DEBT, TAXES, AND THE EFFECTS OF 401(k) PLANS ON HOUSEHOLD WEALTH ACCUMULATION*

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ABSTRACT

This paper examines interactions between tax-preferred assets and tax-preferred debt, each of which has grown dramatically since 1980. For all households and for homeowners, we find that to the extent that eligibility for 401(k) plans raises households' financial assets, the increase is generally offset by reductions in housing equity and in particular by increases in mortgage debt. For renters, the results are somewhat mixed. Because homeowners hold the vast portion of 401(k) balances, our results indicate that, at best, only a small proportion of 401(k) contributions have represented net increments to saving. The results also suggest that the response to 401(k)s can vary across households and highlight an important interaction between household debt and saving.

> "[Financial advisers]...offer lots of formulas to help you save money. Some suggest... increasing your retirement contributions...But doesn't that involve giving up more money right now? Good thing...[we] have come up with [our] own formula: save money by borrowing money, with [our] Home Equity Line of Credit...So call...now, before someone has you saving so much money, you don't have any left." (1997 Radio ad for a midwestern bank)[1]

I. Introduction

Tax-preferred debt and tax-preferred assets have become increasingly important components of households' wealth portfolios. Since 1980, household debt has increased dramatically relative to income or assets. Over the same period, the composition of debt shifted toward mortgages, especially after the Tax Reform Act of 1986 phased out tax deductions for interest on consumer debt. Previous studies indicate that homeowners responded differently to these changes than renters did, but provide little insight on how changes in debt have affected household wealth or saving.[2]

Tax-based saving incentives have also grown rapidly. Contributions to 401(k)s plans, IRAs, and Keogh plans were virtually zero in 1980, but comprised 37 percent of personal saving from 1985 to 1991. From 1986 to 1993, net saving in all tax-preferred plans (including pensions and life insurance saving) was about as large as overall net personal saving (Sabelhaus 1996).

The effects of saving incentives on private saving and wealth have attracted a

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[1]We thank Tom Bowne for bringing this ad to our attention.

[2]Skinner and Feenberg (1990), Manchester and Poterba (1989), Maki (1994) and Scholz (1992b).

great deal of attention, but the literature has largely ignored the role of debt, and has not examined the possibility of different responses by homeowners and renters.[3] Previous studies examined the effects of saving incentives on households' financial assets or net financial assets (financial assets minus non-mortgage debt), but these measures are poor proxies for wealth when debt changes as it has since 1980. In analyses of financial assets, debt-financed purchases of financial assets would be misread as increases in net saving, and debt-financed consumption increases would not be read as reductions in saving. In analyses of net financial assets, shifts in the composition of debt toward mortgages would be misread as increases in saving. These concerns are heightened by the fact that, for the typical household, financial assets are a small fraction of net worth, and the ratio of financial assets to net worth rose significantly over the 1980s.[4]

The lack of research on interactions between debt and tax-preferred saving is surprising given the trends since 1980. In addition, the heart of many tax shelters is

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[3]Bernheim (1996), Engen, Gale and Scholz (1996a), Hubbard and Skinner (1996), and Poterba, Venti and Wise (1996a) provide alternative perspectives on the literature.

[4]In our sample, described below, mean household financial assets were 25 percent of mean net worth in 1984 and rose to 33 percent in 1991. Median holdings of financial assets were 7 percent of median net worth in 1984 and rose to 13 percent in 1991. Aggregate data follow similar trends, with financial assets an even higher proportion of net worth because financial assets are heavily concentrated among the very wealthiest households, which are undersampled in our data set.

precisely the opportunity to invest in tax-preferred assets with borrowed funds that generate tax-deductible interest payments (Steuerle 1985, 1990 and Stiglitz 1988). And, as the quote at the beginning of the paper indicates, financial institutions may have incentives to encourage households to finance tax-preferred assets with tax-preferred debt.[5]

In this paper, we explore one aspect of these interactions by examining the effects of 401(k) eligibility on households' accumulation of wealth--defined as the sum of net financial assets plus housing equity. Using data from the Survey of Income and Program Participation, we find that financial assets and net financial assets increased over time by more for cross-sections of families that had at least one worker eligible for a 401(k) plan than for cross-sections of families where no one was eligible.[6] But wealth did not increase for eligible families relative to other families, and non-401(k) wealth fell by economically and statistically significant amounts that were as large or larger than the increase in 401(k) balances over the period. House value rose for eligible families relative to ineligible families, but mortgage debt grew faster, so that

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[5]The popular financial press has focused significant attention on these issues, with articles in the 1980s sometimes recommending that households borrow money to invest in IRAs (e.g., Anrig 1985, Sudo 1985) and articles in the 1990s examining the extent to which the stock market build-up has been fueled by increases in debt (e.g., Woolley, 1996).

[6]Poterba, Venti, and Wise (1995) obtain similar findings for financial assets using similar data and specification. We discuss the interpretation of this result below.

housing equity fell. Splitting the sample by IRA status, to help control for unobserved tastes for saving, yields similar results.

We also examine the impact of 401(k)s on separate samples of homeowners and renters. Results for the sample of homeowners are similar to the findings for the overall sample, noted above. Results for renters show mixed effects, depending on the sample period. Since about 88 percent of 401(k) balances in our sample are held by homeowners, our results imply that, at most, only a very small proportion of 401(k) contributions have represented net additions to saving.

The results also suggest that families' responses to saving incentives vary in predictable and plausible ways. In particular, groups--like homeowners--that, on average, are older, have higher income and/or wealth, and have greater access to tax-deductible borrowing, find it easier to substitute funds into tax-preferred assets and have greater incentive to do so. Our findings are consistent with results in previous research showing that the impact of saving incentives on broader measures of wealth is typically much smaller than on narrow measures of wealth. Finally, in showing that tax policy, borrowing, and saving can interact in important ways, the results have direct implications for the effects of tax-based saving incentives and broader implications for the design of policy and for future research.

The paper is organized as follows. Section II describes the data. Sections III and IV review trends and previous research on debt and on 401(k)s, respectively. Section V presents estimates of how 401(k) eligibility affects families' wealth accumulation.

Section VI discusses a number of issues in interpreting the results. Section VII provides a short conclusion.

II. Data

We use data from 1984, 1987, and 1991, available in the Survey of Income and Program Participation (SIPP), which is conducted by the Bureau of the Census.[7] Our sample focuses on families where the reference person is 25-64 years old, at least one person is employed, and no individual is self-employed.[8] We use this group for several reasons. 401(k) plans are employment-based and are typically unavailable to the self-employed. For people aged 65 and older, retirement issues may complicate the analysis. SIPP questions about 401(k) plans are asked only of people aged 25

[7]Households are interviewed several times over a period of about two and a half years. Every "wave" collects core data on income, demographics, and other items. We use this information and data from periodic topical modules with information on 401(k) plans, assets and debt. The 1984 SIPP wave 4 was undertaken between September and December 1984. We refer to this as 1984 data. The 1985 SIPP wave 7 and the 1986 SIPP wave 4 surveys occurred between January and April 1987. The relevant variables in these two samples have very similar distributions, so we pool these data sets to form our 1987 data. Interviews for the 1990 SIPP wave 4 occurred between February and May 1991; we refer to this as 1991 data.

[8]The reference person is the person in whose name the family's home is owned or rented. If jointly owned or rented, either spouse may appear as the reference person.

and older. We also exclude households with inconsistent asset data.[9] These criteria leave samples of 9,310 households in 1984, 10,669 in 1987 and 10,266 in 1991.

The SIPP is the only nationally representative survey with data on 401(k) eligibility and wealth, but has several shortcomings. First, there is no information on 401(k) balances for 1984. Second, mortgage debt and house value are top coded. Based on sensitivity analysis, we believe that top coding does not have an important influence on our results.[10] Third, Curtin, Juster, and Morgan (1989) compare the SIPP wealth data to the Survey of Consumer Finances and the Panel Study of Income Dynamics. They conclude (p. 474) that the "striking feature of these comparisons is the substantial similarity in the amounts and distribution of wealth holdings across the three surveys--provided one ignores households with extremely high wealth (in excess of $0.5 million)." The SIPP lacks the oversampling of high-income households needed

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[9]The SIPP records holdings of particular assets for each person in the household, and also provides summary data at the household level for holdings of classes of assets. We exclude households for whom these two sources of data do not match.

[10]The top code for mortgages is $100,000 in 1984 and 1987, and $150,000 in 1991. For house value, the top code is $200,000 in 1984 and 1987, and $300,000 in 1991. Top coding affects only 3.3 percent or less of the sample in each year. To minimize the effect of top coding, we estimate median regressions as well as robust regressions. We also re-examine the central regression results using a sub-sample restricted to households that earn less than $75,000 (in 1991 dollars). In this sub-sample, which constitutes 94 percent or more of each year's sample, top coding affects 2.1 percent or less of the households in each year. Results from this sub-sample were very similar to those for the full sample.

to accurately measure behavior in that group. For analyzing 401(k)s, however, this limitation is not critical, since 401(k) eligibility is distributed widely across the population.

III. Background on Debt

Figure 1 shows that household debt rose relative to disposable personal income in the 1980s. Debt rose from 55 percent of income in 1970, to 62 percent in 1980 and 77 percent in 1990. Consumer debt was a small part of this trend, rising from 18 percent of income in 1980 to 20 percent in 1986. After the tax reform act of 1986 phased out deductions for consumer interest, consumer debt fell below 17 percent of income by 1992, and then rose to over 18 percent by 1994. The increase in aggregate debt was concentrated in mortgages, which rose from 37 percent of income in 1970 to 44 percent in 1980, 58 percent in 1990 and 62 percent in 1994.[11] Mortgage debt rose from 26 percent of the value of owner-occupied real estate in 1980 to 31 percent in 1986, and to 42 percent by 1994.[12] The steady rise in overall mortgage lending masks a combination of changes in home equity loans, mortgage

[11]A mortgage is defined by having a residence as collateral, rather than by the use of the funds.

[12]Board of Governors (1995, 1996). Other studies report much higher values for this ratio. The discrepancy arises because the Federal Reserve Board recently changed the way it calculates the value of owner-occupied land (owner occupied real estate is the sum of owner-occupied residential structures and owner-occupied land). The revised series for land is much larger than the original series.

refinancing, and downpayment levels.

Home equity loans include both second mortgages--closed-end loans that specify monthly payments of principal and interest--and home equity lines of credit--typically revolving accounts that allow households to borrow at their own discretion up to specified limits. Home equity loans rose from an estimated 4 percent of outstanding mortgages in 1981 to 11 percent in 1986 and 12 percent in 1991. Outstanding home equity debt rose from $40 billion in 1981, to $178 billion in 1986 and $357 billion in 1991.[13] In 1989, 15 percent of households held home equity loans, with mean and median balances of $22,534 and $15,905, respectively (Canner and Luckett 1990).

Mortgage refinancing grew rapidly in the late 1980s. Refinancing flows peaked at $200 billion in 1991, up from $115 billion in 1987. The proportion of refinances that extracted equity rose from 50 percent in 1986 to over 80 percent in 1988-90 (Dougherty 1994). In 1989, 20 percent of households with mortgages had refinanced, and 57 percent of those had extracted equity. The mean and median amounts extracted were $25,145 and $15,941, respectively (Canner and Luckett 1990).

Higher loan-to-value ratios (i.e., smaller downpayments) are another channel through which mortgages can rise relative to house value. Aggregate loan-to-value

[13]General Accounting Office (1993). Estimates in Manchester and Poterba (1989), Eugeni (1993), and Canner et al (1989) are consistent with these figures in the approximate magnitude and growth of home equity lending.

ratios on primary mortgages fluctuated between 74 and 77 percent between 1984 and 1991, and then increased to 80 percent in 1994 (Federal Housing Finance Board 1995).

Trends in debt have been the subject of several studies.[14] Manchester and Poterba (1989) estimate that each dollar of second mortgages outstanding is associated with a 75 cent reduction in household net worth, but caution that the results can not be given a causal interpretation. Skinner and Feenberg (1990), Scholz (1992b), and Maki (1994) use a variety of microdata sources and tests to show that the level of household debt rose over the course of the 1980s and that, especially among high-income homeowners, the composition shifted toward mortgage debt after 1986.

Data from the SIPP are consistent with these findings. Real mean and median mortgage debt rose in every income and age group in our sample from 1984 to 1991. Median mortgage debt rose by 14 percent from 1984 to 1987 and by 37

[14]Several surveys ask respondents why they obtained home equity loans. See Manchester and Poterba (1989), GAO (1993), Canner et al (1988), Canner and Luckett (1990), and DeMong and Lindgren (1995). Debt consolidation, home improvements, and investments are the most common responses. Great caution must be used in interpreting these answers, both because they are qualitative responses and because they provide no information on whether the indicated activity would have been undertaken in the absence of the home equity loan.

percent from 1987 to 1991, even though house value changed only slightly.[15] Consumer debt fell from 1987 to 1991 for homeowners with family earnings above $50,000, but rose for renters in the same earnings class.

IV. Background on 401(k) Plans

Deferred compensation, or 401(k), plans are employment-based saving incentives featuring tax-deductible contributions, tax-free accrual of earnings, and annual contribution limits. Withdrawals are taxed as ordinary income and may also be subject to penalties, depending on the age of the account holder and the purpose of the withdrawal. 401(k)s are available only to employees of organizations that sponsor the plans.[16] Relative to more traditional pensions, a novel feature of 401(k)s is that employees may make voluntary pre-tax contributions. Employers may also make tax-deductible contributions on an unconditional or matching basis.[17]

401(k) plans were authorized in 1978, but only began to grow rapidly after clarifying regulations were issued in 1981. The number of active 401(k) participants

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[15]Median house value (1991 dollars) in the SIPP was about $77,000 in 1984, $78,000 in 1987, and $75,000 in 1991. These patterns are consistent with house price data reported by Poterba (1991).

[16]Plans authorized by section 401(k) of the Internal Revenue Code are only available to employees of for-profit firms that offer the plans. Employees of non-profit institutions, and federal, state, and local governments are eligible for similar saving plans. In the empirical work below, we refer to all of these as 401(k) plans.

[17]For further details, see Andrews (1992), Engen, Gale, and Scholz (1994), Papke (1995), and Poterba, Venti, and Wise (1995).

rose from 7.5 million in 1984 to almost 20 million in 1991. Aggregate contributions were $16 billion in 1984. By 1991, contributions had risen to $51.5 billion and accounted for 26 percent of personal saving and 46 percent of all pension contributions (Department of Labor (1995)).

In the SIPP, we consider a family to be eligible for a 401(k) if either the reference person or the spouse is eligible. About 15 percent of families were eligible in 1984, rising to 22 percent in 1987 and 38 percent in 1991. Because our data do not provide information on 401(k) contributions, a family is defined as a participant if the reference person or spouse has a positive 401(k) balance. Participation rates rose from 8.5 percent in 1984 to 27 percent in 1991; participation rates conditional on eligibility rose from 58 percent to 71 percent over the same period. In 1991, the probability of being eligible for a 401(k) rose with family earnings, from 7 percent for families with earnings below $10,000 to 60 percent for families with earnings above $75,000. Conditional on eligibility, participation rates rose slightly with income. Neither eligibility nor participation showed clear patterns with respect to age.[18]

Previous research on how 401(k)s affect saving and wealth has used differing methodologies and reached a variety of conclusions.[19] A key issue is controlling for

[18]Similar data are presented in Engen, Gale, and Scholz (1994) and Poterba, Venti, and Wise (1995). Similar data patterns hold in 1984 and 1987.

[19]See Engen, Gale and Scholz (1996a,b) and Poterba, Venti, and Wise (1996a,b).

the effects of heterogeneity in tastes for saving across different groups and over time. One approach to this problem uses cross-sectional variation in eligibility for 401(k)s. If 401(k) eligibility were distributed independently of propensities to save, the effects of 401(k)s could be measured from simple comparisons of the saving or wealth of eligible and ineligible families. However, if eligibility is positively correlated with tastes for saving, cross-sectional comparisons that do not control for tastes for saving will systematically overstate the effects of 401(k)s on saving. Poterba, Venti, and Wise (1995) claim that 401(k) eligibility is "approximately" independent of tastes for saving, given income. However, Bernheim (1994, 1996b), Bernheim and Garrett (1995), and Engen, Gale, and Scholz (1994, 1996a, b) provide evidence that eligibility is positively correlated with tastes for saving, even after controlling for family characteristics. Appendix A provides more detail on this issue.

An alternative approach--successive cross-sectional analysis--uses random cross-sections of households from two or more years. Families eligible for 401(k)s in later years had access to 401(k)s for longer, on average, than eligible families in earlier years. Thus, other things equal, to the extent that 401(k)s raise wealth, the wealth of cross-sections of eligible families should increase over time and non-401(k) wealth should not decline. Using this approach, Poterba, Venti, and Wise (1995) show that, among cross-sections of eligible families, median holdings of non-IRA, non-401(k) financial assets did not fall from 1984 to 1991, as saving incentive balances rose. They conclude that 401(k) contributions were incremental saving.

Notably, this approach--comparing eligible families over time--is not subject to the criticism that eligible families have stronger tastes for saving than ineligible families. The validity of the approach does depend, however, on average tastes for saving remaining roughly constant among eligible families over time (after controlling for family characteristics). We discuss this issue in section VI.

Another problem is that the approach is only valid if there are no "time" effects--that is, if total financial asset holdings of cross-sections of eligible families would have remained constant over the years if saving incentives had not existed. It is implausible, however, to attribute all or even most of the growth in financial assets to saving incentives: between 1984 and 1991, aggregate real financial assets grew by $4 trillion, while saving incentive balances grew by less than $1 trillion (Board of Governors 1996, EBRI 1995).

In fact, several major changes, none controlled for in the Poterba, Venti and Wise analysis, suggest that financial assets would have increased during this period even in the absence of saving incentives (Engen, Gale and Scholz 1996a). First, in the seven years prior to being observed in the data, the 1991 cross-section of eligible families experienced higher returns on their pre-existing financial assets than the 1984 cross-section did. For example, the S&P 500 stock index rose by 78 percent in real terms from 1984 to 1991, but fell by 5 percent from 1977 to 1984. Real interest rates were also higher between 1984 and 1991 than in the preceding seven years. Second, the decline in inflation and marginal tax rates in the 1980s induced investors to shift

away from tangible capital (like housing) that had been more attractive in the 1970s (Feldstein 1980; Summers 1981; Poterba 1984). Hence, part of the increase in financial assets was just a shift in the composition of assets. Third, mortgages and overall household debt rose relative to income or assets. At least a quarter of the rise in financial assets from 1984 to 1991 was matched by an increase in debt (Federal Reserve Board, 1996). Fourth, declines in the value of Social Security benefits and in non-401(k) private pension coverage and benefits could have induced increases in financial asset holdings. These factors imply that increases in financial assets during the 1980s can not be attributed solely to saving incentives.

A somewhat cleaner test relies on the hypothesis that if 401(k)s increase wealth, wealth should rise more over time for cross-sections of eligible households than for ineligible households, holding other observable factors constant. For example, Poterba, Venti, and Wise (1995) find that from 1987 to 1991 median gross financial assets (not including debt) rose by about $2,900 (in 1987 dollars) for eligible households relative to others. The validity of this test across groups, however, also depends on two key assumptions. The first is that tastes for saving (controlling for observable factors) did not change for one group relative to the other. We examine this issue in section VI.

The second is that changes over time in financial asset holdings of eligible families relative to ineligible families were due only to lengthier access to saving incentive plans. However, some of the changes during the 1980s could plausibly have caused financial

assets to rise more for eligible families than for ineligible families even if 401(k)s did not raise saving. For example, the shift toward financial assets and away from nonfinancial assets (occasioned by the reduction in inflation and tax rates in the 1980s) may have been larger for eligible families because they had greater access to tax-preferred financial assets (e.g., 401(k) plans) and so would have found shifting into financial assets more attractive than ineligible families did. In addition, debt holdings may have risen more for eligible families than for ineligible families. Notably, both of these factors imply that financial assets may not be a consistent measure of wealth over time or across groups, and that using a measure of wealth that includes debt and housing equity could resolve these problems.

V. Econometric Specification and Results

A. Specification

We estimate the effects of 401(k) eligibility for the 1987-91 and the 1984-91 periods using two similar approaches. Both approaches compare increases in wealth over time for eligible families relative to ineligible families. The difference lies in how the parameters are estimated; the "between-group" tests use data from both groups at the same time, whereas the "within-group" tests use data from one group at a time and then compare the results.

Our between-group specification can be formulated as:

(1) [illegible]

where, ß, [illegible] and [illegible] parameters to be estimated, and [illegible] is a residual.

The dependent variable W is a measure of the reference person and spouse's wealth. We estimate regressions for financial assets, net financial assets, and "wealth," defined as the sum of net financial assets and housing equity.[20] Using a broad measure of wealth helps control for shifts between nonfinancial assets and financial assets and any debt buildup. To provide an alternative test, we estimate the impact of eligibility on the same three measures of wealth, excluding 401(k) balances. To assist in interpreting the results, we estimate equations for mortgage debt, house value, housing equity, and wealth minus house value.

The vector X includes determinants of wealth accumulation. We include family size, categorical variables for age, family earnings, and average educational attainment of the reference person and spouse, and dummy variables for defined benefit pension coverage, marital status, the presence of two earners, and the race and sex of the reference person.[21] These choices are motivated by standard life cycle considerations

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[20]Financial assets include checking accounts, U.S. saving bonds, other interest-earning accounts in banks and other financial institutions, other interest-earning assets (such as bonds held personally), stocks and mutual funds, and IRA, Keogh, and 401(k) balances. Net financial assets are financial assets less unsecured debt. Housing equity is the difference between house value and outstanding mortgage debt. Mortgage debt includes first, second and third mortgages against the principal residence, including home equity loans.

[21]The categories for age of the reference person (in years) are 35-44, 45-54, and 55-64. The categories for family earnings (in thousands of 1991 dollars) are 10-20, 20-30, 30-40, 40-50, 50-75, and >75. The categories for education of the reference person (in years) are 12, 13-15, 16, and 17 or more. The omitted categories are 25-34 for age, 0-10 for

and the desire to control for measurable sources of heterogeneity in saving.

The variable IN91 is an indicator that the observation is in the 1991 sample and captures general differences in wealth between 1991 and the earlier year, either 1984 or 1987, depending on the sample. The variable ELIG is an indicator that at least one family member is eligible for a 401(k); it captures average differences in wealth between 401(k)-eligible families and ineligible families not captured by other variables. The coefficient on the key independent variable, IN91*ELIG, measures the amount by which wealth increased for eligible families relative to ineligible families from the earlier year to 1991, after controlling for family characteristics, general changes between the earlier year and 1991, and general differences between the two groups. To the extent that 401(k) eligibility raises wealth, [redacted] should be positive, and economically and statistically significant for the wealth measures that include 401(k) balances, and should be zero for wealth measures that exclude 401(k) balances.

In addition to full-sample estimates, we divide the sample based on households' IRA status, which helps to control for unobservable tastes for saving, and allows us to use the same comparison groups as Poterba, Venti and Wise (1995). We also use separate samples of homeowners and renters, because differential access to tax-deductible debt across groups could lead to different responses to saving incentives. For each specification, we estimate least absolute deviations regressions (with standard errors estimated via bootstrap techniques with 25 iterations) and an

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earnings, and less than 12 for education.

alternative robust regression method.[22]

Our within-group specification for families in group j is of the form

(2) $W = X\beta_j + \gamma_j IN91 + \varepsilon_j$

where W and X are defined as before, and where j indexes eligible (E) and ineligible (I) families. Equations are estimated separately for each group. The coefficient on IN91 measures the increase in wealth over the sample period. Due to the presence of special factors that raised financial assets in the 1980's, it is insufficient to examine only the results for eligibles (γ_E). The appropriate comparisons of the effects for eligible families relative to ineligible families, given by $\gamma_E - \gamma_I$. If eligibles gained wealth, this difference should be positive, large, and statistically significant for wealth measures

[22]The robust regression method is described in the Stata (1993, volume 2, pp. 126-131) manual. The first step is an ordinary least squares regression; outliers (any observation for which Cook's D>1) are then excluded. The procedure then works iteratively: a regression is run, weights are calculated based on the absolute residuals, and then those weights are used in estimating the next regression. This process continues until convergence is obtained in the successive estimates. The first set of weights used are Huber weights. Based on those results, biweights are then used until convergence is obtained. Both weights are used to offset potential problems with using either one or the other. These adjustments are relevant because ordinary least squares regressions for saving are very difficult to interpret precisely because of the presence of extreme outliers that distort the results. (For example, see the discussions and results in Bernheim and Scholz 1993, Gale 1995, Gale and Scholz 1994, or Samwick 1995).

that include 401(k)s and zero for measures that do not.[23]

The between-group approach generates standard difference-in-difference results (the difference in wealth between eligible and ineligible families between 1987 and 1991) in one estimate. A similar approach was taken by Gruber and Poterba (1994). This approach does not allow the coefficients on observed characteristics to vary across eligible and ineligible families. Within-group estimates allow the ß's and error variances to differ across groups. A difference (change in wealth over time) is calculated within each eligibility group using data only for that group, and then the difference-in-differences is obtained by comparing the two separate estimates.[24] In practice, the patterns of results obtained from the two approaches are similar.

B. Characteristics of Eligible and Ineligible Families

Table 1 provides summary data on 401(k)-eligible and ineligible families in the SIPP. The groups are similar with respect to age, but eligible families have higher earnings, financial assets, net financial assets, housing equity, and homeownership rates. Consistent with aggregate data, financial assets and net financial assets represent

[redacted]

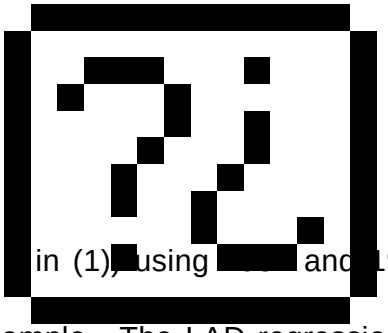
[23]Poterba, Venti and Wise (1995) present within-group estimates and refer to them as estimates for "like" families.

[24]A third option is to estimate equations of the form: W = Xß$_I$*(1-ELIG) + Xß$_E$*ELIG + [redacted] This approach allows for different ß coefficients for the [redacted] different error variances. In practice, estimates using this approach were very similar to the between-group estimates, but several specifications would not converge, presumably because of the large number of parameters that need to be estimated.

only a small portion of overall wealth for eligible and ineligible families, and rise as a proportion of wealth over time. The lower panel shows that in each year eligible families owned more valuable homes, had higher mortgage debt relative to house value, and were more likely to have home equity loans. The increase over time in mortgage debt for eligibles relative to others was large relative to the increase in 401(k) balances in the top panel.

C. Results

Between-Group Estimates, 1987-91

Tables 2-4 present between-group estimates (of  in (1)) using [redacted] and 1991 data. The first two columns of Table 2 use the entire sample. The LAD regressions imply that median financial assets of eligible families rose by $679 from 1987 to 1991 relative to ineligible families and controlling for other factors. The impact of eligibility on net financial assets is half as large, and the impact on wealth is negative. The robust estimates show similar patterns. None of the coefficients, however, is estimated precisely. Rows (4)-(6) show that asset measures that exclude 401(k)s fell by statistically significant amounts. Non-401(k) wealth (row 6) is estimated to fall by about $4,000 from 1987 to 1991 for eligibles compared to ineligible families. This is a large decline relative to the $1,300 ($3,200) rise in median (mean) 401(k) balances for eligibles.

The third and fourth columns provide estimates for the sample of all families with

IRAs.[25] The LAD regressions imply that eligible families raised their financial assets by $4,348 (t=1.84) relative to ineligible families, controlling for other factors. Net financial assets also rose, but eligibility had a negative and statistically insignificant effect on wealth. Rows (4)-(6) show that the change in non-401(k) financial assets and non-401(k) net financial assets was insignificantly different from zero for eligibles relative to ineligibles, but non-401(k) wealth declines by a statistically significant $10,000 for eligible families relative to others. Robust estimates in the third column show similar effects; in particular, they show no increase in wealth and a statistically significant decline of $7,700 in non-401(k) wealth for eligible families relative to others. These declines in non-401(k) wealth are large relative to the $3,000 ($5,200) increase in median (mean) 401(k) balances for eligibles in this group.

The fifth and sixth columns show similar qualitative patterns for families without IRAs, but the results are not estimated as precisely. The impact of 401(k) eligibility on wealth is small and insignificant. Non-401(k) wealth fell by about $1,400 for eligibles relative to others. The decline is sizable relative to the $800 ($3,500) rise in median (mean) 401(k) balances for this group between 1987 and 1991, but is not statistically significant.

Rows (7)-(11) provide a variety of additional results for each group. Row (7) shows that mortgage debt rose by economically and statistically significant amounts

[25]Appendix Table 1 provides the full regression coefficients for the least absolute deviations regressions in column 4 for financial assets and wealth.

for eligible families in each comparison group compared to ineligible families. As shown in subsequent tables, this is the most robust finding in the paper, and will prove important in interpreting the results. Row (8) shows that housing value rose for eligibles relative to others in each comparison, but the finding is of varying statistical significance. Row (9) shows that the effects of eligibility on housing equity--the difference between house value and mortgage debt--are uniformly negative and of varying statistical significance. Similar findings for house value and house equity occur in the subsequent tables.

Rows (10) and (11) provide additional results for wealth minus housing value and non-401(k) wealth minus housing value. These estimates are included because families may regard changes in house value as less certain and hence may be less likely to respond to them by changing other assets. The estimates indicate that wealth measures exclusive of housing value generally fell for eligible families relative to others. For example, as noted above, among families without IRAs, non-401(k) wealth fell by sizable but not statistically significant amounts for eligible families relative to ineligible families. Row (11) shows that the statistical insignificance arises because of changes in house value: that is, non-401(k) wealth other than house value fell by a statistically significant $3,000 for eligible families relative to others among families without IRAs.

The next two tables measure the extent to which the response to 401(k)s depends on homeownership status. Table 3 shows that roughly the same patterns seen in the full sample hold for homeowners. For all homeowners (columns 1 and 2),

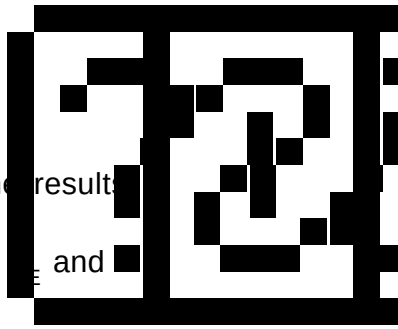
eligibility is associated with increased financial assets and net financial assets, a negative and insignificant impact on wealth, and a statistically significant $4,600 decline in non-401(k) wealth. By comparison, median (mean) 401(k) balances for this group rose by $2,300 ($3,800) from 1987 to 1991.

For homeowners with IRAs, eligibility had a negative and insignificant impact on wealth. The effect on non-401(k) wealth in row (6) indicates a $6,600-$8,500 decline in wealth; this decline is large relative to the $3,500 ($6,800) rise in median (mean) 401(k) balances for this group, but is not estimated precisely.

For homeowners without IRAs, eligibility raised financial assets and had a positive, but insignificant effect on wealth. Row (8), however, shows that eligible families in this group had a large increase in house value and row (10) shows that other than this increase in house value, wealth fell by substantial and significant amounts for eligibles compared to ineligible families. Likewise, row (6) shows that this group had an insignificant decline in non-401(k) wealth, but row (11) shows that the result is due to house value; excluding house value, non-401(k) wealth fell by $6,300 (t=4.2) for eligible families relative to others.

Table 4 shows that, for renters, the effect of eligibility on net financial assets is never positive and significant. The effects on non-401(k) net financial assets are uniformly negative, and significant results obtain for the overall sample of renters and

for renters without IRAs.[26]



Within-Group Estimates, 1987-91

Tables 5-7 present within-group estimates for 1987-91. To make the results easier to interpret, the tables report the estimated difference between E_{87} and E_{91} (from (2)) and the t-statistic for the estimated difference.[27] The full set of underlying estimates of E_{87} and E_{91} is in Appendix Tables A-1-4.

Tables 5-7 generally provide results similar to the between-group estimates. The full sample results in the first two columns of table 5 show that for eligible families relative to ineligible families: financial assets and net financial assets rose; wealth did not rise; non-401(k) financial assets and net financial assets appear to have fallen; and non-401(k) wealth fell by $3,200. For families with IRAs (columns 3 and 4), eligibility had little effect on wealth. Estimates that are significant at the 8 percent and 1 percent level indicate that non-401(k) wealth fell by $7,000-$9,000 for eligibles relative to ineligible families. For families without IRAs, eligibility had no apparent effect on wealth. Non-401(k) wealth fell by $1,500-$1,900, with estimates significant at the 9 percent level, for eligible relative to ineligible families.

[26] To account for topcoding, we re-estimated all of the regressions in tables 2-7 excluding families with earnings above $75,000. Results were very similar to those reported above.

[27] Let σ_{87} and σ_{91} be the standard errors of the estimates of E_{87} and E_{91}, respectively. Then the standard error of the difference, $E_{87} - E_{91}$, is given by $(\sigma_{87}^2 + \sigma_{91}^2)^{1/2}$.

The results for homeowners in table 6 represent the largest differences between the between-group estimates and the within-group estimates. For all comparison groups, eligibility raised financial assets and net financial assets, as in table 3. But the wealth regressions present inconclusive results: eligibility is associated with a positive but insignificant effect on wealth in row (3) and a negative and insignificant effect on non-401(k) wealth in row (6). As in table 3, however, mortgage debt rose by economically and statistically significant amounts for each group of eligible families relative to the appropriate comparison group.

The within-group estimates for renters in table 7 are qualitatively quite similar to the between-group estimates in table 4.

Between-Group Estimates, 1984-91

Tables 8-10 present between-group estimates using data from 1984 and 1991. Due to the absence of 401(k) balances in 1984, results are presented only for dependent variables that exclude 401(k)s. Row (6) of Table 8 shows statistically significant declines in non-401(k) wealth of over $3,100-$3,600 for all eligible families, $8,000-$11,500 for eligibles with IRAs, and $3,700-$6,800 for eligibles without IRAs, relative to the appropriate comparison group of ineligible families. These results cannot be compared to changes in 401(k) balances between 1984 and 1991, but an upper bound for the change is the 1991 level. Median 401(k) balances in 1991 were $2,300 for all eligibles, $6,000 for eligible families with IRAs, and $1,200 for eligibles without IRAs. Thus, table 8 suggests substantial offset of 401(k)s by reductions in

non-401(k) wealth over the 1984-91 period.[28]

Table 9 shows that the same results generally hold for homeowners. In the sample of all homeowners and those with IRAs, large and significant declines in wealth are observed for eligible families relative to ineligible families. For homeowners without IRAs, non-401(k) wealth falls by more than $2,000 for eligible families relative to others, but the decline is not estimated precisely. As before, for homeowners without IRAs, the results appear attributable to changes in house value: row (11) shows that non-401(k) wealth other than housing value fell by substantial and significant amounts for eligible families relative to others.

Table 10 shows that the results for renters for the 1984-91 sample are very different than in the 1987-91. Non-401(k) financial assets and net financial assets rose for eligible renters with IRAs, whereas they fell during the 1987-91 period.

Within-Group Estimates, 1984-91

Tables 11-13 present within-group estimates for 1984-91. The full set of underlying estimates is in Appendix Tables 5-7. Row (6) of table 11 shows large declines in non-401(k) wealth for eligible families, eligibles with IRAs, and eligibles without IRAs relative to the appropriate comparison group. In each comparison group, the estimate from one specification is statistically significant at the 6 percent

[28]Mean 401(k) balances in 1991 were $10,392 for all eligibles, $16,666 for eligible families with IRAs, and $7,419 for eligibles without IRAs. The means are highly influenced by outliers, however, whose effects are minimized in both the least absolute deviations and robust regressions (see footnote 22).

level or higher. Table 12 shows that for homeowners with IRAs, eligibility is associated with a statistically significant $10,000-$17,000 reduction in non-401(k) wealth. For homeowners without IRAs, eligibility is associated with a reduction in non-401(k) wealth, but the decline is estimated imprecisely. As in earlier tables, however, the results for this group appear to be driven by house value; row (11) shows that omitting house value generates a $4,300 decline in non-401(k) wealth for eligibles relative to ineligibles. Table 13 reflects the instability of the estimates for renters over different time periods noted above.

Summary

The tables above present a large number of results, but can be summarized fairly readily. For the overall sample, the results imply that eligibility has not raised broad measures of wealth and has reduced non-401(k) wealth by large and generally significant amounts. Similar results apply for separate samples of families with IRAs and families without IRAs.

For the overall sample of homeowners, the results generally imply little or no effect of eligibility on wealth, and large and generally significant declines in non-401(k) wealth. For homeowners with IRAs, the same results apply. For homeowners without IRAs, the effects of eligibility on wealth are positive, but relatively small and not significantly different from zero, suggesting that 401(k)s do not raise wealth. The effects on non-401(k) wealth are negative, also consistent with the view that 401(k)s have not raised saving, but are not significantly different from zero; however, the statistical

insignificance seems to have arisen from changes in house value. Non-401(k) wealth exclusive of house value fell by large and significant amounts. On the whole, the results for homeowners provide little evidence that 401(k)s raise wealth and substantial evidence that they do not. This is a crucial finding because in both 1987 and 1991, homeowners held 88 percent of 401(k) balances.

For renters, the results are mixed. The 1987-91 results suggest little impact of eligibility on wealth, whereas the 1984-91 results suggest, for renters with IRAs, eligibility raised wealth. Renters with IRAs, however, account for less than 4 percent of 401(k) balances, so the impact of 401(k)s on this group is not a major factor in determining the overall impact of 401(k)s. In addition, to the extent the results differ between the 1984-91 and 1987-91 samples, there may be good reason to prefer the 1987-91 estimates. The 1984 data omit 401(k) balances, which reduces the number of dependent variables that can be examined. In addition, the SIPP in all years omits data from after-tax employer-related thrift saving plans. These were popular before 401(k)s came into use, but by 1987 almost all had been converted to 401(k)s--and therefore included in the SIPP in 1987 and 1991--so the omission would be less important in 1987 than in 1984.

Taking a step back from the specifics, the overall pattern of results is plausible and consistent with economic theory. Families that are older, have higher wealth and/or higher income, or own a home will have more incentive to shift funds into 401(k)s to obtain the tax breaks and more ability to do so. In addition, the potential

illiquidity may be less costly for them since they are closer to the age when penalty-free withdrawals can be made. Thus, we expect to find more substitution of 401(k)s for other wealth among groups with those characteristics.[29] Table 14 generally bears out this point. Homeowners with IRAs are older and have higher income and wealth than other groups. Homeowners without IRAs are typically older than renters, have more wealth than the typical renter and of course have access to home equity loans. Renters in general are younger and have lower income and lower assets than homeowners, although renters with IRAs do exhibit strong tastes for saving.

VI. Discussion

In this section, we examine potential criticisms, biases and interpretations of our results. Additional related issues are explored in Appendix B.

Housing values

Poterba, Venti and Wise (1996b) claim that exogenous changes in housing value bias our results toward finding no effect of 401(k)s on wealth. They claim that eligible families started with higher housing values and all housing values fell over the sample period by about the same percentage, resulting in larger arithmetic declines for eligible families. Since changes in house value may be largely unrelated to financial saving (Hoynes and McFadden 1994, Engelhardt 1996, Skinner 1993), our results may be

[29]Bernheim and Scholz (1993) and Gale (1997) find that the degree to which pensions are offset by reductions in other wealth varies across households in similar fashion. Gale and Scholz (1994) find similar results for substitution between IRAs and other wealth.

biased toward understating the impact of 401(k)s.

We note several items in response. First, although we concur that changes in house value may not be effectively or immediately absorbed into families' saving behavior, our main results are driven primarily by increases in mortgage debt. The regressions in row (7) above present very robust evidence that the impact of eligibility on mortgage debt is large, positive, and significant. Second, the regression results in row (8) above demonstrate that, controlling for other factors, house value generally rose for eligibles compared to ineligibles. This finding is not always significant, but there is very little evidence to support the opposite view, at least after controlling for observable characteristics as done in the regressions above. Third, to address this issue further, we estimated the effects of 401(k)s on wealth exclusive of house value, as reported in tables 2-13. This measure is not contaminated by trends in house value, and the regressions using this dependent variable provide strong evidence that 401(k) eligibility has not served to increased wealth and no evidence to suggest the opposite. Thus, exogenous changes in housing values do not appear to be driving the results in general, but to the extent that they are, inclusion of housing values tends to overstate rather than understate the effects of 401(k)s.

A related concern is that housing values may be measured poorly in the SIPP. This would have generated our results (for homeowners), however, only if the bias in measurement of housing equity became more negative from 1984 or 1987 to 1991 for eligible homeowners relative to ineligible homeowners, which seems unlikely. In any

case, mismeasurement of housing would not explain the results above for wealth measures exclusive of house value.[30]

Dilution or Concentration?

Bernheim (1996b) claims that studies that examine successive cross-sections of eligible families understate the impact of 401(k)s due to "dilution." Within a group, dilution occurs if the average "taste for saving" among families in that group falls over time. Across groups, dilution occurs if tastes for saving decline more (or rise less) for eligible families than for ineligible families over time. "Concentration" is the opposite of dilution, and would occur among eligibles if average tastes for saving rose among eligible families, and would occur across groups if tastes for saving rose for eligible families relative to ineligible families.

Dilution of eligible families relative to ineligible families would cause our estimates to understate the effects of 401(k)s on saving. Likewise, concentration of eligibles relative to ineligibles would cause the estimates to overstate the effects. Dilution or concentration within the group of eligibles, or within the group of ineligible families, is neither necessary nor sufficient to bias our results in any direction, because our results depend on the differential increase in wealth for the two groups over time. Nonetheless, it is useful to examine dilution within and across groups.

[30]We also estimated specifications with regional or state dummies added, to help control for spatial variation in housing prices, with results very similar to those in the text.

At a theoretical level, dilution among eligible families could occur if the most dedicated savers were most likely to become eligible for 401(k)s early on. As less dedicated savers became eligible in later years, average tastes for saving would fall among cross-sections of eligible households. On the other hand, Ippolito (1993) provides a model where the eligible sample could become more concentrated over time. He argues that, because of employers' matching contributions, 401(k)s are more attractive to workers with high tastes for saving. Thus, high savers are attracted to, and remain at, firms that sponsor 401(k)s while low savers are more likely to leave such firms and cash in their 401(k)s. As a consequence, the average tastes for saving plausibly rise over time among eligible workers. Dilution of eligible families relative to ineligible families depends on both subsamples. Since the most dedicated savers among ineligible households are the most likely to become eligible, it seems likely that there is dilution among ineligible households over time. The net dilution across groups is impossible to determine on a theoretical basis.

Ultimately, the extent of dilution or concentration is an empirical issue. Among eligible families, there is little evidence that dilution occured. Participation in a saving incentive plan is widely accepted as an indicator of tastes for saving. Thus, if dilution of the eligible sample were empirically important, then--other things equal--it would be reasonable to expect that the proportion of 401(k)-eligible workers that made 401(k) contributions would have fallen over time. Instead, data from the Current Population Survey show that the participation rate among eligible workers rose from

57 percent in 1988 to 65 percent in 1993 (Bassett, Fleming, and Rodrigues, 1996).[31] Our own analysis using the SIPP indicates that between 1987 and 1991, controlling for household characteristics (including pension coverage), the 401(k) participation rate of eligible households rose by 8 percentage points and was statistically significant, and the likelihood of participating in any saving incentive plan (IRA or 401(k)) rose by 4.4 percentage points and was significant. Similar findings hold for the 1984-91 period. These results are consistent with the view that tastes for saving could have increased, rather than decreased, among cross-sections of eligible households over time.[32]

Across groups, there is little evidence of dilution of eligible families relative to other families and some evidence consistent with concentration. Overall saving incentive participation rose by 15 percentage points from 1987 to 1991 for eligible families relative to others, controlling for other characteristics. Similar results hold for the

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[31] This increase is unlikely to be due to an increase in employer matching. In 1993, 60 percent of eligible workers that did not receive a match contributed. This is larger than the overall 1988 average probability of contributing (Bassett, Fleming, and Rodrigues, 1996).

[32] Other evidence provides further support for the notion that concentration rather than dilution may have occured. Evan and MacPherson (1996), using the CPS, present evidence that the large positive correlation between 401(k) participation and employees' tenure at a firm occurs because employees that tend to stay at their jobs also tend to be high savers. In addition, workers frequently liquidate their 401(k) upon leaving a firm (Chang, 1996; Fernandez, 1992). If, as seems reasonable, these workers have lower tastes for saving, their departure should raise average tastes for saving among eligible families over time.

1984-91 period. This is consistent with the view that the sample of eligible families became more concentrated relative to ineligible families over time. Thus, the empirical findings and other factors discussed above are consistent with the view that the bias created could more plausibly work in the direction of concentration rather than dilution.

Econometric biases

Although our results do not appear to suffer from problems with house value or dilution, the findings (and those of Poterba, Venti, and Wise 1995) overstate the impact of 401(k) eligibility on wealth for at least two reasons. First, balances in 401(k)s are pre-tax amounts. Balances in other financial assets typically represent post-tax amounts. Although adjusting for this factor on a family-specific basis is difficult, Engen, Gale, and Scholz (1996a,b) conclude that at least 20 percent of 401(k) (and IRA) balances should be removed from reported balances in estimating household wealth. The regressions above do not make any such adjustment and so overstate the impact of 401(k) eligibility on financial assets and wealth.

The second problem is that the expansion of 401(k)s has for many firms involved reductions in the generosity of existing pension plans or elimination of those plans. Our regressions control for pension coverage and hence provide a control for plan elimination (see also Papke 1996). The control may be weak, however, since the regressions use a family-level pension coverage variable. The regressions can not control, however, for reductions in the generosity of existing pensions. There is

anecdotal evidence that such changes have been quite important, but hard evidence is difficult to acquire.[33] Nevertheless, the direction of the bias created should be clear. To the extent that initiation or expansion of 401(k) plans is associated with reductions in the value of previously existing pensions, the observed increase in financial assets for eligible families could be due to the pension reduction rather than the 401(k) expansion.

Related Evidence from the literature

Our findings on the effects of 401(k) eligibility on financial assets are consistent with Poterba, Venti, and Wise (1995), but we also find that the impact disappears when the analysis is extended to examine the effects of eligibility on debt and broader measures of wealth. This finding is consistent with several previous analyses of saving incentives and saving behavior that show that the impact of the incentives on wealth dissipates or disappears when the analysis focuses on broader wealth measures that include debt.

Avery, Elliehausen, and Gustafson (1986) examine the effect of pension wealth on various measures of non-pension wealth using data from the 1983 Survey of Consumer Finances. For married couples, they find that a dollar of pension wealth reduces financial assets by 11 cents, but reduces non-pension net worth by 66 cents.

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[33]The following Congressional testimony, by an executive of a major corporation, is not atypical: "A recent major change occured in 1995. We generally reduced the value of our defined benefit plan...Correspondingly, we increased the match in our 401(k) plan." (Sauvigne 1997).

Gale (1997) finds pension offsets of 0-10 percent using financial assets as the dependent variable, but 40-82 percent when using non-pension net worth.

Feenberg and Skinner (1989) examine IRA contributions, interest deductions and other items from a panel of tax returns covering 1979-84. They examine households who itemized their deductions (and hence provided data on interest payments), and stratify households by their initial (1980-81) holdings of financial assets. Among households with less than $2,000 in financial assets in 1980-81, they note that households that contributed to IRAs in all three years from 1982 to 1984 accumulated enough extra non-mortgage debt relative to non-contributors to account for almost two years of IRA purchases. A similar finding holds for households with initial assets between $2,000 and $10,000. Among households with more than $50,000 in initial financial assets, three-year contributors accumulated enough extra non-mortgage debt relative to non-contributors to account for all three years of IRA purchases and more. These results suggest important interactions between IRAs and consumer debt, the interest on which was tax-deductible during the period covered by the Feenberg and Skinner data. As noted below, we would expect even more debt-financing of 401(k)s because of employer-matching provisions.

Engen, Gale, and Scholz (1994), using a similar tax panel sample as Feenberg and Skinner, show that average non-mortgage debt rose by $2,400 for IRA contributors over this period, but by only $1,350 for others. Median non-mortgage debt rose by $2,350 for contributors but only $100 for non-contributors.

Venti and Wise (1992) use panel data on financial assets from the 1983-86 Surveys of Consumer Finances and claim, first, that IRA contributors did not save very much before IRAs became available, and, second, that IRA contributors saved substantial amounts after IRAs became available. They conclude (p. 24) that "the 1986 contributors, prior to 1983, had not been accustomed to saving nearly as much as they saved over the next three years" and that IRAs represent new saving. But financial assets were only one-fifth of wealth for this group in 1983. Engen, Gale and Scholz (1996a, Table 2) show that examination of trends in wealth (net financial assets plus housing equity) leads to dramatically different conclusions. Specifically, the households in the sample had sizable wealth in 1983 and the increase in wealth from 1983 to 1986 was not exceptional. Thus, examining broader measures of wealth suggests that little if any of the IRA contributions were new saving.

Thomas and Towe (1996) use panel data from the 1983-89 Survey of Consumer Finances and conclude that the increase in IRA and 401(k) contributions over this period was financed by a reduction in net housing equity.

Bernheim (1996a) finds that retirement saving by households in the baby boom generation rose significantly from 1994 to 1995 relative to the amount of saving needed to assure adequate living standards in retirement. However, he also finds that overall net financial assets, including non-mortgage debt, rose only slightly, and concludes (page 3) that "the increase in retirement saving appears to reflect a change in the form of saving rather than in the level of saving."

Canadians may make contributions to Registered Retirement Saving Plans (RRSPs), which are similar to U.S. saving incentives. Using aggregate data, Carroll and Summers (1987) show that Canadian saving rates rose relative to American saving in the early to mid-1970s when RRSP contribution limits were raised and RRSP contributions increased. However, Altig (1990) shows that the impact of RRSP contributions on aggregate saving vanishes when the regression includes a borrowing variable that accounts for the fact that mortgage interest is not tax-deductible in Canada.

Interpreting the Evidence: Intentional or Inadvertent Substitution?

Housing wealth and 401(k) plans each represent illiquid, tax-preferred assets that are often held for long periods. Substitution between such assets should not be surprising, especially given the large financial incentives to do so. Employer matching of 401(k) contributions implies that financing a 401(k) with tax-deductible mortgage borrowing can be quite lucrative.[34] Borrowing to finance 401(k) contributions is profitable if the after-tax return on the contribution exceeds the after-tax cost of borrowing. If r_s and r_b are the returns on contributions and the cost of borrowing, m is the employer match rate, p is the penalty for early withdrawal, t is the income tax rate, and H is the holding period, debt-financed contributions are profitable when

[34]Simple calculations show that with typical employer matching contributions, workers should do everything possible to maximize 401(k) contributions at least up to the match limit. Kusko, Poterba, and Wilcox (1994) report that about 75 percent of 401(k) participants in their sample at one firm contributed at or above the match limit.

$$(1-t-p)(1+m)(1+r_s)^H > [1+r_b(1-t)]^H.$$

If t is 0.4, r_s = 0.10, p is 0.1, H is 15 years, and m is 0.5, debt financing will be profitable with r_b as high as 13.1 percent. If m is 0, as in an IRA, borrowing is only profitable if r_b is below 8.3 percent. This suggests that debt-financed contributions may be more likely for 401(k)s than for IRAs.

Thus, our results could have been generated by households that are consciously gaming the tax system. This interpretation would be consistent with a sizable body of evidence indicating that households allocate their assets and debt with tax considerations in mind and with research that indicates that homeowners responded differently than renters to tax law changes in the 1980s.[35] It seems unlikely, however, that all households are gaming the system so aggressively, so we note several points. First, our results do not require that all households with 401(k)s substitute between saving incentives and housing equity. The results refer to changes in mean and median wealth, conditional on explanatory variables, so 401(k)s could actually raise wealth for some people and the results could still hold. Second, households need not be sophisticated, rational, or aggressive in tax planning to have generated our results. Different households may substitute between 401(k)s and housing equity in different ways, and the substitution need not even be intentional.

Some obvious ways to substitute include financing a 401(k) with a refinanced

[35]Feldstein (1976), Feenberg and Poterba (1991), Maki (1994), Scholz (1992a,b) and Skinner and Feenberg (1990).

mortgage, a second mortgage, a home equity line of credit, or with a smaller downpayment on a house. About 14 percent of eligible homeowners had home equity loans in 1991; about 19 percent bought new homes between 1987 and 1991; and a reasonable estimate is that 12 percent extracted equity from their home via a refinancing between 1987 and 1991 (Bernheim 1996b). Thus, a substantial minority of eligible families had direct access to one of these mechanisms. Less obvious, and perhaps less intentional, ways to substitute include not accelerating mortgage payments that otherwise would have been accelerated or not trading up into a bigger house.

Finally, consider different cohorts of new homeowners who are observationally equivalent except that the new homeowners in the later year have had longer exposure to 401(k)s and so have placed more funds in 401(k)s than did those in the younger cohort. Now suppose that households in the later cohort have smaller balances of liquid cash (because they have moved some of their liquid cash into 401(k)s) than those in the earlier cohort. Because they have less cash available, households in the later cohort might purchase the same size home as the earlier cohort, but with a larger mortgage (less initial housing equity). A comparison of households in these two cohorts would reveal that households in the later cohort had less housing equity, more 401(k) wealth, but the same overall wealth compared to households in the younger cohort. As an analytical statement, it is clear that households in the later cohort were in fact substituting 401(k)s for home equity

relative to earlier cohorts, even if this substitution were completely unintentional and even if the household itself were unaware of the comparison. Similar types of "inadvertent" substitution could be quite widespread.[36] Our results do not provide information on the relative importance of each of the channels noted above, nor on the extent to which substitution is intentional or inadvertent. It seems likely that these factors would vary across households.

VII. Conclusion

Despite general agreement in the literature that the allocation of households' debt and the allocation of assets are sensitive to tax considerations, there has been little attention given to the links between tax-preferred assets and tax-preferred debt. The link is crucial, however, because the presence of both presents opportunities for tax arbitrage that subvert the intent of the saving incentives. Links between tax-preferred assets and tax-preferred debt may become even more direct over time, with proposals in the 1990s to allow credit cards that would provide "401(k)-equity" loans, in a manner similar to the home equity lines of credit that became popular in the 1980s (Kiplinger's 1995, Fund Action 1995).

In this paper, we explore these links using the specific example of how 401(k)

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[36]As Stiglitz (1988, p. 595) notes: "The individual may, of course, not consciously perceive himself as borrowing for these purposes; he may say to himself in April that it would be a good idea to put money into an IRA; and then in June, he may decide that he would like to buy a new car; given his available cash, he finds that he needs to borrow more than he otherwise would have."

eligibility influences the accumulation of wealth, where wealth is defined to include financial assets, consumer debt and housing equity. We show that 401(k) accumulations are largely offset by reductions in non-401(k) wealth, and in particular in housing equity.[37] The results are consistent with prior research, and overstate the true effect of 401(k) eligibility on private and national saving, due to at least two biases. We also find variation in the response to 401(k)s, consistent with the view that the offset between tax-preferred saving and other assets should rise with account-holders' age, income, wealth, and access to tax-preferred debt.

Thus, the findings emphasize the numerous potential channels through which substitution can occur, that such channels can differ across households, and that they need not be intentional or even well-understood by households. Unfortunately, our results do not provide information on the relative importance of each of the channels, nor on the extent to which substitution is intentional rather than inadvertent. Exploring these issues is an important direction for future research.

Our findings also suggest that other links between asset and debt holdings need to be considered more thoroughly in future research and policy design. First, the analysis confirms the importance of examining the impact of saving policy on broad measures of wealth, rather than narrow measures such as financial assets. Second, with either intentional or inadvertent substitution, the results suggest that "saving

[37]Although this paper focuses on tests exploiting variation in 401(k) eligibility, in Engen and Gale (1995), we obtain similar results based on regressions that use variations in 401(k) participation.

incentives" need to be designed to reward saving (that is, reductions in consumption), rather than to reward the placement of assets in designated accounts. Third, the results suggest that the expansion of debt may have played an important role in the decline and continuing low levels of saving in the United States, and that tax policy toward debt may be an important component of tax policy toward saving (see also Altig and Davis 1992).

APPENDIX A

This appendix discusses previous evidence concerning whether 401(k) eligibility is exogenous with respect to tastes for saving. At first glance, the idea that 401(k) eligibility is exogenous may seem plausible; as Poterba, Venti, and Wise (1995, p. 10) note, "eligibility is determined by employers." But while employers ultimately decide on the policy, a relevant issue is whether employers take employee preferences into account. In a survey of a broad range of employers, "perceived employee interest" was the second-most frequently stated reason that a firm installed a 401(k) plan and was noted by 63.5 percent of respondents (Buck Consultants, 1989). This should not be surprising; it would be strange if employers created benefits without regard to employee preferences. Moreover, even if firms did provide 401(k)s randomly, we would expect workers with high tastes for saving to seek out firms with 401(k)s or to encourage their firms to provide 401(k)s. These patterns are consistent with theoretical and empirical models of pensions.[38] But if employers do consider employee preferences, or if some employees prefer firms that offer 401(k)s, then eligibility is likely to be positively correlated with tastes for saving.

Ultimately, whether 401(k) eligibility is exogenous is an empirical issue. Poterba, Venti, and Wise (1995) present regressions showing that eligible households have

[38]See Allen, Clark, and McDermed (1993), Curme and Even (1995), Johnson (1993), and Ippolito (1993).

about the same level of non-pension, non-IRA, non-401(k) financial assets as ineligible households, controlling for income and other factors. They interpret these results as evidence that 401(k) eligibility is exogenous with respect to tastes for saving, given income and a few other household characteristics.

But the evidence and interpretation are fragile at best. First, Engen, Gale, and Scholz (1994, Table 8) use a similar sample from the same data set as Poterba, Venti and Wise (1995), a slightly different test format and a longer list of explanatory variables, and find that eligible families have higher levels of non-pension, non-401(k) financial assets, net financial assets and net worth.

Second, Bernheim (1994a, 1996) shows that evidence in Poterba, Venti, and Wise's own work indicates that differences in median financial assets between eligible and ineligible households are, in several income classes, several times as large as median 401(k) balances for eligible households. Bernheim and Garrett (1995), using cross section data that does not control for tastes for saving, find that 401(k) eligibility "raises" total wealth by about four times as much as it "raises" retirement wealth. Unless 401(k) contributions crowd in several times their value in non-401(k) saving, each of these findings suggest that eligibility is positively correlated with tastes for saving.[39]

[39]When Bernheim and Garrett (1995) examine the effects of 401(k) eligibility using initial wealth as a proxy for tastes for saving, they obtain results that Engen, Gale and Scholz (1996b, pp. 14-16) show to be consistent with the view that only a small portion at most of 401(k) balances represent net additions to saving.

Third, Poterba, Venti, and Wise (1995) omit pensions. Families eligible for 401(k)s in 1984, 1987, or 1991 were between 24 and 33 percentage points more likely to be covered by a defined benefit pension plan than other families in the same year, controlling for other factors (Engen, Gale, and Scholz, 1994). Again, this implies that eligible households have higher non-401(k) wealth than ineligible households. If pension coverage is positively correlated with tastes for saving, the difference in coverage is further evidence that eligibility is not exogenous. Even if coverage is independent of tastes for saving, higher pension wealth should, but does not, show up as lower non-pension wealth for eligibles than ineligibles if any of the pension wealth is offset by reductions in non-pension wealth. In short, the pensions findings imply that to believe that eligibility is exogenous requires assumptions that (a) pension coverage is not correlated with tastes for saving, and (b) all pension wealth is new wealth.

Fourth, the Poterba, Venti, and Wise test ignores all 401(k) wealth and thereby assumes that all 401(k) saving is new saving.[40] To determine whether 401(k) eligibility is exogenous requires knowing whether eligible families would have saved more than ineligible families in the absence of 401(k)s. If *x* percent of 401(k) wealth would have existed anyway, an appropriate test of exogeneity compares the non-401(k) assets of ineligible families to the sum of non-401(k) assets plus *x* percent of

[40]Poterba, Venti and Wise (1995, p. 10) make the puzzling and incorrect assertion that their test evaluates the exogeneity of 401(k)s "based on saving behavior before 401(k)s became available." But their earliest data are from 1984, when 401(k)s were obviously available.

the 401(k) wealth of eligible families. Clearly, assuming 401(k)s are all new saving (x=0)--as in the Poterba, Venti and Wise test--creates a bias in favor of finding that eligibility is exogenous.[41]

For all of these reasons, we conclude that eligibility is positively correlated with tastes for saving and that cross-sectional comparisons of eligible and ineligible households that do not control for tastes for saving are biased toward showing that 401(k)s raise saving.

[41]Poterba, Venti, and Wise (1996b) claim that omitting 1984 401(k) balances is not an important omission since these balances were quite small. Data from the 1983 Survey of Consumer Finances indicates that the median balance in tax-deferred compensation accounts among participants was about $3,700. Data from the form 5500s that pension plans file indicate even larger balances (Engen, Gale and Scholz 1996b).

APPENDIX B

This appendix discusses a series of additional issues that have been raised in interpreting the results in this paper. For additional discussion, see Engen, Gale, and Scholz (1996a, b).

Interpreting increases in mortgage debt Poterba, Venti, and Wise (1996b) claim that increasing mortgage debt may, in the long run, increase wealth. This is an odd claim, however, on a number of grounds. Since reducing mortgage debt raises wealth, it is hard to see how increasing mortgage debt also should be interpreted as an increase in future wealth. If households repay the added mortgage by reducing the amount of saving they do in other forms, then net saving will not have increased. Moreover, households can always take out second mortgages or home equity loans in the future. Finally, Manchester and Poterba (1989) show that households with home equity loans have less net worth, controlling for other factors. Thus, the most that can be said is that an increase in mortgage debt is a reduction in current wealth.

Cohort Analysis Poterba, Venti, and Wise (1996b) also claim that cohort analyses of saving incentive balances and home equity in 1984-7 and 1987-91 leads to the conclusion that "the timing of changes in mortgage debt and net home equity is inconsistent with a causal relationship between personal retirement plan contributions and mortgage debt." They find that from 1984-7, a period of rapid increases in contributions to saving incentives, home equity rose and there was no countervailing

increase in mortgage debt. From 1987-91, the increase in personal retirement assets slowed, but mortgage debt rose rapidly.

These results, however, provide no evidence against our results. As we and Poterba, Venti, and Wise (page 48) recognize, market trends in housing that are unlikely to be induced by saving incentives can have large effects on equity values and mortgage markets; and the Tax Reform Act of 1986 led to shifts in the composition of debt toward mortgages. We do not suggest that saving incentives are driving the changes in the mortgage or housing markets. The market for mortgages is very large compared to saving incentive contributions and is affected by many factors. It would be perfectly consistent with our results if those factors led to slow growth of mortgage debt before 1986 and rapid growth after 1986. Our point is that, after allowing for those factors, and controlling for household characteristics, eligible households (say, within each cohort) ended up with less housing equity relative to ineligible households. Examining the trends across cohorts--as Poterba, Venti, and Wise do--does not provide any information on how eligible families fared relative to ineligible families, or homeowners relative to renters.

Do homeowners want to reduce their housing equity? Bernheim (1996b) claims it is implausible that families would use their housing equity to finance 401(k) contributions on the grounds that one study (Venti and Wise 1989) indicates that past retirees have been reluctant to reduce housing equity. However, that study was based on data from the 1970s, when housing prices were rising dramatically. The

study may also suffer from an important sample selection bias: elderly people who reduce their equity (by selling their house and moving to another, or into a nursing home, or in with relatives) may get dropped from subsequent waves of the survey. Other studies of the elderly have reached different conclusions (see Hurd 1995).

In addition, whether the elderly wish to consume their housing equity is not directly relevant for our results, which (a) focus on workers, who are accumulating assets, rather than the elderly, who are dissaving, and (b) find evidence of reshuffling between mortgage debt and 401(k)s, not necessarily between house value and 401(k)s. Thus, even if people do not want to trade off the size of their house for other consumption (elderly) or other assets (workers), they may still reshuffle housing equity with 401(k)s through debt.

Along similar lines, Bernheim notes that many younger households say in surveys that they view their home equity primarily as a source of financial security. The relevance of his observation is unclear. Certainly, surveys of people's intentions, as opposed to their actions, warrant skepticism. Our results measure people's actions. In addition, as noted above, substitution between housing equity and 401(k)s need not be intentional, and in some cases can occur without the household even being aware of the substitution. Finally, survey responses about intentions notwithstanding, there were in fact several home equity lending booms in the 1980s and 1990s indicating that some households were taking equity out of their houses.

The sample of renters: Bernheim (1996b) asserts that the sample of eligible renters appears to be "more selected" than that of eligible homeowners. However, 401(k) participation rates conditional on income, age and eligibility are slightly lower for renters than for homeowners. This is not consistent with Bernheim's assertion. Bernheim (1996b) also suggests there could be greater dilution of eligible renters over time. Based on trends in IRA participation, the data do not support this claim.[42] Finally, Bernheim (1996b) claims low wealth among ineligible renters could invalidate the results, because if economic forces were pushing down wealth in both groups of renters, ineligible renters may not have been able to reduce their wealth much, due possibly to liquidity constraints. This is certainly a possibility. However, if renters save almost nothing, any increase in 401(k) balances should show up very clearly as an increase in wealth. Eligibility did raise financial assets in absolute terms among homeowners, who have higher levels of wealth than eligible renters, so there is little reason to see why, if 401(k)s raise wealth, eligibility should not also have raised renters' financial assets.

[42]In 1987, controlling for other factors, eligible renters were about 2.2 percentage points more likely to hold an IRA. In 1991, the corresponding figure was also 2.2 percentage points, indicating no symptoms of dilution of the sample of eligible renters compared to the sample of ineligible renters.

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