How do 401(k)s Affect Saving? Evidence from Changes in 401(k) Eligibility

Gelber, Alexander M.

Wharton School University of Pennsylvania, NBER

28 December 2010

How do 401(k)s Affect Saving? Evidence from Changes in 401(k) Eligibility*

Alexander M. Gelber

The Wharton School, University of Pennsylvania, and NBER

December 2010

Abstract

This paper investigates the effect of 401(k) eligibility on saving. To address the possibility that eligibility correlates across individuals with their unobserved tastes for saving, I examine a change in eligibility: some individuals are initially ineligible for their 401(k) but become eligible when they have worked at their firm long enough. I find that eligibility raises 401(k) balances. Other financial assets and net worth respond insignificantly to eligibility, but the confidence intervals do not rule out substantial responses. In response to eligibility, IRA assets increase, consistent with a "crowd-in" hypothesis, and accumulation of cars decreases.

* I thank Alan Auerbach, Hanley Chiang, David Cutler, Martin Feldstein, William Gale, Caroline Hoxby, Lawrence Katz, Kirk Moore, James Poterba, Todd Sinai, Jonathan Skinner, Jeremy Tobacman, and three anonymous referees for helpful comments. I am grateful for financial support from the National Institute on Aging, Grant Number T32-AG00186. All errors are my own.

How does 401(k) eligibility affect saving? Individuals could substitute 401(k) saving for saving in other forms, and in principle, the positive income effect of 401(k)s could even decrease personal saving. Moreover, 401(k)s are expensive for the government: the U.S. government lost an estimated $51.2 billion in revenue due to the tax expenditure on defined contribution plans in 2008 (Joint Committee on Taxation 2008). Since a large fraction of personal savings in the U.S. is in 401(k)s, these issues are crucial in designing strategies to affect U.S. savings rates. Recent declines in 401(k) balances associated with the sharp declines in asset values around 2008 have led some to re-evaluate the social value of defined contribution pensions and 401(k) plans, adding urgency to the question of whether 401(k)s have positive effects.

Previous work on the effect of 401(k) eligibility on saving has not reached a consensus, which motivates the new empirical strategy of this paper. James Poterba, Steven Venti, and David Wise (1995) use the Survey of Income and Program Participation (SIPP) to compare the financial assets of those eligible for 401(k)s because they work in firms that offer 401(k) plans, to the financial assets of those in firms that do not offer 401(k)s. They also compare the financial assets of eligible and ineligible households in repeated cross-sections. These strategies indicate that 401(k) savings is not offset by decreases in other financial assets.[1] Venti and Wise (1996) compare the assets of similar individuals from different cohorts, some of which had longer than others to contribute to special saving programs. They find that those cohorts that had longer to contribute to 401(k)s and IRAs have substantially higher assets in these vehicles, but they find no difference between the other financial assets of the older and younger workers. Eric Engen, William Gale, and John Karl Scholz (1994, 1996) use the same data as Poterba, Venti, and Wise (1995) and Venti and Wise (1996) but find that 401(k) eligibility has no effect on overall saving, in part because 401(k) saving is offset by decreases in home equity. Engen and Gale (2000) argue that generalizing the specification in Poterba, Venti and Wise (1995) yields substantially smaller effects of 401(k) eligibility on net worth.

[1] Within the repeated cross-sections, they also stratify individuals by whether they participate in IRAs, finding similar results within these groups to those on the un-stratified repeated cross-sections.

These papers have made important contributions to our understanding of 401(k)s and saving. Nonetheless, there are acknowledged limitations of the approaches they take (B. Douglas Bernheim 2002; R. Glenn Hubbard and Jonathan Skinner 1996). First, workers may have unobserved tastes for saving that are correlated with 401(k) eligibility, even conditional on observables. For example, those with higher unobserved tastes for saving may choose more often to work in firms that offer 401(k)s. Second, turning to the analysis based on repeated cross-sections, the composition of the population of households ineligible and eligible for 401(k)s changed over time, as more employers began to offer 401(k)s. Therefore, the unobserved saving tastes of the ineligible and eligible populations may have changed over time. Third, in the data used in this literature, households' wealth in Defined Benefit (DB) and Defined Contribution (DC) pensions (other than 401(k) wealth) is unobservable. If there is substitution between 401(k) wealth and wealth in these other forms, then the wealth of households ineligible for 401(k)s is understated relative to the wealth of eligibles. Fourth, in the cohort-based analysis of Venti and Wise (1996), other differences across cohorts, including the environment for wealth accumulation that each faced during the years prior to their retirement, could confound comparisons across these groups.

The empirical strategy of this paper aims to address these issues. I identify the effect of 401(k) eligibility using longitudinal data on households' savings decisions from the 1996 SIPP, in combination with a plausibly exogenous within-person change in eligibility. Many firms exclude their employees from participating in the firm's 401(k) plan at the beginning of the employees' tenure at the firm. Federal law dictates that for-profit firms cannot exclude employees from participating in a 401(k) plan for more than one year. My empirical strategy exploits this using a differences-in-differences approach. I examine households containing individuals who report in their first year on a new job that they do not participate in their firm's 401(k) plan because they have not worked at the firm long enough. I compare their saving over this first year to their saving in their second year on the job, when they are now eligible for their firm's 401(k) plan. The change in their saving from the first year to the second year is then further compared to

the change in saving of a control group whose 401(k) eligibility does not change over the same period of time.

My estimates indicate that while 401(k) eligibility raises 401(k) contributions substantially. There is no evidence that eligibility significantly impacts saving in other financial assets or significantly increases liabilities, but an important limitation of the paper is that the confidence intervals are large enough that I cannot rule out substantial changes in other assets or liabilities. Moreover, I do not find a significant or precisely estimated impact on net worth. I do find that among those under 45, saving in IRAs rises in response to 401(k) eligibility, consistent with a "crowd-in" hypothesis (Bernheim 2002). Among those 45 and older, the point estimates indicate that saving in IRAs decreases in response to 401(k) eligibility, although the estimate is insignificant. The estimate for the sample as a whole indicates that 401(k) eligibility raises IRA saving. I also find that the increase in saving corresponds to a decrease in consumer durables. In particular, the total value of families' cars falls substantially in response to 401(k) eligibility.

The remainder of the paper is organized as follows. The data and identification strategy are described in Section I. Section II presents the results. Section III concludes.

I. Data and Empirical Strategy

A. Identification Strategy and Sample Description

In the main results, the change in saving from Year 1 to Year 2 of households who are initially ineligible for their 401(k), but later become eligible, is compared to the change in saving from Year 1 to Year 2 of those who are always eligible.[2] This strategy addresses a number of important issues. First, it exploits within-person variation in 401(k) eligibility, which addresses the possibility of unobserved heterogeneity across

[2] "Year 1" refers to the first year that households are at their firm, whereas "Year 2" corresponds to the second year. "Year 0" refers to the year immediately prior to the first year spent at the firm.

individuals in their tastes for saving.[3] Second, while wealth in DB plans is unobservable in the data, it is reasonable that the present discounted value of DB wealth does not change differentially over time in the treatment and control groups. Third, the strategy compares one group of 401(k)-eligibles to another group of 401(k)-eligibles, which will appear in the data to be closer comparison groups than 401(k)-eligibles and 401(k)-ineligibles.

In the 1996 SIPP, Year 0 corresponds approximately to calendar year 1997, Year 1 corresponds approximately to calendar year 1998, and Year 2 corresponds approximately to calendar year 1999.[4] The data are structured as follows. Assets and liabilities are observed in Waves 3, 6, 9, and 12. Waves occur every four months, so assets are observed once each year, over the course of four years. 401(k) eligibility is observed in Wave 7.[5] The timeline is illustrated graphically in Figure 1.

Individuals must possess several characteristics to be included in the sample. They must work at a for-profit firm (because the law allowing firms to exclude employees for at most one year applies only to for-profit firms). They must have started their current job one year or less before Wave 7, so that individuals in both the treatment and control groups are comparable in the sense that they are all in their first year on the job. Except where otherwise noted, they must also work at a firm that offers a 401(k) plan.[6] Following the previous literature (Poterba, Venti, Wise 1995, 1996; Engen, Gale, and Scholz 1994, 1996; Benjamin 2003), the sample is limited to individuals under 65, thus avoiding issues relating to the decumulation of assets at retirement. In Wave 7, individuals who do not participate in their firm's tax-deferred pension plan are asked, "Reason respondent not covered by pension plan. Why are you not included? Haven't

[3] In the related context of IRAs, Orazio Attanasio and Thomas DeLeire (2002) compare those who were contributing before and after IRA eligibility changes to those only contributing after the changes.
[4] The exact dates depend on the SIPP rotation group to which an individual belongs.
[5] No other SIPP panel has data on both Year 2 saving and whether the individual is temporarily ineligible for the 401(k).
[6] Another control group consists of those who are ineligible for a 401(k) because they work at a firm that does not offer one. I instead examine those who are eligible in both periods because their observable characteristics, and those of their employers, are more similar to those in the treatment group. As a robustness check, I later show results incorporating never-eligibles in the sample.

worked long enough for this employer." If the respondent answers "yes" to this question, then he or she is considered temporarily ineligible (i.e. he or she is a member of the treatment group).

B. Specifications

The main independent variable of interest is a dummy that equals 1 when the individual responds that he or she is temporarily ineligible for a 401(k), which I call the "Become Eligible" dummy because these individuals become eligible for the 401(k) over the period of observation. The dependent variable is the difference in saving between Year 1 and Year 2. In particular, if A_{in} represents the level of a given type of assets or liabilities of individual i in wave n of the 1996 SIPP, then the dependent variable is Y_i $=[\ln(A_{i12})-\ln(A_{i9})]-[\ln(A_{i9})-\ln(A_{i6})]$. Waves 3 and 6 represent the beginning and end, respectively, of Year 0 at the firm; Waves 6 and 9 represent the beginning and end of Year 1 at the firm; and Waves 9 and 12 represent the beginning and end of Year 2. Each value of A_{in} has been replaced by $A_{in}+10$, so that the logarithm of the variable is defined for all observations.[7] The regression equation is specified as:

$$Y_i = \beta_0 + \beta_1 T_i + X_i \beta + \varepsilon_i,$$

where β_0 is the constant term, β_1 is the coefficient of interest on the treatment dummy T_i, X_i represents the control variables, β is a vector of coefficients on these controls, and ε_i is an error term. The logarithmic specification is appropriate because assets and liabilities are approximately log-normally distributed, with a long right tail. I also report a number of related specifications, including a linear specification in which assets are not logged, as well as a specification that controls for initial log assets of the type in question.

The coefficient on the treatment dummy represents the differential increase in saving from Year 1 to Year 2 in the treatment group relative to the control group, as a percentage of initial assets of the type in question. The independent variable could also

[7] In results I report and discuss later, I use the inverse hyperbolic sine to transform the data, a specification that circumvents the need to choose a number to add to the dependent variable before transforming it (Karen Pence 2006).

be considered to be in first differences, since the Become Eligible dummy could be seen as the first difference (from Year 1 to Year 2) of a variable that equals 1 when an individual is eligible for the 401(k) (in Year 2) and equals 0 when ineligible (in Year 1). By first-differencing saving, I remove time-constant differences in saving behavior between the treatment and control groups.

C. Addressing Limitations of the Empirical Strategy

It may be that only individuals who have a particularly high taste for saving respond that they are temporarily ineligible for their firm's 401(k) plan. There may be other employees who are in fact ineligible for this reason but who are not aware of this fact.[8] The estimates should be interpreted as local to the group that responds that they are temporarily ineligible. Although second-differencing assets removes time-constant differences in savings behavior in the treatment and control groups, if the group that reports they are temporarily ineligible consists disproportionately of highly motivated savers for whom the effect of 401(k) eligibility on savings is particularly large, then the estimates of the effect on overall saving for this group may be larger than the effect in the population as a whole.[9]

Nonetheless, it is reassuring that 36% of the sample is in the treatment group, which is in the range of a survey of firms that found that 44% of for-profit employers require employees to wait a full year before participating in a 401(k) (Profit-Sharing/401k Council of America 1998). While we would also need to know the size of employers who temporarily exclude employees in order to compare the samples more directly, this survey also found little heterogeneity in the probability of a waiting period for eligibility across the employer size groups it examines: the survey reports that 45% of employers with fewer than 100 total employees exclude employees for a full year, which is close to the 44% of all employers who require a waiting period. In the SIPP, 37% of those

[8] See Olivia Mitchell (1988) and Alan Gustman, Thomas Steinmeier, and Nabahid Tabatai (2009) on imperfect knowledge of pensions.
[9] On the other hand, if some of those who are actually temporarily ineligible are unaware of this and are therefore placed in the control group, this could result in under-estimates.

working at employers with fewer than 100 total employees are in the treatment group, again in the range of the survey's results. Among employees who have worked for more than one year at for-profit firms that offer 401(k)s—who cannot be temporarily ineligible for the firm's 401(k) since by law these firms can exclude them from the 401(k) for up to one year after starting a job—only 5.8% respond that they are temporarily ineligible.[10] This is much smaller than the 36% of the sample in the treatment group. These considerations point toward the conclusion that it is unlikely that a large number of people are responding inaccurately to the question on temporary ineligibility. However, the possibility that respondents may not know their true classification remains a limitation.

Another limitation of this strategy is that I do not know how long people have actually been excluded from their firm's 401(k) plan. Certain people who respond that they are eligible for their firm's 401(k) could have been excluded from the firm's 401(k) for an unknown period of time prior to the time at which they respond to the SIPP questionnaire. People who respond that they are ineligible will continue to be ineligible for an unknown period of time. Even though I do not know how long individuals are eligible or ineligible, the treatment dummy is positively correlated with the change from Year 1 to Year 2 in the amount of time eligible. In other words, because they respond in Wave 7 that they are temporarily ineligible, they will have been temporarily ineligible for a longer total time during Waves 6-9 than the total period of time for which they are ineligible during Waves 9-12. While I will not be able to determine exactly how much eligibility raises saving, I will be able to compare the rise in 401(k) saving to the fall (or rise) in saving in other forms in response to 401(k) eligibility, thus addressing the question of how much a dollar of 401(k) saving is associated with increases or decreases in other saving.

Data on saving are notoriously noisy. Since measures of saving are used as the dependent variable, this is expected to create noise that will affect the standard errors of

[10] It is also possible that some of this 5.8% are actually temporarily ineligible but are mis-reporting the date at which they started their job.

the estimates. Evidence that measures of saving respond significantly to 401(k) eligibility may be considered all the more striking.

D. Benchmark Cases

It is useful to ground the analysis by discussing some natural benchmark cases. Let us consider a simple life-cycle model in which individuals consume in three Periods (0, 1, and 2) and earn interest from one period to the next.[11] Individuals who know in Period 0 that they will earn a higher after-tax real interest rate from Period 1 to Period 2 should adjust their saving right away in Period 0, relative to their saving if the after-tax real interest rate from Period 1 to Period 2 were lower.

Consider the Euler equations governing an individual's intertemporal consumption decision, which equate the marginal utility of consumption in one period to the marginal utility of consumption in the next period multiplied by the discount factor and one plus the after-tax real interest rate: $u'(c_0)=\delta(1+r_1)u'(c_1)$ and $u'(c_1)=\delta(1+r_2)u'(c_2)$. Raising the after-tax real interest rate r_2 that individuals expect to earn during Year 2–as a 401(k) does for the treatment group–implies that $u'(c_1)$ must rise relative to $u'(c_2)$, since $u'(c_1)=\delta(1+r_2)u'(c_2)$. In order for $u'(c_0)=\delta(1+r_1)u'(c_1)$ to hold, $u'(c_0)$ must also rise relative to $u'(c_2)$ (since $u'(c_1)$ has risen). In other words, consumption is affected immediately in period 0 by the change in the future real interest rate. Savings rises immediately as long as risk aversion is small enough that the substitution effect dominates the income effect; if the income effect is stronger, then conversely savings falls immediately. Either way, there is an immediate effect on saving in the sense that r_2 affects c_0. The growth in consumption c_1/c_0 will depend only on r_1, δ, and the concavity of utility, and would be unaffected by r_2 under CRRA utility. If hypothetically we found that total saving does not change from Year 1 to Year 2 in the treatment group relative to the control group, however, this would not rule out the possibility that 401(k)s affect saving through conventional life cycle channels. If 401(k)s primarily stimulate savings

[11] "Periods" 0, 1 and 2 are distinct from "Years" 0, 1 and 2 to emphasize that the benchmark model may not in fact be operative in the data.

through other mechanisms such as psychological channels or financial education (Brigitte Madrian and Dennis Shea, 2001; Esther Duflo and Emmanuel Saez, 2003; Bernheim, Patrick Bayer, and Scholz, 2009), then my empirical analysis may identify the effect of these mechanisms.[12]

E. Summary Statistics

Table 1 shows summary statistics on the covariates from Wave 6 of the 1996 SIPP. The mean values of age, income, and assets are somewhat higher in the control group than in the treatment group. This is not surprising, since firms with higher turnover (and to a minor extent, smaller firms) are more likely to exclude employees temporarily from participating in the firm's 401(k) because the fixed costs of setting up a 401(k) are more burdensome for these firms. In the SIPP sample that I consider, the evidence suggests that higher-turnover firms tend to have workers who are younger and have lower income and assets.[13] These differences between the treatment and control groups raise the possibility that they are not entirely comparable. I attempt to address this in a number of ways, including controlling for demographic variables, matching observations on still more demographics through propensity score match, and first-differencing the data to remove factors that are constant through time. It remains possible that even after accounting for all of these factors, the savings paths through time of the two groups would not have been the same in the absence of a 401(k). However, it is possible to argue that since employers decide whether to temporarily exclude individuals based on expected turnover and firm size, then we can remove the relevant differences between the treatment and control groups by controlling for firm size and for the main determinants of turnover—age and industry—as I do in the empirical analysis.[14]

[12] If these are indeed the primary drivers of 401(k) saving, then it is relevant to note that the saving response to an anticipated change in 401(k) eligibility such as the one considered in this paper may be the same as the response to an unanticipated change in eligibility.

[13] Specifically, job turnover is negatively correlated with age, negatively correlated with assets, and negatively correlated with income.

[14] I find that the difference in job turnover between the treatment and control groups in Year 1 can be explained by observable differences in demographic characteristics. In particular, I run a probit in which the dependent variable is a dummy for whether the individual remained at their job between Year 1 and Year 2, and the independent variables are the control variables in the main regressions (i.e. those in Panel B of Table 2), as well as on the Become Eligible dummy. The coefficient on the Become Eligible dummy is

It is also worth noting that the mean differences between the treatment and control groups are smaller than those between all 401(k) eligibles and all 401(k) ineligibles reported in Poterba, Venti, and Wise (1995).

II. Results

A. Initial Results

The initial results are displayed in Table 2. Panel A displays the coefficients on the treatment dummy when no controls are included in the regressions.[15] Given a coefficient estimate, it is possible calculate the dollar value of the effect of treatment that is implied by the coefficient, by applying the coefficient estimates to the mean asset values in the treatment group in Wave 6. These dollar equivalents are shown below the R-squared. Panel B displays a specification in which the control variables are age, age squared, household income, dummies for all possible education categories, dummies for all firm size categories, dummies for 1-digit industries, and days on the job. Panel C displays a specification with these controls as well as the log value of the asset in question in Wave 6, since this could influence the percentage increase in the asset. Robust standard errors are clustered by household. The coefficients on the control variables are almost always insignificantly different from zero, and they have been omitted. In the tables, liabilities are all in positive terms, with a larger number representing a larger liability.

Since the results are similar with and without controls, I restrict attention to Panel A of Table 2 in the discussion that follows. In Column 1 of Table 2, in which the

---

-.05, with a standard error of .12, implying that the coefficient estimate is insignificantly different from zero (p>.40). The differences in job turnover between the treatment and control groups (which are presumed in turn to influence the decision to create a waiting period) are not significantly related to temporary eligibility once one accounts for other observable factors that serve as control variables. This is in turn consistent with the claim that the independent variables adequately control for differences between the treatment and control groups (though note that the confidence interval on the Become Eligible dummy does not rule out that there is a substantial relationship between temporary ineligibility and turnover).

[15] Because the coefficient on the treatment dummy represents the estimated effect of eligibility, the estimates imply that saving in the treatment group would have been higher by the amount corresponding to the coefficient, if (as in the control group) the individual had been eligible in both Year 1 and Year 2.

dependent variable is the 401(k) balance, the coefficient on "Become Eligible" (.95) is significantly different from zero at the 1% level. Of course, it is unsurprising that 401(k) eligibility raises saving in 401(k)s. 401(k) eligibility is estimated to cause an increase of $2,759.8 in 401(k) saving, though the 95% confidence interval is large enough that a much smaller increase cannot be ruled out. The stock market performed unusually well during the period under consideration in the late 1990s, and the associated unusual level of interest in the stock market among ordinary investors could have raised the true effect of eligibility. Only 25.6% of the sample had positive 401(k) balances in Wave 3, so there was substantial room to increase 401(k) contributions among a large fraction of the sample.

Interestingly, when the dependent variable is the difference between Year 1 and Year 2 in the increase in the logged IRA balance, the coefficient on "Become Eligible" is positive, with a substantial coefficient that is significant at the 5% level. While the coefficient is surprisingly large, the 95% confidence interval does not rule out a substantially smaller increase in IRA assets. Only 21.1% of individuals in the sample had positive IRA balances in Wave 3, so there was substantial room to increase IRA contributions among the substantial fraction of the sample who become IRA contributors over the period of study. If 401(k) eligibility encourages households to overcome the fixed costs of opening accounts with mutual funds or other investment vehicles, or to learn about financial markets, then it may be less costly to put money in IRA accounts. 401(k) participation can also teach individuals about financial markets. Eligibility often comes with reminders by one's firm to save, pamphlets emphasizing the importance of retirement saving, the necessity of learning about financial markets, and the like. Therefore, individuals could be encouraged by 401(k) eligibility to save in IRAs. The effect of eligibility on other assets is negative (Column 3), but relatively small and insignificant. The point estimates are all insignificant for secured and unsecured debt, with large confidence intervals.

If 401(k) eligibility raises financial assets, then consumption or non-financial assets should correspondingly decrease. I investigate the value of individuals' cars in the

final row of the table, as cars are the major consumer durable with data in the SIPP. Car value could be considered a proxy for consumer durables as a whole, much as food consumption is sometimes used as a proxy for overall consumption.[16] Accumulation of cars falls substantially and significantly in response to eligibility. The dollar equivalent of the effect on car value is large, though the coefficient has a confidence interval large enough that we cannot rule out that the increase in 401(k) saving due to eligibility is the same as the decrease in car value. 39.8% of the sample buys or a sells a car during Year 0; with such frequent car buying and selling, it is less surprising to find such effects on car value. When I investigate how purchases of cars responds, the coefficient on the treatment dummy is insignificantly different from zero (p=.39) but suggests that individuals buy .1 fewer cars as a result of eligibility. The SIPP has only spotty and idiosyncratic measures of nondurables consumption, such as expenditures on commuting and expenditures on utilities. The point estimates suggest small and insignificant responses of these variables, and the results are omitted. I next perform a number of analyses that are oriented toward probing the robustness of these basic results.

B. Addressing Functional Form

Table 3 presents a variety of specifications that explore how the functional form of the dependent variable affects the results. As noted earlier, the initial value of assets or liabilities could influence its percentage change. If initial assets are zero, the percentage increase in assets may be large.[17] It is therefore helpful to see how the results change when I control very flexibly for the initial level of assets. If the coefficient on Become Eligible is much different than its value in Table 2 Panel B, this will suggest that differences in the initial value of assets between the treatment and control groups may be driving the results. In Panel A of Table 3, I control for a 20-piece spline in the level of initial (Wave 6) assets, as well as the control variables used in Panels B and C of Table 2. Reassuringly, the coefficients on the treatment dummy are only slightly changed from the basic specifications in Table 2. To investigate further whether the estimated effect of

[16] Other than cars, the only major durable with data in SIPP is housing; I examine home equity below.
[17] Note, however, that this affects the percentage increase in both Year 1 and Year 2 and thus may be differenced out (to a first approximation).

treatment could be driven by initial differences in assets between the treatment and control groups, I investigate the interaction between the treatment and the level of initial assets. In Panel B of Table 3, I include terms for initial (Wave 6) assets, the Become Eligible dummy, and the interaction of the Become Eligible dummy with Wave 6 assets. The interaction term is never significant and is always estimated with small standard errors. This supports the main analysis because it suggests that the effect of treatment is not greatly affected by the initial size of assets, the latter of which may differ between the treatment and control groups.

Panel C of Table 3 uses a different transformation of the dependent variable: the inverse hyperbolic sine. The inverse hyperbolic sine is defined as:[18]

$$\sinh^{-1}(A)=\ln(A+\text{sqrt}(1+A^2))$$

In Panel C, the dependent variable is then defined as $[\sinh^{-1}(A_{i12})-\sinh^{-1}(A_{i9})]-[\sinh^{-1}(A_{i9})-\sinh^{-1}(A_{i6})]$ for asset or liability category A. Pence (2006) pointed out that the inverse hyperbolic sine can be usefully applied in investigating the effect of 401(k)s on saving because the inverse hyperbolic sine of negative or zero values is defined, thus avoiding the problem of how to treat zeroes (or negative values) of a dependent variable. This transformation also addresses the issue that if the initial level of assets is low, the percentage increase may be large: the inverse hyperbolic sine of zero is zero, and the derivative of the inverse hyperbolic sine function evaluated at zero is equal to 1 (unlike the log function, whose derivative approaches infinity from the right of zero). The estimates are in the range estimated earlier, with the effect of treatment on 401(k) assets, IRA assets, and car value still significantly different from zero. Since the inverse hyperbolic sine of A is defined for negative values of A, I can investigate the effect on home equity or net worth, whose values below zero preclude their inclusion in a logarithmic specification. The coefficients are insignificant: the coefficient on the Become Eligible dummy is .93 with a standard error of .66 when the dependent variable is home equity, and the coefficient is .73 with a standard error of 1.13 when the dependent variable is net worth. The 95% confidence intervals do not rule out a

[18] A more general form of the inverse hyperbolic sine adds a scaling parameter to the function.

substantial decrease or increase in net worth or home equity in response to 401(k) eligibility.

C. Other specifications

Table 4 presents further robustness checks. There are large outlier observations of assets and liabilities, so it is of interest to estimate the results after removing the influence of these outliers. In Panel A of Table 4, I winsorize the dependent variable at the 5th and 95th percentiles. The results are still significant for 401(k)s and IRAs, but the magnitudes are reduced. Since the magnitudes are smaller, I have estimated new equivalent dollar values, which are substantially smaller: the dollar-value equivalent of the eligibility effect for 401(k)s is $1,967.8, and the dollar-value equivalent of the eligibility effect for IRAs is $2,072.3.[19]

As noted earlier, it is possible that individuals in the treatment and control groups differ in ways not accounted for among the control variables used thus far. First-differencing the data helps to mitigate this concern, as it removes individual unobserved effects that are constant over time. However, if the differences between the treatment and control groups differentially influence the time profiles of saving in the treatment and control groups, then even first-differencing may not be sufficient to address this issue. To help address this concern by accounting for other ways that the treatment and control groups may differ, I have also estimated the results using a propensity score match.[20] The propensity score allows me to condition on a greater set of observables in comparing the treatment and control groups (Rosenbaum and Rubin 1983). Observations in the treatment and control groups are matched, using stratified matching, according to age, education, household income, gender, household size, firm size, 1-digit industry, and the value in both Wave 3 and Wave 6 of the 401(k) balance, IRA balance, other assets,

[19] These dollar equivalents rely on applying the coefficient estimates to the raw means of 401(k) and IRA assets in Table 1. After winsorizing 401(k) and IRA assets at the 5th and 95th percentiles, the mean values are reduced substantially, and the dollar equivalents implied by applying the estimates in Table 4A to the winsorized means are $554.3 and $866.9 for 401(k) saving and IRA saving, respectively.

[20] Benjamin (2003) was the first to use a matching estimator to estimate the effect of 401(k) eligibility on savings.

secured debt, unsecured debt, and cars. The first stage results in four blocks, which are balanced along the covariates. Panel B shows that the coefficients and standard errors using the propensity score estimator are similar to those relying on OLS in Table 2. The effect of eligibility on the 401(k) balance is smaller, but the effect of eligibility on other asset categories is quite similar to the basic analysis.

Assuming that the treatment and control groups are comparable except for the ostensibly exogenous dummy for temporary ineligibility in Year 1, I can compare Year 1 saving between the treatment and control groups, as an alternative estimate of the effect of 401(k) eligibility. This is implemented in Panels C and D. The coefficient on the "Become Eligible" dummy in Panels C and D now represents the *negative* of the estimated effect of 401(k) eligibility on saving. This is because individuals in the "Become Eligible" group are ineligible over the period of Year 1—the period considered in Panels C and D—whereas the others in the sample are eligible for the 401(k) in Year 1. Panel C regresses log Wave 9 assets on the treatment dummy, log Wave 6 assets, and controls. Panel C again shows a positive and significant effect of 401(k) eligibility on 401(k) saving, although this effect is smaller than the effects estimated previously. There is again a positive effect of 401(k) eligibility on IRA saving and again a negative effect on car value, though in this specification these are insignificant. Like Table 2, Table 4 shows that 401(k) eligibility has insignificant effects on secured and unsecured debt and other financial assets.

Still another specification is linear, rather than the logarithmic specifications used thus far. In Panel D, assets or liabilities in Wave 9 are regressed on the treatment dummy, assets or liabilities in Wave 6, and controls.[21] There is a positive, significant, and large effect of 401(k) eligibility on the 401(k) balance and the IRA balance. The estimated effect on secured debt is negative and substantial, and the estimated effect on other assets is positive and substantial, but both are insignificant. The estimated effect on

[21] Since a linear specification creates additional noise in the dependent variable (due to non-normality), it makes sense to estimate the results using only data from Year 1 as in Table 3 Panel B, rather than the first-differenced results analogous to Table 2 that will exacerbate noise (particularly given that the results are comparable in Tables 2 and 3B).

car value is insignificant and small, but this is the only specification in which this is true. The estimated effect on unsecured debt is negative, as it is in most other specifications. Since this regression is in levels, I can again investigate the effect on home equity or net worth, whose values below zero preclude their inclusion in a logarithmic specification. When the dependent variable is home equity in Wave 9, and the independent variables are the Become Eligible dummy, home equity in Wave 6, and the controls, the coefficient on the Become Eligible dummy is -1,786.4, with a standard error of 3,380.4, indicating an insignificant rise in home equity in response to eligibility. In the analogous specification when the dependent variable is net worth, the coefficient on the Become Eligible dummy is -10,248.39, with a standard error of 7,848.87. The 95% confidence intervals again do not rule out a substantial increase or decrease in net worth or home equity in response to 401(k) eligibility, though the point estimate suggests a positive effect on net worth that is larger than the combined point estimates of the effects on 401(k) and IRA assets.

Appendix Table 1 presents still further robustness checks. In Panel A, I show the results when the group of individuals reporting that they are temporarily ineligible for the 401(k), but who also report that they have been at their job more than one year, is included in the treatment group. In Panel B, I limit the sample to those who remain on the same job from Year 1 to Year 2. (In the main results, the sample includes those who switched jobs in order to avoid conditioning the sample on a potential outcome variable.) In Panel C, I include in the control group both individuals who are always eligible for their 401(k) (as in the regressions reported so far), *and* individuals who are always ineligible for their 401(k) (because they work at an employer that does not offer a 401(k)). The latter group was excluded from the control group in the analysis so far. The treatment group in Panel C is the same as in the main analysis. In Panel D, I run the specification from Table 2 Panel C but instead control for the Wave 6 level of the asset (rather than the log of the Wave 6 value of the asset as in Table 2 Panel C). All of the results are in the range of those estimated in the main tables.

D. Intertemporal Considerations

In a life-cycle model under the assumptions discussed above, individuals who know that they will become eligible for a 401(k) in the future may change their savings right away in Year 1. Because temporarily ineligible individuals cannot (mechanically) save in a 401(k) while they are ineligible, they would have to change their saving in other forms instead in Year 1. This would imply that Year 1 savings in non-401(k) assets could change for temporarily ineligible individuals, relative to the Year 1 savings in non-401(k) assets of individuals who will always be ineligible for the 401(k).

I can therefore examine the extent to which this is happening by comparing the saving in Year 1 of individuals who are temporarily ineligible for their 401(k), relative to the Year 1 saving of individuals who are ineligible for a 401(k) in both Year 1 and Year 2 (because they work in a firm that does not offer a 401(k)). If temporarily ineligible individuals are raising or lowering their Year 1 saving in anticipation of future eligibility, then those who are temporarily ineligible may save more or less in Year 1 in non-401(k) assets than those who are always ineligible. An even more stringent test compares the Year 1 saving to Year 0 saving among always and temporarily ineligible individuals, to test whether saving rises more or less from Year 0 to Year 1 among those who are temporarily ineligible than among those who are always ineligible. The two tests yield similar results, and Table 4 presents the latter test. Because the dependent variable is the difference in saving between Year 0 and Year 1, the regression exploits within-individual rather than cross-individual variation, with the goal of addressing unobserved heterogeneity.

In Table 5, the dependent variable is the change from Year 0 to Year 1 in the increase in logged assets or liabilities. The sample is individuals who either report being temporarily ineligible for their firm's 401(k), or who report that they work in a firm that does not offer a 401(k). Recall that I seek a treatment dummy that is positively correlated with the first difference of 401(k) eligibility. Individuals are only asked in Wave 7 whether they are temporarily eligible for the 401(k), so if they report being temporarily ineligible, it may be that they have been temporarily ineligible since as long ago as Wave

4. For example, suppose hypothetically that they have indeed been ineligible since Wave 4. Then they would have spent more time ineligible for the 401(k) during waves 3-6 (i.e. a total of eight months) than they spent ineligible for the 401(k) during waves 6-9 (i.e. a total of four months). In other words, during waves 3-6, they spent waves 4-6 ineligible for the 401(k), whereas during waves 6-9, they only spent waves 6-7 ineligible. In this case, the treatment dummy is negatively correlated with the first difference of 401(k) eligibility, which is precisely the opposite of what I seek. I therefore must take additional precautions to make sure that the treatment dummy is correlated with the first difference in 401(k) eligibility. Thus, I limit the sample in Table 5 to those who began their job in Wave 5 or after. Even in spite of the larger sample size of 1,027, none of the coefficient estimates is significant, though the confidence intervals do not rule out substantial changes.[22]

E. IRA Assets

Table 6 investigates further the crowd-in of IRA saving. Columns 1 and 2 of Table 6 break down the sample by prior 401(k) participation. Among those who previously had no 401(k), there is a strong and significant effect of 401(k) eligibility on IRA saving. Among those who previously had a 401(k), there is a weaker and insignificant effect of 401(k) eligibility. These results are consistent with a story of crowd-in in which 401(k) eligibility brings those who did not previously have as much contact with other saving opportunities into greater contact with saving instruments, or raises their awareness of saving opportunities, leading them to save more even in non-401(k) savings vehicles. As discussed earlier, education is important in taking advantage of savings opportunities. It is apparent in Columns 3-4 that the treatment effect is stronger among the more educated. In Columns 5-6, I break down the sample by age. It is possible to argue that crowd-out is likely to be found to a greater extent among those closer to retirement, as they are likely to be more familiar with the possibilities for

[22] Note that this specification may be relevant to the concern that individuals in the treatment group may show an increase their IRA saving in response to 401(k) eligibility because they have more steeply upward-sloping savings in tax preferred savings vehicles such as IRAs. From Year 0 to Year 1, they do not show evidence of this.

substitution between IRAs and 401(k)s. Conversely, crowd-in could be stronger among younger individuals, who are more likely to learn about retirement savings opportunities from contact with them. Columns 5-6 show exactly this pattern: a strong positive effect on IRA saving among those under 45, and an insignificant negative effect among those 45 and older. While the estimates of the coefficients on the treatment dummy are not significantly different at the 10% level when comparing Column 1 to Column 2 or when comparing Column 3 to Column 4, the coefficient estimate is significantly larger at the 5% level for those under 45 in Column 5 than for those 45 and older in Column 6.

IV. Conclusion

This paper introduces a new strategy to identify the effect of 401(k) eligibility on saving, examining saving before and after individuals become eligible for their firm's 401(k) plan. While the empirical strategy has limitations, it also addresses several of the issues present in previous literature. I find evidence that 401(k) eligibility raises 401(k) balances substantially, but I find no evidence that contributions to 401(k)s are offset by significant decreases in holdings of other types of financial assets. Nonetheless, the paper's results are limited by the fact that the confidence intervals do not rule out large changes in other assets or liabilities, and by the insignificant and imprecisely estimated effects on net worth.

A number of informative conclusions about outstanding issues surrounding 401(k)s are possible. The estimates indicate that 401(k) eligibility may increase IRA saving, perhaps because 401(k) participation brings individuals into contact with financial markets in ways that encourage IRA saving. This contrasts with the usual presumption that 401(k) eligibility decreases saving in other forms. It is worth noting that differences in median financial assets between those eligible and ineligible for a 401(k) are several times as large as median 401(k) balances for eligibles (Poterba, Venti, and Wise 1994). This could indicate substantial "crowd-in" of non-401(k) financial assets in response to 401(k) eligibility, which is consistent with my results on IRAs. The evidence is consistent with the view that 401(k)s help younger individuals to overcome

barriers to saving in IRAs, whereas it has no such effect for older individuals who may be more sophisticated about financial matters. It is important to note that since my sample is comprised disproportionately of younger individuals, the estimates on the full sample are weighted toward estimating positive effects on IRA saving. Thus, the estimates suggest that the effect of eligibility on IRA saving may be less positive or even negative in an age-representative sample.

In response to 401(k) eligibility, car value falls substantially. Consumer durables can be considered a form of saving, since they continue to have value to consumers well into the future. To the extent that we consider durable goods to be saving, the results therefore suggest that saving in 401(k)s is offset to some extent by decreases in saving in the form of durables. It is worth noting that cars depreciate quickly: A standard rule of thumb is that cars lose 15-20% of their value each year, so that after 5 years, the car will only retain 30%-45% of its initial value. In comparison, money will normally be withdrawn from a 401(k) or IRA only at retirement.

This paper leaves a number of open questions. I examine the impact of 401(k) eligibility on saving and consumption within one year of eligibility, but one wonders about the impact at other time horizons. The estimates apply most directly to the group that reports being temporarily ineligible, but it is possible that other population groups respond differently. Defaulting individuals into 401(k) plans raises 401(k) contributions dramatically (Madrian and Shea, 2001; James Choi, David Laibson, and Madrian, 2004). An open question is whether defaulting people into a 401(k) has a different effect on their total saving than does 401(k) eligibility without a default. A final important outstanding issue surrounds the large magnitude of the estimated IRA effect. While several possible answers were discussed, this remains an important question about the results. Data with a larger sample size might help in measuring this effect, and the impact of 401(k)s on overall net worth, more precisely.

References

Attanasio, Orazio and Thomas DeLeire. 2002. "The Effect of Individual Retirement Accounts on Household Consumption and National Saving." *Economic Journal,* 112(7): 504-38.

Benjamin, Daniel. 2003. "Do 401(k)s Increase Saving? Evidence From Propensity Score Subclassification." *Journal of Public Economics*, 87(5-6): 1259-90.

Bernheim, B. Douglas. "Taxation and Saving." In *Handbook of Public Economics*. Vol. 3. Edited by A. Auerbach and M. Feldstein. Amsterdam: Elsevier, 2002, pp. 1173-1249.

Bernheim, B. Douglas, Patrick Bayer, and John Karl Scholz. 2009. "The Effects of Financial Education in the Workplace: Evidence from a Survey of Employers," forthcoming, *Economic Inquiry*.

Choi, James, Laibson, David, and Brigitte Madrian. 2004. "Plan Design and 401(k) Savings Outcomes." *National Tax Journal*, 57(2): 275-98.

Duflo, Esther, and Emmanuel Saez. 2003. "The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment." *Quarterly Journal of Economics*, 118: 815-842.

Dynan, Karen. 1993. "How Prudent are Consumers?" *Journal of Political Economy*, 101: 1104-1113.

Engen, Eric, and William Gale. 2000. "The Effects of 401(k) Plans on Household Wealth: Differences Across Earnings Groups." NBER Working Paper 8032.

Engen, Eric, William Gale, and John Karl Scholz. 1994. "Do Savings Incentives Work?" *Brookings Papers on Economic Activity*, 1994(1): 85-180.

Engen, Eric, William Gale, and John Karl Scholz. 1996. "The Illusory Effects of Saving Incentives on Saving." *Journal of Economic Perspectives*, 10(4): 113-138.

Gustman, Alan, Thomas Steinmeier, and Nabahid Tabatai. 2009. "Do Workers Know About Their Pension Plan Type? Comparing Workers' and Employers' Pension Information." In Anna Lusardi, editor, *Overcoming the Saving Slump: How to Increase the Effectiveness of Financial Education and Saving Programs*. Chicago: University of Chicago Press.

Hubbard, R. Glenn. and Jonathan Skinner. 1996. "Assessing the Effectiveness of Saving Incentives." *Journal of Economic Perspectives*, 10(4), 73-90.

Joint Committee on Taxation. 2008. *Estimates of Federal Tax Expenditures for Years 2008-2012*. Washington, D.C.: U.S. Government Printing Office.

Madrian, Brigitte, and Dennis Shea. 2001. "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior." *Quarterly Journal of Economics*, 116(4): 1149-1187.

Mitchell, Olivia. 1988. "Worker Knowledge of Pension Provisions." *Journal of Labor Economics*, 6(1): 21-39.

Pence, Karen. 2006. "The Role of Wealth Transformations: An Application to Estimating the Effect of Tax Incentives on Saving" *The B.E. Journal of Economic Analysis & Policy* 5:1. Available at: http://works.bepress.com/karen_pence/1.

Poterba, James, Steven Venti, and David Wise, "401(k) Plans and Tax-Deferred Saving," in David A. Wise, Ed., Studies in the Economics of Aging, Chicago: University of Chicago Press, 1994.

Poterba, James, Steven Venti, and David Wise. 1995. "Do 401(k) Plans Crowd Out Other Personal Saving?" *Journal of Public Economics* 58(1), 1-32.

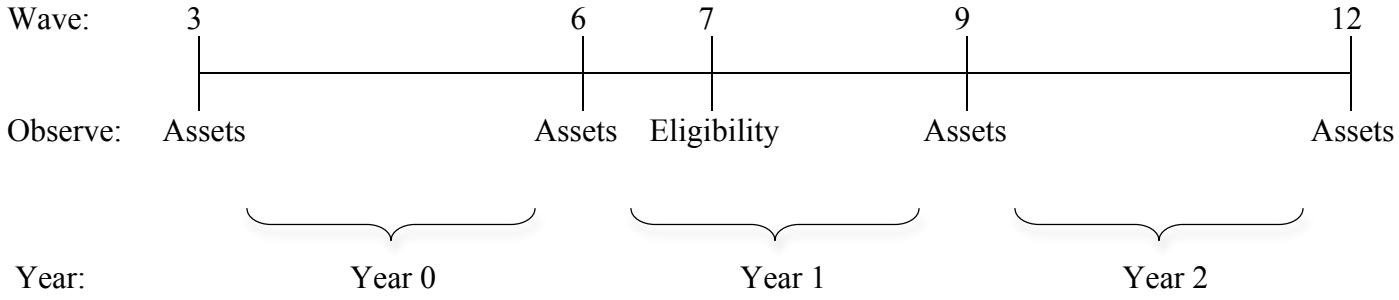
Poterba, James, Steven Venti, and David Wise. 1996. "How Retirement Saving Programs Increase Saving." *Journal of Economic Perspectives*, 10(4): 91-112.

Profit-Sharing/401k Council of America. 1998. "401(k) and Profit Sharing Plan Eligibility Survey." http://www.psca.org/pdfs/elig98.PDF.

Rosenbaum, Paul, and Donald Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70: 41–55.

Venti, Steven, and David Wise. 1996. "The Wealth of Cohorts: Retirement Saving and the Changing Assets of Older Americans." NBER Working Paper 5609.

Figure 1. *Timing of observations*



Notes: Figure 1 shows that assets (or liabilities) are observed in Wave 3, 6, 9, and 12, whereas eligibility for a 401(k) (and whether the individual is temporarily ineligible for a 401(k)) is observed in Wave 7. The period from Wave 3 to Wave 6 is referred to as "Year 0," the period from Wave 6 to Wave 9 is referred to as "Year 1," and the period from Wave 9 to Wave 12 is referred to as "Year 2."

Table 1. *Summary statistics: mean (standard deviation) of main variables*

	(1) All	(2) Treatment Group	(3) Control Group
Age	36.8	35.7	37.5
	(9.8)	(9.4)	(10.0)
Yearly Household Income	56,699.4	52,848.3	58,861.5
	(37,407.3)	(35,987.9)	(38,043.6)
401(k) Assets	6,044.7	4,132.5	7,118.2
	(22,269.0)	(18,079.0)	(24,256.9)
IRA and Keogh Assets	7,834.8	7,464.3	8,042.8
	(27,130.5)	(28,321.0)	(26,463.7)
Other Financial Assets	36,745.8	18,076.2	47,226.8
	(171,386.0)	(61,043.1)	(208,562.7)
Secured Debt	61,681.1	58,166.1	63,654.4
	(77,004.6)	(70,090.7)	(80,623.2)
Unsecured Debt	6,838.0	6,646.1	6,945.8
	(13,860.2)	(13,174.4)	(14,241.7)
Car Value	11,875.0	11,036.64	12,345.6
	(9,268.2)	(8,622.3)	(9,587.5)
N	835	296	539

Notes: Summary statistics are shown for individuals under aged 22-64 who began working at a for-profit firm one year or less before Wave 7 of the 1996 SIPP, who report that their firm offers a 401(k), and for whom there are no missing observations on any of the variables appearing in the main regressions. Values of the variables shown are taken from Wave 6 of the 1996 SIPP. Household income is taken from Year 1. The treatment group is composed of individuals who report that they do not participate in their firm's 401(k) because they have not worked at the firm long enough. The control group represents all others in the sample.

Table 2. *OLS regressions of the change from Year 1 to Year 2 in saving or dissavings, on a dummy for temporary ineligibility (**Become Eligible**) and control variables. Dependent variable shown in column heading*

	(1) 401(k) Assets	(2) IRA Assets	(3) Other Financial Assets	(4) Secured Debt	(5) Unsecured Debt	(6) Car Value
Panel A: No Controls						
Become	.95	.56	-.05	.10	-.09	-.50
Eligible	(.29)***	(.26)**	(.29)	(.35)	(.40)	(.29)*
R-squared	.01	.01	.00	.00	.00	.00
Dollar Equiv.	$2,759.8	$3,319.3	$-927.2	$5,525.8	$-626.8	$-7,650.0
Panel B: With Controls						
Become	.95	.56	-.06	.15	-.10	-.59
Eligible	(.29)***	(.26)**	(.29)	(.36)	(.39)	(.29)**
R-squared	.04	.05	.05	.04	.05	.06
Panel C: With Controls and Initial Balance						
Become	1.04	.52	.02	.16	-.04	-.48
Eligible	(.29)***	(.26)**	(.28)	(.35)	(.37)	(.28)*
R-squared	.07	.08	.11	.08	.13	.12

Notes: The sample includes all individuals aged 22-64 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a firm that offers a 401(k). The dependent variable is defined as $\ln(A_{12})-\ln(A_9)-[\ln(A_9)-\ln(A_6)]$, where A is the category of assets (e.g. 401(k) assets) and the subscript shows the 1996 SIPP wave in question. This represents the change in the individual's accumulation of that asset from Year 1 to Year 2 at the firm. "Become Eligible" is a dummy variable that equals 1 when an individual reports that s/he does not participate in the firm's tax-advantaged pension plan because s/he has not worked long enough at the firm. "Controls" refers to age, age squared, household income, dummies for all possible education categories, dummies for all firm size categories, dummies for 1-digit industry, and days on the job. "Initial balance" refers to a control for $\ln(A_6)$. "Dollar Equiv." refers to the dollar equivalent of the treatment effect implied by the coefficient estimates, calculated by applying the coefficient estimate to the mean Wave 6 value of the asset in question. Dollar equivalents are omitted in Panels B and C because they are similar to those in Panel A. Liabilities are all in positive terms, so that larger numbers represent larger liabilities. All regressions are weighted by the 1996 SIPP final person weights and include a constant term. The sample size is 835 individuals, with 804 household clusters, except in the regressions for "other financial assets," in which the sample is 834 because of an aberrant negative value of one individual's stock holdings. Standard errors are clustered by household. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 3. *Robustness Checks. Column heading shows the dependent variable in question.*

	(1) 401(k) Assets	(2) IRA Assets	(3) Other Financial Assets	(4) Secured Debt	(5) Unsecured Debt	(6) Car Value
Panel A: Controlling for 20-piece spline in initial balance						
Become Eligible	1.04 (.29)***	.55 (.26)**	.08 (.29)	.11 (.35)	-.02 (.37)	-.45 (.29)
R-squared	.08	.07	.11	.10	.14	.12
Panel B: Interacting initial balance with treatment						
Become Eligible	1.10 (.29)***	.51 (.26)*	-.06 (.30)	.33 (.52)	-.34 (.46)	-1.10 (.50)**
Wave 6 Assets* Become Eligible	-.02 (.02)	.01 (.01)	.004 (.004)	-.003 (.004)	.04 (.04)	.05 (.03)
Wave 6 Assets	.04 (.01)***	.01 (.01)	.002 (.001)	.01 (.003)**	.04 (.02)**	.05 (.02)**
R-squared	.07	.04	.04	.03	.06	.06
Panel C: Inverse hyperbolic sine transformation						
Become Eligible	1.34 (.42)***	.82 (.37)**	.09 (.39)	.21 (.50)	-.03 (.57)	-.80 (.42)*
R-squared	.04	.03	.03	.02	.04	.04

Notes: See notes to Table 2. Panel A controls for a 20-piece spline in the Wave 6 balance of the asset in question. Panel B adds an interaction of the initial balance with the treatment dummy, as well as a control for the initial balance. For readability, the coefficients and standard errors on "Wave 6 Assets*Become Eligible" and "Wave 6 Assets" have been multiplied by 1,000. In Panel C, I transform the dependent variable using the inverse hyperbolic sine transformation, as described in the text. All regressions control for age, age squared, household income, dummies for all possible education categories, dummies for all firm size categories, dummies for 1-digit industry, and days on the job.

Table 4. *Robustness Checks. Column heading shows the dependent variable in question.*

	(1) 401(k) Assets	(2) IRA Assets	(3) Other Financial Assets	(4) Secured Debt	(5) Unsecured Debt	(6) Car Value
Panel A: Winsorize Outliers at 5th and 95th percentiles						
Become Eligible	.61 (.19)***	.32 (.16)**	.04 (.23)	.12 (.27)	-.14 (.35)	-.31 (.19)
R-squared	.03	.02	.03	.02	.03	.04
Dollar Equiv.	$1,967.8	$2,072.3	$708.9	$6,591.9	$-1,002.4	$-4,095.1
Panel B: Propensity Score Match						
Become Eligible	.59 (.25)**	.56 (.25)**	.02 (.28)	0.004 (.32)	-0.16 (.38)	-0.50 (.25)**
Panel C: Regressions of Log Wave 9 Assets on Treatment Dummy, Log Wave 6 Assets, and Controls						
Become Eligible	-.46 (.14)***	-.20 (.13)	.06 (.14)	-.13 (.18)	-.11 (.18)	.15 (.14)
Wave 6 Assets	.62 (.04)***	.74 (.03)***	.57 (.03)***	.68 (.03)***	.39 (.03)***	.45 (.05)***
R-squared	.46	.59	.54	.53	.21	.25
Panel D: Regressions of Wave 9 Assets on Treatment Dummy, Wave 6 Assets, and Controls						
Become Eligible	-1,964.0 (1033.6)*	-3,053.7 (1516.1)**	-4,567.9 (4164.7)	2,653.7 (2908.1)	400.9 (819.6)	-149.7 (470.3)
Wave 6 Assets	.44 (.09)***	.77 (.14)***	.29 (.10)***	.74 (.05)***	.24 (.07)***	.53 (.04)***
R-squared	.28	.40	.28	.65	.17	.41

Notes: In Panel A, I winsorize the dependent variable at the 5th and 95th percentiles and then run the main specification. In Panel B, the propensity score match is based on 328 treatment observations and 585 control observations. Observations are matched on age, education, household income, gender, household size, firm size, 1-digit industry, and value in both Wave 3 and Wave 6 of the 401(k) balance, IRA balance, other assets, secured debt, unsecured debt, and car value, using stratified matching. The specification yields four blocks, which are balanced along the covariates. The sample size is smaller than that in Table 2 because some individuals are present in Wave 6 but not Wave 3. Panels C and D use 1096 observations, some of whom are not in Table 2 because data on these individuals does not appear in Wave 12. Results are similar when the sample is limited when the sample is limited to that in Table 2. In Panels C and D, the coefficient on the Become Eligible dummy represents the negative of the estimated effect of 401(k) eligibility on the asset in question.

Table 5. *OLS regressions of the change from Year 0 to Year 1 in saving or dissaving, on a dummy for temporary ineligibility (**Become Eligible**) and control variables*

	(1) 401(k) Assets	(2) IRA Assets	(3) Other Financial Assets	(4) Secured Debt	(5) Unsecured Debt	(6) Car Value
Become Eligible	-.12	-.54	.33	-.27	-.35	.15
	(.26)	(.33)	(.35)	(.44)	(.51)	(.35)
R-squared	.02	.04	.05	.02	.05	.05

Notes: For asset or liability category A, the dependent variable is defined as $\ln(A_9)-\ln(A_6)-[\ln(A_6)-\ln(A_3)]$, where the subscript shows the 1996 SIPP wave in question. The sample size is 1027 individuals, with 996 household clusters. The sample includes those who began work at the firm at most 1 year before Wave 7 of the 1996 SIPP, who are either temporarily ineligible for the 401(k) or who are always ineligible because their firm does not offer a 401(k). The coefficient estimates are similar when estimated through a propensity score match.

Table 6. *Results of propensity score match. Dependent variable: change from Year 1 to Year 2 in IRA saving*

	(1) No Initial 401(k)	(2) Initial 401(k)	(3) Less Education	(4) More Education	(5) Under 45	(6) At Least 45
Become Eligible	.61	.36	.50	.67	.89	-.27
	(.28)**	(.57)	(.45)	(.36)*	(.23)***	(.69)
Dollar Equiv.	$3,554.6	$2,295.1	$3,026.4	$3,827.7	$4,751.4	$2,349.0
N	201	712	355	544	702	211

Notes: Observations are matched using the propensity score described in the notes to Panel A of Table 3. The combined sample size in Columns 3 and 4 is smaller than the combined sample size in Columns 1-2 and 5-6 because 14 observations are excluded from the regressions in Columns 3 and 4 in which education is missing.

Appendix Table 1. *Robustness Checks. Column heading shows the dependent variable in question.*

	(1) 401(k) Assets	(2) IRA Assets	(3) Other Financial Assets	(4) Secured Debt	(5) Unsecured Debt	(6) Car Value
Panel A: Those reporting temporary ineligibility outside of Year 1 included in treatment group						
Become Eligible	.82 (.25)***	.46 (.21)**	-.04 (.24)	.10 (.28)	.32 (.32)	-.48 (.22)**
R-squared	.01	.00	.00	.00	.00	.00
N	1,158	1,158	1,157	1,158	1,158	1,158
Panel B: Limit sample to those on same job in Year 2						
Become Eligible	1.04 (.32)***	.50 (.28)*	-.08 (.32)	.27 (.38)	-.24 (.42)	-.52 (.30)*
R-squared	.04	.04	.05	.03	.04	.04
N	721	721	720	721	721	721
Panel C: Include in control group individuals never eligible for 401(k)						
Become Eligible	.63 (.23)***	.50 (.24)**	-.15 (.26)	.03 (.31)	.26 (.36)	-.34 (.25)
R-squared	.02	.01	.01	.01	.01	.00
N	2,299	2,299	2,296	2,299	2,299	2,299
Panel D: Control for Wave 6 level of assets						
Become Eligible	1.01 (.29)***	.58 (.26)**	.02 (.29)	.15 (.36)	-.09 (.39)	-.51 (.29)*
Wave 6 Assets	.03 (.01)***	.01 (.01)	.002 (.001)**	.005 (.002)***	.06 (.02)	.06 (.02)***
R-squared	.07	.04	.04	.03	.05	.06
N	835	835	834	835	835	835

Notes: See notes to Table 2. All regressions control for age, age squared, household income, dummies for all possible education categories, dummies for all firm size categories, dummies for 1-digit industry, and days on the job. In Panel A, the treatment group includes all those reporting that they are temporarily ineligible for a 401(k), including those who do not report that they are in their first year of the job. The control group consists of always-eligibles, as in the main analysis. In Panel B, the sample is limited to those who stay at the same job from Year 1 to Year 2. In Panel C, I include in the sample all individuals who started their job within 1 year of Wave 7 of the 1996 SIPP. This includes individuals who are temporarily ineligible for their 401(k) in Year 1 (the treatment group), individuals eligible for their 401(k) in Year 1 (who comprise the control group in the main regressions of the paper), and individuals who are in a firm that does not offer a 401(k) and who are therefore ineligible for a 401(k) in both Year 1 and Year 2. In Panel D, I control for the level of Wave 6 assets of the type in question. For readability, in reporting the coefficients in Panel D, the actual coefficients and standard errors on the "Wave 6 Assets" variable have been multiplied by 1,000.