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

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How do 401(k)s Affect Saving and Consumption?

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Abstract

How 401(k) eligibility affects saving is a major unresolved issue. I address this question by exploiting a plausibly exogenous change in 401(k) eligibility: Some individuals are ineligible for their firm's 401(k) plan when they begin to work at the firm, but become eligible when they have worked at the firm long enough. I find that 401(k) eligibility raises saving in the 401(k) substantially, but I find no evidence that 401(k) saving is offset by decreases in other financial assets. I also find no evidence that increases in saving following 401(k) eligibility are driven by intertemporal subsitution. In response to 401(k) eligibility, accumulation of durable goods decreases significantly, providing the most direct existing evidence of a decrease in consumption in response to eligibility.

Keywords: Saving; Retirement

JEL Classification: H2; H31

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1 Introduction

Does 401(k) eligibility raise saving? When eligible for 401(k)s, individuals could save in their 401(k)s rather than saving in other forms. In principle, the positive income effect of tax-advantaged savings programs (such as 401(k)s) could even decrease personal saving. Moreover, as a tax expenditure, 401(k)s could decrease public saving by more than they increase private saving. Since a large fraction of personal savings in the U.S. is in 401(k)s, these issues are crucial in designing strategies to raise U.S. savings rates.

Previous work on the effect of 401(k) eligibility on saving has not reached a consensus. Poterba, Venti, and 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, in a cross-section of households. They also compare the financial assets of eligible and ineligible households in repeated cross-sections. Both of these strategies indicate that 401(k) saving is not offset by decreases in other financial assets.[2] Venti and Wise (1996) compare the assets of individuals who are statistically similar, except that they reached a given age in different calendar years. As a result, some cohorts had longer than others to contribute to special saving programs. They find that those cohorts who 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. Engen, Gale, and Scholz (1994, 1996) use the same data as Poterba, Venti, and Wise (1995) and Venti and Wise (1996) and use similar strategies for identification. However, Engen, Gale, and Scholz (1994, 1996) find that 401(k) eligibility has no effect on overall saving, in part because 401(k) saving is offset by decreases in home equity.

While these papers have made important contributions to our understanding of 401(k)s and saving, there are several acknowledged limitations of the approaches they take (Bernheim, 2002). First, workers may have unobserved tastes for saving that may be correlated, even conditional on observables, with 401(k) eligibility. For example, those with higher unobserved tastes for saving may choose more often to work in firms that offer 401(k)s than those with lower unobserved tastes for saving. Second, turning to the analysis based on repeated cross-sections, the composition of the population of households ineligible and eligible for 401(k)s has changed over time, as more employers began to offer 401(k)s. Therefore, the unobserved savings tastes of the ineligible and eligible populations may not have been constant over time. Third, in the data used in this literature, households' wealth in Defined Benefit (DB) and Defined Contribution 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, turning to the

[2] 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.

cohort-based analysis of Venti and Wise (1996), it is possible that successively younger cohorts had different propensities to save than older cohorts, which would confound comparisons across cohorts. Finally, these papers leave open the question of whether 401(k) saving has an effect on direct measures of the level and form of consumption. If 401(k)s raise saving, then consumption should correspondingly fall.

The empirical strategy of this paper attempts to address all of these issues. In the context of a debate over the existing evidence, there is value in introducing a new empirical strategy to re-evaluate the evidence. I use longitudinal data on households' savings decisions, in combination with a plausibly exogenous within-person change in 401(k) eligibility, to identify the effect of 401(k) 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 compared to the change in saving of a control group: households containing individuals who are eligible for the 401(k) in both their first and second year at their jobs.

My estimates indicate that while 401(k) eligibility raises 401(k) contributions substantially, eligibility does not crowd out saving in other financial assets and does not increase net liabilities. Indeed, I find suggestive evidence that savings in IRAs may rise in response to 401(k) eligibility, consistent with a "crowd-in" hypothesis (Bernheim, 2002). I find that the increase in saving corresponds to a decrease in consumption. Households' accumulation of durable goods falls substantially and significantly when eligible for a 401(k). Since durable goods arguably represent a form of saving, the results indicate that our view on how much 401(k) eligibility raises saving may in turn depend on the extent to which we consider durable goods holdings to be present consumption.

The remainder of the paper is organized as follows. The data and identification strategy are described in Section 2. Section 3 proceeds to the results. Section 4 concludes.

2 Data and Empirical Strategy

A. Identification Strategy and Sample Description

In the main results, the change in savings 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 savings from Year 1 to Year 2 of those who are always eligible.[3] This strategy addresses several important issues. First, it allows individual fixed effects and exploits within-person variation in 401(k) eligibility, which addresses the problem of unobserved heterogeneity. Second, while wealth in DB plans and non-401(k) DC plans is unobservable in the data, it is reasonable that the present discounted value of wealth in these forms does not change differentially over time in the treatment and control groups. Third, the dependent variable is saving or expenditure, rather than net worth. By examining expenditure measures, I conduct an additional test of the hypothesis that 401(k)s raise saving. Finally, I seek to illuminate new and more detailed facets of consumption and saving behavior, by examining the response of different types of consumption and saving to 401(k) eligibility. I now describe the data and regression specification in detail.

The data are drawn from the 1996 SIPP, in which 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, liabilities, and measures of expenditures 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. Whether the individual is temporarily ineligible for the 401(k) is observed in Wave 7.[5]

In constructing the main variables, I use responses to several questions. If individuals answer "yes" to both of the following two questions, then I code their firms as sponsoring a tax-deferred pension plan. Respondents are asked, "Now I'd like to ask about retirement plans offered on this job, not Social Security, but plans that are sponsored by your (job/business). This includes regular pension plans as well as other kinds of retirement plans like thrift and savings plans, 401(k) or 403(b) plans, and deferred profit-sharing and stock plans. Does your (job/business) have any kind of pension or retirement plans for anyone in your company or organization?" They are also asked, "Is the plan something like a 401(k) plan, where workers contribute to the plan and their contributions are tax deferred?" In other words, if the firm offers a pension, and the pension plan is tax-deferred, and the individual works at a for-profit firm, then the individual works at a firm that offers a 401(k). Pensions from both individuals' primary and secondary jobs are included. Individuals who do not participate in their firm's tax-deferred pension plan are also asked, "Reason respondent not covered by pension plan. Why are you not included? Haven't 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).

[3] "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.

[4] The exact dates depend on the SIPP rotation group to which an individual belongs.

[5] No other SIPP panel both has data on whether the individual is temporarily ineligible for the 401(k) and has data on Year 2 saving. All other SIPP panels are shorter than the 1996 panel.

Individuals must possess several characteristics to be included in the sample. They must work at a for-profit firm (because the law requiring firms to exclude employees for at most one year applies only to for-profit firms). They must have started the job they are observed to hold in Wave 7—the wave when the question about temporary ineligibility is asked—one year or less before Wave 7. Except where otherwise noted, they must also work at a firm that offers a 401(k) plan. Limiting the sample to those individuals who remain at the same job from Year 1 to Year 2 yields extremely similar results to those reported in the text. Following the previous literature the sample, is limited to individuals under 65, thus avoiding issues relating to the decumulation of assets at retirement.[6]

B. Main Specification

In the central regressions, 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). The dependent variable is the difference in savings between Year 1 and Year 2. In particular, if A_i^n represents the level of a given type of assets, liabilities, or expenditures of individual i in wave n of the 1996 SIPP, then the dependent variable (in most of the regressions) is $Y_i = [\ln(A_i^{12}) - \ln(A_i^9)] - [\ln(A_i^9) - \ln(A_i^6)]$. Each value of A_i^n has been replaced by $A_i^n + 1$, so that the logarithm of the variable is defined for all observations. 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. The logarithmic specification is appropriate because assets and liabilities are approximately log-normally distributed. Other ways of addressing the non-normality of the distribution include estimating the results in levels but trimming outliers, or by estimating median regressions. Trimming outliers produces similar estimates to those shown here, but the significance of the estimates is reduced. Median regressions show similar results, although unsurprisingly, the treatment dummy almost always has a smaller estimated impact on the median than on the mean. 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 be considered to be in first differences, since the dummy that equals 1 when an individual is temporarily ineligible 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).

The regression equation is therefore specified as:

$$Y_i = \beta_0 + \beta_1 T_i + X_i\beta + \varepsilon_i, \tag{1}$$

[6]Poterba, Venti, Wise (1995, 6), Engen, Gale, and Scholz (1994, 6), and Benjamin (2003) also limit the sample to those under retirement age.

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 represents a vector coefficients on these controls, and ε_i is an error term. Note that in a panel with two periods, this first-differenced regression is equivalent to a differences-in-differences regression with individual fixed effects.

C. Possible Limitations

It may be that only individuals who have a particularly high taste for saving will 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. The estimates should be interpreted as local to this group. However, the percentage of the sample in the treatment group (42%, both weighted and unweighted) is not far below what one would expect from a survey of firms regarding their 401(k) plans (Profit-Sharing/401k Council of America 1998). This survey found that 43.8% of for-profit employers require employees to wait a full year before participating in a 401(k). To address the possibility of unobserved heterogeneity, I also match individuals in the treatment and control groups with a propensity score, which is all the more powerful in combination with fixed effects.

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 savings in other forms in response to 401(k) eligibility, thus answering the question of how much a dollar of 401(k) saving crowds out other saving. Given a certain level of 401(k) contributions, it will then be possible to calculate how much of this represents new saving.

D. Summary Statistics

Summary statistics, which show values of the covariates from Wave 6 of the 1996 SIPP, are shown in Table 1. 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 smaller employers, and those that experience higher turnover, are more likely to exclude employees temporarily from participating in the firm's 401(k). The fixed costs of setting

up 401(k) are more burdensome for smaller and higher-turnover firms. Smaller firms and higher-turnover firms tend to have workers who are younger and have lower income and assets.

3 Results

A. Preliminary Evidence

Figure 1 shows the pattern of 401(k) balance increases in the treatment and control groups in Years 1 and 2. For individuals in the control group, the increase in the 401(k) balance is nearly identical in Year 1 and Year 2. For individuals in the treatment group, the increase is dramatically larger in Year 2 than in Year 1. The increases in the balance represent both the increase in the price of existing 401(k) assets and the value of new 401(k) contributions (minus the value of 401(k) withdrawals, which are rare in this sample). The stock market was increasing in value quickly during the period in question (the late 1990s), so existing assets usually increased in value quickly, which contributes heavily to the increase in 401(k) balances in the treatment group in Year 1. The percentage increases are large in part because these individuals are young and have small 401(k) balances, so that each year's worth of contributions represents a substantial percentage increase in the 401(k) balance. To adjust for the influence of other covariates, I turn to the regressions.

B. Main Results

The main results are displayed in Table 2. The first column displays the coefficients on the treatment dummy when no controls are included in the regressions. Given a coefficient estimate, it is possible calculate the dollar value of the effect of treatment that is implied by the coefficient. The second column displays these dollar equivalents, calculated by applying the coefficient estimates to the mean asset values in the treatment group in Wave 6. The third column displays a specification in which the control variables are age, age squared, household income, and dummies for four education categories (no high school diploma, exactly a high school diploma, some college, and college graduate). The fourth column shows the dollar equivalents of the coefficient estimates in the third column. Results are generally extremely similar under the two different specifications, and are similar with other choices of the control variables (including controlling for whether the individual has a defined benefit pension). Robust standard errors are clustered by household. The coefficients on the control variables are almost always insignificantly different from zero at the 30% significance level, and they have not been shown in the tables. The R-squared is always small (usually below .01), which is unsurprising given the large amount of noise in measuring assets and the first-differenced specification, and it has also been omitted from the tables. In the tables, liabilities are all in positive terms, with a larger number representing a larger

liability (so that a larger value of a liability represents smaller net worth, all other things equal).

In the discussion that follows, I restrict attention to columns 3 and 4 of Table 2, since the results are similar with and without controls. In row 1 of Table 2, in which the dependent variable is the 401(k) balance, the coefficient on "Temporarily Ineligible" (.32) is significant at the 1% level. Of course, it is unsurprising that 401(k) eligibility raises saving in 401(k)s. Column 4 shows that 401(k) eligibility is estimated to cause an increase of $1172 in 401(k) savings. If initial assets are zero, the percentage increase in assets may be large. To address this concern, I also estimate regressions that include a dummy for beginning Year 1 with a zero 401(k) balance (or zero of another asset), or by adding a dummy for beginning Year 1 with a small 401(k) balance (e.g. a balance below $100). The results are substantively unchanged by adding such a dummy.

Interestingly, in row 2, in which the dependent variable represents the difference between Year 1 and Year 2 in the increase in the logged IRA balance, the coefficient on "Temporarily Ineligible" is positive, with a substantial coefficient that is significant at the 10% level. While the dollar equivalent of the point estimate is large, it is also imprecisely estimated. 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 often teaches individuals about financial markets. Eligibility often comes with reminders by one's firm to save, pamphlets emphasizing the importance of retirement savings, 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 (row 3), is negative, but relatively small and insignificant.

Turning to secured and unsecured debt, the point estimates are all insignificant and negative. The negative sign may appear puzzling, and I return to this issue shortly. The 95% confidence interval bounds the possible increase in debt at a relatively low level. As before, the results are similar under OLS and under the matching estimator.

If 401(k) eligibility raises saving, then consumption should correspondingly decrease. I investigate holdings of durable goods in the final row of the table. The SIPP contains measures of the total value of various durable goods that households may purchase. Here, durable goods accumulation over a period of time is defined as the change in the total value of households' durable goods holdings over that time.[7] The SIPP has a measure of the value of households' holdings of several durable goods: cars, boats, motorcycles, and RVs. Therefore, I use the change in the total value of households' holdings of all of these goods as the measure of durable goods expenditure. While this

[7]Note that a change in the value of durable goods holdings could result from purchases or sales of durable goods, or from appreciation or depreciation of the value of existing durable goods holdings.

does not measure the value of all durable goods that one might purchase, it represents a proxy for total durable goods holdings, just as food consumption is often used as a proxy for total consumption.

Accumulation of durable goods falls substantially in response to eligibility, and the coefficient estimate is highly significant. The dollar equivalent of the effect on durable goods value is surprisingly large, though imprecisely estimated with 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 durable goods value.[8] Since there are large outlier observations of durable goods value, it is not surprising that I estimate sizeable but imprecisely estimated coefficients.

It is possible that the fall in durable goods value could account for the fall in debt observed earlier. If households spend less on durable goods, they need not accrue as much debt to pay for these durable goods.[9] Indeed, a regression of the change in secured debt accumulation from Year 1 to Year 2 on the change in durable goods value from Year 1 to Year 2 and the controls yields a coefficient on the change in durable goods value of .30 (with a standard error of .06)l. Running this regression on only the sample of those who report being temporarily ineligible for the 401(k) yields similar results (a coefficient of .33 on the treatment dummy, with a standard error of .10). Thus, it seems that decreases in durable goods accumulation could account for decreases in debt accumulation.

The SIPP has only a handful measures of nondurables consumption, such as expenditures on commuting and expenditures on utilities. The point estimates suggest small and insignificant responses of these variables. Since these measures are spotty and idiosyncratic measures of nondurables consumption, the results are omitted.

In sum, the main results show a substantial increase in 401(k) saving in response to 401(k) eligibility. Little offset is seen in other financial assets or liabilities. Indeed, the point estimates indicate a weakly significant increase in IRA saving. 401(k) eligibility leads to a decrease in accumulation of durable goods in response to eligibility, which is further evidence for an increase in saving.

C. Validity Checks

As noted earlier, it is possible that individuals in the treatment and control groups differ along unobserved dimensions. The presence of individual fixed effects helps to

[8] If durable goods are indivisible and individuals are liquidity constrained, 401(k) eligibility could even cause one to hold off on durable goods purchases whose value might exceed one's 401(k) contributions.

[9] This is consistent with several possible models of rational behavior. For example, if durable goods are indivisible and individuals are liquidity constrained, then when individuals save more, they could forego a durable goods purchase that they would otherwise have financed with debt.

mitigate this concern, as they remove individual unobserved effects that are constant over time. However, if the unobserved differences differentially influence the savings paths of households in the treatment and control groups, then even fixed effects may not be sufficient to address unobserved hetergeneity. To help address this concern, I have also estimated the results using a propensity score match in Table A1.[10] Observations in the treatment and control groups are matched according to years of education, age, household income, marital status, household size, and firm size, using stratified matching. The first stage results in five blocks, which are balanced along the covariates. The coefficients and standard errors using the propensity score estimator are extremely similar to those relying on OLS.

Table 3 presents an alternative set of estimates of the effect of 401(k)s on saving. 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. Table 3 regresses Year 1 saving on the treatment dummy and controls. To compare the Table 2 point estimates with those in Table 3 more readily, the treatment dummy is defined differently in Table 3 than in Table 2. In Table 3, the treatment dummy equals 0 when the individual *does* report being temporarily ineligible for the 401(k), and equals 1 when the individual *does not* report being temporarily ineligible for the 401(k). Thus, the estimated coefficient on the treatment dummy in Table 3 represents the estimated effect of 401(k) eligibility on the dependent variable (as it does in Table 2).

The regressions in row 1 of Table 3 again show a positive and significant effect of 401(k) eligibility on 401(k) savings. There is again a positive and significant effect on IRA savings (significant at 5% with no controls), and again a negative and highly significant of 401(k) eligibility on durable goods accumulation. As expected, the estimated effect of eligibility is always similar in Tables 2 and 3. Like Table 2, Table 3 shows that 401(k) eligibility causes a decrease in debt and an increase in other assets, though these estimates are again insignificant. The propensity score results are again similar to those from the OLS regression and have been omitted.

Still another robustness check involves a linear specification, rather than the logarithmic specification employed thus far. In a linear version of the regressions in Table 3, assets (or liabilities) in wave 9 are regressed on the treatment dummy, assets in wave 6, and controls.[11] These regressions are displayed in Table 4. In the version with controls,

[10] A matching estimator was first used to estimate the effect of 401(k) eligibility on savings by Benjamin (2003), who compares the net worth of those eligible and ineligible for a 401(k) in a cross-section.

[11] Since a linear specification will create additional noise in the dependent variable (due to non-normality), it makes sense to estimate the results on the cross section as in Table 3, rather than the first-differenced results analogous to Table 2 (particularly given that the results are similar in Tables 2 and 3).

there is a positive, significant, and large effect of 401(k) eligibility on the 401(k) balance and the IRA balance. All other effects are insignificant, though the point estimate of the effect on durable goods is still negative. Since this regression is in levels, I can investigate the effect on net worth. (Earlier it was not possible to investigate the effect on net worth, since net worth is sometimes negative and it is not possible to take its log.) There is a positive and statistically significant effect on net worth. Focusing on the version with controls, while the point estimate of the effect of eligibility on net worth is large ($11,110), it is imprecisely estimated, with a 95% confidence interval that does not rule out a relatively small effect of $1,000. Thus, the true effect on net worth could be much smaller. Given the large amount of noise in measuring net worth, it is unsurprising to find a large but imprecisely estimated effect. The size of the effect also makes sense because the estimated effect of eligibility on IRA assets was positive. 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; Engen, Gale, and Scholz 1994). It is possible that this indicates substantial "crowd-in" of non-401(k) savings in response to 401(k) eligibility, which could also be the case here.

In principle, the results thus far could in part represent intertemporal substitution. In other words, temporarily ineligible households could be waiting to save until they become eligible for their 401(k)s, holding down their saving while they are temporarily ineligible. While intertemporal substitution is possible in theory, empirical work usually finds a very small elasticity of intertemporal substitution (e.g. Dynan, 1993), suggesting that this should not be a major concern in practice. The hypothesis that households are intertemporally substituting predicts that the 401(k) saving of temporarily ineligible individuals should fall from Year 0 to Year 1 as they wait to save in their 401(k)s. Meanwhile, there is no expected future increase in the rate of return on saving in other forms. Saving in other forms could rise as those temporarily ineligible for their 401(k)s substitute into saving other forms.

If individuals are engaging in intertemporal substitution, then savings in Year 1 will be lower than it would have been, if individuals were *always* ineligible for the 401(k). This implies that I can test for intertemporal substitution 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 engaging in intertemporal substitution, then those who are temporarily ineligible should save less in Year 1 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 savings drops more 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. Table 5 presents the latter test.

In Table 5, the dependent variable is the change in logged assets, liabilities, or

expenditures from Year 0 to Year 1. 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. This reduces the sample size to 656 observations. The treatment dummy in Table 5 (and all subsequent tables) is defined as it was in Table 2. The dummy equals 1 when the individual *does* report being temporarily ineligible for the 401(k), and equals 0 when the individual *does not* report being temporarily ineligible for the 401(k). Thus, the estimated coefficient on the treatment dummy in Table 4 represents the estimated effect of 401(k) eligibility in Year 2 on the dependent variable (as in all other tables).

In Table 5, row 1, where 401(k) saving is the dependent variable, the coefficient on the treatment dummy is -.03 (p>.40) with a small equivalent dollar value of -$110. This suggests that any temporary decrease in 401(k) saving in Year 1 is small. Furthermore, individuals will be eligible for their 401(k)s long into the future. Presumably, the elevated level of saving in Year 2 in response to 401(k) eligibility, maintained over a large number of years, will be much larger than a potential small decrease in 401(k) saving in Year 1.[12] Thus, even if a bit of intertemporal substitution is occurring, 401(k)s appear to raise saving overall. As before, two variables with many large outlier observations (secured debt and durable goods) unsurprisingly have sizeable but imprecisely estimated and insignificant coefficients. I find some evidence of substitution into other forms of saving while individuals are temporarily ineligible for 401(k)s, as saving in other assets rises (though not significantly). The results are again similar when estimated through a propensity score match, and these results have been omitted.

[12] As noted above, the treatment dummy may be correlated with the change from Year 1 to Year 2 in the amount of time eligible for the 401(k) to a different degree in Table 4 than in Table 2 (or Table 3). Thus, the main question of interest is how the fall in the rate of increase of the 401(k) balance from Year 0 to Year 1, relative to changes from Year 0 to Year 1 in the accumulation rate of other assets or liabilities, compares to the rise in the rate of increase of the 401(k) balance from Year 1 to Year 2, relative to changes from Year 1 to Year 2 in the accumulation rate of other assets or liabilities. These relative comparisons also suggest that intertemporal substitution is not a cause for worry.

D. Durable Goods

I further examine durable goods in Table 6. I first investigate the value of cars, which are a major expenditure item for the young individuals in this sample. Indeed, in the control and treatment groups combined, 24% of people purchased at least one new vehicle in Year 0. Car value increases are affected by 401(k) eligibility. The coefficient on the treatment dummy is -.33, significantly different from 0 at the 1% level. This corresponds to a strong effect of 401(k) eligibility on the number of vehicles that individuals own. Column 2 shows that eligibility causes a mean decrease in the number of vehicles of -.38, with an estimate that is also extremely significant. The value of other vehicles is not significantly affected, with a very small equivalent dollar value. This is unsurprising given that relatively few people own other sorts of vehicles. Interestingly, the point estimate for the value of housing is positive, though insignificant and imprecisely estimated. Though the coefficient appears small, the equivalent dollar value is large, since housing assets are large.

In sum, Table 6 suggests that individuals hold off on purchases of new cars (or selling existing ones) in response to 401(k) eligibility, and there is a corresponding decrease in the value of their vehicles. The Table 6 results are again similar when estimated with a propensity score match, and these results have been omitted.

E. IRA Assets

Evidence that 401(k) eligibility "crowds in" of other forms of saving is interesting in part because it contradicts the usual presumption that 401(k) eligibility decreases saving in other forms. As noted earlier, "crowd-in" is nonetheless consistent with a number of theoretical frameworks and certain existing empirical evidence. If savings programs serve in part to increase individuals' propensity to save even in non-favored forms of saving, then models of saving must take this into account. This paper finds evidence that IRA saving increases in response to 401(k) eligibility. Columns 1 and 2 of Table 7 break down the sample by prior 401(k) participation. Column 1 indicates that among those who previously had no 401(k), there is a strong and highly significant effect of 401(k) eligibility on IRA savings. By contrast, Column 2 shows that among those who previously had a 401(k), there is an insignificantly negative effect of 401(k) eligibility. These results are consistent with the typical story of crowd-in, in which 401(k) eligibility brings those who did not previously have a 401(k) into greater contact with saving instruments, thus leading them to save more even in non-401(k) savings vehicles. Bernheim, Garrett, and Maki (2001) show that education is important in taking advantage of savings opportunities. It is apparent in Columns 3 and 4 that the treatment effect is much stronger among the more educated.

4 Conclusion

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 decreases in holdings of other types of financial assets. In fact, 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. Consistent with a low intertemporal elasticity of substitution, I find no evidence that these results stem from intertemporal substitution. While Engen, Gale, and Scholz (1996) find that 401(k) saving is offset by decreases in home equity, no evidence is found that home equity decreases in response to 401(k) eligibility. The point estimates suggest that both secured and unsecured debt fall in response to 401(k) eligibility, albeit insignificantly.

I find that in response to 401(k) eligibility, accumulation of durable goods falls substantially and significantly. This represents the most direct existing evidence of a decrease in consumption in response to 401(k) eligibility, since smaller durable goods stocks correspond to smaller associated consumption flows. Consumer durables holdings can be considered a form of saving, since they continue to have value to consumers well into the future (e.g. Fernandez-Villaverde and Krueger, 2001; Moulton, 2001). 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. If, on the other hand, we consider consumer durables to be consumption at the time of their purchase (as they are treated in the National Income and Product Accounts), then this paper finds no evidence of any decreases in saving outside the 401(k) in response to 401(k) eligibility.

It is worth nothing that the durable good (cars) for which the crowdout results are strongest depreciates 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 an IRA) only at retirement. Hence, 401(k) (or IRA) saving shifts consumption much further into the future than does durable goods "saving" in the form of a car. While the estimates are insignificant, the point estimates for debt suggest that the fall in durable goods accumulation may correspond to a decrease in debt, as households need to accumulate less debt to finance their durable goods purchases. Overall, the results suggest a multifaceted relationship between 401(k)s and saving, as falling durables purchases are perhaps accompanied by a fall in debt.

This paper leaves a number of open questions. First, the identification strategy pursued here examines the impact of 401(k) eligibility on saving and consumption within one year of eligibility. One wonders about the impact at other time horizons. Nonetheless, identifying the effect of 401(k) eligibility at longer time horizons would run into the usual problem that identification becomes more difficult at long time

horizons as other factors affect the dependent variable. Arguably, in this context as in others, identifying the effect from the short-term impact yields the best possible estimate. Second, defaulting individuals into 401(k) plans raises 401(k) contributions dramatically (Madrian and Shea, 2001; Choi, Laibson, and Madrian, 2004). One wonders whether defaulting people into a 401(k) has a different effect on their total saving than does 401(k) eligibility without a default.

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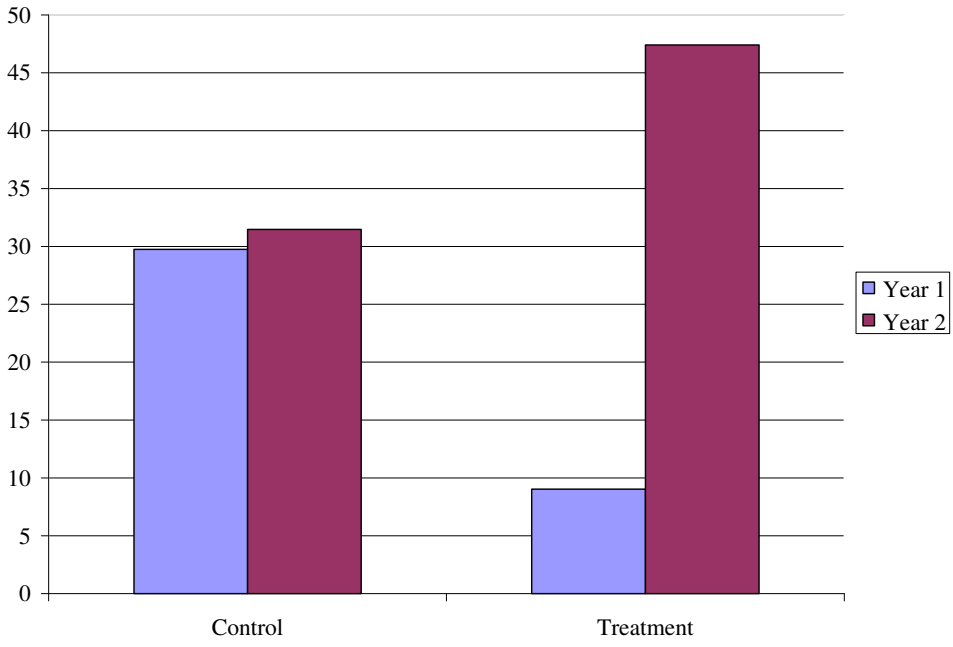
Figure 1. *Percentage increase in mean 401(k) balance in control and treatment Groups, Year 1 and Year 2*



Notes: The figure shows the percentage change in Year 1 and Year 2 in the mean 401(k) balance in the treatment and control groups. Summary statistics are shown for individuals under age 65 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 regressions. 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. Since the figure shows the increase in 401(k) balances, this increase reflects both 401(k) contributions and appreciation of existing 401(k) assets. The figure shows unadjusted mean differences between the treatment and control groups, whereas the regressions in the Tables adjust for a variety of covariates.

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

	(1) All	(2) Treatment Group	(3) Control Group
Age	35.3 (10.7)	33.4 (10.5)	36.7 (11.9)
Yearly Household Income	61795 (39171)	57843 (37594)	64668 (40074)
401(k) Assets	6369 (22621)	3664 (17076)	8334 (25758)
IRA and Keough Assets	7365 (25556)	6942 (26854)	7672 (24595)
Other Assets	2209 (16383)	1202 (3280)	3062 (21322)
Secured Debt	59843 (71789)	53868 (67238)	64186 (74695)
Unsecured Debt	6462 (13411)	6373 (12745)	6526 (13887)
Durables Value	13225 (10423)	12796 (9812)	13374 (10629)
N	799	333	466

Notes: Summary statistics are shown for individuals under age 65 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 regressions. Values of the variables shown are taken from Wave 6 of the 1996 SIPP. In each cell, the mean of the variable in question is shown, followed by its standard deviation in parentheses. 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 savings or dissavings, on a dummy for temporary ineligibility ($TEMP_i$) and control variables*

	No Controls		With Controls	
	(1) Coefficient on $TEMP_i$	(2) Equivalents ($)	(3) Coefficient on $TEMP_i$	(4) Equivalents ($)
401(k) Assets	.28 (.12)**	1026	.32 (.12)***	1172
IRA Assets	.20 (.11)*	1388	.21 (.11)*	1458
Other Financial Assets	-.08 (.16)	-96	-.03 (.16)	-36
Secured Debt	-.02 (.16)	-1077	-.01 (.16)	-323
Unsecured Debt	-.06 (.15)	-382	-.06 (.15)	-382
Durable Goods	-.34 (.11)***	-4351	-.33 (.12)***	-4223

Notes: All regressions control for age, age squared, household income, dummies for four education categories, and a constant term. Standard errors are clustered by household. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a for-profit firm that offers a tax-advantaged employer-sponsored defined contribution pension. For asset or liability category A, the dependent variable is defined as $\ln(A^{12})$-$\ln(A^{9})$-[$\ln(A^{9})$-$\ln(A^{6})$], where the superscript shows the 1996 SIPP wave in question. This represents the change in the individual's saving in that asset from Year 1 to Year 2 at the firm. "Temporarily Ineligible" 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. Liabilities are all in positive terms, so that larger numbers represent larger liabilities. All observations are weighted by the 1996 SIPP final person weights. The sample size is 799 individuals, with 773 household clusters. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 3. *OLS regressions of Year 1 savings or dissavings, on a dummy for always being eligible (AE_i) and control variables*

	No Controls		With Controls	
	(1) Coefficient on AE_i	(2) Equivalents ($)	(3) Coefficient on AE_i	(4) Equivalents ($)
401(k) Assets	.20 (.07)***	733	.20 (.07)***	733
IRA Assets	.15 (.07)**	1041	.13 (.07)*	902
Other Financial Assets	-.03 (.09)	-36	-.001 (.10)	-1
Secured Debt	-.02 (.09)	-1077	-.02 (.09)	-1077
Unsecured Debt	-.06 (.15)	-382	-.06 (.15)	-382
Durable Goods	-.18 (.07)**	-2303	-.15 (.07)**	-1919

Notes: All regressions control for age, age squared, household income, dummies for four education categories, and a constant term. Standard errors are clustered by household. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a for-profit firm that offers a tax-advantaged employer-sponsored defined contribution pension. For asset or liability category A, the dependent variable is defined as $\ln(A^9)$-$\ln(A^6)$, where the superscript shows the 1996 SIPP wave in question. In other words, the dependent variable is saving in Year 1 in a given type of asset. AE_i is a dummy variable that equals 1 when an individual does *not* report 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. Liabilities are all in positive terms, so that larger numbers represent a larger liabilities. All observations are weighted by the 1996 SIPP final person weights. The sample size is 1050 individuals, with 1014 household clusters. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 4. *OLS regressions of Year 1 savings or dissavings, on a dummy for always being eligible (AE_i) and control variables*

	No Controls	With Controls
	Coefficient on AE_i	Coefficient on AE_i
401(k) Assets	3732	2916
	(1146)***	(1107)***
IRA Assets	5054	3217
	(1800)***	(1602)**
Other Financial Assets	6545	1000
	(5997)	(4569)
Secured Debt	-1717	-3261
	(2892)	(2868)
Unsecured Debt	-183	-167
	(850)	(810)
Durable Goods	-156	-347
	(506)	(481)
Total Net Worth	17169	11110
	(7296)**	(5672)**

Notes: All regressions control for age, age squared, household income, dummies for four education categories, and a constant term. Standard errors are clustered by household. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a for-profit firm that offers a tax-advantaged employer-sponsored defined contribution pension. For asset or liability category A, the dependent variable is defined as $\ln(A^9)$-$\ln(A^6)$, where the superscript shows the 1996 SIPP wave in question. In other words, AE_i is a dummy variable that equals 1 when an individual does *not* report 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. Liabilities are all in positive terms, with a larger number representing a larger liability. All observations are weighted by the 1996 SIPP final person weights. The sample size is 1050 individuals, with 1014 household clusters. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 5. *OLS regressions of the change from Year 0 to Year 1 in savings or dissavings, on a dummy for temporary ineligibility ($TEMP_i$) and control variables*

	No Controls		With Controls	
	(1) Coefficient on $TEMP_i$	(2) Equivalents ($)	(3) Coefficient on $TEMP_i$	(4) Equivalents ($)
401(k) Assets	-.04	-147	-.03	-110
	(.10)		(.09)	
IRA Assets	-.16	-1111	-.16	-1111
	(.15)		(.14)	
Other Financial Assets	.17	204	.20	240
	(.17)		(.17)	
Secured Debt	.07	3770	.02	1077
	(.17)		(.18)	
Unsecured Debt	-.02	-127	-.02	-127
	(.16)		(.16)	
Durable Goods	.11	1408	.14	1791
	(.13)		(.14)	

Notes: All regressions control for age, age squared, household income, dummies for four education categories, and a constant term. The sample includes individuals under 65 in their first year of employment at a firm, who are ineligible for the firm's tax-advantaged employer-sponsored defined contribution pension in Wave 7 of the 1996 SIPP. Standard errors are clustered by household. 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 superscript shows the 1996 SIPP wave in question. For nondurable goods expenditures and utilities, the dependent variable is defined as $\ln(A^1)$-$\ln(A^0)$, where A represents the yearly level of expenditures and the superscript shows the year in question. $TEMP_i$ is a dummy that equals 1 if the individual is temporarily ineligible for a 401(k) in Wave 7, and 0 otherwise. Liabilities are all in positive terms, with a larger number representing a larger liability. All observations are weighted by the 1996 SIPP final person weights. The sample size is 656 individuals, with 623 household clusters. The sample is different than that in Table 2 because Table 4 includes both those temporarily ineligible for the 401(k) and those who are always ineligible. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 6. *OLS regressions of the change from Year 1 to Year 2 in durable goods accumulation, on a dummy for temporary ineligibility ($TEMP_i$) and control variables*

	(1) Cars	(2) Number of Vehicles	(3) Other Vehicles	(4) Housing
$TEMP_i$	-.33 (.12)***	-.38 (.17)**	.06 (.12)	.11 (.11)
Equivalents ($):	3960	N/A	43	9085

Notes: All regressions control for age, age squared, household income, dummies for four education categories, and a constant term. Standard errors are clustered by household. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a firm that offers a tax-advantaged employer-sponsored defined contribution pension. $TEMP_i$ is a dummy that equals 1 if the individual is temporarily ineligible for a 401(k) in Wave 7, and 0 otherwise. For all assets A except number of vehicles, the dependent variable is defined as $\ln(A^{12})$-$\ln(A^9)$-$[\ln(A^9)$-$\ln(A^6)]$, where the superscript shows the 1996 SIPP wave in question; for number of vehicles, the dependent variable is $[A^{12}$-$A^9]$-$[A^9$-$A^6]$. "Temporarily Ineligible" 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. Liabilities are all in positive terms, with larger numbers representing a larger liabilities. All observations are weighted by the 1996 SIPP final person weights. The sample size is 799 individuals, with 773 household clusters. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Table 7. *OLS regressions of the change from Year 1 to Year 2 in IRA savings, on a dummy for temporary ineligibility ($TEMP_i$) and control variables*

	(1) No Previous 401(k)	(2) Had Previous 401(k)	(3) More Educated	(4) Less Educated
$TEMP_i$	.37 (.12)***	-.27 (.30)	.32 (.16)**	.07 (.17)
Equivalents ($)	2569	-1874	2221	486
N	606	193	467	332

Notes: All regressions control for age, age squared, household income, dummies for education categories, and a constant term. Standard errors are clustered by household. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a firm that offers a tax-advantaged employer-sponsored defined contribution pension, and the sample is further broken down in each column, as indicated by the column titles. $TEMP_i$ is a dummy that equals 1 if the individual is temporarily ineligible for a 401(k) in Wave 7, and 0 otherwise. Column 3 includes only those individuals with at least some college; column 4 includes only individuals with less than some college. The dependent variable is defined as $\ln(A^{12})$-$\ln(A^{9})$-$[\ln(A^{9})$-$\ln(A^{6})]$, where A represents IRA assets and the superscript shows the 1996 SIPP wave in question. "Temporarily Ineligible" 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. Liabilities are all in positive terms, with a larger number representing a larger liability. All observations are weighted by the 1996 SIPP final person weights. The sample size is 799 individuals, with 773 household clusters. *** indicates significance at the 1% level; ** at the 5% level; and * at the 10% level.

Appendix

Appendix Table 1. *Propensity score results. Dependent variable: change from Year 1 to Year 2 in savings or dissavings*

	Coefficient on $TEMP_i$	Equivalents ($)
401(k) Assets	.29 (.11)***	1063
IRA Assets	.19 (.11)*	1319
Other Financial Assets	-.08 (.16)	-96
Secured Debt	-.02 (.16)	-1077
Unsecured Debt	-.06 (.15)	-382
Durable Goods	-.26 (.12)**	-3327

Notes: A propensity score is used to match individuals by age, years of education, household income, marital status, household size, and firm size, with stratified matching. The first stage results in five blocks, which are balanced along the covariates. The sample includes all individuals under age 65 who report in Wave 7 of the 1996 SIPP that they are in their first year of employment at a firm that offers a tax-advantaged employer-sponsored defined contribution pension. For asset or liability category A, the dependent variable is defined as $\ln(A^{12})$-$\ln(A^{9})$-[$\ln(A^{9})$-$\ln(A^{6})$], where the superscript shows the 1996 SIPP wave in question. "Temporarily Ineligible" 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. Liabilities are all in positive terms, with larger numbers representing larger liabilities. All observations are weighted by the 1996 SIPP final person weights. The sample size is 799 individuals.