individual. We then retain the first high earner. In more than 80 percent of families, the high earner is the survey-designated household head. In almost 95 percent of families, the high earner is the survey-designated household head or spouse.

[10] Under the 1983 reform, federal employees began to be covered by Social Security. However, we cannot distinguish federal employees from state or local employees. Many state and local employees are covered by Social Security, but others are not.

[11] For income, we add up wages, business income, farm income, rent, dividends, interest, pension income, Social Security benefits, Supplementary Security Income, unemployment compensation, workers compensation, welfare payments, scholarships, food stamps, contributions from others (including alimony and child support), lump sums (e.g., from inheritances), and insurance refunds. We then subtract contributions made to others (including alimony and child support).

[12] The change in net worth sums pension and retirement contributions, changes in checking and savings account balances, changes in stocks and bonds, investments made in a farm or business, net properties purchased, additions and alterations made to properties, and the net reduction in debt.

defines saving as contributions to retirement accounts. We deflate these dollar amounts to 1980q1 dollars using the average CPI-U over the four quarters that each family is in the survey.

We estimate the following difference-in-differences model:

$$s_{it} = \delta_1 post_t + \delta_2 post_t T_i + \delta_3 incom_{it} + \delta_4 age_{it} + \mu_t + \epsilon_{it} \quad (5)$$

Here, s_{it} is defined as total savings for family i in year t, $post_t$ is an indicator for years after 1983, T_i is a dummy for the treatment group (defined as either self-employed individuals, who experienced a tax increase, or individuals born in 1938 or later, who experienced an increase in full retirement age), age_{it} is a set of age dummies for the head of household, μ_t is a set of year dummies, and ϵ_{it} is a stochastic error term.

To determine the impact of the degree of tax increase or benefit reduction, we modify equation (5) as follows:

$$s_{it} = \delta_1 taxrate_{it} + \delta_2 incom_{it} + \delta_3 ag_{it} + \mu_t + \epsilon_{it} \quad (6)$$

$$s_{it} = \delta_1 post_t + \delta_2 post_t * fra_{it} + \delta_3 inco_{it} + \delta_4 age_{it} + \mu_t + \epsilon_{it} \quad (7)$$

Equation (6) replaces the first two terms on the right-hand-side of equation (5) with $taxrate_{it}$, the exact OASDI statutory tax rate (employer plus employee share) faced by the head of household. The tax rate varies in each year only according to the employment status (self-employed versus wage earner) of the head of household. Equation (7) replaces the treated dummy in equation (5) with fr_{it}, the exact post-reform full retirement age faced by the head of household, thereby allowing larger increases in the full retirement age to have larger effects on saving.

We examine the impact of the elimination of student benefits by comparing life insurance premiums paid for households with and without dependent children. Using data on individual family members, we identify households with children who are living at home. Unfortunately, the data do not allow us to identify whether there are any children who are not living at home, such as college students who live in dorms. While it is college students who were most immediately affected by the elimination of

student benefits, the reform still lowered the present value of the benefit paid to all children at or below college age. We estimate the following difference-in-differences model:

$$l_{it} = \delta_1 transition_t + \delta_2 post_t + \delta_3 transitio_{\ t} kids_{it} + \delta_4 post_t kids_{it} +$$

$$\delta_5 incom_{\ it} + \delta_4 ag_{\ it} + \mu_t + \epsilon_{it} \quad (8)$$

Here, l_{it} is life insurance premiums (in 1980q1 dollars) paid by household i in year t, $transition_t$ is an indicator for time periods from the third quarter of 1981 through the first quarter of 1985 (the period over which student benefits were being phased out), $post_t$ is an indicator for time periods after the first quarter of 1985 (when student benefits had been fully eliminated), and $kid_{\ it}$ is an indicator for having kids under the age of 18 living at home.[13] If the student benefit elimination reduced life insurance holdings for affected families, we would expect δ_3 and δ_4 to be positive.

Because the reform primarily affected benefits for college students, life insurance value was primarily reduced for children who were likely to attend college. This allows us to add a third difference by using the head of household's education level to proxy for the likelihood of children under 18 attending college. We do this by estimating (8) with three-way interactions between time periods during and after the reform, having kids under 18, and an indicator for the head of household having some college education. Theory predicts that the coefficient on these three-way interactions will be positive.

IV. Empirical Results

a. Panel Study of Income Dynamics

[13] Having children 18-21 living at home is relatively uncommon. Results are largely similar if $kids_{it}$ is defined as having kids 21 and younger living at home.

Table 2 shows the results from estimating equation (1) for the full sample (column 1) as well as various subsamples. As identified in the last two rows of the table, the subsamples restrict the head of household's year of birth to 1910-1923 (the 7 cohorts on either side of the reform), the survey year to 1972-1981, or both. The first four columns present results for the first measure of saving: whether the household currently has two months of income in savings. The next four columns present results for the second measure of savings: whether the household had two months of income saved at any point during the previous five years. None of the coefficients on the interactions between post-1977 and the treatment group indicator are statistically significant, though the point estimates are mostly positive as theory would predict. However, as shown in Table 3, when the treatment group indicator is replaced with the percent reduction in benefits (equation (2)), the signs of the coefficients are mixed, and the coefficients are still insignificant. Excluding fixed effects produces similar results, with uniformly insignificant coefficients. Thus, we find no evidence that the 1977 reform reduced the probability of having 2 months of income saved. However, the size of the standard errors does not allow us to conclude that the reform had no effects.

b. National Longitudinal Study

Table 4 shows the results from estimating equation (3). The interaction coefficient of interest is positive but small and insignificant for the savings regression. Moreover, the coefficients for the bonds, investments, and total savings regressions are all negative and insignificant. Overall, the evidence suggests that people did not increase their savings behavior in response to reduced Social Security benefits.

Table 5 shows the results from estimating equation (4). The interaction coefficients for the savings and bonds specifications are both positive, but insignificant. Again, the coefficients on the investments

and total savings specifications are negative and insignificant. These regressions also suggest that the reduction in Social Security benefits did not have large impacts on people's savings behaviors. However, the large standard errors do not allow us to conclude that the reform had no economically meaningful effects.

Additional specifications tested for heterogeneity across education and income groups, since it is possible that particular education and income groups responded to Social Security reductions. However, we did not find any significant changes to savings behavior for different groups.

c. Consumer Expenditure Survey

Table 6 shows the impact of the payroll tax increase for the self-employed on savings (equation (5)). In the first two columns, the dependent variable is total payroll (Social Security and Medicare) taxes paid by the household head as a share of the household head's earnings. Payroll taxes for wage earners are multiplied by two to account for the employer's contribution. The coefficient on the interaction term in these equations indicates the increase in the average payroll tax rate for the self-employed relative to wage earners following the 1983 reform. As Figure 1, which was based on statutory tax rates, suggests, observed tax rates indeed increased for the self-employed relative to wage earners.[14] Columns (3) and (4) suggest that following the 1983 reform, the self-employed reduced their saving by $1,733 (if all age groups are included in the analysis) or $2,381 (if only individuals aged 40-64 are included in the analysis) relative to wage earners. These two columns measure savings as disposable income minus consumption.[15] The

[14] Observed tax rates differ from statutory tax rates because they also include contributions to Medicare, and because the measure of earnings reported in the data may not correspond exactly to income that was subject to the payroll tax, possibly due to the payroll tax cap, noncompliance, or measurement error.

[15] Since taxes – including payroll taxes – are subtracted from gross income to arrive at disposable income, and since payroll tax rates increased for the self-employed following the reform, one might be concerned that there is a mechanical relationship between the savings definition and the reform. But this specification is consistent with the theory, which predicts that the payroll tax increase reduces saving rather than consumption.

two other definitions of savings suggest a more mixed picture – either no statistically significant change or a statistically significant increase (when savings is measured as the change in assets and only individuals aged 40-64 are included in the analysis). Thus, we have evidence that the payroll tax increase reduced saving by one definition of saving. However, this finding is not robust across alternative definitions of saving.

Table 7 presents estimates of equation (6), in which the key independent variable is the exact statutory tax rate faced by the head of household. The results in columns (1)-(2) show that again, as expected, the observed average tax rate for the head of household is highly correlated with the statutory tax rate. The results in columns (3)-(8) are similar to those in Table 6. They suggest that the reform reduced one measure of saving (disposable income minus consumption) for the self-employed, with mixed results for the remaining definitions of saving. Between 1983 and 1984, the statutory payroll tax rate increased by 2.75 percentage points for the self-employed relative to wage earners. The results in column (3) suggest that this tax increase caused a $1,676 ($60,956 x .0275) decrease in saving for the full sample. The results in column (4) suggest that the tax increase caused a $2,272 ($82,613 x .0275) decrease in saving for individuals aged 40-64.

Table 8 shows the results from estimating equation (5) for the full retirement age increase. The interactions between the indicator for being born in 1938 or later and the years after the reform have mixed signs and are statistically insignificant. Table 9 shows similar results for equation (7), where the indicator for the treated group is replaced with the individual's full retirement age. Thus, we find no evidence that the reduction in lifetime benefits induced by the increase in the full retirement age led to an increase in saving. However, the standard errors are large, so we cannot rule out the possibility that there was an effect.

Finally, Table 10 shows the results from estimating equation (8). In the first column, the coefficients on the interaction terms between having kids and being in the transition or post-reform

period have the expected positive sign. However, they are not individually or jointly significant. In the second column, the three-way interactions between having kids, being in the transition or post-reform periods, and having some college education also have the expected positive sign. Furthermore, the coefficients on the two-way interactions between having kids and being in the transition or post-reform periods are smaller in magnitude (and negative), suggesting that the reform had an effect for individuals with some college education but not those without. However, the coefficients on the three-way interaction terms are again not individually or jointly significant. We conclude again that, while we cannot rule out the possibility that the reform affected life insurance purchases, we have no evidence to suggest that it did.

V. Discussion

As we have shown, the standard neoclassical model generates large predicted declines in savings from the introduction of Social Security in aggregate. Theory also predicts that the life insurance aspect of Social Security causes declines in life insurance holdings. On the other hand, our empirical analysis reveals that changes in Social Security policy do not have a statistically significant impact on saving or life insurance holding. There are several reasons why such policy changes may generate less dramatic changes in observed savings than the predicted results from the model.

First, our standard errors are large. In our life insurance regressions, the coefficients of interest all have the expected signs; they are just not statistically significant. In all our regressions, our standard errors do not rule out the possibility of an economically meaningful effect that is consistent with theory. In recent decades, datasets like the Health and Retirement Study and Survey of Consumer Finances have started to provide more detailed pictures of household savings and asset holdings. In comparison, the

datasets we use provide much less detail about saving behaviors, although they are the best available datasets that cover the period of the reforms we study. It is possible that the quality of the savings data from the 1970s and early 1980s is simply too noisy to precisely estimate these relationships.

Second, it is possible that individuals chose to delay retirement, rather than save more, in response to the benefit cuts studied. That may especially be the case for the 1983 reform, which cut lifetime benefits by raising the full retirement age. For example, Behaghel and Blau (2012) show that individuals tend to retire at whatever age is designated the full retirement age, possibly because they view this age as either a reference point or a recommendation by the government. On the other hand, Kruger and Pischke (1992) find little evidence that the 1977 reform had an impact on the labor force participation of affected older males.

Third, it is possible that individuals simply lack knowledge about their Social Security benefits and were unaware of the policy changes. While Smith and Couch (2014) show that younger workers are aware of the broad provisions of Social Security – for example that it provides benefits to retirees and the families of deceased workers – Leibman and Luttmer (2015) find that many individuals are unaware of its specific design features. Gustman and Steinmeier (2004) further show that even older individuals make large errors in estimating their Social Security benefit levels. Thus, it is likely that many individuals are unaware of or do not pay attention to how changes in policy rules affect their benefits, particularly for small benefit changes. Lack of knowledge or attention is consistent with finding weak evidence of responsiveness to a tax increase but no evidence of responsiveness to a benefit cut. Since payroll taxes are deducted immediately from individuals' paychecks (or calculated and paid quarterly or annually for self-employed individuals), a tax increase is likely to be more salient and well-understood than a benefit cut.

Fourth, the timing of information about the policy change can be important in identifying any effects. If individuals learn about policy changes before they are implemented, then behavioral responses may not align with the timing of the policy change. As discussed by the Social Security Administration

(n.d.), the problems created by the flawed indexation formula of the 1972 law became apparent almost immediately. The Social Security Trustees report of 1974 suggested that the program had a large long-term actuarial imbalance; by 1976 it had become "overwhelmingly clear" that reform would be required. Over this period, there was much public discussion about how to correct the flaw in the 1972 law. However, our identification strategy rests on the fact that the 1977 reform affected different groups in different ways. And it is not clear to what extent the exact design of the reform – including which cohorts would be affected and by how much – could have been anticipated, as numerous alternative proposals were considered. Similarly, the financial shortfall that led to the 1983 reform was also apparent well in advance, and proposals to cut benefits were debated as early as 1981 (see Light 2005). Proposals to eliminate student benefits were considered as early as the late 1970s (DeWitt 2001). But here too, it is not clear to what extent the exact design of the reform could have been anticipated.

Fifth, individuals also likely lack information about whether the policy change is transitory or permanent. Households can easily observe that tax rates and benefit levels change frequently. For example, Shoven and Slavov (2006) document that promised internal rates of return vary considerably over each cohort's lifetime due to policy changes. Individuals may not respond much to any one change if they are not sure how long that change will last.

Finally, expectations may also affect the timing of behavioral responses. Expectations could matter in several ways. First, any policy change should be taken in context of the overall Social Security budget. Our theoretical model assumes a balanced government budget. However, our empirical work relies on benefit cuts and tax increases designed to restore fiscal balance. In an environment where Ricardian equivalence holds, the timing of policy changes to restore long-run fiscal balance to the Social Security system should not influence behavior if it does not change individual expectations about future policy. As discussed above, the actuarial shortfalls that triggered the 1977 and 1983 reforms were apparent well in advance, although it is not clear to what extent the differences in impact across

individuals (on which our identification rests) could have been anticipated. Alternately, Dominitz, Manski, and Heinz (2003) argue that many individuals do not expect to receive any Social Security benefits, so changes in rules may not influence behavior. More generally, several papers including Gomes, Kotlikoff, and Viceira (2007), van der Wiel (2008), and Caliendo, Gorry, and Slavov (2016) study the impact of uncertainty on savings decisions and welfare.

VI. Conclusions

In this paper, we review the theoretical implications of Social Security on private savings, establishing that Social Security strongly crowds out private savings behavior. With these theoretical predictions in mind, we empirically evaluate the effect of Social Security on savings and insurance purchasing behavior using three different policy changes. Despite the strong theoretical predictions, we find little evidence to support that Social Security crowds out private savings. We posit that lack of knowledge about Social Security program details in general or the specific implications of the policy change in particular could mitigate any effect of the policy on individual savings behavior.

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Figure 1: Statutory Payroll Tax Rate by Employment Status

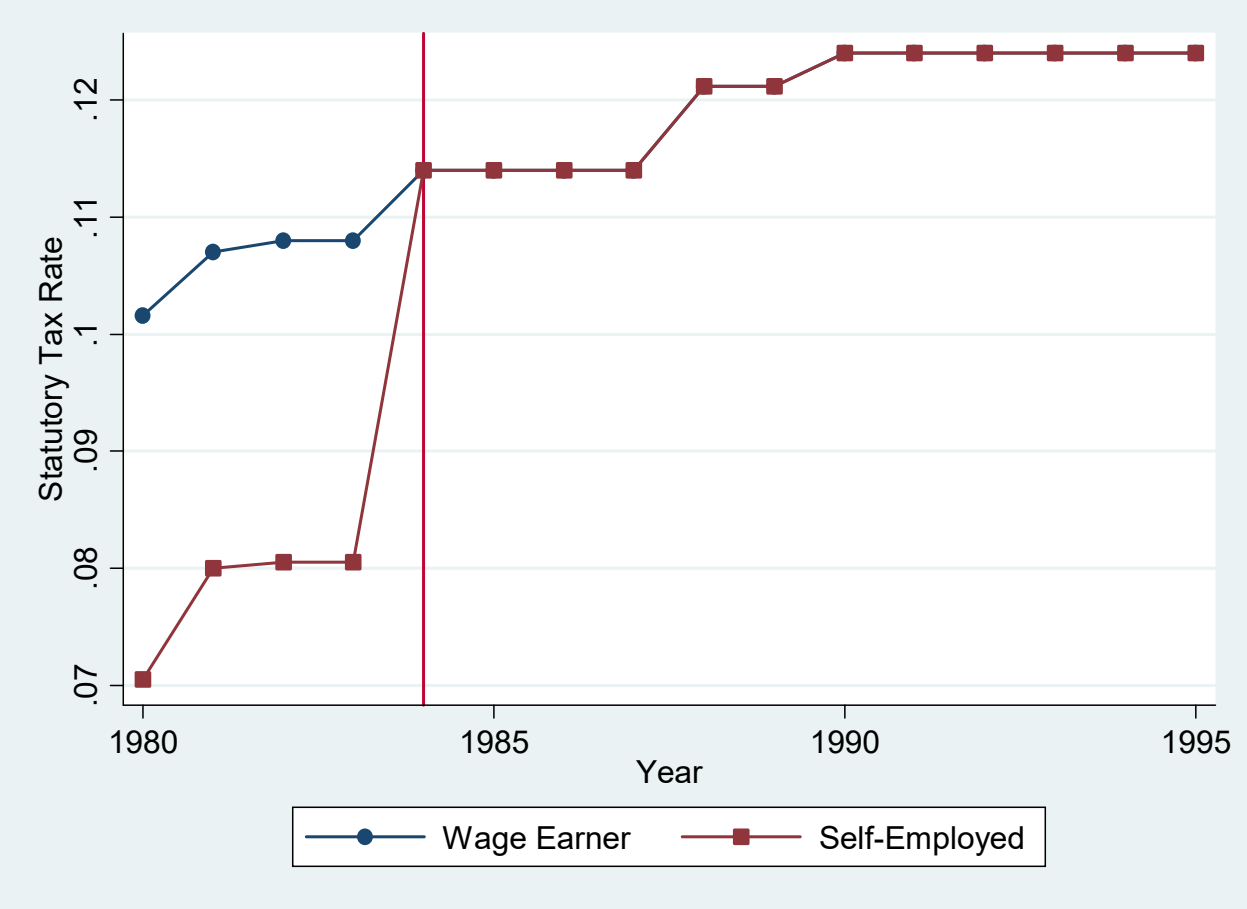

Table 1: Assets at Retirement as a Fraction of Lifetime Income by Wage Type

	Wage Type			
	.2E(w)	E(w)	1.24E(w)	2.47E(w)
No Social Security	41%	41%	41%	41%
Social Security	-6%	11%	12%	18%

Table 2: Impact of Receiving Benefit Cut on Savings and Consumption (PSID)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P(Currently Have Two Months' Income Saved)				P(Had Two Months' Income Saved in Past 5 Years)			
Post-1977 * Treated	-0.00606	0.000113	0.0384	0.0603	0.00307	0.00787	0.0435	0.0623
	(0.0360)	(0.0617)	(0.0501)	(0.0684)	(0.0277)	(0.0489)	(0.0397)	(0.0554)
Family Income	3.16e-06***	3.30e-06***	2.64e-06***	4.09e-06***	1.19e-06***	1.14e-06**	1.05e-06**	1.57e-06**
	(6.47e-07)	(9.09e-07)	(8.64e-07)	(1.26e-06)	(3.50e-07)	(5.53e-07)	(4.88e-07)	(7.33e-07)
Observations	19,024	4,742	10,374	2,412	18,834	4,700	10,316	2,399
Number of Families	2,601	604	2,601	604	2,601	604	2,601	604
Cohorts	All	1910-1923	All	1910-1923	All	1910-1923	All	1910-1923
Years	1968-1980	1968-1980	1972-1980	1972-1980	1968-1980	1968-1980	1972-1980	1972-1980

Notes: All regressions also include year dummies, household head age dummies, and family fixed effects. Longitudinal weights used.
Standard errors clustered by family in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Impact of Percentage Reduction in Benefits on Savings and Consumption (PSID)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P(Currently Have Two Months' Income Saved)				P(Had Two Months' Income Saved in Past 5 Years)			
Post-1977 * Reduction	-0.0940	-0.179	0.0764	0.116	-0.0284	-0.104	0.137	0.186
	(0.128)	(0.248)	(0.187)	(0.301)	(0.102)	(0.197)	(0.153)	(0.253)
Family Income	3.17e-06***	3.28e-06***	2.64e-06***	4.09e-06***	1.19e-06***	1.14e-06**	1.05e-06**	1.58e-06**
	(6.48e-07)	(9.09e-07)	(8.65e-07)	(1.26e-06)	(3.51e-07)	(5.54e-07)	(4.89e-07)	(7.30e-07)
Observations	19,024	4,742	10,374	2,412	18,834	4,700	10,316	2,399
Number of Families	2,601	604	2,601	604	2,601	604	2,601	604
Cohorts	All	1910-1923	All	1910-1923	All	1910-1923	All	1910-1923
Years	1968-1980	1968-1980	1972-1980	1972-1980	1968-1980	1968-1980	1972-1980	1972-1980

Notes: All regressions also include year dummies, household head age dummies, and family fixed effects. Longitudinal weights used. Standard errors clustered by family in parentheses.

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

Table 4: Impact of Receiving Benefit Cut on Savings (NLS)

	(1) Savings	(2) Bonds	(3) Investments	(4) Total Savings
Post*Treated	261.4	-25.48	-2,144	-2,310
	(1,946)	(349.8)	(4,272)	(4,951)
Income	0.118***	0.0246***	0.299***	0.560***
	(0.043)	(0.006)	(0.111)	(0.126)
Observations	18,740	20,577	20,621	21,006

Notes: All regressions include year dummies, age dummies, and individual fixed effects. Survey years include 1966, 1969, 1971, 1976, 1981, and 1990. Robust standard errors in parentheses and survey weights are used.

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

Table 5: Impact of Percentage Reduction in Benefits on Savings (NLS)

	(1) Savings	(2) Bonds	(3) Investments	(4) Total Savings
Post*Reduction	3,471	207.3	-3,730	-3,117
	(7,428)	(1,393)	(16,937)	(19,347)
Income	0.118***	0.0246***	0.300***	0.561***
	(0.043)	(0.006)	(0.111)	(0.126)
Observations	18,740	20,577	20,621	21,006

Notes: All regressions include year dummies, age dummies and individual fixed effects. Survey years include 1966, 1969, 1971, 1976, 1981, and 1990. Robust standard errors in parentheses and survey weights are used.

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

Table 6: Impact of Receiving Tax Increase on Saving (CE)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Tax Rate		Income - Taxes - Consumption		ΔAssets		Retirement Contributions	
Self-Employed	-0.0454***	-0.0448***	-2,184***	-2,554**	729.4	-1,241	-209.2	-486.0
	(0.00118)	(0.00174)	(704.9)	(1,082)	(894.5)	(1,492)	(205.2)	(441.4)
Self-Employed x Post 1983	0.0203***	0.0189***	-1,733**	-2,381*	800.5	4,032**	179.6	467.4
	(0.00142)	(0.00215)	(837.7)	(1,306)	(1,186)	(1,931)	(205.0)	(433.2)
Family Income	-4.51e-07***	-5.10e-07***	0.381***	0.373***	0.120***	0.181***	0.0304***	0.0354***
	(2.10e-08)	(3.44e-08)	(0.0100)	(0.0153)	(0.0420)	(0.0696)	(0.00185)	(0.00293)
Observations	25,950	12,230	25,950	12,230	25,950	12,230	25,950	12,230
Household Head Ages	All	40-64	All	40-64	All	40-64	All	40-64

Notes: Dependent variable in columns (1)-(2) is household head's payroll taxes as a share of household head's earnings. Dependent variable in columns (3)-(6) is household saving in 1980q1 dollars. All regressions also include year dummies, household head age dummies, and household head cohort dummies. Attrition adjusted weights used. Robust standard errors in parentheses.

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

Table 7: Impact of Statutory Tax Rate on Saving (CE)

	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)
VARIABLES	Tax Rate		Income - Taxes - Consumption		ΔAssets		Retirement Contributions	
Statutory Tax Rate	0.708***	0.653***	-60,956**	-82,613*	31,293	141,108**	6,316	16,023
	(0.0497)	(0.0754)	(28,779)	(44,556)	(41,984)	(68,477)	(7,125)	(14,798)
Family Income	-4.51e-07***	-5.10e-07***	0.381***	0.373***	0.120***	0.181***	0.0304***	0.0354***
	(2.10e-08)	(3.44e-08)	(0.0100)	(0.0153)	(0.0420)	(0.0696)	(0.00185)	(0.00293)
Self-Employed	-0.0251***	-0.0260***	-3,918***	-4,932***	1,547*	2,792**	-29.55	-20.11
	(0.000809)	(0.00119)	(448.0)	(717.3)	(804.5)	(1,280)	(59.51)	(97.12)
Observations	25,950	12,230	25,950	12,230	25,950	12,230	25,950	12,230
	0.175	0.120	0.331	0.295	0.027	0.042	0.103	0.094
Household Head Ages	All	40-64	All	40-64	All	40-64	All	40-64

Notes: Dependent variable in columns (1)-(2) is household head's payroll taxes as a share of household head's earnings. Dependent variable in columns (3)-(6) is household saving in 1980q1 dollars. All regressions also include year dummies, household head age dummies, and household head cohort dummies. Attrition adjusted weights used. Robust standard errors in parentheses.

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

Table 8: Impact of Receiving FRA Increase on Saving (CE)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Income - Taxes - Consumption		ΔAssets		Retirement Contributions	
Born in 1938 or later x Post 1983	-45.68	778.8	-1,101	302.8	116.9	1.239
	(545.5)	(1,580)	(925.2)	(2,308)	(330.0)	(184.0)
Family Income	0.376***	0.345***	0.122***	0.103***	0.0304***	0.0331***
	(0.0100)	(0.0177)	(0.0417)	(0.0366)	(0.00187)	(0.00506)
Observations	25,950	4,662	25,950	4,662	25,950	4,662
Household Head Cohort	All	1933-1942	All	1933-1942	All	1933-1942

Notes: Dependent variable is household saving in 1980q1 dollars. All regressions also include year dummies, household head age dummies, and household head cohort dummies. Attrition adjusted weights used. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Impact of Full Retirement Age on Saving (CE)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Income - Taxes - Consumption		ΔAssets		Retirement Contributions	
Full Retirement Age x Post 1983	-2.904	228.0	-92.04*	79.79	5.638	-1.072
	(31.81)	(299.4)	(51.67)	(432.7)	(17.89)	(35.58)
Family Income	0.376***	0.346***	0.122***	0.103***	0.0304***	0.0331***
	(0.0100)	(0.0177)	(0.0417)	(0.0366)	(0.00187)	(0.00506)
Observations	25,950	4,662	25,950	4,662	25,950	4,662
Household Head Cohort	All	1933-1942	All	1933-1942	All	1933-1942

Notes: Dependent variable is household saving in 1980q1 dollars. All regressions also include year dummies, household head age dummies, and household head cohort dummies. Attrition adjusted weights used. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Impact of Eliminating Student Benefits on Life Insurance Premiums Paid (CE)

	(1)	(2)
VARIABLES	Life Insurance Premiums	
Have Kids	87.31***	76.76**
	(24.65)	(31.11)
Have Kids x (1981q3 - 1985q1)	20.35	-10.11
	(29.10)	(36.51)
Have Kids x Post 1985q1	9.047	-0.239
	(27.03)	(33.57)
College		27.36
		(43.38)
College x (1981q3 - 1985q1)		-59.38
		(46.35)
College x Post 1985q1		-14.07
		(45.20)
College x Have Kids		34.42
		(55.78)
College x Have Kids x (1981q3 - 1985q1)		53.85
		(63.71)
College x Have Kids x Post 1985q1		16.67
		(60.45)
Family Income	0.0103***	0.0101***
	(0.000941)	(0.000983)
Observations	25,950	25,950

Notes: Dependent variable is household life insurance premiums paid in 1980q1 dollars. All regressions also include year dummies, household head age dummies, and household head cohort dummies. Attrition adjusted weights used. Robust standard errors in
*** p<0.01, ** p<0.05, * p<0.1