Volume Title: Frontiers in the Economics of Aging

Volume Author/Editor: David A. Wise, editor

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-90304-4

Volume URL: http://www.nber.org/books/wise98-1

Publication Date: January 1998

Chapter Title: Personal Retirement Saving Programs and Asset Accumulation: Reconciling the Evidence

Chapter Author: James M. Poterba, Steven F. Venti

Chapter URL: http://www.nber.org/chapters/c7297

Chapter pages in book: (p. 23 - 124)

1 Personal Retirement Saving Programs and Asset Accumulation: Reconciling the Evidence

James M. Poterba, Steven F. Venti, and David A. Wise

A large fraction of American families reach retirement age with virtually no personal financial assets. The median level of all personal financial assets of families with heads aged 55–64 was only $8,300 in 1991; excluding individual retirement accounts (IRAs) and 401(k) balances the median was only $3,000. Mean values are substantially higher. Almost 20 percent of families had no financial assets at all. In 1991, the median value of the future social security benefits of retired families with heads aged 65–70 was about $100,000, the median value of housing was about $50,000, and the median value of future employer-provided pension benefits was about $16,000. But other than social security and pension benefits, and illiquid housing wealth, the typical family has very limited resources to meet unforeseen expenses.

Two saving programs introduced in the early 1980s were intended to encourage individual saving. IRAs rapidly became a very popular form of saving in the United States after they became available to all employees in 1982. Any employee could contribute $2,000 per year to an IRA and a nonworking spouse could contribute $250. The contribution was tax deductible. Annual contributions grew from about $5 billion in 1981 to about $38 billion in 1986, approximately 30 percent of total personal saving. Contributions declined precipi-

James M. Poterba is professor of economics at the Massachusetts Institute of Technology and director of the Public Economics Research Program at the National Bureau of Economic Research. Steven F. Venti is professor of economics at Dartmouth College and a research associate of the National Bureau of Economic Research. David A. Wise is the John F. Stambaugh Professor of Political Economy at the John F. Kennedy School of Government, Harvard University, and the director for Health and Retirement Programs at the National Bureau of Economic Research.

This research was supported primarily by a series of grants from the National Institute on Aging. The authors also acknowledge the support of the Hoover Institution (Wise), the National Science Foundation (Poterba), and the National Bureau of Economic Research. The authors are grateful to Bill Gale, Jon Gruber, Jon Skinner, and Richard Thaler for comments on an earlier draft of the paper.

tously after the Tax Reform Act of 1986, even though the legislation limited the tax deductibility of contributions only for families who had annual incomes over $40,000 and who were covered by an employer-provided pension plan. By 1994, only $7.7 billion was contributed to IRAs, and while over 15 percent of tax filers contributed in 1986, less than 4 percent contributed in 1994.

The other program, the 401(k) plan, grew continuously and almost unnoticed, with contributions increasing from virtually zero at the beginning of the decade to over $51 billion by 1991, when almost 25 percent of families contributed to a 401(k). Deposits in 401(k) accounts are also tax deductible, and the return on the contributions accrues tax free; taxes are paid upon withdrawal. But these plans are available only to employees of firms that offer such plans. Prior to 1987 the employee contribution limit was $30,000, but the Tax Reform Act of 1986 reduced the limit to $7,000 and indexed this limit for inflation in subsequent years. The contribution limit was $9,235 for both the 1994 and 1995 tax years.

Although very small at the beginning of the decade, by 1989 contributions to all personal retirement saving plans exceeded contributions to traditional employer-provided pension plans, as shown in figure 1.1. It seems evident that were it not for the Tax Reform Act of 1986, personal retirement plan saving would have been much larger. Whether these programs increase net saving can be of critical importance to future generations of older Americans and to the health of the economy in general. The issue remains an important question of economic debate. In a series of papers based on very different methods of analysis we have concluded that a large fraction of the contributions to these accounts represent new saving. Our previous research is summarized here, along with several new results.

As interest in the saving effect of these programs evolved, several other investigators also directed attention to the issue. In some instances, alternative analyses came to conclusions that differed dramatically from ours. Thus in describing our results we have tried to point out the differences between our methods and alternative approaches that have been used to address the same questions. We have not, however, attempted to comment on all analyses of the relationship between retirement plan saving and total personal saving.

The key impediment to determining the saving effect of IRAs and 401(k)s is saver heterogeneity. Some people save and others do not, and the savers tend to save more in all forms. For example, families with IRAs also have more conventional savings than families without IRAs. Thus a continuing goal of our analyses has been to consider different methods of controlling for heterogeneity. The methods that could be used when each analysis was conducted were largely dependent on the available data. As new data became available we used alternative and possibly more robust methods to control for heterogeneity.

The paper has several sections: Sections 1.1 through 1.5 present our results and are organized by the method used to control for heterogeneity. In each case the question is whether IRA and 401(k) contributions substitute for conven-

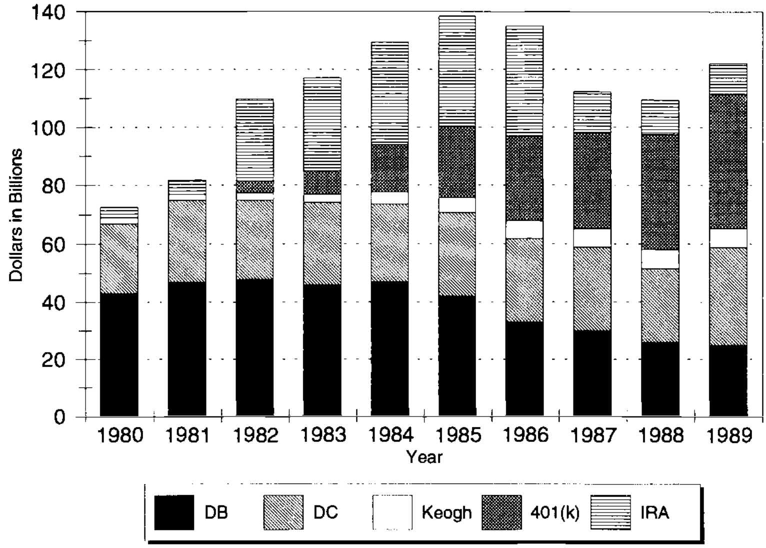

Fig. 1.1 Retirement plan contributions

tional financial asset saving. These sections also contain some discussion of closely related results reported by others. While early work in this area focused on the potential substitution between IRA assets and liquid financial assets, subsequent analyses considered the potential substitution between personal retirement saving plan assets and employer-provided pension assets and housing equity. Section 1.6 considers others margins of substitution, particularly the possibility that saving in these programs is financed by drawing down home equity. Section 1.7 addresses the divergence between our conclusions based on Survey of Consumer Finances 1983–86 summary data—introduced in section 1.2—and the parametric analysis of the same data by Gale and Scholz (1994). Conclusions are presented in section 1.8.

1.1 Early Parametric Analysis of Substitution at the Outset of the IRA Program

When Venti and Wise began work on the saving effect of IRAs in the mid-1980s, data on asset holdings were available for a limited time period. Assets could typically be measured at only two points in time, one year apart. To use these data, Venti and Wise developed an econometric model that could be used to estimate the relationship between IRA saving and other saving. Within a framework that allowed for any degree of substitution between IRA and non-IRA saving, the analysis asked whether persons who save more in IRAs in a particular year save less in other financial asset forms, controlling for age, income, other personal characteristics, and accumulated housing and financial assets. Given age and income, this approach used accumulated financial assets to control for "individual-specific" saving effects. The analysis accounted for the explicit limit on IRA contributions and placed substantial emphasis on the change in non-IRA saving after the IRA limit is reached.

The first results using this approach were based on data from the 1983 Survey of Consumer Finances (Venti and Wise 1986, 1987; Wise 1987). Subsequent analysis was based on the 1980–85 Consumer Expenditure Surveys (Venti and Wise 1990) and the 1984 panel of the Survey of Income and Program Participation (Venti and Wise 1991).[1]

The results suggested that the majority of IRA saving, even at the outset of the program, represented net new saving and was not accompanied by substantial reduction in other financial asset saving. These findings imply that increasing the IRA limit would lead to substantial increases in IRA saving and very little reduction in other saving. If the IRA limit were raised, one-half to two-thirds of the increase in IRA saving would be funded by a decrease in current consumption and about one-third by reduced taxes; only a very small proportion—at most 20 percent—would come from other saving.

The widely cited study by Gale and Scholz (1994), based on the 1983–86 Survey of Consumer Finances, was in some respects in the same spirit as these analyses, but their conclusions were radically different, suggesting that raising the IRA limit would have virtually no effect on total personal saving. A detailed analysis of the findings in this study is presented in section 1.7 of this paper.

Our subsequent analyses have taken a very different turn, using better data and more robust methods to control for heterogeneity. Our findings based on these approaches are discussed in the next four sections.

1.2 Following Individuals over Time at the Outset of the IRA Program

To frame the discussion in this and subsequent sections, we give a simple algebraic description of the key features of each method that we use to control for heterogeneity. We establish some notation and key ideas at the outset. Consider the flow of saving S_{it} of person i in year t. To capture saving heterogeneity among families, suppose that saving of person i depends on an unobserved individual-specific saving effect $\boldsymbol{m}_i$. This effect is large for more committed savers and small for less eager savers. Saving may also depend on a program effect, which is denoted by $\boldsymbol{p}_{it}$. For program participants, $\boldsymbol{p}_{it}$ is the component of saving that is due to the program; for nonparticipants, $\boldsymbol{p}_{it}$ is zero. If person i

1. In an earlier study, Hubbard (1984) found that the ratio of assets to income was higher for IRA participants, controlling for individual attributes and eligibility. He concluded that the results "provide strong evidence that contributions to IRAs and Keogh plans do increase individual saving." Feenberg and Skinner (1989) show that IRA participants save *more* than nonparticipants, controlling for initial wealth.

is a saving program participant and person j is a nonparticipant, $S_{it} = \boldsymbol{m}_i + \boldsymbol{p}_{it}$ and $S_{jt} = \boldsymbol{m}_j$. The difference in saving between these individuals is

$$(1) \qquad S_{it} - S_{jt} = (\boldsymbol{m}_i - \boldsymbol{m}_j) + \boldsymbol{p}_{it}.$$

This difference confounds the program effect with the difference in the taste for saving.

In this simple example the difference in saving rates between participants and nonparticipants does not provide an unbiased estimate of the program effect because the unobserved taste for saving is correlated with program participation. This form of heterogeneity is probably the most important source of potential bias, but there are others as well. In the following sections we present several methods to control for heterogeneity. Each method controls for important sources of heterogeneity, but no single method—other than a randomized controlled trial—can control for all possible sources. Each of the methods in this and the following sections is described in a consistent way, in an attempt to highlight both the way that heterogeneity is addressed as well as the potential types of heterogeneity that each method may not address. A specific form of heterogeneity that may confound an estimate obtained by one method may not present the same problem within the context of another method. Thus there is an important advantage to using several methods to address potential heterogeneity.

In practice, each estimate is a difference obtained in one of three ways: (1) by comparing the saving or assets of a "treatment" group in a later period with saving or assets of the same group in an earlier period, relying on *within*-group changes; (2) by comparing saving or assets of two different groups in the same period, relying on *between*-group comparisons; or (3) by comparing the assets at a given age of persons who attain that age in different calendar years, using "cohort analysis."

1.2.1 Change in Other Saving When IRA Status Changes

The Method

The most direct way to control for heterogeneity is to follow the same household over time, observing the change in S_{it} when program participation changes. Saving in periods t and $t+1$ for household i can be described by

$$S_{it} = \boldsymbol{m}_i + \boldsymbol{p}_{it},$$

$$S_{i,t+1} = \boldsymbol{m}_i + \boldsymbol{p}_{i,t+1}.$$

The "within-household" change in saving is therefore

$$(2) \qquad S_{i,t+1} - S_{it} = (\boldsymbol{m}_i - \boldsymbol{m}_i) + (\boldsymbol{p}_{i,t+1} - \boldsymbol{p}_{it}) = \boldsymbol{p}_{i,t+1} - \boldsymbol{p}_{it},$$

which yields an estimate of the program effect for households that participate in one period but not in the other (p_{it} is zero in the nonparticipating period). The unobserved individual-specific saving effects are "differenced" out.

If the heterogeneity is limited to differences in saving commitment among households, and the problem is simply that more committed savers are more likely to be program participants, then this within-household change in saving provides a clean estimate of the program effect. But this estimate can be confounded by another possible source of heterogeneity: differences in saving commitment over time within the same household. If individual saving commitment changes at the same time that participation status changes, this estimate will capture the effect of a change in the taste for saving as well as the participation effect. With this coincidence, the difference would be

$$S_{i,t+1} - S_{it} = (m_{i,t+1} - m_{it}) + (p_{i,t+1} - p_{it}). \tag{3}$$

If the household began to participate in period $t+1$ at the same time that the unobserved propensity to save increased, the difference in saving would overestimate the program effect.

The Results

Venti and Wise (1995a) used this method in analyzing data from consecutive waves of the 1984 Survey of Income and Program Participation (SIPP). The SIPP panel data allow calculation of the change in non-IRA saving when IRA contributor status changes, although non-IRA saving must be inferred from asset income. They considered the change in non-IRA saving between 1984 and 1985 by IRA contributor status.[2] If non-IRA saving is reduced when IRA saving is increased, then when a household that was not contributing begins to contribute, that household should reduce its non-IRA saving. Likewise, when a household that was contributing stops contributing, non-IRA saving should increase. Venti and Wise find, however, that when the same families are tracked over time, there is little change in other financial asset saving when families begin to contribute to an IRA, or when they stop contributing. Illustrative results are shown in table 1.1.

These data reveal little substitution. The key information in this approach is the change in other financial assets when families began to contribute to an IRA. In particular, the non-IRA financial asset saving of families that did not contribute in 1984 but did contribute in 1985 declined by only $193 between 1984 and 1985.[3] This decline in other saving is only a small fraction of the increase in saving from the typical family IRA contribution, $2,300. These

2. Non-IRA saving is inferred from capitalized asset income at three points in time, measured in current-year dollars. Non-IRA assets include all interest-bearing financial assets including stocks and bonds.

3. If the underlying assets are measured in constant dollars, instead of current-year dollars, the change is $186.

Table 1.1 **Change in Non-IRA Saving When IRA Contributor Status Changed between 1984 and 1985**

	1985 Noncontributor	1985 Contributor
1984 Noncontributor	89.4	−193.5
	(102.1)	(413.6)
1984 Contributor	630.3	186.2
	(527.2)	(303.9)
	$F = 0.698$	

Source: Venti and Wise (1995a).
Note: Numbers in parentheses are standard errors.

data suggest that even near the outset of the IRA program there was only a small reduction in non-IRA saving when IRA contributions began.[4]

As emphasized above, this procedure does not correct for "within-individual" change in saving behavior. For example, suppose that the saving behavior of persons who began to contribute in 1985 changed between 1984 and 1985 and that this change happened to coincide with the newly available IRA option. If the IRA option had not been available, it could be argued, the person would have saved in the non-IRA form, but since it was available, the newly awakened saver stored assets in the more advantageous IRA instead. The alternative, of course, is that the IRA option induced the person to save in that form and that the new IRA contribution would not have been saved in another form. To us, the results seem more consistent with the conclusion that the two forms of saving are largely independent, with changes in IRA saving having little effect on other saving. It is clear that those who began to contribute in 1985 had not been saving $2,300 annually prior to 1984. Indeed, their estimated asset balance in 1984 was only $3,362. The same is true for persons who had contributed to an IRA in 1984 but quit contributing in 1985; their 1984 balance was $4,816.

It is nonetheless possible that individual behavior could have changed over time, and the method used here cannot formally correct for this. The cohort analysis discussed below, however, accounts for this possibility, and the results are consistent with the conclusions drawn here.

1.2.2 Attanasio and De Leire's Study of "Old" versus "New" Contributors

Attanasio and De Leire (1994; hereafter AD) analyze Consumer Expenditure Survey (CES) data to evaluate the substitution between IRA and other

4. The increase of $630.3 when contributions are curtailed also suggests some substitution as well, although the estimate is not significantly different from zero. Again, the amount is much less than the typical family IRA contribution. Asset balances are measured in May of each year because IRA contributions can be made through the 15 April tax-filing deadline.

financial assets. The CES data essentially provide a series of independent cross sections, but each cross section is in fact a short panel, providing asset balances at two points in time, one year apart. AD consider the difference between the annual non-IRA saving—measured by the change in asset balances—of "old" and "new" contributors. In a given year, old contributors are families that contributed in the previous year (and possibly earlier years as well); new contributors are those who did not contribute in the previous year. Within a regression framework, they find that old contributors save \$1,740 more than new contributors.

At first blush, the results may appear to contradict the evidence just presented. On closer inspection, however, the method used by AD—the comparison of new contributors with old contributors—can say very little about the saving effect of the IRA program. Indeed, if taken at face value, the AD result suggests that in the *first year* that an IRA contribution is made, there is a drop in non-IRA financial assets, but in the next year and in future years when the household continues to contribute, other saving reverts to its pre-IRA level. So in the long run there is essentially *no offset* of IRA saving by a reduction in other saving.[5]

The key features of the AD method are shown in Figures 1.2*A* through 1.2*C*. These *illustrative* figures assume that old contributors start to make IRA contributions in the 1982–83 period and that new contributors start to contribute in the 1983–84 period, that both old and new contributors save \$2,000 per year in the absence of the IRA, and that an IRA contribution is \$2,000. The figures compare the other saving of old and new contributors in the 1983–84 period.

Figure 1.2*A* illustrates the situation when there is no substitution at all. Other saving remains at \$2,000 when IRA contributions begin. When the new contributors begin to contribute, there is *no difference* in the other saving of old and new contributors. Figure 1.2*B* illustrates the situation when there is complete substitution. When the old contributors begin to contribute, their other saving falls from \$2,000 to zero and remains at zero thereafter. When the new contributors begin to contribute one year later, their other saving also falls from \$2,000 to zero. When the two groups are compared in 1983–84, saving is zero for both groups. Like the no substitution case, there is *no difference* in the other saving of new and old contributors. Thus the value computed by AD cannot distinguish between the two polar cases of no substitution and complete substitution.

Neither of the processes illustrated in figures 1.2*A* or 1.2*B* are consistent with the difference between the saving of old and new contributors that AD find. Figure 1.2*C* illustrates a process that is consistent with their finding. When old contributors first contribute, their other saving falls from \$2,000 to zero. But in the next year other saving reverts to the previous level of \$2,000. In this year, new contributors begin to contribute, and their other saving falls

5. A similar point is made by Hubbard and Skinner (1995).

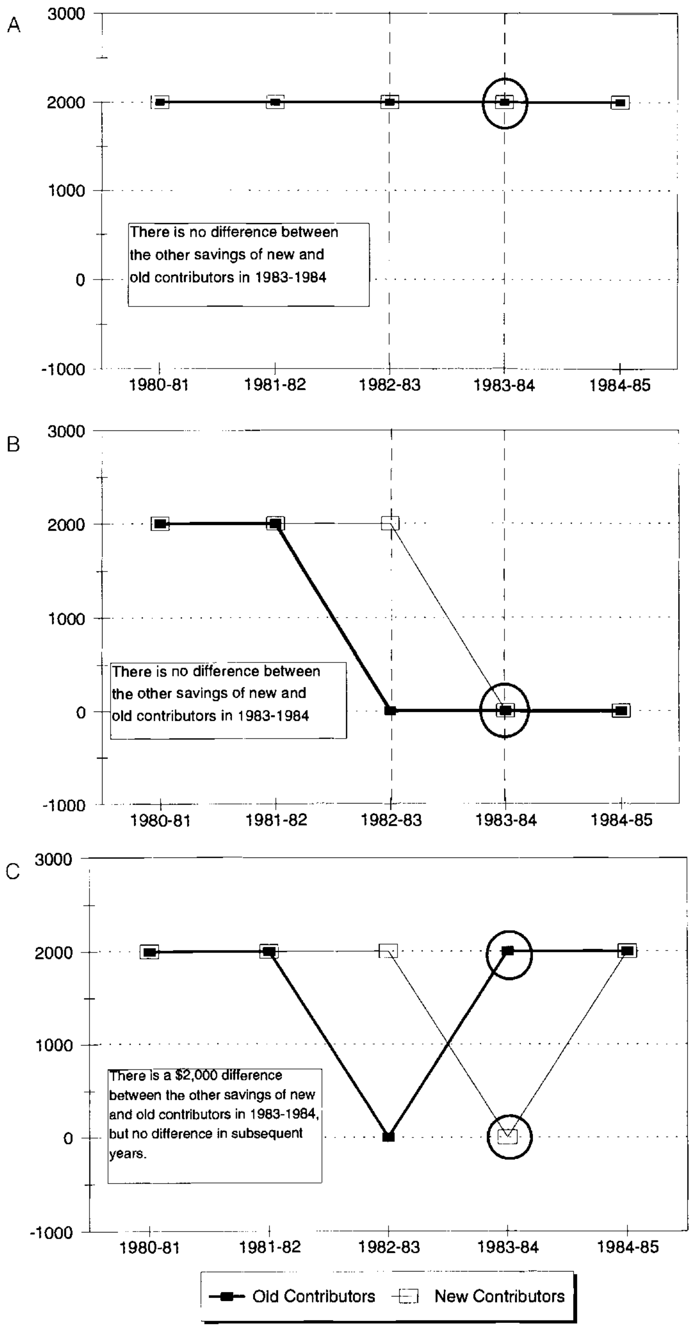

Fig. 1.2 The Attanasio and De Leire method

Note: (*A*) No substitution at all. (*B*) Complete substitution. (*C*) One-year substitution.

Table 1.2 **Inferred Non-IRA Financial Asset Balances in 1984, 1985, and 1986, by Asset and by Change in IRA Contributor Status between 1984 and 1985 (in current dollars)**

	1985 IRA = 0			1985 IRA > 0		
	May 1984	May 1985	May 1986	May 1984	May 1985	May 1986
1984 IRA = 0	1,210	1,587	2,053	3,362	5,051	6,546
	(98)	(98)	(98)	(399)	(399)	(399)
1984 IRA > 0	4,816	5,896	7,606	7,457	9,659	12,048
	(509)	(509)	(509)	(293)	(293)	(293)
		$R^2 = 0.212$			$F = 434.60$	

Source: Venti and Wise (1995a).
Note: Numbers in parentheses are standard errors.

from \$2,000 to zero. The difference between the other saving of old and new contributors is \$2,000 in this year, approximately AD's finding. The next year, however, new contributors become old contributors, and their other saving reverts to its previous level of \$2,000. There is a one-year reduction in other saving but no offset thereafter, and thus little substitution in the long run.

More detail on the non-IRA saving of households who do and do not change IRA status helps to show the limitations of the AD method. The asset data from which the numbers in table 1.1 were derived are shown in table 1.2.[6] For example, the change in the other saving of persons who began to contribute to an IRA in 1985 is derived from the three asset levels in the upper right corner of table 1.2 ((6,546 − 5,051) − (5,051 − 3,362) = −193.5). Two things are clear from these data: First, the assets of old contributors—in the lower right corner—are substantially greater than the assets of new contributors—in the upper right. Their annual saving is different as well—about \$1,600 for new contributors, about \$2,300 for old contributors. Second, there was little change in the other saving behavior of either group over the two-year period, −193.5 for new contributors and +186.2 for old contributors. Looking at these data, however, the method used by AD would show a difference between the non-IRA financial assets of old and new contributors of −\$894 in 1985 ((6,546 − 5,051) − (12,048 − 9,659) = −894), which bears no particular relationship to substitution.

1.2.3 Joines and Manegold's Analysis of Saving Change When the IRA Limit Increases

Joines and Manegold (1995) use the "change" in IRA contribution limits determined by the Economic Recovery Tax Act (ERTA) of 1981 to estimate the

6. These asset levels are inferred from asset earnings reported in SIPP at three points in time, approximately one year apart. Inferred assets based on alternative methods are presented in Venti and Wise (1995a). Although the asset levels differ by method and the growth in assets differs as well, the basic difference-in-difference results, as in table 1.1, are very similar.

saving effect of the IRA program. Prior to ERTA, only wage earners without an employer-provided pension plan were eligible to contribute to an IRA. The contribution limit for wage earners in this group was $1,500. ERTA extended eligibility to all wage earners, beginning in 1982, and increased the limit for each wage earner to $2,000. In addition, nonworking spouses of wage earners could contribute $250.

Joines and Manegold consider the *change in the total annual financial asset saving* of *contributor* households between the 1979–81 and 1982–85 periods as a function of the *change in the IRA limit* between these time periods. Their analysis is based on a panel of individual tax returns. Saving in each period is determined by the change during the period in total financial assets, which are estimated by capitalizing reported asset income. Two groups of households are considered: "new" contributor households first contributed at some time in the later (1982–85) period, and "continuing" contributor households contributed in both the earlier and later periods. The "change" in the limit for new contributors was from zero to $2,000, $2,250, or $4,000 for single wage earner families, couples with a single wage earner, and two wage earner families, respectively. The limit change for continuing contributors was from $1,500 to $2,000 for single wage earners, from $1,500 to $2,250 for couples with a single wage earner, and from $3,000 to $4,000 for two wage earner couples.

Joines and Manegold estimate a relationship of the form

$$\text{Saving}_{82-85} - \text{Saving}_{79-81} = \beta(X) * (\text{Limit change}) + \gamma X, \tag{4}$$

where the key parameter β is the relationship between the limit change and the change in saving. In some specifications β is estimated as a single parameter; in others it is a function of a vector of covariates X, describing primarily household tax status.[7] The covariates also enter separately, with coefficient γ. The parameter β is *not* the saving effect of IRA contributions. It does not represent the relationship between IRA contributions and saving but rather the relationship between saving and the change in the IRA limit. Thus it is not comparable to most other estimates discussed in this paper, which consider the proportion of contributions that represent new saving. For example, suppose that a single new contributor deposited $1,000 in an IRA. If the $1,000 were entirely new saving, β would be 0.50 (1,000/2,000), not 1.00. If the $1,000 were the only deposit of a single wage earner couple, β would be 0.44 (1,000/2,250). If the $1,000 were the only deposit of a two wage earner family, β would be 0.25 (1,000/4,000).[8]

7. The variables include a mortgage deduction indicator, number of exemptions, marital status, the first-dollar marginal tax rate, gender, and transitory income (measured by the mean deviation of income from average income over the 1979–86 period).

8. Although Joines and Manegold compare their estimates to those of Venti and Wise (1986, 1987, 1990, 1991) and Gale and Scholz (1994), their comparisons are inappropriate. Venti and Wise consider the proportion of IRA contributions that represent new saving, and to indicate the implications of the results, they simulate the proportion of the *increase in IRA contributions resulting from an increase in the limit* that would be new saving. Gale and Scholz direct their analysis to this proportion as well, as discussed in section 1.7 below.

When β is estimated as a single parameter, Joines and Manegold obtain values ranging from 0.17 to 0.73, depending on the method of estimation. The largest estimate is obtained by ordinary least squares and is very imprecise. Joines and Manegold favor robust least squares estimates, with β parameterized as a function of X. Their "best guess" estimate of β is 0.26. This implies that substantially more than 26 percent of the IRA contributions associated with an increase in the limit would represent new saving.

Joines and Manegold find that the median of estimated total financial assets of new contributors was only $4,396 in the 1979–81 period. The typical IRA contribution in the 1982–85 period was about $2,300. Since most contributors in this period were new contributors, the typical contribution of new contributors was clearly much greater than these new contributors had been accustomed to saving prior to the advent of the IRA program. Thus, although the analysis does not purport to estimate the net saving effect of IRA contributions, the Joines and Manegold summary data suggest that the saving effect is likely to have been substantial.

1.2.4 Analyzing the Change in the Assets of IRA Contributors

The Method

We now consider the change in non-IRA saving of IRA *contributors* as their IRA savings accumulate. The specification above relates saving in year t to an individual-specific taste effect $\boldsymbol{m}_i$ and a program effect $\boldsymbol{p}_i$. Most surveys do not obtain direct measures of saving, however, but instead collect information on asset balances; saving must be estimated from changes in the balances. Since asset balances reflect the accumulation of past saving decisions, they also reflect individual-specific saving effects. Suppose an IRA contributor with individual-specific component $\boldsymbol{m}_i$ has been saving for s years and has participated in a retirement saving program for n of these years. Then the household's asset balance, after s years of saving and n years of program saving, is

$$(5)\quad A_{si}(n) = (1 + r)^s A_0 + \boldsymbol{m}_i[(1 + r)^s - 1)]/r + \boldsymbol{p}_i[(1 + r)^n - 1)]/r$$
$$= h(s) + \boldsymbol{m}_i f(s) + \boldsymbol{p}_i g(n),$$

where A_0 is the level of assets when saving began (possibly zero), r is the rate of return, and $h(\cdot)$, $f(\cdot)$, and $g(\cdot)$ are defined by context. This very stylized formulation need not be interpreted literally and indeed does not reflect saving behavior that might limit accumulation to some precautionary level, for example. Here the formulation simply serves to emphasize that the program effect as well as the individual saving effect are magnified by the number of years over which saving occurs. To illustrate the key features of this and other methods, we write the relationship in the simplified form, highlighting the key parameters in boldface.

Now assume that we observe *contributors* after n years of exposure and then

Table 1.3 **Survey of Consumer Finances Data Summary (in current dollars)**

Contributor Status and Asset	Year 1983	Year 1986	Percentage Change
Contributors in 1986			
Non-IRA assets	**9,400**	13,500	43.6
IRA assets	1,000	7,000	600.0
Total assets	12,075	**24,000**	98.8
Noncontributors in 1986			
Total assets	729	1,000	37.2

again after $n+k$ years of exposure. After $n+k$ years of program saving, and $s+k$ years of nonprogram saving, the assets of participants will be

$$A_{si}(n+k) = h(s+k) + \boldsymbol{m}_i f(s+k) + \boldsymbol{p}_i g(n+k).$$

The change in assets over the k years is given by

$$A_{si}(n+k) - A_{si}(n) = h(s+k) - h(s) + \boldsymbol{m}_i[f(s+k) - f(s)]$$
$$+ \boldsymbol{p}_i[g(n+k) - g(n)].$$

If $A_0 = 0$, this expression becomes

$$(6) \qquad A_{si}(n+k) - A_{si}(n) = \boldsymbol{m}_i[f(s+k) - f(s)] + \boldsymbol{p}_i[g(n+k) - g(n)].$$

The change in assets reflects the program effect plus the saving that $\boldsymbol{m}_i$ type families would have done over k years in the absence of the program. To isolate the program effect, we use cross-sectional data at the earliest observation date to approximate $\boldsymbol{m}_i[f(s+k) - f(s)]$, the expected change in saving over the next k years in the absence of the IRA program. We then compare this estimate with the actual change in assets for IRA contributors.

The Results

Using 1983 and 1986 Survey of Consumer Finances (SCF) data it is possible to compare the asset balances of the same households over time. Venti and Wise (1992) considered how the assets of IRA contributors changed over this time period. The results are reported in table 1.3. Households that made IRA contributions over this period began the period with a median of $9,400 in other financial assets in 1983. Between 1983 and 1986, the IRA assets of these families increased from $1,000 to $7,000. Other financial assets increased from $9,400 to $13,500. These families ended the period with total financial assets, including IRAs, of $24,000, an increase of 100 percent over assets in 1983. Venti and Wise determined that an increase of this magnitude could not be accounted for by change in age, income, or rate of return between 1983 and

1986.[9] In particular, they find that the increase in other financial assets is no less than would have been expected in the absence of the IRA program. Thus they conclude that it is unlikely that the IRA contributions simply substituted for saving that would have occurred anyway.

Once again, however, it is possible that at least some IRA contributors experienced a shift in saving commitment that happened to coincide with the emergence of the IRA option and that the new commitment to saving was realized through contributions to an IRA instead of contributions to conventional saving accounts. But for the results to be explained by a within-household change in saving behavior would require that most IRA contributors over the 1983–86 period had not been committed savers prior to this period (to be consistent with the low 1983 asset balances) but became committed savers just as the IRA program became available and would have become committed savers in the absence of the program. This seems to us an unlikely coincidence of events.[10]

The numbers in table 1.3 come from the same data used by Gale and Scholz (1994) in their analysis of the saving effect of the IRA program. In section 1.7 below we return to consideration of their methodology and how the conclusions of their formal analysis could be so different from what we believe these simple data suggest.

1.3 Comparing the Assets of "Like" Saver Groups Over Time

1.3.1 Within-Group Comparisons

The Method

Each of the foregoing methods rests on comparing the same individuals over time, so that similar saving propensities can be "differenced out." Another way to eliminate the unobserved saving effect is to group households with similar saving propensities and then estimate the program effect by using the *within*-group difference in exposure to retirement saving programs. Poterba, Venti, and Wise (PVW 1994a, 1995) use saving program participation itself as a signal of taste for saving. "Like saver" groups are determined by observed saving behavior: families participating in an IRA only are one group, families who participate in both an IRA and a 401(k) are another, and so forth.

9. Prediction of the expected increase in non-IRA saving in the absence of the program is discussed in section 1.7 below.

10. The test reported in Venti and Wise (1990) provides more formal evidence against the coincidence hypothesis. Unlike the SCF data that pertain to the same households in 1983 and 1986, the CES data used in the Venti and Wise (1990) analysis is based on random samples of similar households for the period 1980–85. E.g., the 1980 survey respondents were about the same age as the 1985 respondents. If the saving behavior of contributors changed just as the IRA program was introduced, estimates of saving based on post-1982 data should predict pre-1982 saving poorly. But the formal model estimated on post-1982 data predicts well the pattern of saving by income in the pre-1982 period, prior to the advent of IRAs. If the saving behavior of contributors had changed dramatically over this time period, one would expect a poor match between actual and predicted pre-1982 saving.

We consider the *within*-group difference between the assets of a like group at two points in time, but we do not compare the *same* households in two periods. Rather the groups are obtained from random cross sections of households surveyed in different calendar years. Because the cross-sectional surveys are representative, the demographic attributes of the cross sections are approximately the same each year. The like saver groups will be the same if households that save in a given way in one year are like the households that save in that way in another year. The hope is that two randomly chosen cross sections from the same like group share the same unobserved saving propensities and thus would have the same asset balances, except for differential exposure to the special saving programs, which identifies the program effect. Families observed in 1984 had had about two years of exposure to the IRA and 401(k) programs, families observed in 1987 about five years of exposure, and families in 1991 about nine years.

Two factors may complicate this analysis of the effect of program exposure. First, although the IRA program expanded rapidly between 1982 and 1986, the Tax Reform Act of 1986 reduced the attraction of IRAs for households with incomes above $30,000 and led to a massive reduction in IRA participation by households at all income levels, even those who were unaffected by the legislation. There were few new contributors after 1986. Second, the 401(k) program grew rapidly throughout the 1980s, with more and more firms offering such plans. In both cases, but especially with respect to IRAs, the characteristics—and thus the saving commitment—of participants may have changed over time. In principle, there could also be year-specific macroeffects that might affect saving of both program participants and nonparticipants. (The results below show no effects for nonparticipants.)

Now consider explicitly the assets of a like saver group surveyed in two different randomly selected cross sections, conducted k years apart. The two random samples of a particular group have been saving in any form for approximately the same number of years s, but the sample surveyed in the earlier year has had n years of exposure to the program and the sample surveyed in the later year has had $n+k$ years of exposure. (It is not important that s be known, but only that s be the same for each random cross section.) Assume that $\boldsymbol{m}_i$ is the typical saving propensity of the sample surveyed at the later date and that $\boldsymbol{m}_{i'}$ is the typical saving propensity of the sample surveyed at the earlier date. Then

$$A_{si}(n) = h(s) + \boldsymbol{m}_{i'}f(s) + \boldsymbol{p}_i g(n),$$

$$A_{si}(n + k) = h(s) + \boldsymbol{m}_i f(s) + \boldsymbol{p}_i g(n + k).$$

This implies

$$(7) \qquad A_{si}(n + k) - A_{si}(n) = (\boldsymbol{m}_i - \boldsymbol{m}_{i'})f(s) + \boldsymbol{p}_i[g(n + k) - g(n)].$$

If $\boldsymbol{m}_i = \boldsymbol{m}_{i'}$, then the difference in assets of the two random samples represents the program effect. If $\boldsymbol{m}_i \neq \boldsymbol{m}_{i'}$, then the difference represents a combination of the program effect and the different saving propensities of the two samples.

If, as seems likely, less committed savers are drawn into the program as it matures, then $m_i < m_{i'}$, and the first term is negative. In this case, the direction of the "bias" is clear; the difference in assets underestimates the program effect.[11]

Results

PVW (1994a, 1995) used several saving choices to identify like saver groups. We grouped families in two ways: first, according to whether they contributed to an IRA, a 401(k), or both; and second, according to whether they were eligible for a 401(k) plan and whether they had an IRA. Altogether, we considered six different groups of "saver types," not counting those without IRA or 401(k) saving. We focused on the within-group change in the other saving of families in these groups using data from the SIPP for 1984, 1987, and 1991. Random samples of saver types are similar in each of these years, but the 1984 sample had had only about two years (1982–84) to accumulate 401(k) and IRA balances, the 1987 sample about five years, and the 1991 sample about nine years. The central question is whether longer exposure to these plans results in higher levels of saving by families who participate in the programs.

The key test for substitution is whether non–IRA-401(k) assets are lower for the random samples that had been exposed to the IRA and 401(k) programs for longer periods of time and that had accumulated more IRA and 401(k) assets. The answer is typically no. The data for six saver groups are shown in table 1.4 (abstracted from PVW 1995). The key finding is that, with one partial exception, within each saver group the level of other financial assets for the 1991 sample is not noticeably lower than the level of other financial assets for the 1987 and 1984 samples. Indeed, within each saver group, the level of total financial assets for the 1991 sample exceeds the level for the 1987 sample (the total is not available for 1984 because 401(k) assets were not obtained in that year). The only apparent aberration is a decline in the median of other financial assets of 401(k)-only savers between 1984 and 1987. For this group, there was a noticeable increase in total financial assets, but little change in non-401(k) assets, between 1987 and 1991. But there was a noticeable increase in the total assets of families who made IRA and 401(k) contributions or were eligible for a 401(k) (whether or not they had an IRA). Since there is no evidence of a reduction in other assets for any of these groups, we conclude that the increase in retirement plan assets was not funded by a reduction in other financial assets.

Consider, for example, families with an IRA only (group 2a). A comparison of the 1984 and 1991 samples reveals that the median total financial assets of such families increased from \$19,068 to \$23,892. But there was little change in other financial assets, which declined from \$11,595 to \$10,717. Or consider families with an IRA who were eligible for a 401(k) (group 5a). Because

11. This is what Bernheim (1994) refers to as the "dilution effect."

Table 1.4 **Conditional Median Assets by Saver Group, 1984, 1987, and 1991 (in 1987 dollars)**

Saver Group and Asset Category	1984	1987	1991
By IRA-401(k) Saver Group			
IRA and 401(k)			
1a. Families with IRA and 401(k)			
Total financial assets	–	42,655	45,724
Other than IRA or 401(k)	**15,653**	**16,795**	**16,253**
1b. Families with neither IRA nor 401(k)			
Total financial assets	1,060	972	939
IRA Only			
2a. Families with IRA only			
Total Financial Assets	19,068	20,969	23,892
Other than IRA	**11,595**	**10,818**	**10,717**
2b. Families without IRA			
Total financial assets	1,274	1,274	1,509
Other than 401(k)	1,180	1,091	1,089
401(k) Only			
3a. Families with 401(k) only			
Total financial assets	–	8,566	9,808
Other than 401(k)	**3,723**	**2,587**	**2,498**
3b. Families without 401(k)			
Total financial assets	3,570	3,602	3,312
Other than IRA	2,472	2,339	2,145
By 401(k) Eligibility and IRA Saver Group			
All Families			
4a. Eligible for a 401(k)			
Total financial assets	–	16,763	19,608
Other than IRA or 401(k)	**6,924**	**6,796**	**7,037**
4b. Not eligible for a 401(k)			
Total financial assets	4,516	4,607	4,573
Other than IRA or 401(k)	**3,075**	**3,010**	**3,025**
Families with an IRA			
5a. Eligible for a 401(k)			
Total financial assets	–	37,882	44,432
Other than IRA or 401(k)	**16,881**	**16,032**	**17,212**
5b. Not eligible for a 401(k)			
Total financial assets	20,686	23,537	27,094
Other than IRA or 401(k)	**13,098**	**13,269**	**13,355**
Families without an IRA			
6a. Eligible for 401(k)			
Total financial assets	–	5,748	7,013
Other than IRA or 401(k)	**2,992**	**2,737**	**2,757**
6b. Not eligible for a 401(k)			
Total financial assets	1,261	1,202	1,210

Source: Poterba, Venti, and Wise (1995).

Note: The estimates are conditional on age, income, education, and marital status. The medians are evaluated at the means of these variables.

401(k) asset balances were not reported in 1984, total financial assets are not available in that year, but between 1987 and 1991, total financial assets of this group increased from $37,882 to $44,432. Yet there was no decline in other financial assets, which increased slightly from $16,881 to $17,212.

Although the key comparison here is the within-group change over time in the other financial assets of persons who participated (or were eligible for) the IRA and 401(k) programs, we also show data for families that did not participate in one or both of these programs. In each of these groups, except 5b for which where was a slight increase, there was a decline in other financial assets between 1984 and 1991. For example, the median assets of persons with neither an IRA nor a 401(k) declined from $1,060 to $939. The assets of families without an IRA and who were not eligible for a 401(k) declined from $1,261 to $1,210. Because the assets of program participants and nonparticipants are typically very different, however, we avoid between-group comparisons of these very dissimilar saver groups. Because their saving propensities are apparently so different, there seems little reason to believe that they would experience similar changes in asset balances in the absence of the saving programs. We return to this issue below.

It is sometimes suggested that these programs may affect households with limited assets but have little effect on wealthier households. We have addressed this issue by comparing the distribution of assets in 1984 and 1991. Again we rely on the fact that households in the 1991 survey had had much more time than their counterparts in earlier years to contribute to the saving programs. As shown in PVW (1994b), the higher levels of total financial assets held by IRA and 401(k) participant families in 1991 was not limited to families with large or small asset balances. Rather the effect was evident across the entire distribution of households, from those with the least to those with the greatest assets. On the other hand, across the entire distribution, there was almost no change between 1984 and 1991 in the non–IRA-401(k) assets of contributors. At all points in the distribution there was a fall over time in the assets of noncontributors.

For these estimates to control for heterogeneity, it is important that the typical person within a like saver group not change substantially over time—that is, that the unobserved difference in saving propensity $m_i - m_{i'}$ be close to zero. To help to assure that this is true, we have controlled for age, income, education, and marital status in calculating all the numbers presented in table 1.4. Nonetheless, it is possible that there were changes not accounted for by these covariates.

1.3.2 Engen, Gale, and Scholz Between-Group Comparisons

The Method

The critical feature of the PVW like group comparison is the *within*-group change in other assets as the retirement assets of a group accumulate with increasing program exposure. This is the technique used to "difference out" the group-specific saving effect. Engen, Gale, and Scholz (1994; hereafter EGS) follow a very different *between*-group approach and present an alternative comparison as evidence of substitution between retirement saving program assets and other saving. EGS combine two of the PVW like groups, and they compare the assets of the combined group to the assets of another of the PVW like saver groups. Using a difference-in-difference approach, EGS compare the change in the assets of two very different saver groups. The first group—call it the "treatment" group—is composed of all 401(k) *participants.* This is a composite group, some of whom participate only in the 401(k) program and some of whom participate in both the 401(k) and the IRA programs. Assume that this composite program effect is $\boldsymbol{cp}_i$ and that members of this group have saving commitment $\boldsymbol{m}_i$ in the most recent year and $\boldsymbol{m}_{i'}$ in the earlier year. The difference in the assets of two random samples of group i surveyed k years apart is given by

$$A_{si}(n+k) - A_{si}(n) = (\boldsymbol{m}_i - \boldsymbol{m}_{i'})f(s) + \boldsymbol{cp}_i[g(n+k) - g(n)].$$

The EGS second group is composed of IRA participants not eligible for a 401(k) and is thus exposed only to the IRA program, with an IRA program effect denoted by $\boldsymbol{b}_j$. Let $\boldsymbol{m}_j$ represent the saving commitment of the second group in the most recent period and $\boldsymbol{m}_{j'}$ the saving commitment of this group in the earlier period. Then the difference in assets of two random samples of group j surveyed k years apart is

$$A_{sj}(n+k) - A_{sj}(n) = (\boldsymbol{m}_j - \boldsymbol{m}_{j'})f(s) + \boldsymbol{b}_j[g(n+k) - g(n)].$$

The difference-in-difference estimate used by EGS is

$$(8)\quad [A_{si}(n+k) - A_{si}(n)] - [A_{sj}(n+k) - A_{sj}(n)]$$
$$= [(\boldsymbol{m}_i - \boldsymbol{m}_{i'}) - (\boldsymbol{m}_j - \boldsymbol{m}_{j'})]f(s) + (\boldsymbol{cp}_i - \boldsymbol{b}_j)[g(n+k) - g(n)].$$

In principle, this method will estimate the program effect if $\boldsymbol{m}_i = \boldsymbol{m}_{i'}$ and $\boldsymbol{m}_j = \boldsymbol{m}_{j'}$, or if $\boldsymbol{m}_i - \boldsymbol{m}_{i'} = \boldsymbol{m}_j - \boldsymbol{m}_{j'}$, but there are several confounding effects. First, the estimate is the difference $(\boldsymbol{cp}_i - \boldsymbol{b}_j)$ between the composite group (IRA and 401(k)) program effect and the IRA program effect. Second, the program effect is assumed to be the same for both groups, but it is likely that the same program will have different effects on very dissimilar saver groups, even without a within-group change in saving propensity. Third, because IRA participants are a much more select group of savers than 401(k) participants, the between-group difference in the saving commitments of the composite group and the IRA group at a point in time $(\boldsymbol{m}_i - \boldsymbol{m}_j$ or $\boldsymbol{m}_{i'} - \boldsymbol{m}_{j'})$ may be very large.[12] Thus there is reason to question whether the within-group change in saving commitments will be the same for both groups.

In the EGS case, the within-group change in saving commitment $(\boldsymbol{m}_i - \boldsymbol{m}_{i'})$

12. The IRA participation rate never exceeded 16 percent; the 401(k) participation rate has been at least 60 percent among eligible households.

of the composite group is especially large. The two subgroups of the composite group have very different saving commitments. And the subgroup proportions in the composite group change over time. There is a substantially smaller proportion of committed savers and a larger proportion of less committed savers in the most recent year. Thus $\boldsymbol{m}_{i}$ is much less than $\boldsymbol{m}_{i'}$.

Comparison of Results

Our reproduction of the central EGS results is reported in the top panel of table 1.5. EGS compare all participants in a 401(k) plan—combining PVW groups 1a and 3a in table 1.4—with IRA contributors who were not eligible for a 401(k)—PVW group 5b in table 1.4. EGS interpret the fall in the assets of 401(k) participants *compared* to the increase in the assets of the "control" group as evidence that 401(k) contributions did not lead to an increase in financial assets between 1987 and 1991.

The critical feature of our approach to controlling for heterogeneity is comparison of the within-group change in non–IRA-401(k) assets as IRA and or 401(k) assets grow, for each like saver group. Based on this reasoning, we find that the EGS comparison has two important shortcomings. First, the two groups EGS compare surely exhibited very different saving behavior before the advent of the IRA and 401(k) programs. In 1987, the non–IRA-401(k) assets of the EGS control group were almost twice as large as those of the treatment group of 401(k) participants ($11,823 vs. $6,635). From table 1.4, it can be seen that similar differences existed in 1984. Thus, in our view, the two groups should not be treated as like saver groups and the comparison *between* them is not meaningful.

Furthermore, the increase in the total financial assets of the control group is entirely due to the increase in the IRA assets of this group. There was virtually no change in the non-IRA assets of this group. This can also be seen in the PVW data for group 5b in table 1.4. The non-IRA assets of this group remained almost constant, increasing from $13,098 to $13,355 between 1984 and 1987. In arguing that IRA and 401(k) plans have no effect on personal saving, it seems awkward to use evidence that suggests a substantial effect of IRAs on saving to show that the 401(k) plan had no effect.

The second problem with the EGS comparison is also fundamental and leads to an incorrect interpretation of the fall in the assets of the composite 401(k) participant group. This group is in fact composed of two very different groups: 401(k) participants without an IRA and 401(k) participants with an IRA. The misleading interpretation created by combining these very different groups can be explained with reference to the bottom panel of table 1.5. The 1987 non–IRA-401(k) assets of the second group—401(k) participants with an IRA—are about 10 times as large as those of the first group—401(k) participants without an IRA.[13] It is clear that the past saving behavior of the two groups

13. The other financial assets of the "control" group are three or four times as large as the other assets of group 1 and only a third as large as the other assets of group 2. Thus the past saving

Table 1.5 **EGS Comparison and the Composition Fallacy (in 1991 dollars)**

Saver Group and Asset Category	1987	1991	Change	Percentage Change
EGS Comparison: All 401(k) Participants vs. "Control" Group—IRA Participants Not Eligible for a 401(k)				
1. All 401(k) participants[a]				
Total financial assets	20,630	19,300	−1,330	−6.4
Net total financial assets[b]	**17,710**	**15,999**	**−1,711**	**−9.7**
Other than IRA and 401(k)	6,635	4,747	−1,888	−28.5
Number	1,489	2,773		
Percentage of total	**100.0**	**100.0**		
2. "Control" group: IRA participants not eligible for a 401(k)				
Total financial assets	24,129	28,974	4,845	20.1
Net total financial assets	**21,052**	**26,100**	**5,048**	**24.0**
Other than IRA and 401(k)	11,823	11,000	−823	−7.0
Decomposition of EGS: All 401(k) Participant Group				
3a. 401(k) Participants **without** an IRA				
Total financial assets	8,686	10,000	1,314	15.1
Net total financial assets	**5,550**	**7,149**	**1,599**	**28.8**
Other than 401(k)	2,774	2,400	−374	−13.5
Number	780	1,744		
Percentage of total	**52.4**	**62.9**		
3b. 401(k) Participants **with** an IRA				
Total financial assets	44,638	50,275	5,637	12.6
Net total financial assets	**41,622**	**46,099**	**4,477**	**10.8**
Other than IRA or 401(k)	29,844	30,000	156	0.6
Number	709	850		
Percentage of total	**47.6**	**37.1**		

Source: Authors' tabulations from 1987 and 1991 SIPP.

[a]Group 1 is our reproduction of the EGS numbers. Although the match is not exact, it is very close and qualitative relationships are the same.

[b]Net of nonmortgage debt.

was very different. This confounds inferences made from changes in the assets of the combined group, particularly if the proportions of the two subgroups in the composite group change over time, as they do between 1987 and 1991. The proportion of the second (high saver) group declined from 47.6 percent to 37.1 percent of the total combined groups, leading to a fall in the assets of the com-

behavior of the EGS "control" group does not approximate the saving behavior of either of the component groups of the combined group.

posite group. The proportion of the low-saver group increased from 52.4 percent to 62.9 percent. Thus the non–IRA-401(k) assets of the composite group declined. In fact, the total assets of each group separately *increased.* Total financial assets of 401(k) participants without an IRA increased by $1,314, and total financial assets of 401(k) participants with an IRA increased by $5,637. In neither case was there an important change in non–IRA-401(k) assets. This is exactly the result shown by PVW for groups 3a and 1a in table 1.4. Thus the composition problem inherent in the EGS comparison creates the illusion of substitution when in fact the data do not show that.[14] The regressions run by EGS and reported in table 5 of their paper (1994) suffer from the same problem.

In all three of the groups used by EGS, there was an increase in total financial asset saving as IRA and or 401(k) assets grew. In none of the three groups was there a substantial change in non–IRA-401(k) financial assets. The increase in the financial assets of the EGS control group was due to an increase in IRA assets, which—when compared to the fall in the assets of the composite group—lead EGS to conclude that the 401(k) plan had no effect. But the fall in the assets of the composite group is an illusion created by the changing composition of the group.

1.4 The 401(k) Eligibility "Experiment"

Another approach relies on the "experiment" that is provided by the largely exogenous determination of 401(k) eligibility, *given income.* It considers whether eligibility is associated with higher levels of total saving, holding income and other demographic characteristics constant. In this case the key question is whether families who were eligible for a 401(k) in a given year had larger total financial asset balances than families who were not eligible, or, equivalently, did non-401(k) financial assets decline enough to offset the 401(k) contributions of eligible families? This approach is used in PVW (1994a, 1995).

1.4.1 The Method

Unlike the IRA program, only persons whose employers establish a 401(k) plan are eligible to contribute to such a plan. This creates a natural opportunity to compare the saving of eligible and noneligible households. In this case, we make a *between*-group comparison of assets at a point in time. The data set for each year represents a random cross section of respondents in that year. Thus

14. This composition fallacy is a classic error in empirical analysis made clear by Bickel, Hammel, and O'Connell (1975) in their analysis of graduate student admissions at the University of California, Berkeley. While grouping departments made it seem as though there was discrimination against women, looking at individual departments made it clear that in no single department was there discrimination. It was just that women were applying to departments where the admission rate was low and men to departments where the admission rate was high.

the random samples in different years have essentially the same demographic characteristics. But samples drawn in later years have had longer exposure to retirement saving plans. This means that s—the number of years of saving in any form—remains the same even though retirement program exposure is greater for more recent samples.

Suppose that the saving commitment of the typical eligible household in the most recent period is represented by $\boldsymbol{m}_i$ and the commitment of noneligible households by $\boldsymbol{m}_j$. In an earlier period, the commitment of the typical eligible household is represented by $\boldsymbol{m}_{i'}$ and the commitment of noneligible households by $\boldsymbol{m}_{j'}$. After exposure to the program for n and $n+k$ years, respectively, the assets of eligible households are given by

$$A_{si}(n) = h(s) + \boldsymbol{m}_{i'}f(s) + \boldsymbol{p}_i g(n),$$

$$A_{si}(n+k) = h(s) + \boldsymbol{m}_i f(s) + \boldsymbol{p}_i g(n+k),$$

and the assets of noneligible households are given by

$$A_{sj}(n) = h(s) + \boldsymbol{m}_{j'}f(s),$$

$$A_{sj}(n+k) = h(s) + \boldsymbol{m}_j f(s).$$

To determine the program effect, $\boldsymbol{p}_i$, we consider the between-group difference in the assets of eligible and noneligible households at a *point in time,* which after $n+k$ years of exposure is given by

$$(9) \qquad A_{si}(n+k) - A_{sj}(n+k) = (\boldsymbol{m}_i - \boldsymbol{m}_j)f(s) + \boldsymbol{p}_i g(n+k).$$

If $\boldsymbol{m}_i = \boldsymbol{m}_j$, the difference represents the program effect. Thus a critical question is whether the saving propensities of the two groups are in fact equal. At the outset of the program, when $n = 0$, the assets of the two groups will differ only if $\boldsymbol{m}_{i'}$ and $\boldsymbol{m}_{j'}$ differ, with

$$A_{si}(0) - A_{sj}(0) = (\boldsymbol{m}_{i'} - \boldsymbol{m}_{j'})f(s).$$

Thus, if the two groups have equal assets at the outset of the program, the implication is that the saving commitments of the two groups are equal, and vice versa. We use this test to establish approximate equality of taste for saving near the outset of the program.

The estimate presented in equation (9), however, depends not on the equality of saving commitments at the outset of the program but on equality of saving commitments at a later point in time (1991 in our case). What would assure that this equality is maintained? Suppose that the two groups are composed of equally committed savers at the outset of the program, with $\boldsymbol{m}_{i'} = \boldsymbol{m}_{j'}$. Over time, more households became eligible for a 401(k). As long as newly eligible households are a representative sample of the former noneligible households, the two groups will continue to be composed of equally committed savers and the difference in assets at a point in time will represent the program effect.

Table 1.6 **Conditional Median Asset Balances by 401(k) Eligibility: Families with Income $40,000 to $50,000**

Asset Category and Eligibility Status	Results for 1991 (1991 $)	Results for 1984 (1984 $)
Total financial assets		
Eligible for a 401(k)	14,470*	–
Not eligible for a 401(k)	6,206	–
Non–IRA-401(k) assets		
Eligible for a 401(k)	4,724	5,027
Not eligible for a 401(k)	4,250	5,082

Source: Poterba, Venti, and Wise (1995).

Note: These are medians controlling for age, marital status, and education.

*Difference between eligibles and noneligibles is statistically significant at the 95 percent confidence level.

1.4.2 The Results

Inferences about the net saving effect of 401(k) contributions depend on the similarity of the saving behavior of families who are and are not eligible for a 401(k), m_i versus m_j, controlling for income. It is important, for example, that the eligible group not be composed disproportionately of savers. The data show little evidence of this type of difference in saving behavior. The most compelling evidence is for 1984. In that year eligibles and noneligibles had about the same level of other financial assets, controlling for income. Thus these data suggest that near the outset of the 401(k) program families that were newly eligible for a 401(k) exhibited about the same previous saving behavior as families that did not become eligible—m_i and m_j were about the same.

Data for families with incomes between $40,000 and $50,000, presented in table 1.6, illustrate the findings. In 1984, newly eligible and noneligible 401(k) families had almost identical non–401(k)-IRA assets—$5,027 and $5,082, respectively. By 1991, however, the median of total financial assets of eligible families was $14,470, compared to $6,206 for noneligible families. But in 1991, the *non–IRA-401(k) assets* of the two groups were still about the same, $4,724 for eligible and $4,250 for the noneligible group.[15] If families reduced saving in other forms when they became eligible for a 401(k) plan, the typical eligible family in 1991 would have accumulated less wealth in other financial assets than the typical noneligible family. This was not the case.

Similar comparisons are reported in appendix table 1C.1 for all income groups. In 1984, the ratio of median non–IRA-401(k) assets of eligibles to noneligibles, weighted by the number of observations within income intervals,

15. The apparent reduction in the non-401(k)-IRA assets of both groups between 1984 and 1991 is due largely to earnings growth. The income intervals are not indexed, and thus families in a given interval in 1984 will tend to have greater wealth than families in that same interval in 1991. Comparable calculations with the intervals indexed to 1987 dollars are discussed in section 1.6.2 and reported in tables 1.10 and 1.11 and in appendix tables 1C.3 and 1C.4.

was *exactly one.* The ratio of means was 0.87, indicating that the mean of non–IRA-401(k) assets of the eligible group was *lower* than the mean of noneligibles. By 1987 the ratio of total financial assets of eligible to noneligible families was 1.62, and by 1991 this ratio was 2.22. This evidence suggests a sizable effect of 401(k) saving on the accumulation of financial assets and shows little if any substitution of 401(k) contributions for other financial asset saving.

Indeed, for all income groups, eligible households have greater total financial assets than noneligible households at virtually all points across the entire distribution of financial assets. But there is virtually no difference across the entire distribution of the other financial assets of eligible and noneligible households, as shown in PVW (1995).

For comparisons between eligible and noneligible households to shed light on the net saving effect of 401(k) plans, it is important that the saving behavior of the eligible and noneligible groups be comparable. As noted above, after controlling for income, the accumulated assets of the two groups were very close at the outset of the program. Nonetheless, there could have been some change in the composition of the two groups over time, even if eligibles and noneligibles were very similar in 1984. Data on measured household attributes, however, suggest that there was little composition change.

Many studies of saving behavior have shown that saving commitment is related to household demographic attributes, such as age and education. As appendix table 1C.5 shows, these characteristics did not change substantially over time. The average age of the head of eligible households was 41.8 in 1984 and 41.4 in 1991. The average years of education of the head of eligible households was 13.6 in 1984 and 13.7 in 1991. Within income interval, there was also very little change in the average age or education of eligible families. Similarly, there was little change in the age or education of noneligible households. The proportion of households with husband and wife present, which is typically found to be positively related to saving behavior, declined by 7 percentage points, on average, for both eligible and noneligible households. Much of saving commitment, however, cannot be explained by observed household attributes, and we rely on the cohort approach discussed below to provide a check on the eligibility experiment results. The cohort analysis is not confounded by the potential difference between the saving commitment of eligible and noneligible households.

EGS question the validity of our comparisons between 401(k) eligible and noneligible households. They argue that "401(k) eligible families save more in non-401(k) assets than observationally equivalent noneligible families, even after controlling for other factors." In our view, however, their numbers differ little from ours. They say, for example, that the two groups had different asset levels in 1984. But they estimate a (statistically insignificant) difference in *median* financial assets of only $173. They estimate a difference in median net financial assets of only $346. Given that our analysis controls for demographic characteristics and compares households within income intervals, while the EGS approach simply includes income as a single variable in a regression equation, their estimates seem hardly different from our findings.

EGS find a difference of $2,500 in 1984 in the net worth of eligible and noneligible households. The median for the entire sample is about $30,000 so the estimated difference represents a percentage difference of under 9 percent. In section 1.6 below, we repeat our analysis including housing equity, and controlling for income interval as above. We find essentially no difference in the housing equity of eligible and noneligible families in 1984.

EGS also find that eligible families are more likely than noneligible families to have a traditional defined benefit employer-provided pension plan. Whether this reflects a difference in saving propensity is questionable. That depends first on whether people choose jobs based on the pension plan. And, if they do, it is not at all clear that wanting a good pension plan means a stronger preference for saving. It could mean just the opposite. It may well be that choosing a job where the employer saves for you is a means of self-control. If a person is unlikely to save and would not do so were it not for the employer-guaranteed retirement income, then the people who choose jobs with pensions may be nonsavers, not savers.

Assuming further that persons with defined benefit pensions save less in other forms, Engen and Gale (1995) seem to argue that they should have lower financial assets in 1991 than noneligibles, if eligibility is *independent* of the taste for saving. In this case, according to their reasoning, a finding that eligible families have about the same, or even *more,* assets as noneligible households in 1991 confirms that they have a stronger commitment to save than noneligibles. Engen and Gale (1995) conclude that even a finding that eligible families save the same amount in non-401(k) assets as noneligible families can be interpreted as evidence that 401(k) eligibility is not exogenous, since eligibles should be saving less, if eligibility is exogenous with respect to saving. This reasoning seems to us self-fulfilling, assuming substitution to demonstrate substitution. It is a remarkable change from all earlier studies of IRAs, in which the central hypothesis was that IRA participants should have *less* non-IRA financial wealth than nonparticipants, if IRAs and other financial assets are substitutable. Here, the possibility that 401(k) eligibles may have *more* non-401(k) assets than noneligibles is used as evidence for substitution.

The weight of the evidence, however, is that persons with pensions do not reduce saving much, if at all, relative to persons without pensions. Gale (1995) argues that the methods that have been used by others to address this question are plagued by a series of biases that lead to an underestimate of the reduction in other saving for persons with employer-provided pensions. The key method that he proposes to avoid bias in his empirical analysis is a derived adjustment factor that multiplies pension wealth in regression equations relating other saving to pension wealth.

While Gale (1995) has pointed to a number of possible difficulties with prior estimates, his methodology, particularly his derived adjustment factor, raises new questions of interpretation. He derives the adjustment factor in a stylized life cycle model that assumes that a household's consumption and saving, even before retirement, are proportional to the present discounted value of lifetime wage and pension income. There are three difficulties with this approach. First, the model assumes that persons view pension wealth and other financial assets as perfect substitutes, that there is a "complete offset between pensions and other wealth" (Gale 1995, 16–17). Because the adjustment factor used in the empirical analysis is derived assuming complete substitution, the analysis cannot provide an unambiguous test of the extent of substitution. Much of the empirical research on saving, including the analysis of tax-deferred saving summarized here, suggests that the assumption is inappropriate. More conceptual "behavioral" explanations of saving behavior, in particular the work that emphasizes the "mental accounts" approach to saving, also bring into question this assumption. Second, the approach abstracts from the likely possibility that many households face liquidity constraints that make it difficult to consume out of pension wealth before retirement. Third, only a small fraction of persons who take a job with a pension early in the life cycle will retire from that job and acquire the rights to the defined pension benefit at age 65. Kotlikoff and Wise (1985, 1988, 1989) explain that a person who leaves a firm at age 40, for example, will have accrued only a small fraction of the age 65 pension. Such considerations imply that Gale's (1995) findings are subject to new biases that cloud interpretation of his results.

1.4.3 401(k) Eligibility and Other Pension Plans

Our analysis has focused on the substitution between 401(k) assets and other financial assets. Another potential trade-off is employer substitution of 401(k) plans for other employer-provided pension plans. The possibility of such substitution arises more directly with 401(k) plans than with IRAs because 401(k) plans are part of the workplace benefits package and their availability, like the availability of defined benefit or defined contribution pension plans, is subject to employer choice.

Although substitution between traditional pensions and 401(k) plans is a theoretical possibility, existing empirical evidence provides little support for such substitution in practice. As discussed above, EGS present evidence that workers who are eligible for 401(k) plans are *more* likely to be covered by a defined benefit pension plan than are workers without 401(k) eligibility. Although they interpret this as evidence of saver heterogeneity, it is prima facie evidence against the pension substitution hypothesis. Papke (1995) uses data from 1985 and 1991 IRS Form 5500 filings, and Papke, Petersen, and Poterba (1996) use data from a survey of 401(k) providers, to provide further evidence on this question. There is essentially no evidence that large firms offering 401(k) plans substituted these plans for other pension plans; the first 401(k)s were typically offered as "add-on" plans in large firms with preexisting defined benefit pension plans. Papke (1995) finds some evidence of substitution at smaller firms that have introduced 401(k) plans in recent years.

1.5 Cohorts and the Effects of Retirement Saving Programs

This method compares the assets of persons who are the same except that they reached a given age in different calendar years. Hence some cohorts had longer than others to contribute to special saving programs. For example, families that reached age 65 in 1984 had had only two years to contribute to an IRA or to a 401(k) plan, but families who attained age 65 in 1991 had had nine years to contribute. If these programs affect personal saving, they should lead to differences in asset accumulation by cohort.

1.5.1 The Cohort Method

Consider a random sample of *all* families, and assume for the moment that the typical family i has saving commitment $\boldsymbol{m}_i$. Cohorts are distinguished by age (c) in 1984. We assume that cohort c has had s years to save. Suppose that in 1984 cohort c has had s years to save and during n of these s years was able to contribute to special retirement saving programs. We follow each cohort from 1984 to 1987 to 1991. For simplicity, we can assume that the cohort c *began* the period having saved for s years and ended the period having saved for $s+k$ years. By 1991, each cohort has had $n+k$ years to contribute to special retirement saving programs. (In 1984, n is about 2; in 1991, $n+k$ is about 9.) For cohort c, assets after n and $n+k$ years of exposure are given by

$$A_{ci}(n) = h(s) + \boldsymbol{m}_i f(s) + \boldsymbol{p}_i g(n),$$

$$A_{ci}(n+k) = h(s+k) + \boldsymbol{m}_i f(s+k) + \boldsymbol{p}_i g(n+k).$$

Consider now an older cohort ($c+k$) that *began* the period having saved for $s+k$ years. For this cohort, assets after n and $n+k$ years of exposure are given by

$$A_{c+k,i}(n) = h(s+k) + \boldsymbol{m}_i f(s+k) + \boldsymbol{p}_i g(n),$$

$$A_{c+k,i}(n+k) = h(s+k+k) + \boldsymbol{m}_i f(s+k+k) + \boldsymbol{p}_i g(n+k).$$

This second cohort is k years older than the first cohort in the same calendar year. Thus the difference in the assets of the two cohorts when both have saved for $s+k$ years is given by

$$(10)\quad A_{ci}(n+k) - A_{c+k,i}(n)$$
$$= (\boldsymbol{m}_i - \boldsymbol{m}_i) f(s+k) + \boldsymbol{p}_i[g(n+k) - g(n)] = \boldsymbol{p}_i[g(n+k) - g(n)].$$

Thus the difference in the assets of families who reached the same age in calendar years n and $n+k$ is due to the program effect.

Table 1.7 Summary of Cohort Effects at Ages 60–64 (in 1991 dollars)

Asset	1984[a]	1991
Contributors and Noncontributors Combined		
Mean		
Personal retirement assets	5,118	14,156
Other personal financial assets	**37,132**	**36,263**
Total personal financial assets	42,250	50,419
Contributors		
Percentage of cohort	38	42
Median		
Personal retirement assets	8,171	22,148
Other personal financial assets	**22,983**	**21,528**
Total personal financial assets	34,975	50,182
Noncontributors		
Percentage of cohort	62	58
Median		
Total personal financial assets	2,687	2,134

Source: From Venti and Wise (1997), converted to 1991 dollars.

[a]The means and medians reported in this table are controlling for age, income, marital status, and education. The 1984 totals exclude 401(k) assets, which were small at that time. Thus the data for personal retirement and for total personal financial assets are affected to some degree by this omission. But the data on other personal financial assets are unaffected.

If the different cohorts had different saving commitments, however, the term $m_i - m_i$ in equation (10) would not be zero, and the difference would reflect this, as well as the program effect. Judgments about the likely importance of such differences may be based on several features of the analysis. Cohort effects are obtained for a succession of cohorts ranging in age from 42 to 70 in 1984. These cohort effects are obtained for several asset categories: special retirement saving program assets, total financial assets, and conventional financial assets. Differences in the cohort effects for different assets can be used to judge whether there was a systematic change in taste for saving over time. It is also possible to compare cohort effects for participants and nonparticipants in retirement saving programs.

1.5.2 The Results

The cohort method was used by Venti and Wise (1997). They find that households who attained a given age in 1991 had consistently larger total real financial assets than households who reached that age in 1984. The larger assets of the younger cohorts is accounted for almost entirely by more assets in IRA and 401(k) plans. There is on average no difference between the other financial assets of the older and younger cohorts. The results can by illustrated by comparing the assets of families who reached ages 60–64 in 1984 with the assets of families that attained those ages in 1991, as shown in table 1.7.

To control for heterogeneity, the data for all families—both contributors and noncontributors combined—are the most compelling. In this case it is the typical saving propensity m_i over all families that is important, and the possible effect of the changing composition of participant and nonparticipant families is avoided. (Because fewer than half of all families participate in these programs, the median of program assets for all families is zero and thus not informative.) The *mean* of total financial assets of all families that attained ages 60–64 in 1984 was $42,250; the mean of those who attained this age in 1991 was $50,419 (both values are in 1991 dollars and control for income, age, education, and marital status). The increase was accounted for almost entirely by personal retirement saving—$5,118 for the cohort that attained ages 60–64 in 1984 compared to $14,156 for the cohort that attained this age range in 1991. There was essentially no cohort difference in other financial assets ($37,132 for the older cohort and $36,263 for the younger cohort). Thus there is little evidence of substitution of personal retirement saving for other financial assets.

The data for families who participated in personal retirement saving plans provide a better measure of the potential of the plans to augment the financial assets of retirees. The *median* level of total personal financial assets of contributor families that attained ages 60–64 in 1984 was $34,975, compared with $50,182 for families who attained that age range in 1991. The median level of personal retirement plan assets of the families that reached this age range in 1984 was $8,171, compared with $22,148 for families who reached ages 60–64 in 1991. On the other hand, the other financial assets of these families were about the same in 1984 and 1991 ($22,983 and $21,528, respectively). Although not as compelling as the data for both groups combined, these data also provide little evidence of substitution. In contrast, the financial assets of families that attained ages 60–64 in 1991 and did not participate in personal retirement plans were somewhat lower than the assets of similar families who reached this age range in 1984.

The results for other age groups are summarized in figure 1.3. To understand the figure, consider age 66: The cohort that reached this age in 1984 had about $5,000 less in personal retirement assets *(heavy lines)* than the next younger cohort that reached that age about four years later. The difference in the total financial assets *(light lines)* of these two cohorts is also about $5,000. But there is very little difference in the other financial assets of these two cohorts.

Results of more formal estimation of cohort effects are shown in table 1.8. The estimates are obtained by fitting a cubic function in age to the cohort means, allowing for cohort shifts—the cohort effects—in the relationship between age and assets.[16] The estimate of the youngest cohort effect for personal

16. We fit the actual cohort means with a specification of the form

$$A_{ic} = \alpha + \beta_c + \gamma_1(\mathrm{Age}_i) + \gamma_2(\mathrm{Age}_i)^2 + \gamma_3(\mathrm{Age}_i)^3 + \varepsilon_{ic}, \tag{11}$$

where A represents an asset category—personal retirement assets, other personal financial assets, total personal financial assets—c indexes cohort, and i denotes the ith cohort mean. The β_c are

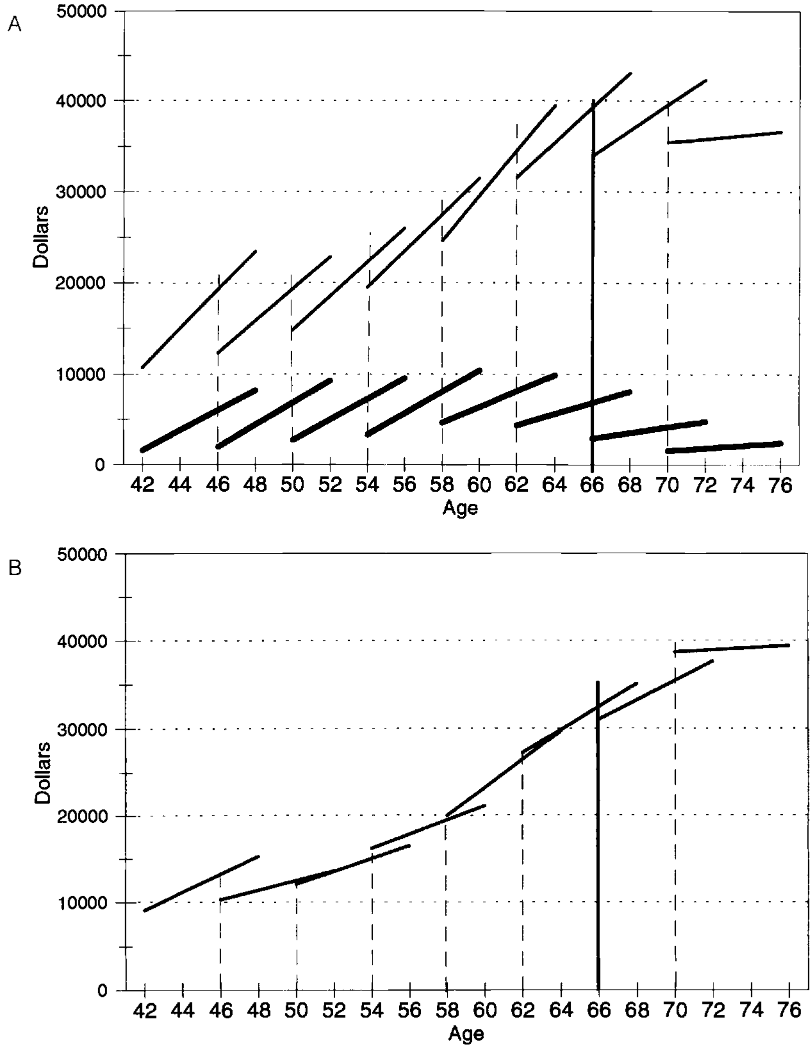

Fig. 1.3 Mean assets: total and retirement (*A*) and other (*B*), for contributors and noncontributors combined—indexed

Note: In panel *A*, light lines graph total assets, and heavy lines graph retirement assets.

cohort effects with $\Sigma\beta_c = 0$. Thus the individual estimates represent deviations from the mean effect, which is set to zero. The specification is intended to fit the age-asset accumulation pattern, allowing the differences in the levels of the assets between successive cohorts to be maintained and to cumulate as the cohorts age. It is assumed, e.g., that the estimated difference between the assets of the two youngest cohorts, C42 and C46, will be maintained as the cohorts age. It is likely that this assumption implies a conservative estimate of the projected cohort differences. Constant percentage differences as the cohorts age, e.g., imply much larger absolute differences at advanced ages than this model does.

Table 1.8 **Estimated Cohort Effects for Means by Asset: Both Contributors and Noncontributors (in 1984 dollars)**

	Personal Retirement Assets		Total Personal Financial Assets		Other Personal Financial Assets	
Cohort	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
C42	14,076	19.0	16,002	8.2	1,927	1.0
C44	11,085	17.9	12,024	7.3	939	0.6
C46	9,997	17.3	9,568	6.3	−428	−0.3
C48	7,821	14.8	6,556	4.7	−1,264	−0.9
C50	5,759	11.9	4,132	3.2	−1,626	−1.3
C52	3,814	8.6	1,459	1.2	−2,354	−2.1
C54	1,944	4.7	452	0.4	−1,492	−1.4
C56	363	0.9	734	0.7	370	0.4
C58	−1,604	−3.9	−1,682	−1.6	−78	−0.1
C60	−3,815	−8.7	−5,165	−4.5	−1,349	−1.2
C62	−5,813	−12.1	−3,796	−3.0	2,017	1.7
C64	−8,130	−15.4	−5,234	−3.7	2,895	2.2
C66	−10,345	−18.0	−8,766	−5.8	1,578	1.1
C68	−12,049	−19.2	−12,203	−7.3	−154	−0.1
C70	−13,103	−17.8	−14,081	−6.1	−981	−0.4

Source: Venti and Wise (1997).

retirement assets is $14,076 *above* the mean while the estimate for the oldest cohort is $13,103 *below* the mean, a difference of $27,179. If there were no counterbalancing cohort effects with respect to other personal financial assets, the total personal financial asset cohort effects should approximately parallel the retirement asset cohort effects. The estimates show that the total personal financial asset cohort effect for the youngest cohort is $16,002 *above* the mean and the cohort effect for the oldest cohort is $14,081 *below* the mean, a difference of $30,083. The other personal financial asset cohort effects are typically small and not statistically different from zero. An F-test does not reject the hypothesis that all the cohort effects with respect to other personal financial assets are zero.

The analysis suggests that if current patterns persist families who reach retirement age 25 or 30 years from now will have much more in financial assets than families currently attaining retirement age, and the difference will be due solely to assets in personal retirement accounts.

We believe that the cohort approach provides the surest way of controlling for heterogeneity. When both contributors and noncontributors are considered jointly, the overall saving effects are not contaminated by potential changes in composition of the two groups. Nor are the cohort estimates confounded by the "coincidence" possibility that may affect the difference-in-difference estimates discussed in section 1.2. In principle, the cohort analysis compares families who differ only in the calendar year in which they reached a given age and therefore in their exposure to retirement saving programs.

A potential, although we believe unlikely, confounding influence would be an overall change in saving behavior, with each successively younger cohort wanting to save more than its older cohorts. The evidence suggests that such a systematic increasing taste for saving must have been realized only in contributions to the special retirement saving programs. We find this an unlikely possibility for two reasons. There are no cohort effects in other financial assets, as we would expect if there were an underlying change in taste for saving. Nor are there cohort effects for nonparticipants, as we would also expect if there were an overall change in the taste for saving. Therefore, we interpret the cohort results as supporting the results of the other methods of correcting for heterogeneity.

1.5.3 Further Results

Registered Retirement Saving Plans (RRSPs) were first introduced in Canada in 1957. As with the IRA in the United States, an individual can make contributions to an RRSP and deduct the contributions from income for tax purposes. Interest accrues tax free until withdrawal, when taxes are paid. The contribution limits were increased substantially in the early 1970s, and RRSPs were widely promoted. Since then, they have become a very prominent form of saving. Annual contributions grew from $225 million in 1970 to almost $3.7 billion in 1980 to $16 billion by 1992, when they accounted for about one-third of aggregate personal saving. In 1992 about 33 percent of families contributed, with an average contribution of $4,180. Now RRSP contributions exceed the total of employee and employer contributions to employer-provided pension plans.

Based largely on "cohort" analysis like the procedure described above, Venti and Wise (1995b) conclude that RRSPs have contributed substantially to personal saving in Canada. In virtually no case do the data suggest substitution of RRSP saving for other forms of retirement saving. In the two decades prior to the growth in RRSP popularity, the personal saving rate in Canada was typically below the U.S. personal saving rate. Since that time, the personal saving rate in Canada has become much higher than in the United States. Although it is difficult to make judgments about the RRSP saving effect based only on the trends in U.S. and Canadian aggregate saving rates, the cohort analysis suggests that a large fraction of the current difference can be accounted for by RRSP saving. Engelhardt (1996b) analyzes the similarly tax-advantaged Registered Home Ownership Saving Program (RHOSP), designed to encourage saving for home purchase. He finds that the RHOSP program also increased total personal saving.

1.6 Other Margins of Substitution: Home Equity

The foregoing discussion focuses on the substitution between contributions to special retirement saving plans and other financial assets. There are at least two other potential margins of substitution: employer-provided pension assets and home equity. As mentioned above, many analysts have considered the substitution between employer-provided pension assets and personal financial assets. The results are mixed, but the weight of the findings suggests little substitution.[17] Venti and Wise (1997) have also addressed this question, considering the assets of retired persons for whom pension assets are known, and using social security benefit percentiles to control for lifetime income. They conclude that there is essentially no relationship between employer-provided pension assets and either personal retirement saving plan assets or other financial assets. They considered the same question for retired persons in Canada, where the RRSP program has been widely used for several decades, again finding essentially no relationship between employer-provided pension assets and personal financial assets. We will not address that question further here.

We will, however, consider the potential substitution between housing equity and retirement saving plan assets. Our focus on the relationship between retirement saving assets and other financial assets neglects the possible interaction between these retirement plan assets and home equity, which is the largest asset of a large fraction of households. While many of the factors that are likely to determine whether to purchase a home, the value of the home, and how to finance a home purchase may be unrelated to the accumulation of retirement saving assets, it is possible that some of the buildup in these accounts has been financed through reduced accumulation of housing equity.

Several studies have considered the relationship between housing *prices* and financial assets. In his review article, Skinner (1994) finds little relationship between exogenous shocks to housing value and personal financial assets. Several other studies are based on the Panel Survey of Income Dynamics: Skinner (1996) finds a small relationship for younger households and no relationship for older households. Hoynes and McFadden (1994) find little relationship between exogenous changes in home values and changes in financial assets. Engelhardt (1996a) finds no decrease in financial asset saving among households with an increase in home values but finds a small increase among households with falling home values. Engen and Gale (1995) have considered the relationship between home equity and 401(k) assets based on SIPP data. While their results largely confirm our findings on the relationship between 401(k) and other financial assets, they conclude that the increase in the financial asset saving of 401(k) participants (or eligibles) between 1987 and 1991 was offset by a reduction in home equity.

We consider the relationship between retirement saving plan contributions and home equity using cohort analysis as in section 1.5 and comparison of 401(k) eligible and noneligible families as in section 1.4. The most important conclusion from the cohort analysis is that the timing of changes in mortgage debt and net home equity is inconsistent with a causal relationship between

17. Gale (1995) has summarized the results in table 1 of his paper.

personal retirement plan contributions and mortgage debt. With respect to 401(k) contributions in particular, we conclude from the eligibility comparison that there was no apparent offset to 401(k) contributions through a reduction in home equity. We consider briefly one possible reason for the difference between our results and those reported by Engen and Gale (1995).

1.6.1 Cohort Analysis

Cohort data make it easy to compare the trends in personal retirement saving and housing assets. As in the analysis above, we consider IRA and 401(k) participants and nonparticipants together.[18] The interpretation of financial asset versus housing equity trends must be tempered by at least two factors. First, market trends in housing values and financing practices that are unlikely to be induced by IRA and 401(k) contributions can have substantial effects on housing equity. There was probably little relationship between retirement saving plan contributions and the concerns that led to elimination of tax deductibility of nonmortgage interest as part of the Tax Reform Act of 1986. But this provision may have had a substantial effect on home mortgage debt.[19] Thus the home equity data may be subject to very important time effects.

Second, unlike other consumer debt, mortgage debt may, in the long run, increase future saving. Many financial planners tout mortgage debt repayment as the surest way for a household to commit to a long-term saving strategy. Regularly scheduled mortgage payments can thus be viewed as means of self-control as stressed by Thaler and Shefrin (1981), Shefrin and Thaler (1988), and Thaler (1990). While increased mortgage debt may appear as a reduction in wealth today, it may assure greater rather than reduced wealth at retirement. Similarly, a home equity loan that is repaid before retirement may not affect wealth at retirement.

The central results of the cohort analysis are presented in figure 1.4, which shows the relationships between contributions to personal retirement saving plans and housing market data. Like the results presented in section 1.5, the analysis here is based on 1984, 1987, and 1991 data for 15 cohorts: the youngest was age 42 and the oldest was age 70 in 1984.[20] The cohort data on housing value, mortgage debt, and home equity are shown in appendix tables 1C.2A through 1C.2C. Data for selected cohorts are graphed in figures 1.4*A* through 1.4*D*. Figure 1.4*A* shows data for mean personal retirement assets (including IRA, 401(k), and Keogh saving balances). Figure 1.4*B* pertains to home value, figure 1.4*C* to mortgage debt, and figure 1.4*D* to net home equity.

18. To maintain comparability with the cohort analysis discussed above, we have converted current dollar amounts to 1991 values using the Bureau of Labor Statistics earnings index. Using a price index instead has little effect on the trends.

19. Skinner and Feenberg (1990) find that each dollar of reduced consumer debt following the Tax Reform Act of 1986 was offset by a 67 cent increase in mortgage debt.

20. In principle, we would like to consider younger cohorts as well, but we wanted these data to be comparable to our earlier analysis of financial asset data that was directed to families approaching and entering retirement.

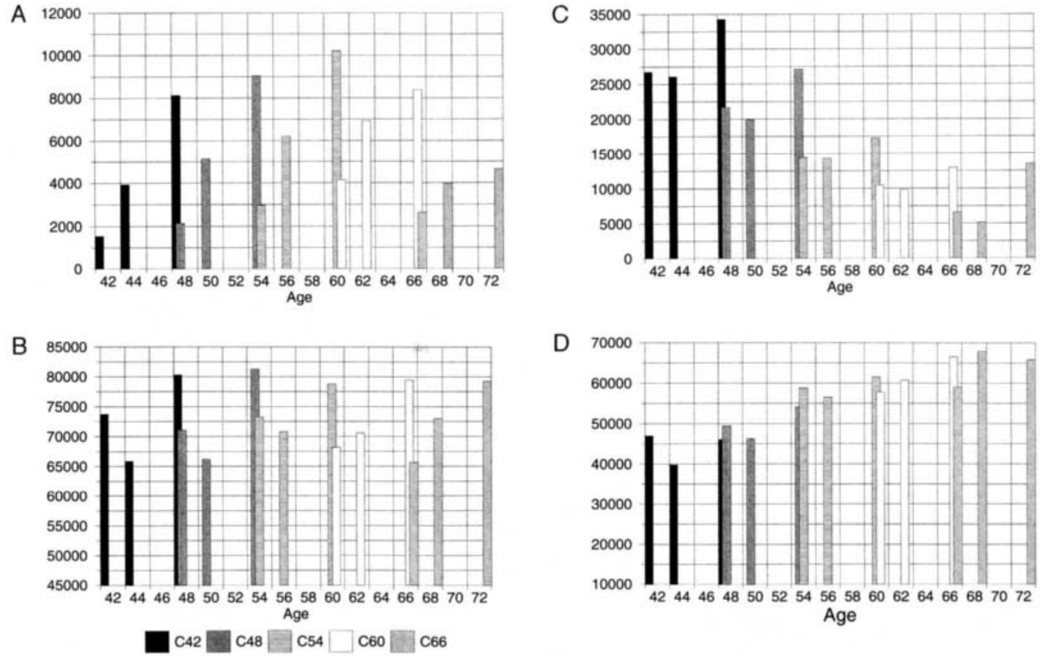

Fig. 1.4 Contributions to personal retirement saving plans and housing market data

Note: (*A*) Retirement assets summary. (*B*) Home value summary. (*C*) home mortgage summary. (*D*) Home equity summary.

All values are in 1991 dollars. The figures can be explained with reference to figure 1.4*A*. For each of the cohorts, mean retirement assets are shown for 1984, 1987, and 1991. For example, cohort C42 was age 42 in 1984, 44 in 1987,[21] and 48 in 1991. By 1991, this cohort had had nine years to contribute to the retirement saving program and had mean assets of $8,000 in these accounts at age 48. In contrast, cohort C48 had had only about two years to contribute to such accounts when first observed in 1984 and had only about $2,000 in these retirement assets at age 48. Similar comparisons can be made at ages 54, 60, and 66. The cohort that attained the given age later had much larger amounts in these retirement assets at that age. Figures 1.4*B* through 1.4*D* present housing data for the same cohorts, and the trends can be compared to the cohort trends for retirement assets.

Figure 1.4*B* shows a substantial fall in real home value between 1984 and 1987 for younger cohorts but an increase for older cohorts. For all cohorts, but especially for the younger cohorts, there was a large increase in home values between 1987 and 1991. Given that housing values were falling during the rapid rise in retirement saving plan assets—and only rising later on—these trends apparently reflect housing market effects that are unrelated to 401(k) and IRA contributions. It is clear, however, that at ages where direct comparisons can be made, the home values of younger cohorts are much greater than those of older cohorts. For example, the cohort that reached age 48 in 1991 had a real mean home value of about $80,000. The cohort that attained age 48 in 1984 had a mean home value at that age of about $72,000, in 1991 dollars.

Figure 1.4*C* shows a *fall* in mortgage debt between 1984 and 1987 for all cohorts. This pattern persists even for older cohorts that experienced an increase in home values between 1984 and 1987. Yet over this period there was a sharp *increase* in the IRA and 401(k) assets of these cohorts, as shown in figure 1.4*A*. Between the early 1980s and 1986 contributions to these programs grew from about $3 billion to almost $74 billion. Contributions to 401(k) plans almost doubled between 1984 and 1986. Yet it is clear that over this period when contributions to special retirement saving plans were growing dramatically there was no countervailing increase in home mortgage debt.

There was an enormous increase in home mortgage debt between 1987 and 1991 for all cohorts. Although assets in personal retirement saving plans continued to grow over this period, the increase was not as rapid as over the earlier period, when mortgage debt was declining. Indeed, new contributions to special retirement saving programs *declined* between 1986 and 1991. Because of the 1986 cutback in the IRA program, contributions to that program fell from almost $40 billion in 1986 to less than $10 billion by 1991. Contributions to all special retirement programs decreased from about $74 billion in 1986 to about $68 billion in 1991, a decline of about 9 percent. Thus when contribu-

21. The 1984 survey was administered between September and December 1984, and the 1987 survey between January and April of 1987, a difference of approximately 28 months.

tions to these programs were growing dramatically there was a fall in mortgage debt, and when contributions to the retirement saving programs were declining there was a dramatic increase in mortgage debt. This pattern does not appear to be consistent with substitution of IRA and 401(k) assets for housing equity.

The cohort data confirm that changes in mortgage debt, as well as changes in home value, were not induced by contributions to retirement saving plans. It seems likely that the increase in mortgage debt for all cohorts after 1987 was prompted by the provisions of the Tax Reform Act of 1986 that eliminated the tax deductibility of nonmortgage debt. We consider, though, whether there was a difference in the behavior of younger and older cohorts over this period.

Figure 1.4*D* summarizes the cohort data for home equity, which of course is the difference between housing value and mortgage debt. There is a change in the cohort relationships, starting with the cohort that attained age 54 in 1984. The youngest cohorts have lower home equity than successively older cohorts up to the age 54 cohort. For example, the younger cohorts that reached ages 48 and 54 in 1991 had lower mean values of home equity than the older cohorts that attained those ages in 1984. But for older cohorts, the reverse is true, younger cohorts have greater housing equity than successively older cohorts—at ages 60 and 66, for example. The cohort effects in home equity are very dissimilar from the cohort effects readily apparent in retirement saving assets and thus we judge were not prompted by contributions to special retirement saving programs.

The time effects in home value and mortgage debt complicate the identification of cohort effects (this issue is discussed further in appendix A). Nonetheless, to provide some indication of the housing equity of successively older cohorts, we have estimated cohort effects (as above) by fitting the cohort means with a function cubic in age. The results are shown in figure 1.5. The first series shows estimated home value cohort effects, the second series shows mortgage debt effects, and the third series shows home equity effects. The home value effects range from +25,667 for the youngest cohort to −36,407 for the oldest cohort, a difference of 62,074. The mean home value of each successively older cohort is lower than the mean for the immediately younger cohort. Interpreted literally, if there were no changes in the housing market, these data would suggest that when the current youngest cohort attains the age of the oldest cohort, the mean home value of the current youngest cohort will be $62,074 more than the mean of the oldest cohort.

The home mortgage cohort effects show a similar pattern, ranging from a high of +26,180 to a low of −20,951. Again, interpreted literally, these estimates would suggest that when the youngest cohort attains the age of the oldest cohort, that (future old) cohort will have $47,131 more in mortgage debt than the current old cohort. But here it becomes clear that the projections are likely to be exaggerated. Mortgage debt is likely to be paid down. If it were completely paid off by age 72, say, then the current young cohort would be wealthier than the current old cohort—by an amount given by the difference in their

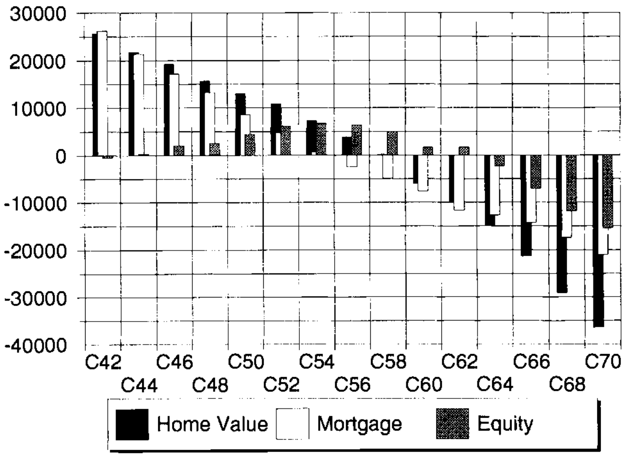

Fig. 1.5 Estimated cohort effects in home value, mortgage, and equity

home value cohort effects ($62,074). The key question, which is not addressed by these data, is how much the current mortgage debt of the younger cohorts will be paid down.

The home equity cohort effects mirror the pattern shown in figure 1.4*D*. The estimated effect for the youngest cohort is −513, while for the oldest cohort the effect is −15,456. If the home mortgage were not reduced, the difference of $14,943 indicates that when the youngest cohort attains the age of the oldest cohort, the youngest cohort would have $14,943 more in home equity than the current oldest cohort.[22] On balance, the home equity cohort effects *magnify* the financial asset cohort differences, showing successively greater financial assets with each younger cohort (see fig. 1.6). But if, as emphasized above, all mortgage debt is reduced with age and the trend in housing value persists, the difference in the assets of the younger and the older cohorts at retirement would be more closely indicated by the difference in home value. Since most retirement assets are likely to be accumulated until retirement, if mortgage debt is paid off by retirement age, wealth at that time will include retirement saving balances plus home value.

1.6.2 The 401(k) Eligible-Noneligible Comparison: Evidence on Housing Equity

An approach that is not complicated by a coincidental growth in retirement saving and mortgage debt is to compare the assets of 401(k) eligible and non-

22. The difference between the home equity cohort effects varies with age, however. The effects increase from the youngest cohort to the cohort that is age 54 in 1984. Thereafter, the cohort effects decline, with successively older cohorts having less home equity. The difference between the youngest and the C54 cohort is $7,193. The difference between the C42 and the oldest cohort is $22,136.

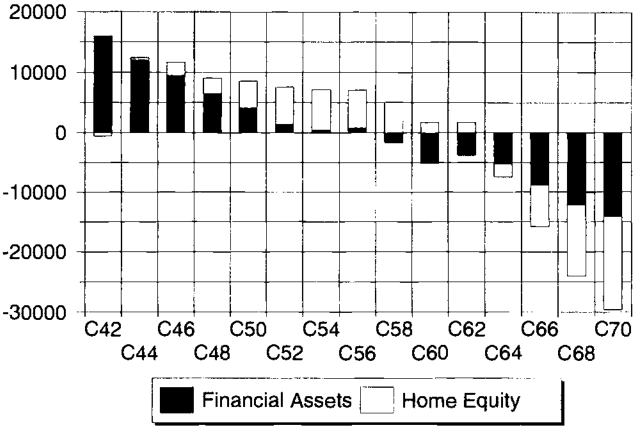

Fig. 1.6 Cohort effects in financial assets and home equity

eligible families in a given year, based on a random cross section of respondents of all ages. As described in section 1.4, for this comparison to be compelling, it is important that the eligible and noneligible households be similar with respect to saving propensity, controlling for income interval. As above, we use 1984 data, near the outset of the 401(k) program, to demonstrate the similarity of the saving propensities of eligible and noneligible families. We first discuss data on trends in the housing equity of eligible and noneligible households.

Trends in the Housing Equity of Eligible and Noneligible Households

The cohort data described above show changes in home mortgage debt and home values from 1984 to 1987 and from 1987 to 1991. Here we consider changes in home equity for families that are and are not eligible to contribute to a 401(k) plan. Table 1.9 shows that the trend was essentially *the same for both groups.* (These data show differences in the mean levels of home equity of eligible and noneligible households, *without* controlling for income interval. Within income interval, the differences are typically small, as discussed below.) There was very little change between 1984 and 1987 in mean home equity for eligible or for noneligible households. This is consistent with the cohort data, which shown a decrease for some cohorts and an increase for others. Between 1987 and 1991 there was a substantial decline in home equity for both eligible and noneligible households. The absolute decline is larger for eligible than for noneligible households, reflecting their larger absolute level of housing equity at the beginning of the period. The percentage declines were approximately the same for both groups, about 17 percent for noneligible families and about 19 percent for eligible families.[23] Given that the absolute effects

23. Home ownership declined 10 percent for noneligible and 4 percent for eligible families, and mean home value of home owners declined 12 percent for noneligible and 16 percent for eligible families.

Table 1.9 **Trends in Home Equity by 401(k) Eligibility, 1984, 1987, and 1991 (in 1991 dollars)**

	Year		
Measure	1984	1987	1991
Percentage own			
Eligible	0.78	0.75	0.75
Not eligible	0.63	0.60	0.57
Percentage own, relative to 1984			
Eligible	1.00	0.96	0.96
Not eligible	1.00	0.96	0.90
Mean home equity given own			
Eligible	70,723	71,189	59,880
Not eligible	61,197	61,688	54,629
Mean home equity given own—relative to 1984			
Eligible	1.00	1.01	.085
Not eligible	1.00	1.01	0.89
Mean home equity			
Eligible	49,747	47,685	40,425
Not eligible	34,073	33,088	28,273
Mean home equity, relative to 1984			
Eligible	1.00	0.96	0.81
Not eligible	1.00	0.97	0.83

of both market-determined housing price changes and availability of home equity loans are functions of initial housing equity, it is not surprising that the changes are roughly proportional to initial equity.

Although percentage changes in mean values were about the same for both groups, the proportionate decline in medians, and other quantiles, was much greater for noneligible than for eligible households. Quantile values (50th, 75th, and 90th) for eligible and noneligible households are shown in figure 1.7. Because a large fraction of households do not own a home, medians can be substantially affected by small changes in mean values. Like the means, the quantile changes between 1984 and 1987 were much smaller than the changes between 1987 and 1991. Between 1987 and 1991, median home equity for eligible households declined by 40 percent, the 75th percentile by 18 percent, and the 90th percentile by 5 percent; the declines for noneligible households were 71 percent, 25 percent, and 10 percent, respectively. These tabulations suggest that the forces that induced changes in home equity applied more or less equally to eligible and noneligible households during the 1987–91 period.

The Eligibility Comparison

We expanded our comparison of the assets of eligible and noneligible households at a point in time to include net housing equity. As emphasized above, the validity of this difference as an estimate of the eligibility effect depends on

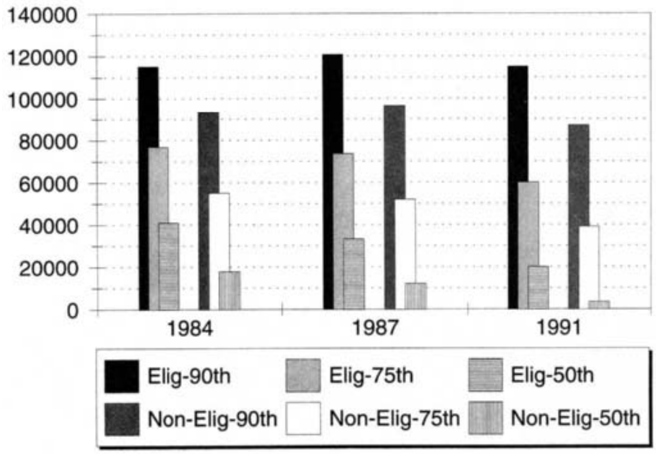

Fig. 1.7 Home equity quantiles by eligibility and year

the similarity of the underlying taste for saving of the two groups. Again, we rely on comparison of the assets of eligible and noneligible households near the outset of the program, in 1984, to establish the extent of similarity.

Median 1984 asset balances are shown in table 1.10, by income interval.[24] Within an income interval, the medians control for age, marital status, and education. The assets of eligible and noneligible families were roughly the same at the outset of the 401(k) program, whether the asset measure includes or excludes net housing equity.[25] There is, however, a noticeable difference for the $75,000+ income interval. There are only 83 families in the 401(k) eligible group in this interval. Because this top interval is open ended, the incomes of eligible and noneligible households in this interval may be quite different. For most families, net non–IRA-401(k) assets were negative or very small in 1984. Thus any significant contributions to a saving plan, from which assets are not withdrawn, would therefore represent a net increase in the financial asset saving of most families.

Median assets balances in 1991 are shown in table 1.11. Although at the outset of the program eligible and noneligible families had approximately the same level of net financial assets, by 1991 eligible families had substantially greater median levels of net total financial asset balances, and greater levels of financial asset plus home equity balances, than noneligible families.

The first two panels of table 1.11 show net total financial assets and net total

24. The estimates are evaluated at the median of sample values for age, marital status, and education. Thus they differ from similar calculations reported in PVW (1995), which are evaluated at the means of control variables.

25. Appendix table 1C.3 shows that eligibles and noneligibles had similar levels of other asset measures at the outset of the 401(k) program in 1984. We were unable to calculate conditional medians for home value and home mortgage.

Table 1.10 **Conditional Median Asset Balances by 401(k) Eligibility and Income Interval, 1984 (in 1984 dollars)**

Asset Category and Eligibility Status	Income Interval[a]						
	<10	10–20	20–30	30–40	40–50	50–75	75+
Net non–IRA-401(k) financial assets							
Eligible	−1,288	−651	302	716	2,815	6,241	22,068
Not eligible	−607	−348	130	775	2,080	5,208	17,802
Difference	−681	−304	172	−60	735*	1,034*	4,267*
Net non–IRA-401(k) financial assets plus home equity							
Eligible	11,594	16,616	21,371	28,136	38,799	53,060	104,748
Not eligible	11,293	14,398	18,632	28,461	36,327	44,462	83,338
Difference	301	2,218	2,739	−325	2,472	8,598*	21,410*

Source: Authors' tabulations from the 1984 SIPP.

[a]Income intervals indexed to 1987 dollars.

*Statistically significant at the 5 percent level.

Table 1.11 **Conditional Median Asset Balances by 401(k) Eligibility and Income Interval, 1991 (in 1991 dollars)**

Asset Category and Eligibility Status	Income Interval[a]						
	<10	10–20	20–30	30–40	40–50	50–75	75+
Net total financial assets							
Eligible	1,102	1,073	2,464	7,554	17,022	34,726	67,878
Not eligible	−483	−57	370	2,307	3,652	11,597	39,218
Difference	1,585*	1,130*	2,094*	5,247*	13,370*	23,129*	28,660*
Net total financial assets plus housing equity							
Eligible	14,509	14,150	20,538	32,875	49,361	84,511	151,834
Not eligible	9,185	13,121	15,106	28,502	38,139	60,945	122,341
Difference	**5,324**	**1,029**	**5,432***	**4,373***	**11,222***	**23,566***	**29,499***
Net non–IRA-401(k) financial assets							
Eligible	−491	−262	−95	1,089	3,094	8,838	18,925
Not eligible	−327	−142	116	907	1,968	5,667	26,909
Difference	−164	−120	−211	182	1,126*	3,171*	−7,984*
Net non–IRA-401(k) financial assets plus home equity							
Eligible	9,030	10,361	14,017	24,168	34,682	61,358	108,290
Not eligible	8,059	11,557	13,522	25,468	35,275	56,360	105,294
Difference	**971**	**−1,196**	**495**	**−1,300**	**−593**	**4,998***	**2366**

Source: Authors' tabulations from the 1991 SIPP.

[a]Income intervals indexed to 1987 dollars.

*Statistically significant at the 5 percent level.

financial assets plus home equity, for eligible and noneligible households. For the most part, the difference between the assets of eligible and noneligible families remains about the same when home equity is added to net total financial assets. At the outset of the program, the financial assets and the home equity of eligible and noneligible families were about the same. They were also about the same in 1991 (more detail is shown in appendix table 1C.4). The difference between median levels of net non–IRA-401(k) financial assets plus housing equity of eligible and noneligible families is about the same as the difference in net non–IRA-401(k) financial assets. Thus these data suggest that the greater financial assets of 401(k) eligible families were not offset by a disproportionate reduction in the housing equity of eligible families. This is consistent with the data that show approximately equal proportional changes in the housing equity of eligible and noneligible families between 1984 and 1991.

1.6.3 The Engen and Gale Between-Group Results

Using a different approach, Engen and Gale (1995; hereafter EG) conclude that the increase in the financial assets of eligible families was offset by a reduction in home equity. We do not explore the differences between their results and ours in detail here, but we do describe the key elements of their method and provide some conjectures about possible reasons for the differences.

The Method

To study the relationship between 401(k) saving and home equity, EG use an approach similar to the one described in section 1.3.2 above. They consider several *between*-group comparisons, including 401(k) eligible versus noneligible families and 401(k) eligibles who have an IRA versus 401(k) noneligibles who have an IRA. For illustration, we consider first the former comparison, denoting the first group by i and the second group by j. We treat the Tax Reform Act of 1986 (TRA86) as a "program," with an effect on both groups. Using the same terminology as in equation (7), the "treatment" group (i) is subject to both the 401(k) and the TRA86 program effects, $\boldsymbol{p}_i$ and $\boldsymbol{r}_i$, respectively, while the comparison group (j) is subject only to the TRA86 effect. The difference-in-difference estimate including both the saving program and the TRA86 effects would be

$$\begin{aligned} &[A_{si}(n+k) - A_{si}(n)] - [A_{sj}(n+k) - A_{sj}(n)] \\ (12) \quad &= [(\boldsymbol{m}_i - \boldsymbol{m}_{i'}) - (\boldsymbol{m}_j - \boldsymbol{m}_{j'})]f(s) \\ &\qquad + \{\boldsymbol{p}_i[g(n+k) - g(n)] + (\boldsymbol{r}_i - \boldsymbol{r}_j)[q(u+k) - q(u)]\}. \end{aligned}$$

To make clear that years of exposure to TRA86 may differ from exposure to the saving program, we let u indicate the number of years of exposure to TRA86 at the first observation (e.g., 1987) and $u+k$ the number of years of exposure to

TRA86 at the second observation (e.g., 1991). This method will estimate the 401(k) program effect $\boldsymbol{p}_i$ if two conditions are met: $\boldsymbol{m}_i = \boldsymbol{m}_{i'}$ and $\boldsymbol{m}_j = \boldsymbol{m}_{j'}$, or if $\boldsymbol{m}_i - \boldsymbol{m}_{i'} = \boldsymbol{m}_j - \boldsymbol{m}_{j'}$, as discussed above, and $\boldsymbol{r}_i = \boldsymbol{r}_j$. But if the two groups have very different levels of home equity at the outset, it is unlikely that the effects of TRA86, $\boldsymbol{r}_i$ and $\boldsymbol{r}_j$, will be equal, at least in levels.

If the comparison is between 401(k) eligibles who have an IRA versus 401(k) noneligibles who have an IRA, the treatment group (i) is subject to three program effects: 401(k), IRA, and TRA86—$\boldsymbol{p}_i$, $\boldsymbol{b}_i$, and $\boldsymbol{r}_i$, respectively. The comparison group (j) is subject to two program effects: IRA and TRA86—$\boldsymbol{b}_j$ and $\boldsymbol{r}_j$. In this case, the difference-in-difference estimate is

$$\begin{aligned} &[A_{si}(n+k) - A_{si}(n)] - [A_{sj}(n+k) - A_{sj}(n)] \\ (13) \quad &= [(\boldsymbol{m}_i - \boldsymbol{m}_{i'}) - (\boldsymbol{m}_j - \boldsymbol{m}_{j'})]f(s) + [(\boldsymbol{p}_i + \boldsymbol{b}_i) - \boldsymbol{b}_j][g(n+k) - g(n)] \\ &\quad + (\boldsymbol{r}_i - \boldsymbol{r}_j)[q(u+k) - q(u)]. \end{aligned}$$

The program effect $\boldsymbol{p}_i$ will be isolated if three conditions are met: $\boldsymbol{m}_i = \boldsymbol{m}_{i'}$ and $\boldsymbol{m}_j = \boldsymbol{m}_{j'}$, or if $\boldsymbol{m}_i - \boldsymbol{m}_{i'} = \boldsymbol{m}_j - \boldsymbol{m}_{j'}$, $\boldsymbol{r}_i = \boldsymbol{r}_j$, and $\boldsymbol{b}_i = \boldsymbol{b}_j$. Again, whether these conditions are approximately met is likely to depend on the initial conditions of the two groups. As emphasized above, if the initial conditions of the two groups are very different it is more likely that the program effects on the two groups will differ as well. The effects of TRA86 are likely to depend on the initial home equity levels, and the potential effects of the saving programs may vary in nonsystematic ways with the initial financial assets of the two groups. Some committed *non*savers may be completely unaffected by the programs, for example. Thus it is problematic whether any differencing procedure will adequately account for differences in the potential program responses of very dissimilar saver groups.

Results versus Method: Some Illustrations

A complete understanding of why our results differ from those obtained by EG will have to await further analysis and discussion, but we believe one explanation is their use of dissimilar groups in computing a difference-in-difference estimator. Recall that PVW (1994a, 1995) emphasize *within*-group estimates in the like saver group comparisons discussed in section 1.3. EG use a *between*-group approach.[26]

The within-group approach that we used to evaluate the effect of the saving programs on financial asset saving may not extend satisfactorily to include housing equity. Although housing equity may be affected by 401(k) eligibility, it is also likely to have been affected by TRA86. A within-group estimator cannot distinguish the separate effects of the two programs. Thus it is natural to seek a saver group affected by TRA86 but not by the 401(k) plan, with which

26. EG cite PVW (1995) as the source for their method. This is a misunderstanding of our method.

the 401(k) group can be compared. This is what the between-group estimate that EG use is intended to do. But typically their comparisons are between dissimilar saver groups with very different saving behavior. The question then is how to obtain reliable estimates from between-group comparisons when the groups are so different. There may be no completely satisfactory way to do this—other than a randomized controlled trial—and we do not try to solve the problem.

We do, however, illustrate the issue using data for the saver groups defined by 401(k) eligibility and IRA participation status. These groups were considered by PVW in the like group analysis discussed in section 1.3. (The financial asset data for the groups are shown in the bottom panel of table 1.4.). In their within-like-group analysis, PVW emphasized that there was virtually no change between 1984 and 1991 in non–IRA-401(k) assets of 401(k) eligible households, 401(k) eligible households with an IRA, or 401(k) eligible households without an IRA (or in the non–IRA-401(k) assets of 401(k) noneligible households with an IRA). Yet for each of these saver groups there was a large increase in total financial assets. Based on between-group comparisons, EG argue that the increase in financial assets between 1987 and 1991 was offset by a reduction in home equity.

But this conclusion depends critically on whether the groups compared are similar or dissimilar, as the data in table 1.12 show. The question is whether there was a differential effect of TRA86 on the 401(k) eligible households, compared to the comparison households. Consider first 401(k) eligibles with an IRA compared to 401(k) noneligibles with an IRA. These two groups had similar housing assets in 1984 at the outset of the 401(k) program and experienced similar declines in housing equity (10.2 percent for the eligible and 10.6 percent for the noneligible group). In this case, the dollar declines were about the same as well. The decline for 401(k) eligible households was $379 greater than the decline for noneligible households. Were one to assume that this is the decline due to 401(k) eligibility—which we would not—this amount would offset very little of the increase in the total financial assets of 401(k) eligible households between 1987 and 1991.

When very dissimilar groups are compared, however, this approach can yield misleading results. For example, even though all 401(k) eligible and all 401(k) noneligible households experienced similar proportional declines in housing equity (15.7 vs. 13.7 percent), the *dollar* declines were very different ($4,513 vs. $2,254) because the two groups had very different levels of housing equity at the outset of the program. Thus it is misleading to ascribe the greater decline in the housing equity of the 401(k) eligible group to 401(k) eligibility per se, as the between-group comparison does. The greater decline for eligibles may simply reflect their larger initial housing equity. The groups also had very different levels of financial assets. This is why we emphasize within-group comparisons, and avoid inferences based on between-group comparisons, in our like group analysis.

One way to estimate the reduction in housing equity attributable to the

Table 1.12 **Home Equity by Saver Group and Year, with Between-Group Estimates: Conditional Medians in 1987 Dollars**

Saver Group	1984	1987	1991	Difference 1984 to 1991		Difference to 1987 to 1991	
				Percentage	Level	Percentage	Level
All families							
401(k) Eligible	32,658	28,743	24,230	−25.8	−8,428	−15.7	−4,513
401(k) Not eligible	18,699	16,469	14,215	−24.0	−4,484	−13.7	−2,254
Difference in difference				**−1.8**	**−3,944**	**−2.0**	**−2,259**
Equal percentage reduction[a]					**−597**		**−579**
Families with an IRA							
401(k) Eligible	52,621	48,451	43,531	−17.3	−9,090	−10.2	−4,920
401(k) Not eligible	46,385	42,913	38,372	−17.3	−8,013	−10.6	−4,541
Difference in difference				**0.0**	**−1,077**	**0.4**	**−379**
Equal percentage reduction[a]					**0**		**207**
Families without an IRA							
401(k) Eligible	22,905	19,704	15,578	−32.0	−7,327	−20.9	−4,126
401(k) Not eligible	12,399	10,575	8,696	−29.9	−3,703	−17.8	−1,879
Difference in difference				**−2.1**	**−3,624**	**−3.2**	**−2,247**
Equal percentage reduction[a]					**−486**		**−625**

[a]Difference between actual reduction of eligibles and the reduction had eligibles experienced the same percentage decline as noneligibles.

401(k) program would be as the difference between the actual reduction in home equity (28,743 − 24,230) and the reduction had the treatment group experienced the same *percentage* reduction as the noneligible group. This yields an estimate of $579, which is small compared to the increase in the financial assets of this group. This approach seems plausible in this case because programs that affect housing values and mortgages are likely to have effects proportional to initial housing values.

1.7 SCF: Summary Data and Gale and Scholz Parametric Analysis

In section 1.2.4 above, we discussed the change in the financial assets of IRA contributors between 1983 and 1986, as their IRA assets accumulated. We concluded that these data, from the Survey of Consumer Finances, showed no substitution of IRA contributions for other forms of saving, and that the IRA *contributions* between 1983 and 1986 represented largely new saving. Based on parametric analysis of the same data, Gale and Scholz (1994; hereafter GS) concluded that virtually none of the additional IRA saving resulting from an *IRA limit increase* would be new saving. This result has often been interpreted to imply that none of the IRA saving undertaken during the 1983–86 period represented new saving, although GS are careful to emphasize that their analysis pertains to a limit increase. They conclude that, of the increase in IRA contributions resulting from an increase in the limit, 31 percent would be financed by lower taxes, 2 percent would be funded by a decrease in consumption, and 67 percent would come from a reduction in other saving. Our conclusion and that of GS are not *necessarily* inconsistent, although it seems unlikely that they could both be true. Thus we now consider what lies behind our different conclusions.

We first discuss the data on which our results in section 1.2.4 and the GS results are based. We consider the deletion of observations that preceded the GS estimation, and we draw attention to the potentially important effect of sample selection on the GS results. Then we discuss more carefully the change in the non-IRA saving of contributors that would have been expected in the absence of the IRA program. We conclude that it is virtually impossible that IRA contributions between 1983 and 1986 came entirely from a reduction in other saving. We consider whether this conclusion could be consistent with the possibility that an increase in the IRA limit would result in no new saving. Finally, we explore the GS estimation procedure in detail and find that their results are not a robust reflection of the SCF data but rather are an artifact of their specification and estimation procedure.

1.7.1 The Data

The data and the GS estimation sample are described with reference to table 1.13. For background, several features of the SCF data are important: (1) Only households who were surveyed in both the 1983 and 1986 waves of the SCF

Table 1.13 **Observations in Matched 1983–86 Survey of Consumer Finances Sample, by IRA Contributor Status, Definition, and Observation Deletions**

	Using 1986 IRA Balance		Using GS Contributor Assignment		
				With GS Savings Deletions	
IRA Status	Without GS Savings Deletions (1)	With GS Savings Deletions (2)	Without GS Savings Deletions (3)	PVW Replication (4)	GS Estimation Sample (5)
All	1,670	1,486	1,670	1,486	1,483
Area probability	1,489	1,445	1,489	1,445	
High income	181	41	181	41	
Noncontributors	1,021	988	1,099	1,038	1,035
Area probability	996	982	1,042	1,025	
High income	25	6	57	13	
Contributors	649	498	571	448	448
Area probability	493	463	447	420	
High income	156	35	124	28	
Within limits			403	341	331
Area probability			340	324	
High income			63	17	
At limit			168	107	117
Area probability			107	95	
High income			61	11	

can be used in the analysis. (2) Some households are deleted because they did not meet the criteria for IRA participation or for other reasons were unlikely to contribute to an IRA. (3) The SCF data comprise of two samples: an "area probability" sample and a "high income" sample that oversampled high-income households. (4) Whether a family contributed to an IRA during the period is not reported in the SCF, so contributor status must be inferred. (5) Flow saving in other conventional forms must also be inferred from the reported levels of assets in 1983 and 1986. Although we have been unable to match exactly the GS estimation sample, we believe that the differences do not materially affect the conclusions that we draw below.

The 1983 SCF sample included 4,262 respondents, of whom 3,824 were in the area probability sample and 438 in the high-income sample. Of these, 2,791 were surveyed in 1986 as well.[27] Excluding families in which either the respondent or the spouse were self-employed, the age of the head was less than 25 in 1986, the age of the head was greater than 65 in 1983, or there was a change

27. The 1983 survey was conducted between February and August 1983, with the majority of interviews in March and April. Thus 1983 IRA balances represent 1982 contributions for the most part. The 1986 survey was conducted in June through September 1986, so 1986 IRA balances represent contributions through the 1985 tax year.

in marital status between 1983 and 1986 leaves a total of 1,670 households. Column (1) of table 1.13 gives a breakdown of this sample by area probability versus high-income sample status and by IRA contributor status.

Because the SCF reports IRA balances but not annual contributions, to determine whether a household contributed to an IRA between 1983 and 1986 requires assumptions about the return on assets, as well as other conventions. The GS assumptions are explained in appendix A to their paper. Column (1) in table 1.13 shows the number of observations using whether or not the family had an IRA balance in 1986 to indicate IRA contributor status. Of course some of these households could have contributed in 1982 but not thereafter and thus were not active contributors in the 1983–86 period. There were 1,021 households without an IRA balance in 1986 and 649 with a positive IRA balance. Of the households with an IRA balance in 1986, 24 percent of the respondents (156 of 649) were from the high-income sample. Appropriately weighted, only 4.5 percent of all contributors would be from households in this high-income group.

Column (3) shows the breakdown of "contributors" and "noncontributors" based on the GS contributor status assignment conventions. They use balances in 1983 and 1986 together with an assumed rate of return on 1983 balances to infer new contributions between the two years. These assignments yield fewer contributors than the number of households with a positive 1986 IRA balance (571 vs. 649), as expected. GS also use their assumptions to allocate households to limit contributor status (those with estimated three-year contributions greater than the estimated three-year limit) and nonlimit contributor status.

To estimate their model, GS eliminate a large number of additional observations, those with 1983–86 estimated saving less than −$100,000 or greater than +$100,000. The resulting sample is labeled "With GS Savings Deletions" in table 1.13. Their procedure removes 184 of 1,670 households, 61 noncontributors and 123 contributors. Of the 168 limit contributors, 61 are removed, including 82 percent (50 of 61) of the high-income sample limit contributors. Estimates based on the remaining 107 limit contributors determine the results of the GS estimation procedure. We were unable to replicate exactly the 117 observations used by GS. Column (5) shows the number of observations used in the GS estimation procedure, as reported in their paper.

These deletions have an enormous effect on the distribution of saving and assets in the estimation sample and on formal parameter estimates, as GS show. Mean and median estimated saving between 1983 and 1986 with and without these deletions, as well as non-IRA financial assets with and without the deletions, are shown in table 1.14. As the table shows, the sample deletions have enormous effects on the sample means. The GS estimation procedure is based on means, and their results are essentially determined by the few limit contributors in the sample, so sample deletions can have an enormous effect on the results. While in principle there is nothing wrong with "trimming" the data,

Table 1.14 **Change in Mean and Median Saving and 1983 Non-IRA Financial Assets with and without Sample Deletions**

Measure and Deletion Choice	Mean	Median
Saving		
Without GS deletions	−13,303	1,132
With GS deletions	2,378	1,044
1983 Non-IRA financial assets		
Without GS deletions	217,668	5,700
With GS deletions	22,244	4,222

we show below that the key parameter estimate is extremely sensitive to exactly which observations are deleted, and the sample deletions that are made essentially determine the conclusions that GS report.

1.7.2 A Simple Reality Check

In section 1.2.2 above, we discussed summary data (table 1.3) based on these same SCF surveys. We return to a similar discussion here, based on data reported in table 1.15. As explained above, using the 1983 and 1986 waves of the SCF it is possible to compare the asset balances of the *same* households over time. We begin with respondents to the 1986 survey. We exclude households with self-employed members and households with a change in marital status between 1983 and 1986. There are two reasons why the values in table 1.15 may differ from "comparable" values reported by GS: First, we restrict the sample to all households between ages 25 and 65 in 1986. Because GS limit their sample to households with heads aged 65 and under in 1983, some heads are as old as age 68 in 1986. We believe that our sample is a better representation of the pool of potential contributors, that is, nonretirees. Second, we also use a narrower definition of financial assets, including only those assets that we believe are most likely to be substituted for IRAs. Our measure includes checking accounts and statement, passbook, share draft, and other saving accounts; stocks and mutual funds, saving bonds, and corporate, municipal, and all other bonds; and money market accounts and CDs. The GS measure includes in addition the cash value of life insurance, trusts, managed investment accounts, and notes and land contracts owed to the household.[28] In addition, the summary data reported by GS in their tables 1 to 3 are based on different age criteria than their estimation sample, including *all* households over age 25, even those over age 65.[29]

28. These additions change the magnitude but not the pattern of the data reported in table 1.15. Data based on the GS definitions are shown in table 1.17 below.

29. In addition, although the pattern revealed by the data is the same in both cases, the values reported here differ slightly from the numbers reported in table 1.3 for three reasons: (1) The 1986 SCF combines IRA and Keogh balances. GS present a method for inferring the 1986 IRA balance based on the 1983 response, and we use the GS method here. (2) We use the newer set of sample weights here. (3) We use the GS definition of a change in marital status here.

Table 1.15 **Survey of Consumer Finances Summary Data: Using GS Estimation Sample Definitions (in current dollars)**

Contributor Status and Asset	Year 1983	Year 1986	Percentage Change
	Medians		
Contributors in 1986			
Non-IRA assets	8,800	13,400	52.3
IRA assets	600	6,857	1043.0
Total financial assets	11,800	23,000	94.9
Noncontributors			
Total financial assets	750	1050	40.0
	Median of Natural Logarithms[a]		
Contributors in 1986			
Non-IRA assets	9.083	9.503	42.0
IRA assets	6.397	8.833	243.6
Total financial assets	9.376	10.043	66.7
Noncontributors			
Total financial assets	6.620	6.957	33.7

Source: Authors' tabulations using the 1983 and the 1986 SCF.

[a]The percentage change is approximated by the difference in the logarithms.

Median IRA and non-IRA financial asset balances in 1983 and 1986, and the change in balances between these years, are shown in the first panel of table 1.15, by whether the respondent had a positive 1986 IRA balance. The table also shows total assets of contributors, including both IRA and non-IRA balances. (In anticipation of estimation results discussed below, the table also includes the median of the logarithm of assets.)[30] Several features of the data are important: (1) The median 1983 non-IRA asset balance of households with IRA accounts in 1986 was $8,800. Clearly, prior to 1983, this group had not been accumulating assets at the rate of the typical household IRA contribution, about $2,300 per year. (2) The $6,257 increase in IRA balances (from $600 in 1983 to $6,857 in 1986) clearly was not funded by transferring funds from the 1983 balance in non-IRA accounts, which was only $8,800 at the beginning of the period. (3) Indeed, the non-IRA assets of contributors did not decline at all as IRA assets increased between 1983 and 1986. On the contrary, they increased over 52 percent, from $8,800 to $13,400.

Without the IRA program, what increase in the 1983 non-IRA asset balance would have occurred over the next three years? The observed 52.3 percent increase was equivalent to an annual asset growth rate of 15 percent. If IRA contributions were funded either by withdrawing funds from non-IRA bal-

30. When logarithms are used, assets of zero are set to one and assigned a logarithm of zero.

ances or by reducing new saving in non-IRA assets, then the increase in non-IRA assets between 1983 and 1986 should have been much less than would have been expected in the absence of the IRA program. That is, the expected increase in non-IRA assets should have been much more than the observed increase—from \$8,800 to \$13,400. We consider a simple prediction of asset growth in the absence of IRAs.

Assets tend to increase with age and income. A simple way to estimate the expected increase in non-IRA assets between 1983 and 1986 is to predict the increase based on the 1983 relationship between age and income on the one hand and assets on the other, with some allowance for change in the rate of return on assets.

Even simple estimates of the income-asset profile are confounded by the nature of the data. There is enormous "residual" variance with respect to assets. For example, a linear regression of assets on age and income yields an R^2 value of about 0.06 with a residual standard deviation of \$2,400,000. If the data are weighted by the appropriate sampling probabilities, the R^2 is about 0.07 and the residual standard deviation is about \$540,000. (This portends the finding that sample deletions can have an enormous influence on the results.) In addition, the data exhibit enormous heteroscedasticity, which we attempt to correct for by using a semilog specification of the form

$$\log A_{83} = a + b(\text{Income}_{83}) + c(\text{Age}_{83}) + e. \tag{14}$$

The predictions for 1986 are based on

$$\log A_{86} = \log A_{83} + b(\text{Income}_{86} - \text{Income}_{83}) + c(\text{Age}_{86} - \text{Age}_{83}). \tag{15}$$

Thus the predictions account for the change in assets associated with an increase in age between 1983 and 1986 and for the change associated with the change in income.

Predictions based on equation (15) are shown in table 1.16. The predicted increase in median assets based on a weighted median regression is about 23 percent, which is less than the actual increase (based on the difference in logs) of about 45 percent. The predicted increase in mean assets is about 29 percent, compared to an actual increase of about 56 percent.

Thus we predict a 1986 non-IRA asset level that is *lower* than the observed level, not higher than the observed level as would be expected if IRA contributions simply substituted for saving that would have occurred anyway.

The 1983 cross-sectional regression implies a difference in the assets of families by age. We want to predict the increase for families who age three years and whose earnings change over these three years. The 1986 prediction based on the 1983 cross-sectional estimates accounts for the increase in age

Table 1.16 Logarithm of 1983 Median and Mean Actual Assets and Predicted Median and Median 1986 Assets (in current dollars)

Measure	1983	1986 Predicted	1986 Actual
Weighted			
Median	9.048	9.280	9.503
Mean	8.786	9.072	9.344
Unweighted			
Median	9.598	9.817	9.957
Mean	9.594	9.839	10.555

between 1983 and 1986, and it accounts for the change in earnings by using in the prediction the 1986 earnings of the respondents. But the prediction does not account for any change due to the return on initial asset holding. Inherent in the 1983 regression estimates of the difference in assets of people differing in age by three years is also a rate of return, but for an earlier period. If the prior return differs from the 1983–86 return, the projected asset increase may not apply to this later period. The magnitude depends on the difference between the prior and ex post rates of return. Consider, for example, the AAA bond rate in 1980–82 versus 1983–85. The average during the first period was 13.30 and during the second period 12.04. So correction for the rate of return would *reduce* the estimated increase. The return on other assets may give a different sign; more detail on this issue is presented in appendix B.

1.7.3 The Saving Effect of Program Contributions versus the Saving Effect of a Limit Increase

The foregoing analysis suggests to us that it is very unlikely that the bulk of IRA contributions were financed at the expense of withdrawals from non-IRA accounts, or from a reduction in new saving in non-IRA accounts. Indeed, if anything, the data taken at face value suggest that other saving increased as IRA contributions increased. Thus from these data alone we would argue that the contributions under the existing program represented largely new saving. Yet GS conclude, and they say explicitly in their paper, that an increase in the IRA *limit* would not increase saving. Here we consider the summary data that GS highlight in foretelling their formal results. In particular, we consider how the inferences that GS draw from the summary data can be so different from our judgments based on the same data.

GS argue that limit contributors in particular, but nonlimit contributors as well, had substantial non-IRA financial assets in 1986. The implication is that if the limit were raised, these families could easily fund an IRA by transferring assets from non-IRA to IRA accounts without increasing net saving, and that because they could do that, they would. We emphasize the low level of non-IRA assets in 1983, at the outset of the program, and the increase in these non-IRA assets as IRA contributions were accumulating. We infer from these data that the IRA accumulation could not possibly have been funded by withdrawing funds from non-IRA balances and was unlikely to have been funded by reducing new non-IRA saving that otherwise would have occurred. We want to understand what accounts for the difference between the $8,800 level that we emphasize and the $41,269 for limit contributors emphasized by GS. Part of the difference is simply their emphasis on 1986 assets versus our emphasis on 1983 assets. Part of the difference is the definition of non-IRA financial assets. Part of the difference comes from differences in the meaning of limit contributor. We consider the last issue first and then turn to differences in financial asset definitions.

Limit Contributors versus All Contributors

We have framed our judgments in terms of the addition to net saving represented by the contributions of all contributors, both limit and nonlimit. But because such a large fraction of contributions were at the limit, we believe that a higher limit would have led to still greater net saving. Based on an analysis of 1983 tax returns, Burman, Cordes, and Ozanne (1990) find that 75.3 percent of all IRA contributions were at the family limit and that an additional 11.3 percent were at the limit for one spouse in households filing joint returns. EGS report that 63.3 percent of contributions were at the family limit over the period 1982–86. With such a large fraction of households at the limit, if all limit contributors funded IRA contributions by transferring funds or by reducing other saving, the summary data would show that. But they do not. We infer, therefore, that if the limit had been higher, we would have seen a greater increase in assets by 1986 than actually occurred.

In considering the effect of a higher limit, the number of *individual annual* contributions made at the limit is the relevant statistic. Presumably, each contribution at the limit would have been at least somewhat greater had the limit been higher. Thus we should have in mind that between 60 percent and 85 percent of contributions are by families in this category.

GS point to an entirely different measure, suggesting that only 22 percent of contributions are at the limit. The families that GS call "limit contributors" are those who are assigned limit status in each of *three consecutive years* (1983, 1984, and 1985) based on *their assignment* criteria. They report 21.8 percent at the limit based on these criteria. Thus the actual proportion of contributions at the limit is three or four times as large as the proportion assumed by GS. In considering whether persons at the limit would have contributed more, and saved more, had the limit been higher, recognizing that a much larger share of contributions are at the limit may well alter one's prior expectations about the saving effect of raising the limit.

Table 1.17 **Survey of Consumer Finances Summary Data: Using GS Definitions (in current dollars)**

Contributor Status and Asset	Year 1983	Year 1986	Percentage Change
Contributors in 1986			
Non-IRA assets	13,085	19,000	45.2
IRA assets	600	6,857	1,043.0
Total financial assets	14,100	30,000	112.8
Noncontributors			
Total financial assets	1,200	2,269	89.1

Source: Authors' calculations based on the 1983 and 1986 SCF.

Assets of All Contributors and Limit Contributors

To understand the differences in the asset levels that we emphasize and those reported by GS, begin with the non-IRA financial asset values reported in table 1.15, which are $8,800 and $13,400 for 1983 and 1986, respectively. Following the presentation of the data in their table 3, GS would emphasize the 1986 balance, corresponding to $13,400. The 1986 balance reflects the *increase* in non-IRA assets during the time that IRA assets were accumulating.[31]

The GS asset definition also differs from ours. Based on the GS non-IRA financial asset definition—but still considering the assets of households with IRA balances in 1986, not the GS assignment procedure—we find the values reported in table 1.17, for households aged 25–65 in 1986.[32] The pattern is the same as that reported in the top panel of table 1.15. In particular, non-IRA assets by this definition are $13,085 and $19,000 in 1983 and 1986, respectively. But even including asset balances from which we believe IRA contributions are unlikely to be taken, the $13,085 balance in 1983 suggests that contributors had not previously been accumulating assets at the rate of $2,300 annually.

The asset balances reported by GS differ in still other respects from those in table 1.17. GS report 1986 non-IRA financial assets of $21,695 for households with *inferred* contributions between 1983 and 1986. In addition to the inferred contributor definition, this estimate incorporates a broader age range, including all persons over age 24, even those who are over age 65 and unlikely to make

31. Indeed, in the 1990 version of their paper, GS emphasized that the level of 1986 non-IRA assets of contributors was $13,500, very close to our measure of $13,400.

32. GS use an expanded definition of non-IRA financial assets that includes the cash value of life insurance, trusts, managed investment accounts, and note and land contracts owed to the household. This sample also includes all households over age 25 in 1986, including those over age 65.

IRA contributions. Because older households tend to have greater assets than younger households, expanding the upper age limit may significantly affect the results.

Based on the three-year inferred limit criterion, the more inclusive definition of financial assets, and the all-inclusive age range, GS report median financial assets of their "three-year limit" contributors of $41,269.[33] Because GS so severely underestimate the proportion of contributions at the annual limit, the assets of the much larger number of persons who actually contribute at the limit is probably lower than this. But there is no data-based value to compare with this figure, since the SCF does not report contributions between 1983 and 1986. Using the 1983 and 1986 CES, we calculate that the median non-IRA financial assets of limit contributors were $14,250 in 1983 and $19,500 in 1986.[34]

In our view, the summary data reported in table 1.15 suggest that most contributions between 1983 and 1986 represented a net addition to saving and, in addition, are inconsistent with the possibility that families who contributed at the IRA limit did not increase net saving. Since a large fraction of contributions are at the limit, most of the increase in non-IRA financial assets as IRA contributions were accumulating must be attributed to limit contributors. Thus it is implausible that if the limit had been higher, these limit contributors would not have increased total saving still more. GS emphasize large financial asset values for contributors by citing assets in 1986 instead of 1983, by using a broader definition of financial assets, and, in the case of limit contributors, by citing the assets of families at their "three-year limit" rather than at the annual limit.

1.7.4 The Gale and Scholz Model

GS present estimates of a formal model that they believe supports the view that limit contributors would not increase net saving if the limit were increased. We explain here why we believe that their conclusion is not supported by the data. We first describe their model and then present and discuss two-stage "consistent" estimates of it that may be easier to understand than the full maximum likelihood estimates. Finally, we explore directly the properties of the maximum likelihood specification used by GS. In each case, we decompose the specification to identify its critical features. The important features of the specification may be lost when looking at the whole, but they are easily discerned if the procedure and results are decomposed and built up step by step.

33. Based on our approximation to their sample, prior to their saving deletions, their estimation sample included approximately 168 persons at this limit. The number used to obtain this median asset balance will be somewhat larger than this because it included more older households.

34. The CES financial asset definition matches approximately the definition used in table 1.12 but is less inclusive than the definition used by GS.

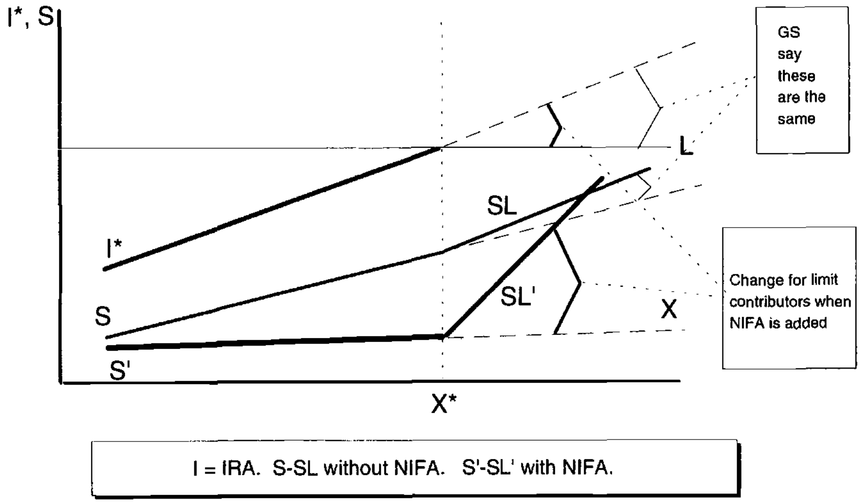

Fig. 1.8 The Gale and Scholz (GS) model
Note: NIFA = non-IRA financial assets.

We believe that the key GS results are an artifact of their model specification.
The GS specification can be written as

$$(16)\qquad I^* = X\beta + u,$$

$$(17)\qquad S = X\gamma_1 + \varepsilon_1 \qquad \text{if } I < 0,$$

$$(18a)\qquad S = X\gamma_2 + \varepsilon_2 \qquad \text{if } 0 < I < L,$$

$$(18b)\qquad \begin{aligned} S &= X\gamma_2 + \eta(I^* - L) + \varepsilon_2 \\ &= X\gamma_2 + \eta(X\beta - L) + \eta u + \varepsilon_2 \\ &= X\gamma_2 + \eta X\beta - \eta L + \eta u + \varepsilon_2 \\ &= X(\gamma_2 + \eta\beta) - \eta L + \eta u + \varepsilon_2, \qquad \text{if } I > L \end{aligned}$$

$$(19)\qquad \eta = X\delta,$$

where I^* represents desired IRA saving and S represents non-IRA financial assets saving. The variables X are a set of household attributes. The specification can be described with reference to figure 1.8. The line labeled I^* represents equation (16), which is desired IRA saving and is limited at L. Other financial asset saving of IRA contributors is represented by the line labeled S, and then S_L, and has two parts. Up to the kink point—associated with some X^*—it reflects equation (18a). We sometimes refer to this component as "underlying" saving. The change in slope after X^* reflects equation (18b) and in particular the value of η. After the kink, the steeper slope recognizes the possibility that desired IRA saving in excess of the limit may be made up by increasing other saving. If $\eta = 1$, the difference between desired IRA saving and the limit L is the same as the difference between S and S_L. Thus the key parameter is η.[35] Equation (17), which describes the other saving of noncontributors, is essentially irrelevant in this specification and is not represented in the figure. In fact, in the GS specification, the saving of non-IRA contributors provides no information about the substitution between IRA and non-IRA saving.

An identical figure was used by Venti and Wise to describe their specification (e.g., fig. 4.2 in Venti and Wise 1991). Thus in spirit the two specifications are very similar. But here the similarity ends. The method used to identify the change in the slope of S after the IRA limit is reached is very different in the two approaches.

How is η identified? We show below that in the GS model specification a downward bias in the "underlying" other saving function for limit contributors is balanced by an upward bias in η. One may conclude therefore that the estimation procedure does not identify an η, the key behavioral parameter. Before turning to this matter, however, we consider how η might in principle be identified and what the estimated value might mean.

Assume that β—the effect of X variables on IRA contributions—is identified from equation (16). Then η is identified by variation in L. Since most variation in L is due to marital status, η is determined in large part by marital status. If the limit for married couples is $2,000 higher than the limit for single persons, for example, and if $\eta = 1$, married limit contributors should save $2,000 less in other financial assets than single persons. If this is not the difference, in principle, η would change accordingly. Of the 107 limit contributors in our estimation sample, 80 are married and 27 are single. This does not provide much evidence on which to base an estimate of η. In addition, the specification assumes that marital status does not influence other financial asset saving, except through the lower IRA spillover effect. This raises a further confounding issue. Marital status is not allowed to affect other saving directly, nor to affect IRA saving. Thus it only enters the equation for limit contributors. If, as most prior research suggests, marital status should properly be an explanatory variable in the underlying other saving equation, then, for limit contributors, η will pick up this effect, in addition to any spillover effect.

Although marital status is critical in the identification of η, GS add to the

35. As GS point out, the difference in the tax treatment of IRA and conventional saving could lead to values of η greater than one. E.g., if households wanted to reach a given asset goal by retirement age, the amount saved in conventional forms would have to be greater than the amount saved in an IRA.

specification some complexity, which also influences the estimated value of η. They allow η to be a function of covariates X, with $\eta = X\delta$, but they do not allow a constant term in the relationship. (The absence of a constant is associated with an important error in the GS interpretation of the results, and this issue is taken up below.) Now the specification for limit contributors becomes

$$X\gamma_2 + (X\delta)(X\beta) - (X\delta)L = X[\gamma_2 + (X\delta)\beta] - (X\delta)L.$$

In this case, the effect of marital status is allowed to depend on other covariates.[36]

To demonstrate the critical features of the GS model we begin with a simplified version, using only income as an explanatory variable. Then we proceed to the full GS specification. We use both a two-stage procedure that provides consistent estimates under the GS assumptions and the maximum likelihood procedure used by GS. All of our estimates are based on our replication of the GS sample, described in table 1.13. We use all the GS variable definitions as well as their procedure to assign limit and nonlimit contributor status.

A Two-Step Procedure

Suppose that equation (16) is estimated independently using a Tobit specification to obtain estimates of the β coefficients. The other saving equation is

$$(20) \qquad S = X\gamma_2 + \eta(X\hat{\beta} - L) * D + (\eta u * D + \varepsilon_2),$$

where D identifies limit contributors. Under the assumptions of the GS model, the expected value of S can be written as

$$E(S) = X\gamma + \eta(X\beta - L) * D + \eta E(u \mid u > L - X\beta) * D$$
$$+ E(\varepsilon_2 \mid u > L - X\beta) * D + E(\varepsilon_2 \mid -X\beta < u < L - X\beta) * (1 - D).$$

Using standard results, we can write

$$(21) \qquad E(u \mid u > L - X\beta) = \sigma_u \frac{\phi[(L - X\beta)/\sigma_u]}{1 - \Phi[(L - X\beta)/\sigma_u]} = \sigma_u\lambda_1,$$

$$(22) \qquad E(\varepsilon_2 \mid u > L - X\beta) = \rho\sigma_\varepsilon \frac{\phi[(L - X\beta)/\sigma_u]}{1 - \Phi[(L - X\beta)/\sigma_u]} = \rho\sigma_\varepsilon\lambda_2,$$

$$(23) \qquad E(\varepsilon_2 \mid -X\beta < u < L - X\beta)$$
$$= \rho\sigma_\varepsilon \frac{\phi[(L - X\beta)/\sigma_u] - \phi(-X\beta/\sigma_u)}{\Phi(-X\beta/\sigma_u) - \Phi[(L - X\beta)/\sigma_u]} = \rho\sigma_\varepsilon\lambda_3.$$

Under the assumptions of the GS model, consistent estimates can be obtained by estimating

36. The multiplication of terms in X is also likely to make identification tenuous.

Table 1.18 Values of η Estimated from Two-Step Procedure by Specification

	Variables			
Specification	In IRA and Other Saving Equations	In η	Estimated η	Standard Error
1	Income	Constant	−1.193	1.879
2	Income and NIFA	Constant	0.835	1.828
3	All *X*s excluding NIFA	Constant	−1.668	1.762
4	All *X*s including NIFA	Constant	0.103	1.712
5	Income	Income, no constant	−1.591[a]	
6	Income and NIFA	Income and NIFA, no constant	−0.413[a]	
7	All *X*s excluding NIFA	All *X*s excluding NIFA, no constant	−1.889[a]	
8	All *X*s including NIFA	All *X*s including NIFA, no constant	0.192[a]	
9	All *X*s excluding NIFA	All *X*s excluding NIFA, plus constant	−1.668[a]	
10	All *X*s including NIFA	All *X*s including NIFA, plus constant	0.103[a]	

[a]Evaluated at the mean of predicted η for limit contributors.

$$\begin{aligned} S &= X\gamma + \eta(X\beta + \sigma_u\lambda_1 - L) * D + D * \rho\sigma_\varepsilon\lambda_2 + (1 - D) * \rho\sigma_\varepsilon\lambda_3 \\ &= X\gamma + \eta(X\beta + \sigma_u\lambda_1 - L) * D + \rho\sigma_\varepsilon[D * \lambda_2 + (1 - D) * \lambda_3], \end{aligned}$$

where β and σ_u are estimated from the first-stage Tobit equation and γ, η, and $\rho\sigma_\varepsilon$ are estimated in the second stage.

We have estimated several specifications using this procedure, and the estimated values of η are reported in table 1.18. The equations use a variety of covariates *X*, ranging from income only to the full set of variables used by GS, and use several different specifications of η. There are two important features of these results. First, in the most inclusive specifications, η is small. The estimate is 0.10 in specification 4 and 0.192 in specification 8. Specification 8 is analogous to the GS specification. If only a constant is included in the set of explanatory variables η, the resulting estimate is $\eta = 0.103$. Second, the inclusion of 1983 non-IRA financial assets (NIFA) produces a large jump in the estimated value of η. This is a feature of both the two-stage procedure and the maximum likelihood procedure discussed next.

Why does the inclusion of the 1983 level of NIFA lead to such large changes in η? For illustration, we compare specifications 7 and 8. Non-IRA financial assets are included presumably to control for past saving behavior. In specification 8, aside from income, NIFA is the only statistically significant variable

Table 1.19 **Predicted Underlying Other Saving, Total Other Saving, and Actual Other Saving For Limit and Nonlimit Contributors, Based on Two-Step Estimates**

Contributor Type and Specification	Predicted Underlying Other Saving: $X\gamma$ Only (1)	Predicted Underlying Other Saving: $X\gamma$ Plus λ_2 and λ_3 (2)	Predicted Other Saving: $X\gamma$ Plus λ_2 and λ_3 and λ_1 (3)	Actual Other Saving (4)
Nonlimit				
Specification 7	−2,452	2,084	2,084	1,989
Specification 8	682	2,149	2,149	1,989
Limit				
Specification 7	759	10,782	2,455	3,089
Specification 8	−1,959	1,287	2,920	3,089

of the 11 variables in the non-IRA saving equation for IRA contributors. Its estimated coefficient is *negative,* −3.447 with a *t*-statistic of 9.429. Taken literally, this result says that the greater the level of non-IRA assets in 1983—controlling for age, income, and other variables—the *lower* the level of saving over the subsequent three years. Thus NIFA is clearly not serving as a control for past saving behavior. Instead, it seems apparent that the coefficient reflects enormous error in the measurement of 1983 NIFA. Recall that saving is inferred from 1983 and 1986 NIFA balances, using a variant of $S = \text{NIFA}_{1986} - \text{NIFA}_{1983}(1 + r)^3$. Thus any error in NIFA_{1983} will impart a negative bias to the coefficient on this variable, and in this case the measurement error is surely very large—large enough to more than offset the intended role of NIFA as a control for heterogeneity.

The effect of NIFA on η can be understood by considering the components of the specification that determines η. It is useful to recall that to a first order of approximation, η can be thought of as

$$\eta = [(\text{Actual other saving}) - (\text{Underlying other saving})]/(I^* - L),$$

where underlying other saving refers to other saving as a function of X before the IRA limit is reached. The important aspect of this formula is that, given actual other saving, if the prediction for underlying saving is arbitrarily low, η will be arbitrarily large to compensate for the low underlying saving. In particular, if predicted underlying saving for limit contributors is lower than the actual saving of limit contributors, the shortfall between underlying and actual saving can be bridged by a large value of η. We show that adding NIFA to the specification yields implausible estimates of underlying saving and corresponding large offsetting increases in η. Column (1) in table 1.19 shows the mean value of estimated $X\gamma$. Column (2) shows $X\gamma + \rho\sigma_\varepsilon[D * \lambda_2 + (1 - D) * \lambda_3]$ and represents the predicted value of *underlying* saving for limit and nonlimit contributors. Column (3) shows predicted values of other saving, accounting for the upper slope component of other saving for limit contributors.

It is easy to see from this table why the effect of NIFA is so large. Without NIFA (specification 7) the systematic component of underlying saving ($X\gamma$) for nonlimit contributors is predicted to be −2,452 and for limit contributors 759. When NIFA is added the predicted underlying saving for limit contributors is *reduced* from 759 to −1,959 and for nonlimit contributors is *increased* from −2,452 to 682. Including the λ_2 and λ_3 terms, without NIFA, predicted underlying saving for limit contributors is substantially higher than for nonlimit contributors (10,782 vs. 2,084). But when NIFA is added, predicted underlying saving of limit contributors is *reduced* from 10,782 to 1,287 and underlying saving for nonlimit contributors is increased somewhat from 2,084 to 2,149. With reference to figure 1.8, the underlying saving function for limit contributors is lowered. In particular, predicted underlying saving is now lower for limit than for nonlimit contributors, which is inconsistent with the prevailing heterogeneity concern, that is, that contributors want to save more in all forms than noncontributors and that limit contributors want to save more than contributors. This means that η, the slope of the portion above the kink point, must be increased to compensate for the low predicted value of underlying saving for limit contributors. Indeed, the difference is made up by the larger η. The average predicted value of total other saving for limit contributors is close to the actual average—2,920 versus 3,089. Taken literally, specification 7 says that limit contributors save more in non-IRA financial assets than nonlimit contributors, as might be expected. But specification 8, with NIFA included, says just the reverse, that limit contributors have a lower propensity than nonlimit contributors to save in other forms.

This feature of the specification is created by the large negative coefficient on NIFA, which seems clearly to reflect error in measurement and not, as intended, a control for saving propensity. Thus the error in measurement of this variable imparts substantial bias to the results. We show below that this feature of the specification applies equally to the joint maximum likelihood estimation.

Joint Maximum Likelihood Estimation

If the GS model specification is a correct representation of the data, then both the two-step procedure and joint maximum likelihood estimation provide consistent estimates of the model parameters.[37] This, of course, is not true if the specification does not capture the empirical regularities in the data. As with the two-step procedure, we begin by using only one X variable—income—and then expand the specification to include the full set of variables used by GS.

37. In appendix A of their paper, GS describe the components of the likelihood function used in their analysis. The third component for limit contributors is incorrect. The numerator of the first term should include $\hat{S}_1 - L$, but the L is not included. We assume that this is only a typographical error in the paper and that the likelihood function is in fact programmed correctly.

Table 1.20 **Joint Maximum Likelihood Estimates of η By Specification**

	Variables			
Specification	In IRA and Other Saving Equations	In η	Estimated η	Standard Error
A	Income	Constant	−0.790	0.180
B	Income and NIFA	Constant	4.644	0.887
C	All *X*s excluding NIFA	Constant	−1.468	1.115
D	All *X*s including NIFA	Constant	4.355	0.904
E	**All *X*s excluding NIFA**	**All *X*s, no constant**	**−0.209**[a]	
F	**All *X*s including NIFA**	**All *X*s, no constant**	**1.170**[a]	
G	All *X*s excluding NIFA	All *X*s, plus constant	−0.006[a]	
H	All *X*s including NIFA	All *X*s, plus constant	1.254[a]	
I	All *X*s excluding NIFA	All *X*s, no constant	−0.222[a]	
J	All *X*s including NIFA	All *X*s, no constant	1.116[a]	

Note: β is fixed at single-equation Tobit estimates, except in specifications I and J.
[a]Evaluated at the mean of the *X* values for limit contributors.

To emphasize the key features of the model, in most specifications, we fix the β parameters at those obtained in a single-equation Tobit estimate of the IRA equation.[38] The results are presented in table 1.20.

As with the two-step estimates, the results change dramatically when NIFA is included among the *X* variables. No matter what the model specification, the estimated η jumps wildly when NIFA is added. For example, when NIFA is added to the specification including all *X* variables but with η estimated as a constant, the estimated value of η jumps from −1.468 to 4.355 (specification D vs. C).

Although we were unable to match the GS sample precisely, specification J is the same specification used by GS, and specification I is the GS specification excluding NIFA. Key parameters of these specifications along with the GS estimates are summarized in table 1.21, for four key variables: income, debt, nonliquid assets, and NIFA. (None of the estimated coefficients on the six other variables included in the GS analysis is statistically different from zero in any of these relationships.) The model J parameter estimates are very close to the estimates presented by GS, although our estimate of η differs somewhat from the value obtained by GS—1.116 versus 1.85 reported by GS. The difference arises for two reasons. First, our estimation sample differs from the GS sample (107 vs. 117 limit contributors). Second, η is parameterized without a constant, and estimated values are extremely sensitive to even small changes in the *X* values that may arise from slight differences in the sample. (This feature of the estimation procedure is documented below.) The critical features

38. This has very little effect on the results. In specifications I and J in table 1.20, β is estimated jointly with all other model parameters.

Table 1.21 Model I without NIFA, Model J with NIFA (GS Equivalent), and GS Estimates: Selected Coefficients

	Noncontributor Other Saving Equation			Contributor Other Saving Equation			η Equation		
Variable	Without NIFA	With NIFA	GS Estimate	Without NIFA	With NIFA	GS Estimate	Without NIFA	With NIFA	GS Estimate
Income	5.654	5.638	6.012	**−0.963**	**7.765**	7.784	**−0.322**	**−2.773**	−3.170
	(0.952)	(1.243)	(1.026)	(1.026)	(1.505)	(1.391)	(0.693)	(0.821)	(0.836)
Debt	1.175	1.187	1.056	1.412	1.154	1.151	−0.735	−0.486	−0.466
	(0.126)	(0.142)	(0.248)	(0.332)	(0.356)	(0.373)	(0.248)	(0.255)	(0.192)
Nonliquid assets	−3.874	−3.866	−3.998	0.419	−0.120	−0.599	1.094	0.802	0.471
	(0.330)	(0.357)	(0.492)	(0.712)	(1.375)	(0.577)	(0.885)	(0.869)	(0.541)
NIFA		−0.001	0.077		**−3.512**	−3.686		**0.782**	0.985
		(0.045)	(0.059)		(0.281)	(0.402)		(0.182)	(0.235)

Note: Numbers in parentheses are *t*-statistics.

of the maximum likelihood estimates are the same as those of the two-step procedure.

Estimates for the key variables in table 1.21 indicate that the influence of NIFA is enormous. In the contributor saving equation—the underlying level of saving for IRA contributors that have not reached the limit—NIFA has a large and negative coefficient (our estimate is −3.512 and the GS estimate is −3.686). This suggests that a one standard deviation increase in the 1983 NIFA level (about $100,000) is associated with a *decrease* in saving of over $35,000 in the 1983–86 period! Apparently the measurement error in 1983 NIFA, from which the dependent variable is constructed, swamps any role NIFA might play as a control for heterogeneity.

On the other hand, when NIFA is added to the η equation the estimated coefficient is large and positive, offsetting the large negative effect in the underlying saving equation. The size of the coefficient (0.782 in our model J and 0.985 in GS) is implausibly large, implying that a one standard deviation increase in the 1983 NIFA level (about $100,000) will increase η by almost 10!

As above, we can understand better the effect of NIFA on η by considering predicted values of underlying saving with and without this variable in the specification. The appropriate predictions are shown in table 1.22. Without NIFA, predicted underlying saving of limit contributors is higher than that of nonlimit contributors (4,533 vs. 1,651). But when NIFA is added, the underlying saving of limit contributors is *lowered* from 4,533 to −760, leaving a large gap between underlying and actual other saving, which is 3,089. The underlying saving of nonlimit contributors is increased somewhat, from 1,651 to 1,713. Once again, to fit the actual saving data, the gap between underlying and actual saving of limit contributors is bridged by the large estimated value of η. In this case, predicted other saving of limit contributors is well above actual other saving—4,461 versus 3,089.[39] That is, when underlying saving is depressed, the slope η of S_L must be raised. With reference to figure 1.8: when S is reduced to S', S_L must be increased to S'_L. Or the measurement error in NIFA biases the underlying saving downward, and this is offset by an upward bias in η. Thus, at least in the presence of NIFA, no behavioral interpretation can be ascribed to the estimated value of η.

Although the addition of NIFA has an enormous effect on the other saving equation for contributors, the addition of this variable has virtually no effect on estimates in the other saving equation for families who do not contribute to an IRA, as shown in the first two columns of table 1.21.

In addition to the artificial increase in η caused by the introduction of NIFA, the GS parameterization of η—excluding a constant term—means that predicted changes in η with changes in X are likely to have little meaning. Without

39. GS do not report predicted other saving of limit and nonlimit contributors separately, but the predicted value for all contributors, which they do report, is far less than actual other saving of contributors ($806 vs. $2,184).

Table 1.22 **Predicted Underlying Other Saving, Total Other Saving, and Actual Other Saving for Limit and Nonlimit Contributors, Based on Maximum Likelihood Estimates**

	Predicted Underlying Other Saving			
Contributor Type and Specification	$X\gamma$ Only (1)	$X\gamma$ Plus λ_2 and λ_3 (2)	Predicted Other Saving: $X\gamma$ Plus λ_2 and λ_3 and λ_t (3)	Actual Other Saving (4)
Nonlimit				
Specification I				
(excludes NIFA)	704	1,651	1,651	1,989
Specification J				
(includes NIFA)	1,082	1,713	1,713	1,989
Limit				
Specification I				
(excludes NIFA)	1,082	4,533	3,298	3,089
Specification J				
(includes NIFA)	−2,160	−760	4,461	3,089

a constant term in the specification, the expected value of η, given X, is not captured by $X\hat{\delta}$ unless the constant is in fact zero.[40] Predictions of η vary wildly in response to small changes in X variables. Indeed, by judicious selection of X values, a wide range of results can be obtained. GS have highlighted the estimated values of η based on selected X values. For example, GS show values for a "typical 35-year-old," defined by particular X values. The "typical 35-year-old" does not have an IRA in 1983 and does not have an employer-provided pension. Their "predicted" value of η is 0.68. But if, in addition to the X values GS use, the person is defined to have an IRA and a pension, the value of η is -1.565. Based on other X values, almost any η could have been emphasized. Table 1.23 makes the possibilities clear. For each specified change in an X variable the table gives the change in η implied by *the GS estimates,* reported in their table 5. For each of the continuous variables, the indicated change in the X variable is approximately one standard deviation. Thus the GS estimates themselves yield implausible responses to changes in household characteristics.

As indicated above, estimation results may vary enormously based on the sample used in estimation. Since the results depend critically on the small number of limit contributors—117 in the GS estimates—any selection that changes this number can change the results enormously, especially given the vast variation in assets and saving. The reported estimates are based on a sample deleting households with inferred 1983–86 saving less than or greater than

40. Specifications G and H in table 1.20 are estimated including a constant term in η, as well as the X variables. Although the constant in these specifications is not significantly different from zero, it is measured extremely imprecisely and identification is tenuous.

Table 1.23 Change in η for Selected Changes in X, Based on GS Estimates

Change	Variable	Change[a]	Change in η
1	Age	Increase by 12 years	0.331
2	Income (3 years)	Increase by $120,000	−3.170
3	Pension	No to yes	−1.376
4	Education	Increase by 3 years	0.666
5	Family size	Increase by 1.5 persons	1.424
6	1983 NIFA	Increase by $100,000	9.850
7	Debt	Increase by $40,000	−1.864
8	Nonliquid assets	Increase by $200,000	0.942
9	IRA in 1983	No to yes	−0.869

[a]For each of the continuous variables the indicated change in the X variable is approximately one standard deviation.

$100,000. Although they do not report the η values with samples based on different saving thresholds, GS do report their estimates of the proportion of an increase in IRA saving, resulting from an increase in the IRA limit, that would be net new saving. Here are their examples, which document how sensitive their results are to the choice of a saving threshold.

Sample Saving Deletions	Net Saving (%)
More or less than ±$75,000	−17.5
More or less than ±$100,000	2.1
More or less than ±$200,000	−382.2

Based on our estimates of the GS model (including NIFA), small differences in this critical sample selection criterion yield very different values of η. Estimates for selection thresholds ranging from ±$50,000 to ±$200,000 are shown in figure 1.9. Not only is the variation great, but most of the estimates are very small, even including NIFA.

1.7.5 Summary

In our judgment, a descriptive summary of the data used by GS suggests very strongly that the contributions of participants to the IRA program between 1983 and 1986 represented largely new saving. It is also clear from these data that the typical IRA contributor had not been saving close to the typical annual IRA contribution of $2,300 per year. Simple predictions of 1986 non-IRA financial assets based on the 1983 cross-sectional relationship between age, income, and non-IRA financial assets (and accounting for differences in rate of return) bolster the message of the raw data, suggesting that most IRA contributions must have represented new saving. These "reality checks" would seem to be inconsistent with the GS results.

But GS frame their conclusions in terms of the effect of an increase in the IRA limit, arguing that virtually none of the increase in IRA contributions

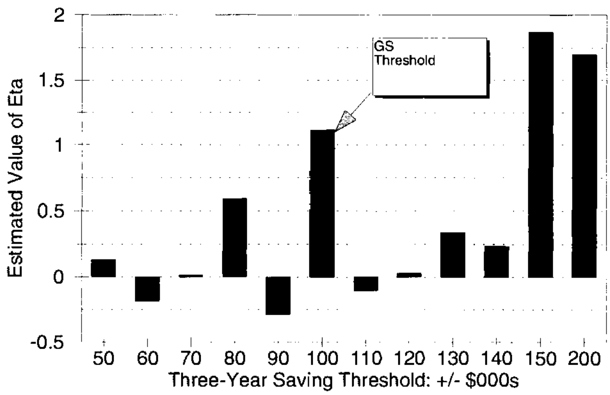

Fig. 1.9 Estimated η values by three-year saving threshold
Note: GS = Gale and Scholz (1994).

resulting from a limit increase would represent new saving. The summary data also suggest that this conclusion is inconsistent with the underlying data. A large fraction—60 to 85 percent—of annual IRA contributions are at a family or an individual IRA limit. Thus most of the increase in net saving that seems evident from the summary data must be attributed to persons who contributed at the limit. And it seems implausible to conclude that if the limit had been higher these people would not have increased their saving even more.

GS acclimatize the reader to their conclusion by highlighting the 1986 non-IRA financial assets of persons at a constructed IRA limit. GS must *infer* IRA contributions from 1983 and 1986 IRA balances, and from these inferred contributions, they infer limit status in the three consecutive years between 1983 and 1986. They classify 117 families in this group and conclude that about 22 percent of contributors are at this "constructed three-year limit." They point to the rather large non-IRA financial assets of this group, suggesting that because these families did have assets that could be transferred to an IRA, that is what they would do if the IRA limit were reached. Even with this unverified possibility in mind, the number GS highlight is misleading for several reasons: (1) While GS point to the assets of 22 percent of participants at a constructed limit, the proportion of contributions at either an individual or a family annual limit is three or four times this large. It is the assets of this much larger group that are relevant. (2) GS further exaggerate relevant non-IRA financial assets by including the assets of all families over age 25, including those over age 65, who are unlikely to contribute to an IRA. GS also use a very inclusive definition of financial assets, including assets that we believe are unlikely to be substituted for IRA assets. (3) Finally, GS emphasize 1986 asset levels. The 1986 data are relevant if one is drawing attention to what might be expected from a "future" increase in the IRA limit. But the 1986 number is at the same time misleading because there was a substantial increase in non-IRA financial assets of contributors during the 1983–86 period, during which IRA contributions were made. In considering the summary data, we emphasize 1983 assets, drawing attention to the fact that contributors had not been saving at the typical IRA annual contribution rate before the advent of the program. Indeed, the median level of the non-IRA financial assets of 1983 IRA contributors was only about 20 percent of the assets of inferred "three-year limit contributors" emphasized by GS. And the median 1983 non-IRA financial assets of the persons who contributed at an IRA limit during the 1983–86 period was probably less than 40 percent of the level emphasized by GS.

The formal GS results are not based on summary data, however, but rather on a complex estimation method. Having reproduced their estimation procedure and analyzed it closely, we conclude that the data provide little support for their conclusions. The value of η, the key substitution parameter reported by GS, is estimated with substantial bias because of measurement error in 1983 non-IRA financial assets. Furthermore, because the parameterization of η used by GS does not include a constant term, the reported variations in η by family attributes have no behavioral meaning whatsoever. These features of the GS estimation procedure, together with values of η that vary wildly with small changes in the sample used in estimation, mean that judicious choice of sample family attributes at which to evaluate η can produce virtually any result.

But can any specification of the GS model be given credence? In particular, can estimates that exclude NIFA and that estimate a constant η be viewed with confidence? We know that even in this case, the estimates vary widely depending on the sample selected for estimation. This is especially critical given that the key parameter η is essentially determined by limit contributors. Excluding NIFA, all specifications yield a negative value of η, which in many but not all instances is not significantly different from zero. We also find that although inclusion of NIFA changes the estimated value of η, it does not change the residual correlation between IRA and other non-IRA financial asset saving. When all X variables other than NIFA are used, the estimated correlation is essentially zero. And when NIFA is added the correlation remains essentially zero. Thus NIFA does not seem to serve its intended goal of providing further control for heterogeneity but does impart substantial bias. It seems evident that the results provide little support for a positive η, and thus little support for substitution of non-IRA financial asset saving for IRA saving. But the results obtained without NIFA may also be so fragile as to provide unreliable evidence of no effect.

1.8 Conclusions

Over the past several years we have undertaken a series of analyses of the effect of IRA and 401(k) contributions on net personal saving. We have summarized this research here, together with additional results. Saver heterogeneity is the key impediment to determining the saving effect of these plans, and in our studies we have used different methods to address this issue. We have organized the discussion according to the method used to correct for heterogeneity. We emphasize that no single method can provide sure control for all forms of heterogeneity. Taken together, however, we believe that the analyses address the key complications presented by heterogeneity. In our view, the weight of the evidence, based on the many nonparametric approaches discussed here, provides strong support for the view that contributions to both IRA and 401(k) plans represent largely new saving. Some of the evidence is directed to the IRA program, other evidence to the 401(k) plan, and some of the evidence to both plans jointly. We believe that the evidence is strong in all cases.

Several other investigators have used different methods to consider the effect of these retirement saving programs on personal saving and in some cases have reached very different conclusions from ours. Thus we have devoted particular effort to explaining why different approaches, sometimes based on the same data, have led to different conclusions. In some instances, we believe that the limitations of the methods used by others have undermined the reliability of the results. Particular attention is devoted to a paper by Gale and Scholz (1994) that is widely cited as demonstrating that IRAs have no saving effect. Based on our analysis of the data used by Gale and Scholz, including calculations based on a replication of their model, we find that their conclusions are at odds with the patterns of asset holding and saving in the raw data.

Appendix A
Cohort and Period Effects

Although the data make clear that the timing of housing market trends and trends in mortgage debt do not coincide, the apparent market, or "period," effects in the housing market complicate the estimation of precise housing data cohort effects. For example, the mortgage data by cohort between 1984 and 1987 show very small cohort effects. But if only the 1987 and 1991 data are considered, it would appear that there were substantial cohort effects. But it is likely that these within-cohort changes reflect period effects that can show up as cohort effects. This is illustrated in figure 1A.1, which shows mortgage debt for the C42 and C48 cohorts. Looking at the 1984 and 1987 data only, no cohort effect is apparent, as shown by the narrow line. But if the data are fitted by cohort, including the 1991 values, there appears to be a cohort effect. If a 1991 year effect were accounted for, the apparent cohort effect would essentially disappear. Suppose the increase in mortgage debt between 1987 and 1991 resulted from the Tax Reform Act of 1986. Then the data might be interpreted this way: The 1986 legislation induced an increase in the debt of the

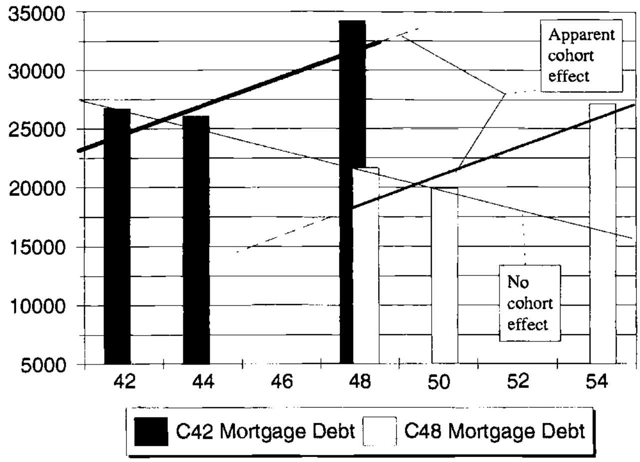

Fig. 1A.1 Period vs. cohort effect?

younger cohorts between ages 44 and 48. For the older cohort, the increase was induced between ages 50 and 54. This period effect raises the debt of the younger cohort at age 48 and the debt of the older cohort at age 54. This creates the illusion of a cohort effect, illustrated by the vertical distance between the two heavy lines. In this case, the apparent cohort effect is really a period effect and should be distinguished from a true cohort effect. But with so few observation per cohort, we have not tried to do that.[41]

Appendix B
Rate-of-Return Effects

The potential magnitude of the rate-of-return effect can be approximated as follows: Consider the predicted (by eq. [13]) assets of persons aged $a+3$ in 1983. The average asset level predicted by the 1983 cross-sectional regression could—if the appropriate data were known—be decomposed this way:

$$A_{a+3} = A_a(1 + \underline{r})^3 + S(A_a, Y; \underline{r}). \tag{B1}$$

41. As is well known, it is not possible to distinguish age effects from time effects within the same cohort. But if age effects are assumed not to depend on cohort—as is assumed when the effect of age is parameterized as in eq. (11) in n. 16—then, in principle, time effects can be estimated. In effect, shifts that correspond to changes between years are interpreted as year effects rather than within-cohort age effects. It is seems evident from the summary graphs that the data is somewhat more complicated than this, because the year effects seem in some cases to have a differential effect on young versus older cohorts.

Here A_a represents the assets that persons aged $a+3$ had three years earlier. The r_- pertains to the rate of return that applied during the three years preceding 1983. New saving, $S(A_a, Y; r_-)$, is some function of income over the three-year period. The r_- in this function recognizes that people might save less if r is higher, because the gain from existing assets is greater. Similarly, the assets of persons aged a could be decomposed as

$$A_a = A_{a-3}(1 + r_-)^3 + S(A_{a-3}, Y; r_-). \tag{B2}$$

The difference between assets at $a+3$ and assets at a can then be described as

$$A_{a+3} - A_a = (A_a - A_{a-3})(1 + r_-)^3 + S(A_a, Y; r_-) - S(A_{a-3}, Y; r_-). \tag{B3}$$

Accounting for the change in income between 1983 and 1986, the difference predicted by equation (14) could be decomposed this way. But the rate of return that determines the difference in assets with a three-year age difference in 1983 may be different from the rate that obtained in the next three years. To predict over the next three years, we would want to use the rate that applied during those years. In this case, we would have

$$A_{a+3} - A_a = (A_a - A_{a-3})(1 + r^+)^3 + S(A_a, Y; r^+) - S(A_{a-3}, Y; r^+), \tag{B4}$$

where r^+ is the rate of return that applied between 1983 and 1986. The difference between the two predictions is

$$\begin{aligned}(A_a - A_{a-3})[(1 + r^+)^3 - (1 + r_-)^3] + [S(A_a, Y; r^+) - S(A_a, Y; r_-)] \\ - [S(A_{a-3}, Y; r^+) - S(A_{a-3}, Y; r_-)].\end{aligned} \tag{B5}$$

Assume that the last two terms approximately cancel. Then the difference is given by the first term. Consider, for example, the AAA bond rate in 1980–82 versus 1983–85. The average during the first period was 13.30, and during the second period 12.04. The second component of this term is thus negative. So correction for the rate of return would *reduce* the estimated increase. The return on other assets may give a different sign, but it seems evident that differences in the rate of return could not account for much of the difference between 1983 and 1986 non-IRA financial assets.

Appendix C

Table 1C.1 Conditional Median Asset Balances by 401(k) Eligibility and Income

Asset Category and Eligibility Status	Income <10	10–20	20–30	30–40	40–50	50–75	>75
				A. Results for 1991 (1991 dollars)			
Total financial assets							
Eligible	2,033	4,045*	5,499*	8,683*	14,470*	26,093*	51,080*
Not eligible	1,378	1,997	2,558	3,256	6,206	10,080	29,842
Non–IRA-401(k) assets							
Eligible	538	1,138	1,500	2,835*	4,724	8,699*	18,188*
Not eligible	663	1,063	1,411	2,052	4,250	5,437	17,000
401(k) assets							
Eligible	1,171	1,008	1,211	2,092	3,073*	4,833*	14,300*
Not eligible	0	0	0	0	0	0	0
IRA assets							
Eligible	0	0	0	0	0	1,437	6,029*
Not eligible	0	0	0	0	0	978	2,882
				B. Results for 1987 (1987 dollars)			
Total financial assets							
Eligible	2,061	2,404	4,206*	9,062*	12,588*	24,384*	57,348*
Not eligible	1,581	1,902	2,624	4,605	6,726	14,108	30,971
Non–IRA-401(k) assets							
Eligible	591	1,029	1,711	3,398*	5,663*	10,776*	24,044*
Not eligible	799	1,004	1,554	2,904	4,246	8,462	20,383
401(k) assets							
Eligible	456	474	607	895	1,255*	1,755*	8,056*
Not eligible	0	0	0	0	0	0	0
IRA assets							
Eligible	0	0	0	0	0	3,564	9,064*
Not eligible	0	0	0	0	0	2,770	4,950
				C. Results for 1984 (1984 dollars)			
Non–IRA-401(k) assets							
Eligible	561	1,042	1,988	3,861*	5,027	11,683*	28,824*
Not eligible	754	1,138	1,746	3,076	5,082	10,846	21,485
IRA assets							
Eligible	0	0	0	0	0	2,250	3,181
Not eligible	0	0	0	0	0	1,484	2,084

Source: Poterba, Venti, and Wise (1995).

*Difference between eligibles and noneligibles is statistically significant at the 95 percent confidence level.

Table 1C.2 **Summary of Cohort Trends in Home Value, Home Mortgage, and Home Equity**

	Cohort														
Age	C42	C44	C46	C48	C50	C52	C54	C56	C58	C60	C62	C64	C66	C68	C70
	A. Mean Home Value Cohort Data														
42	73,740														
44	65,869	72,052													
46		65,675	70,479												
48	80,294		65,741	71,084											
50		80,102		66,224	71,091										
52			83,570		68,135	75,685									
54				81,275		68,374	73,289								
56					80,829		70,817	71,229							
58						79,290		70,456	68,968						
60							78,785		69,293	68,201					
62								81,552		70,564	70,842				
64									85,807		68,824	68,734			
66										79,437		69,097	65,632		
68											80,365		73,018	58,343	
70												82,451		70,354	62,770
72													79,269		64,279
74														84,061	
76															84,089
	B. Mean Home Mortgage Cohort Data														
42	26,737														
44	26,066	24,380													
46		23,955	23,415												
48	34,244		21,311	21,648											
50		31,920		19,919	19,219										
52			29,717		18,139	16,901									

54				27,106		16,267	14,462								
56					23,021		14,262	12,316							
58						20,710		12,482	11,855						
60							17,234		10,721	10,466					
62								15,876		9,822	8,857				
64									15,022		6,683	7,217			
66										12,935		5,823	6,572		
68											9,550		5,150	5,009	
70												12,811		5,150	3,683
72													13,505		5,044
74														10,189	
76															5,766

C. Mean Home Equity Cohort Data

42	47,003														
44	39,802	47,672													
46		41,720	47,064												
48	46,050		44,430	49,436											
50		48,182		46,305	51,872										
52			53,853		49,996	58,784									
54				54,169		52,107	58,827								
56					57,808		56,554	58,914							
58						58,580		57,974	57,113						
60							61,551		58,572	57,735					
62								65,675		60,742	61,984				
64									70,784		62,141	61,516			
66										66,502		63,273	59,059		
68											70,815		67,867	53,334	
70												69,640		65,204	59,087
72													65,764		59,235
74														73,873	
76															78,323

Note: All values are in 1991 dollars.

Table 1C.3 **Conditional Median Asset Balances by 401(k) Eligibility and Income, 1984**

Asset Category and Eligibility Status	Income Interval[a]						
	<10	10–20	20–30	30–40	40–50	50–75	75+
Non–IRA-401(k) financial assets							
Eligible	147	550	1,454	2,404	4,732	7,901	31,485
Not eligible	220	545	1,034	2,043	3,748	7,059	21,778
Difference	−73	6	420*	361*	985*	842*	9,708*
Net non–IRA-401(k) financial assets							
Eligible	−1,288	−651	302	716	2,815	6,241	22,068
Not eligible	−607	−348	130	775	2,080	5,208	17,802
Difference	−681	−304	172	−60	735*	1,034*	4,267*
Home equity							
Eligible	11,377	16,210	17,486	26,138	31,101	43,185	65,232
Not eligible	12,384	13,725	16,007	25,123	30,833	34,348	52,746
Difference	−1,007	2,486	1,478	1,014	268	8,837*	12,486*

Net non–IRA-401(k) financial assets less mortgage debt							
Eligible	−5,329	−5,370	−4,328	−12,021	−16,737	−16,163	−15,560
Not eligible	−4,468	−3,670	−4,386	−7,810	−14,317	−18,665	−10,264
Difference	−861	−1,700	58	−4,212*	−2,420*	2,503*	−5,297*
Net non–IRA-401(k) financial assets plus home equity							
Eligible	11,594	16,616	21,371	28,136	38,799	53,060	104,748
Not eligible	11,293	14,398	18,632	28,461	36,327	44,462	83,338
Difference	301	2,218	2,739	−325	2,472	8,598*	21,410*
IRA							
Eligible	0	0	0	0	0	1,083	5,100
Not eligible	0	0	0	0	0	0	4,200
Difference	0	0	0	0	0	1,083	900

Note: Evaluated at the medians of age, marital status, and education.

[a]Income intervals are indexed to 1987 dollars.

*Difference between eligibles and noneligibles statistically significant at the 95 percent confidence level.

Table 1C.4 **Conditional Median Asset Balances by 401(k) Eligibility and Income, 1991**

Asset Category and Eligibility Status	Income Internal[a]						
	<10	10–20	20–30	30–40	40–50	50–75	75+
Total financial assets							
Eligible	2,323	2,473	5,443	10,263	19,628	37,166	70,954
Not eligible	563	1,030	1,630	3,863	5,523	15,109	42,953
Difference	1,760*	1,443*	3,813*	6,400*	14,105*	22,057*	28,002*
Net total financial assets							
Eligible	1,102	1,073	2,464	7,554	17,022	34,726	67,878
Not eligible	−483	−57	370	2,307	3,652	11,597	39,218
Difference	1,585	1,130*	2,094*	5,247*	13,370*	23,129*	28,660*
Non–IRA-401(k) financial assets							
Eligible	488	616	1,325	3,128	5,692	11,487	21,414
Not eligible	264	544	1,086	2,504	3,973	8,944	27,120
Difference	224	72	239*	624*	1,719*	2,543*	−5,709*
Net non–IRA-401(k) financial assets							
Eligible	−491	−262	−95	1,089	3,094	8,838	18,925
Not eligible	−327	−142	116	907	1,968	5,667	26,909
Difference	−164	−120	−211	182	1,126*	3,171*	−7,984*
Home equity							
Eligible	8,105	10,973	13,937	20,293	25,400	44,839	76,176
Not eligible	9,210	11,044	11,863	18,751	27,132	34,834	58,420
Difference	−1,105	−71	2,073*	1,541	−1,732	10,005*	17,756*

Net total financial assets plus housing equity							
Eligible	14,509	14,150	20,538	32,875	49,361	84,511	151,834
Not eligible	9,185	13,121	15,106	28,502	38,139	60,945	122,341
Difference	5,324	1,029	5,432*	4,373*	11,222*	23,566*	29,499*
Net non–IRA-401(k) financial assets less mortgage debt							
Eligible	−7,112	−7,534	−10,790	−23,701	−41,359	−39,584	−63,414
Not eligible	−5,800	−5,689	−7,795	−22,799	−29,345	−38,977	−16,320
Difference	−1,312	−1,845*	−2,995*	−902	−12,014*	−607	−47,094*
Net non–IRA-401(k) financial assets plus home equity							
Eligible	9,030	10,361	14,017	24,168	34,682	61,358	108,290
Not eligible	8,059	11,557	13,522	25,468	35,275	56,360	105,924
Difference	971	−1,196	495	−1,300	−593	4,998*	2,366
IRA							
Eligible	0	0	0	0	0	0	0
Not eligible	0	0	0	0	0	0	
Difference	0	0	0	0	0	0	0
401(k)							
Eligible	405	431	1,164	2,072	4,053	6,942	15,832
Not eligible	0	0	0	0	0	0	0
Difference	405	431	1,164*	2,072*	4,053*	6,942*	15,832*

Note: Evaluated at the medians of age, marital status, and education.

[a]Income intervals are indexed to 1987 dollars.

*Difference between eligibles and noneligibles statistically significant at the 95 percent confidence level.

Table 1C.5 **Mean Demographic Characteristics by Income Interval, Eligibility, and Year**

Eligibility Status and Characteristic	Income Interval[a] <10	10–20	20–30	30–40	40–50	50–75	75+	All
				1984				
401(k) Eligible								
Income	6,641	16,276	25,418	34,733	44,772	59,170	97,115	41,393
Age	40.8	41.5	41.5	40.9	40.5	43.0	46.2	41.8
Education	12.9	12.1	13.0	13.5	14.1	14.4	15.2	13.6
Married	0.28	0.42	0.64	0.75	0.86	0.93	0.98	0.76
Number	31	140	243	306	212	262	96	1,290
401(k) Not eligible								
Income	6,562	15,163	25,009	34,528	44,539	58,600	96,893	29,276
Age	40.8	40.0	39.4	41.3	41.9	43.2	45.4	40.8
Education	11.9	12.0	12.6	12.9	13.4	14.1	14.8	12.7
Married	0.23	0.45	0.66	0.84	0.88	0.90	0.91	0.65
Number	712	1,820	1,875	1,388	798	645	188	7,426
				1991				
401(k) Eligible								
Income	7,415	15,594	24,876	34,573	44,695	59,565	91,499	38,895
Age	41.0	40.2	40.0	41.4	41.9	43.0	43.6	41.4
Education	12.5	12.2	13.2	13.7	14.2	14.7	15.6	13.7
Married	0.21	0.37	0.55	0.75	0.82	0.90	0.93	0.69
Number	84	543	807	784	564	633	266	3,681
401(k) Not eligible								
Income	6,631	14,949	24,525	34,521	44,623	59,506	92,111	26,533
Age	40.0	40.2	40.4	40.6	41.3	43.3	45.3	40.7
Education	11.6	12.3	13.1	13.5	14.1	14.8	15.3	12.9
Married	0.28	0.41	0.63	0.78	0.84	0.90	0.89	0.58
Number	853	1,928	1,487	880	507	404	171	6,230

[a]The income intervals are indexed to 1987 dollars.

References

Attanasio, Orazio, and Thomas De Leire. 1994. IRAs and household saving revisited: Some new evidence. NBER Working Paper no. 4900. Cambridge, Mass.: National Bureau of Economic Research, October.

Bernheim, B. Douglas. 1994. Comments on: Do saving incentives work? *Brookings Papers on Economic Activity* 1:152–66.

Bickel, Peter J., Eugene A. Hammel, and J. William O'Connell. 1975. Sex bias in graduate admissions: Data from Berkeley. *Science* 87:398–404.

Burman, Leonard, Joseph Cordes, and Larry Ozanne. 1990. IRAs and national savings. *National Tax Journal* 43:259–83.

Engelhardt, Gary V. 1996a. House prices and homeowner saving behavior. *Regional Science and Urban Economics* 26:313–36.

———. 1996b. Tax subsidies and household saving: Evidence from Canada. *Quarterly Journal of Economics* 61:1237–68.

Engen, Eric M., and William G. Gale. 1995. Debt, taxes and the effects of 401(k) plans on household wealth accumulation. Washington, D.C.: Brookings Institution. Unpublished.

Engen, Eric, William G. Gale, and John Karl Scholz. 1994. Do saving incentives work? *Brookings Papers on Economic Activity* 1:85–151.

Feenberg, Daniel, and Jonathan Skinner. 1989. Sources of IRA saving. In *Tax policy and the economy,* vol. 3, ed. L. Summers, 25–46. Cambridge, Mass.: MIT Press.

Gale, William G. 1995. The effects of pensions on wealth: A re-evaluation of theory and evidence. Washington, D.C.: Brookings Institution. Mimeograph.

Gale, William G., and John Karl Scholz. 1994. IRAs and household saving. *American Economic Review* 84:1233–60.

Hoynes, Hilary, and Daniel McFadden. 1994. The impact of demographics on housing and non-housing wealth in the United States. NBER Working Paper no. 4666. Cambridge, Mass.: National Bureau of Economic Research, March.

Hubbard, R. Glenn. 1984. Do IRAs and Keoghs increase saving? *National Tax Journal* 37:43–54.

Hubbard, R. Glenn, and Jonathan S. Skinner. 1995. The effectiveness of saving incentives: A review of the evidence. Hanover, N.H.: Dartmouth College. Mimeograph.

Joines, Douglas H., and James G. Manegold. 1995. IRAs and saving: Evidence from a panel of taxpayers. Los Angeles: University of Southern California. Mimeograph.

Kotlikoff, Laurence J., and David A. Wise. 1985. Labor compensation and the structure of private pension plans: Evidence for contractual versus spot labor markets. In *Pensions, labor, and individual choice,* ed. D. Wise. Chicago: University of Chicago Press.

———. 1988. Pension backloading, wage taxes, and work disincentives. In *Tax policy and the economy,* vol. 2, ed. L. Summers. Cambridge, Mass.: MIT Press.

———. 1989. Employee retirement and a firm's pension plan. In *The economics of aging,* ed. D. Wise. Chicago: University of Chicago Press.

Papke, Leslie E. 1995. Does 401(k) introduction affect defined benefit plans? *Proceedings of the National Tax Association–Tax Institute of America,* 173–77. Columbus, Ohio: NTA-TIA.

Papke, Leslie E., Mitchell Petersen, and James M. Poterba. 1996. Did 401(k) plans replace other employer provided pensions? In *Advances in the economics of aging,* ed. D. Wise. Chicago: University of Chicago Press.

Poterba, James M., Steven F. Venti, and David A. Wise. 1994a. 401(k) Plans and tax-deferred saving. In *Studies in the economics of aging,* ed. D. Wise. Chicago: University of Chicago Press.

———. 1994b. Targeted retirement saving and the net worth of elderly Americans. *American Economic Review* 84:180–85.
———. 1995. Do 401(k) contributions crowd out other personal saving? *Journal of Public Economics* 58:1–32.
Shefrin, Hersh M., and Richard H. Thaler. 1988. The behavioral life-cycle hypothesis. *Economic Inquiry* 26:609–43.
Skinner, Jonathan. 1994. Housing and saving in the United States. In *Housing markets in the United States and Japan,* ed. Y. Noguchi and J. Poterba. Chicago: University of Chicago Press.
———. 1996. Is housing wealth a sideshow? In *Advances in the economics of aging,* ed. D. Wise, 241–68. Chicago: University of Chicago Press.
Skinner, Jonathan, and Daniel Feenberg. 1990. The impact of the 1986 Tax Reform Act on personal saving. In *Do taxes matter? The economic impact of the Tax Reform Act of 1986,* ed. J. Slemrod. Cambridge, Mass.: MIT Press.
Thaler, Richard H. 1990. Saving, fungibility, and mental accounts. *Journal of Economic Perspectives* 4:193–205.
Thaler, Richard H., and H. M. Shefrin. 1981. An economic theory of self-control. *Journal of Political Economy* 89:392–406.
Venti, Steven F., and David A. Wise. 1986. Tax-deferred accounts, constrained choice, and estimation of individual saving. *Review of Economic Studies* 53:579–601.
———. 1987. IRAs and saving. In *The effects of taxation on capital accumulation,* ed. M. Feldstein. Chicago: University of Chicago Press.
———. 1990. Have IRAs increased U.S. saving? Evidence from consumer expenditure surveys. *Quarterly Journal of Economics* 55:661–98.
———. 1991. The saving effect of tax-deferred retirement accounts: Evidence from SIPP. In *National saving and economic performance,* ed. B. D. Bernheim and J. B. Shoven. Chicago: University of Chicago Press.
———. 1992. Government policy and personal retirement saving. In *Tax policy and the economy,* vol. 6, ed. J. Poterba. Cambridge, Mass.: MIT Press.
———. 1995a. Individual response to a retirement saving program: Results from U.S. panel data. *Ricerche Economiche* 49:235–54.
———. 1995b. RRSPs and saving in Canada. Mimeograph.
———. 1997. The wealth of cohorts: Retirement saving and the changing assets of older Americans. In *Public policy toward pensions,* ed. S. Schieber and J. Shoven. Cambridge, Mass.: MIT Press.
Wise, David A. 1987. Individual retirement accounts and saving. In *Taxes and capital formation,* ed. M. Feldstein. Chicago: University of Chicago Press.

Comment David Laibson

What does behavioral economics have to say about the savings incentives debate? That is the question that David Wise assigned me when he asked me to

David Laibson is assistant professor of economics at Harvard University and a faculty research fellow of the National Bureau of Economic Research.

This work has been supported financially by the National Science Foundation (SBR-95-10985) and the MacArthur Foundation. The author is grateful to Yu-Chin Chen and Xiaomeng Tong for excellent research assistance, and to Richard Zeckhauser for thoughtful and copious comments.

be the official discussant of his survey paper coauthored with Jim Poterba and Steven Venti.[1] I imagine that behavioral economics still does not have a lot of name recognition, so it may be helpful to start by defining my terms. In practice, behavioral economists tend to emphasize experimental evidence,[2] validation of modeling assumptions, synergies between psychology and economics, and skepticism regarding strong rationality assumptions. Why might this perspective be helpful?

First, let me address the economists who believe that the IRA-401(k) debate is unresolved (i.e., economists who believe that it is not yet empirically clear to what extent IRAs and 401(k)s increase aggregate savings). Where would these economists direct future research? More tests with heroic identification assumptions? Efforts to gather new data (e.g., data on pension offsets)? Truly random eligibility experiments? Such proposals have merit, but I propose another alternative: test the theoretical microfoundations of the purported efficacy of IRAs and 401(k)s. First, those microfoundations must be identified. But the microfoundations will not be found in mainstream economic models, as Engen, Gale, and Scholz (1994) have argued in theory and shown with simulations. These simulations imply that during the first decades after an IRA or 401(k) is introduced, most of the investment in the asset will reflect asset shifting. Even after 30 years, the IRA or 401(k) generates no new net capital accumulation. By contrast, psychological models suggest a host of reasons why IRAs and 401(k)s—particularly 401(k)s—can work to quickly generate higher levels of net savings. Identifying these psychological factors—for example, commitment and social modeling—and directly testing their impact may help to move the IRA-401(k) debate forward.

Other economists believe that Poterba, Venti, and Wise have already won the IRA-401(k) debate. These economists should still be interested in the behavioral perspective for three practical reasons suggested by Bernheim (1996). First, understanding why IRAs and 401(k)s work is necessary for welfare analysis. For example, mainstream models do not measure the possible welfare gains that would arise if a defined contribution plan helped a consumer overcome a self-control problem. Second, knowing why IRAs and 401(k)s work is necessary for policy analysis. Proposals to improve or expand these instruments can be analyzed ex ante only if we have theories about which features of these instruments drive their effectiveness. For instance, are the savings effects driven by marginal interest effects on rates or self-control effects arising from

1. Engen, Gale, and Scholz (1996a, 1996b) have already written critiques of the Poterba, Venti, and Wise manuscript. Bernheim (1996) and Hubbard and Skinner (1996) have also published surveys that evaluate the Poterba, Venti, and Wise research program. My comments build on the insights of Thaler (1994) and Bernheim (1996), who also relate the behavioral economics research program to the savings incentives debate.

2. However, behavioral economists do not view experimental evidence as a substitute for field data. Behavioral economists value both kinds of evidence and recognize that high-quality field data always trump laboratory evidence.

penalties and commitment? Third, understanding why IRAs and 401(k)s work may generate general insights about consumer behavior that will be applicable in domains far removed from the savings incentives literature. For example, evidence on self-control problems in the savings domain may have implications for self-regulation in other areas, such as teenage promiscuity, procrastination in the workplace, and drug addiction, and may suggest successful public or private interventions to combat these problems.[3]

Finally, a third group of economists believe that Engen, Gale, and Scholz are basically right. These economists need not read on, as standard economic theory is consistent with the Engen, Gale, and Scholz results. However, the behavioral perspective may be compatible with their findings. As I will point out below, behavioral models sometimes predict that 401(k)s and IRAs are not efficacious. Perhaps the most widely discussed example is target behavior. If savers have a target level of retirement savings, they will respond to higher after-tax interest rates by saving less. Such target behavior has been formally documented in the daily actions of cabdrivers deciding when to end their workdays. On days when the shadow wage is high, cabdrivers work shorter hours (Camerer et al. 1997). In the analysis that follows I discuss behavioral arguments that both support and oppose the claim that IRAs and 401(k)s raise net national savings.

I now turn to the general set of insights that behavioral economics brings to the IRA-401(k) discussion. The body of these comments discusses four categories of behavioral phenomena: bounded rationality, self-control problems and dynamic inconsistency, peer group sensitivities, and overoptimistic beliefs. Three caveats apply to this classification. First, in identifying these categories, I have looked for psychological primitives—foundational theories of cognition and motivation. Important behavioral phenomena like mental accounts will be discussed in relation to their associated primitives—in this case bounded rationality and self-control problems. Second, I imagine that some of the primitives may be interpreted as derivative of others. For example, anomalously high sensitivities to peer group behavior may result from bounded rationality: "It's hard to calculate the optimal policy, but it's easy to mimic my neighbor." Third, my list of primitives is undoubtedly incomplete. The psychology literature is vast, and it would be foolhardy to believe that behavioral economists have already identified all of the relevant phenomena in that enormous parallel literature. This observation should reinforce the obvious conclusion that the behavioral research program is just getting started.

The first section discusses the four behavioral phenomena identified above. The second analyzes empirical strategies motivated by this behavioral theory. The third section concludes.

3. See O'Donoghue and Rabin (1997a, 1997b) for a discussion of procrastination and incentives.

Four Behavioral Phenomena

Bounded Rationality

A number of commentators (e.g., Zeckhauser 1986; Thaler 1994; Bernheim 1994, 1995) have noted that classical arguments for rationality fall short when applied to the life cycle savings problem. Retirement savers do not get a second chance to correct or learn from their mistakes. There is little recourse for the 75-year-old who finds herself short of assets. Moreover, we should expect such mistakes to be made often since the lifetime accumulation problem is extraordinarily complex.[4] Expert advice only further complicates the picture. The advice offered by professional financial planners bears little resemblance to the prescriptions of economic theory (Bernheim 1994). Expert advice is usually organized around simple rules of thumb—for example, save 15 percent of your income each year. But simple rules of thumb do not come close to approximating the accumulation dynamics predicted by current optimization theory. Finally, suboptimal retirement savings does not drive the consumer out of the market. How can an arbitrageur exploit someone else's mistaken decision to underaccumulate for retirement?

These arguments, and many others like them in the articles cited above, suggest that lifetime consumption behavior may be poorly approximated by the predictions of the rational actor model. But it is not at all clear what alternative to adopt. There are no well-studied and generally applicable psychological replacements for the rational choice model.[5] Although behavioral economics cannot offer a general replacement for the rational choice model, there are two widely held behavioral principles that are useful for thinking about choices in a boundedly rational world: simplification and salience. I will discuss each of these areas in turn.

Consumers simplify their decision problems by adopting rules of thumb and heuristics (e.g., never go into debt). One particularly important heuristic is to

4. Assuming only three state variables, Hubbard, Skinner, and Zeldes (1995) were compelled to use the Cornell supercomputer to compute optimal lifetime consumption rules.

5. Reinforcement models, in which decision makers repeat actions that have had relatively high payoffs in the past, have only just begun to be studied empirically by economists (e.g., Roth and Erev 1995; Erev and Roth 1997) and extended to incorporate limited forward-looking properties (Camerer and Ho 1996). Moreover, most of this work analyzes subject choices in repeated games, where subjects receive payoffs at the end of every round. Even if we had confidence in such models in the repeated game setting, the reinforcement model would be difficult to apply to the savings context since it is not at all clear why savings is rewarding in the short run. How can reinforcement models explain savings activity that generates payoffs that are not realized for several decades? Other leading psychological models of choice, like Herrnstein's "melioration and matching" paradigm suffer from similar critiques about the timing of rewards (see Rachlin and Laibson, 1997, for a survey). Melioration predicts that decision makers always choose the activity with the highest instantaneous felicity, ignoring future consequences. Such radically myopic psychological models seem to be inherently ill equipped for application to savings behavior. Such models could explain savings activity if the primary return to saving were the intrinsic rewards of the process, but that seems unlikely.

partition the decision problem into simpler subproblems, just as economists work with partial equilibrium analysis despite spillovers beyond the arena studied. Such partitioning generates separate "mental accounts" for day-to-day consumption needs and retirement accumulation (Thaler and Shefrin 1981; Thaler 1985) and associated anomalous wealth nonfungibilities (Thaler 1990). For example, consumers may feel that it is appropriate to consume a wage windfall (e.g., overtime pay), but inappropriate to increase consumption at all in response to an equally large capital gain windfall in their retirement account.

The second behavioral principal is information salience. Salient information and rewards disproportionately influence behavior (e.g., see the surveys by McArthur 1981; Taylor and Fiske 1978). Current rewards are more salient than future rewards. When one spends $15 on a CD it is hard to imagine the retirement consumption sacrifice that this current splurge necessitates. This creates a bias for current consumption over retirement savings.

Instruments like 401(k)s and IRAs engage these two behavioral principles. For example, a retirement account should be viewed as a mental account that has been coded as off-limits to current consumption. Thaler (1994) points out that investments in these accounts may raise long-term accumulation levels, even if the original investment was generated by asset shifting. These long-run effects arise because the asset is moved from an account with a high marginal propensity for consumption (MPC)—say a demand deposit—to a retirement account with a low MPC. More generally, providing retirement accounts like 401(k)s and IRAs creates new "basins of attraction" for savings that would otherwise eventually be consumed. Hence, mental accounting arguments suggest that 401(k)s and IRAs raise accumulation levels. Similarly, salience effects generally strengthen the case for 401(k)s and IRAs. These savings instruments directly (through their existence) and indirectly (through the associated activities of firms and coworkers) focus attention on retirement needs.

Other decision-making heuristics, however, imply that special savings instruments will lower savings. For example, target saving—as discussed above—implies that higher returns would lower current contributions. Likewise, a fixed savings rule—save 15 percent of my gross income every year—implies that 401(k)s and IRAs lower net national savings, due to the tax break associated with these instruments.

Simplification and salience effects coexist in the behavioral mechanisms discussed by Ross and Nisbett (1991). These leading social psychologists emphasize that behavior is powerfully influenced by subtle situational factors that they call "channel factors." The success of behavioral interventions "depends not just on persuading people to hold particular beliefs, or even to develop particular intentions, but also on facilitating a specific, well-defined path or channel for action" (Ross and Nisbett 1991, 47). The channel factors that prove most effective drastically simplify the action space available to the decision maker. This simplification occurs on two fronts. An effective channel factor first narrows and simplifies the choice set and second strongly discourages the option of postponing the decision. Hence, the channel factor makes the action salient.

Ross and Nisbett cite numerous examples of effective channel factors, including the techniques used during the 1940s U.S. war bond campaigns. As Cartwright (1949) reports, these campaigns proved most successful when they identified a clear action (buy an extra $100 bond) and a specific time at which to implement that action (buy it when the solicitor at your workplace asks you to sign up). As Cartwright notes, "The essential function of solicitation lay in the fact that it required the person to make a decision" (1949, 266). The Home Shopping Network also seems to have adopted these lessons. Consider the flashing phone number that accompanies an announcement that a discount will expire in x minutes. Or consider a TV charity telethon that solicits an immediate phone call to lift the fund-raising drive over some "critical" threshold: "Once you take that initial step by making the phone call, they take care of everything. In other words, they create a behavioral channel that very reliably transforms a long-standing but vague intention, or even a momentary whim, into a completed donation" (Ross and Nisbett 1991, 48).[6]

Channel factors lower the action hurdle for cognitively overloaded decision makers. If decision makers were sufficiently cognitively sophisticated, channel factors such as arbitrary deadlines, or a narrowing of the choice set, would not work. By contrast, boundedly rational decision makers would be expected to respond to the simpler and immediate choices associated with channel factors.

For my purposes, the most important attribute of channel factors is their close correspondence to 401(k)s, and to a lesser degree IRAs. A 401(k) simplifies the choice set by (1) identifying a narrow range of contribution rates (zero to 15 percent of income), (2) identifying a specific salient contribution level (e.g., the highest contribution rate at which the firm will match the employee), and (3) identifying a narrow set of investment options (e.g., five mutual funds). In addition, 401(k)s provide a sign-up deadline, discouraging the option of postponing action. The salary reduction forms are distributed to employees in early December and are due back to the fringe benefits office by the end of the month. In this way 401(k)s transform a vague intention to save into a series of concrete events (automatic withdrawals). Analogous arguments can be made for IRAs. This analysis emphasizes that IRAs and 401(k)s increase accumulation by facilitating and focusing attention on the retirement accumulation decision.

Self-Control and Dynamically Inconsistent Preferences

There is a substantial gap between actors' long-term intentions and their short-term actions. When two rewards are both due far away in time, decision makers will generally choose the larger later reward over the smaller earlier

6. For another interesting example of a channel factor at work see Leventhal, Singer, and Jones (1965).

reward (e.g., the $100 restaurant meal in 150 weeks is preferred to the $90 meal in 145 weeks). But, when both rewards are brought forward in time, preference tilts toward the earlier reward (the $90 meal in one week is preferred to the $100 meal in six weeks). Experiments of this form have been done with a wide range of real rewards, including money, durable goods, fruit juice, sweets, video rentals, relief from noxious noise, and access to video games.[7] Such reversals should be well understood by academics, whose long-term intentions, "revising that paper over the summer," often conflict with their actual choices, "I had too many interruptions and didn't end up finding time for it until January." Invariably, our long-term intentions to delay gratification are at least partially defeated by our day-to-day temptations to seize immediate payoffs.

This gap between intentions and actions also arises in the life cycle savings domain. Three types of evidence highlight this gap. First, popular and professional financial advice emphasizes the need to commit oneself to a savings plan. For example, "Use whatever means possible to remove a set amount of money from your bank account each month before you have a chance to spend it" (Rankin 1993). Or "If you wait until the end of the month to put your money into investments, you'll probably encounter months in which there's nothing left over. To keep this from happening, pay yourself first by having money set aside from each paycheck into a savings account or 401(k) plan" (American Express 1996). Financial planners routinely advise their clients to cut up credit cards, to put credit cards in a safe deposit box, to use excess withholding as a forced savings device,[8] and to use Christmas clubs, vacation clubs, and other low-interest, low-liquidity goal clubs to regulate savings flows. American consumers deposited their holiday savings in roughly 10 million Christmas club accounts in 1995 (Simmons Market Research Bureau 1996). Such commitment devices are only appealing because consumers recognize their self-control problems.

Self-reports about preferred consumption paths provide a second type of evidence for the gap between intentions and actions. Consumers generally report a preference for flat or rising real consumption paths, even when the real interest rate is zero (Barsky et al. 1997; see Loewenstein and Sicherman, 1991, for related evidence). But consumers actually implement downward-sloping consumption paths when they are not effectively liquidity constrained (Gourinchas and Parker 1996). Moreover, the typical baby boomer household is saving at one-third the rate required to finance a standard of living during retirement comparable to the standard of living that the household enjoys today (Bernheim 1995).[9] Hence, U.S. consumers report a preference for rising consump-

7. See Solnick et al. (1980), Navarick (1982), Millar and Navarick (1984), King and Logue (1987), Kirby and Herrnstein (1995), Kirby and Marakovic (1995, 1996), Kirby (1997), and Read et al. (1996). See Ainslie (1992) for a partial review of this literature.

8. For interesting evidence on the relatively widespread use of intentional overwithholding, see Shapiro and Slemrod (1995).

9. Bernheim points out that this calculation assumes a best-case scenario. He assumes that all savings is available for retirement and that mortality rates, tax rates, social security benefits, Medicare benefits, and health care costs do not change during the next 50 years.

tion profiles—holding the net present value constant—but actually implement profiles that are downward sloping.

Finally, comparison of target and actual savings rates provides a third type of evidence for the gap between intentions and actions. Baby boomers report a median target savings rate of 15 percent and a median actual savings rate of 5 percent (Bernheim 1995). Baby boomers apparently understand that they save less than they should.

Numerous authors have used multiple-self frameworks to formally model the gap between intentions and actions (e.g., Thaler and Shefrin 1981; Schelling 1984; Hoch and Loewenstein 1991; Akerlof 1991; Ainslie 1992; Laibson 1996, 1997; O'Donoghue and Rabin 1997a, 1997b). These models highlight the contest between the short-run desire for instantaneous gratification and the long-run desire to be patient. IRAs and 401(k)s provide a set of commitment technologies that enable and encourage the present self to lock in choices that ensure that one's long-term, patient interests are heeded in the future. For example, 401(k)s compel consumers to set up an automatic deposit system; changes to the preset deposit levels are sometimes difficult or impossible to make on short notice. Assets in IRAs and 401(k)s are partially protected from splurges since preretirement withdrawals from these accounts generally face a 10 percent penalty. Finally, for limit contributors, withdrawals cannot be redeposited, implying that the consumer is penalized by both the 10 percent penalty and the loss of future tax deferrals.

Laibson (1996) shows that an appropriate combination of penalties and tax deferrals will implement the first-best consumption path for a multiple-self consumer with a "hyperbolic" discount function—a discount function characterized by short-term impatience and long-term patience. This work also demonstrates that currently enacted penalty and subsidy magnitudes are approximately optimal with respect to the calibrated model. Moreover, if consumers have hyperbolic preferences, the welfare gains associated with 401(k) availability are quite large—approximately equal to one year of output. However, this theoretical work needs to be generalized to more realistic economic environments that allow for uncertainty and assume the availability of other preexisting commitment devices, like defined benefit pensions.

What do these self-control models predict when 401(k)s are introduced into an economy in which self-control mechanisms already exist? Might consumers simply shift their assets or their marginal savings from preexisting self-control assets—for example, an illiquid asset like home equity or a defined benefit pension plan—to the new 401(k)? Perhaps mental accounts provide enough implicit self-control to make 401(k)s redundant. A 401(k) might not provide any additional commitment if a sufficient array of commitment technologies are already available in its absence. In such cases, 401(k) availability might lower savings, since the 401(k) would simply have an income effect without any corresponding marginal impact on the capacity for commitment.

However, 401(k)s probably provide better commitment opportunities than other widely discussed commitment mechanisms. Consider home equity;

401(k)s are often harder to borrow against and better diversified than home equity, making 401(k)s a more desirable retirement savings instrument for an actor with a self-control problem. Moreover, 401(k)s may also be more effective than mental accounts; 401(k)s create external penalties that are far more forceful than the psychic costs that regulate mental accounting rules. Mark Twain tried to use mental accounts to limit himself to one cigar per day: "I was getting cigars *made* for me—on a yet larger pattern. . . . Within the month my cigar had grown to such proportions that I could have used it as a crutch" (Twain [1899] 1906, 10).[10] W. C. Fields (n.d.) also had trouble effectively implementing mental accounting rules. Fields viewed alcohol as nothing more than a snakebite remedy, "which I always keep handy." He only permitted himself a drink "after first being bitten by a snake . . . which I also keep handy." Mental accounts may be far too labile to provide meaningful self-discipline. Other commitment mechanisms, like defined benefit pension plans, and other illiquid assets should be evaluated as possible substitutes for 401(k)s. The 401(k) will increase net savings to the extent that 401(k)s provide new, more effective, and more valued commitment technologies relative to these preexisting retirement instruments.

Finally, it is useful to revisit the discussion of channel factors in light of the self-control issues raised in this subsection. I have already noted that 401(k)s and IRAs reduce the complexity of the accumulation decision. In addition, 401(k)s and IRAs should be interpreted as channel factors that help would-be savers overcome self-control problems, especially procrastination.

Like the war bond solicitation techniques analyzed by Cartwright (quoted above), 401(k)s and IRAs impose decision-making deadlines. For procrastinators who would rather postpone any difficult or unpleasant task until tomorrow, such deadlines may make an important difference in their outcomes. Ross and Nisbett conclude that the most effective interventions are "channel factors that facilitate the link between positive intentions and constructive actions" (1991, 227). Whether the behavioral hurdle is problem complexity or a self-control problem like procrastination, 401(k)s and IRAs serve as canonical channel factors that make it easier to do the right thing.

Peer-Group Sensitivities

"When trying to get people to change familiar ways of doing things, social pressures and constraints exerted by the informal peer group represent the most . . . powerful inducing force than can be exploited to achieve success" (Ross and Nisbett 1991, 9). Myriad studies have shown that social modeling can have a disproportionate impact on outcomes. For example, Borgida and Nisbett (1977) gave undergraduates mean course evaluations based on ratings of students who had already taken the courses. This information did not influence

10. The Twain quote and the Fields quote that follows were brought to my attention by Ainslie (1992).

subsequent course choices. By contrast, brief face-to-face comments about the courses had a substantial impact. Rushton and Campbell (1977) showed that requests for blood donation pledges that were successful 25 percent of the time in the absence of any social model produced a positive response 67 percent of the time when an unknown confederate made a pledge just before the subject was asked. In addition, none of the subjects in the no-model condition showed up to give blood, while half of the subjects who agreed to give blood in the model condition ultimately did.[11] "The lesson is among social psychology's most important ones. When we want people to translate their positive intentions into equally positive actions, and when exhortations and reasoned appeals seem to be of limited effectiveness, a little social demonstration can be invaluable" (Ross and Nisbett 1991, 222–23).[12]

Social demonstration effects interact with 401(k)s in two ways. First, 401(k)s increase social learning. Contrast a firm in which all workers invest on their own to one in which all workers invest in the same 401(k) plan. In the 401(k) firm, the workers face similar narrow choice sets, making it easier to learn from each others' experiences. Hence, the 401(k) effectively coordinates all of the employees' investment decisions and enhances learning externalities. Moreover, in the 401(k) firm, dialogue about the investment decision is more likely to be formally and informally encouraged at the workplace, thereby facilitating social learning and increasing the salience of savings choices and the attention devoted to such decisions.

Second, 401(k)s increase social competition in the savings domain. Frank (1985) summarizes a wide range of evidence that actors care about their relative social ranking. Such competition is more likely in domains where choices are easily compared, like consumption. In a firm with a 401(k), workers can also compare their retirement savings with those of other workers. The workers share a common savings benchmark—the contribution rate—and a workplace norm that is likely to encourage discussion about savings choices. Such communications augment interworker competition in the savings domain, generating a predicted increase in accumulation rates. However, this increase is mitigated by the fact that the competition is likely to be focused exclusively on the 401(k) accumulation choice, generating an incentive for asset shifting out of other less public savings categories.

Overoptimism

When subjects evaluate their past performance, future prospects, and attributes, they consistently exhibit self-enhancing beliefs. Although there exists substantial controversy over the source of this bias—the biased beliefs may be

11. Rushton and Campbell's results need to be replicated with a larger sample. They report results for 35 subjects, but only 8 of these subjects were in the no-model condition.

12. Other dramatic social demonstration effects have been documented by Sherif (1937), Asch (1951, 1952, 1955, 1956), Lewin (1952), Rohrer et al. (1954), Jacobs and Campbell (1961), Bryan and Test (1967), and Aronson and O'Leary (1983).

"motivated," like wishful thinking, or they may be unmotivated cognitive errors—the bias itself is well documented. Only depressed subjects appear to have correctly calibrated beliefs (Taylor and Brown 1988).

In one commonly used test of self-enhancing beliefs, researchers ask subjects to rate themselves relative to a comparison group. For example, Svenson (1981) asks subjects to evaluate their skill and safety as drivers in comparison to the other subjects in the study; 49 percent of the subjects reported that they were above the 70th percentile in skill and 68 percent of the subjects reported that they were above the 70th percentile in safety. Weinstein (1980) asks subjects to evaluate whether they were more or less likely than their peers to experience a set of positive and negative life events (e.g., starting salary > $15,000 or not finding a job for six months). The beliefs reflected a self-enhancing bias for 88 percent of the life events. Studies like these have been replicated dozens of times.[13]

A small set of papers have evaluated these biases in settings where real rewards were at stake. For example, Ito (1990) analyzes the forecasts made by foreign exchange experts employed by Japanese firms. Forecasters whose firms benefit from depreciations tend to forecast a weaker yen than forecasters whose firms benefit from an appreciation. Kidd and Morgan (1969) and Kidd (1970) report the forecasts made by plant engineers regarding completion times for plant repairs. Actual completion times usually fell outside of the 99 percent confidence intervals estimated by the engineers. This bias persisted during the study period, despite repeated educational initiatives and the establishment of incentives for accurate reporting. Lovallo and Camerer (1996) conduct a laboratory experiment in which subjects decide whether to enter a market in which they compete based on their answers to trivia questions. The subjects repeatedly exhibit overentry, generating negative average payoffs across entrants.

Such biases seem to influence the retirement accumulation decision as well. Bernheim (1995) reports that the typical baby boomer expects his or her standard of living in retirement to be about the same as it is today. But Bernheim's calculations suggest that this subjective belief is unwarranted. As discussed above, the typical baby boomer household is saving at one-third the rate required to finance a standard of living during retirement comparable to the one enjoyed today.

If consumers are overoptimistic, 401(k)s will raise their retirement accumulation levels. Consider a consumer who overoptimistically forecasts too few negative wealth shocks (e.g., car or home repairs, medical and dental bills). In the absence of a 401(k), the overoptimistic consumer will forecast a level of

13. The literature on self-enhancing beliefs is quite large. Some of the more prominent experimental papers include those of Marks (1951), Irwin (1953), Fischhoff and Beyth (1975), Langer (1975), Miller (1976), Snyder, Stephan, and Rosenfield (1976), Stephan, Rosenfield, and Stephan (1976), Larwood and Whittaker (1977), Sicoly and Ross (1977), Riess et al. (1981), Zakay (1983), Alicke (1985), Brown (1986), Campbell (1986), and Perloff and Fetzer (1986). For helpful reviews, see Zuckerman (1979) and Taylor and Brown (1988).

savings that she will not be likely to attain. Such consumers may end up generating negligible accumulations. However, in the presence of a 401(k), such consumers will "lock in" a high savings rate at the beginning of each year. When the inevitable negative income shocks occur, the consumer will be more likely to cut consumption rather than savings. Such effects will be weakened to the extent that the consumer can either dissave from non-401(k) forms of financial wealth or readily reduce her 401(k) contribution rate.

Field Data Tests: A Few Suggestions

The behavioral perspective on 401(k)s suggests numerous empirical strategies. Some of these have already been explored. For example, Bayer, Bernheim, and Scholz (1996) find that participation in and contributions to voluntary savings plans are significantly higher when employers offer retirement seminars. Of course, perfectly rational (and fully informed) consumers would not be affected by such initiatives. Bayer et al. also find that *written* materials, such as newsletters and summary plan descriptions have no effects on savings, providing support for social psychology theories that emphasize the central role of social demonstration and group participation. Bernheim and Garrett (1996) find complementary evidence that financial education at the workplace increases household retirement accumulation levels.

Other sources of variation should be used to test behavioral hypotheses. For example, many of the commitment properties of 401(k) plans, discussed above, vary across plans. Some plans enable participants to quickly and frequently fine-tune their contribution levels, while other plans make such changes difficult or effectively impossible to implement. Borrowing rules also differ across plans, with variation arising in the consumption categories for which borrowing is allowed (e.g., tuition, medical expenses, home purchase, cars, vacations), as well as the simplicity or speed of the borrowing procedure.[14] As one pension consultant describes it, in certain cases "all employees do is punch a couple of numbers into the phone and a check magically appears" (quoted in Schultz 1995). Behavioral theories predict that plans with weaker commitment properties will tend to engender less long-run wealth accumulation. However, there may be a trade-off here. Extremely low levels of flexibility may discourage contributions in the first place.

Default features of 401(k) plans provide another useful source of variation. Many plans at large firms now make plan participation the default assignment. Moreover, within this set of firms, the default contribution levels vary substan-

14. BancOne recently developed a program to give 401(k) participants credit cards with which they could borrow against their 401(k) accumulations. Such a simple borrowing procedure would dramatically undermine the commitment properties of this instrument. Perhaps this is why the proposal was opposed by U.S. Representative Charles Schumer, who introduced a bill to restrict such cards, foreshadowing BancOne's decision to cancel the program ("BankOne drops Credit Card" 1996).

tially. Differences in the default assignment should be used to test the behavioral hypotheses that boundedly rational consumers tend to take the path of least resistance, or that consumers fear the consequences of changes, or weigh heavily the regret of errors of commission versus those of omission. Samuelson and Zeckhauser (1988) call such propensities status quo bias.

Social demonstration effects may also vary in measurable ways across firms. Some types of workers have relatively few opportunities for interemployee communication—for example, mail and package delivery workers and interstate truck drivers—while other workers have substantial opportunities for interaction. In addition, some firms organize savings discussion groups among their employees. Finally, some firms may actually encourage cross-worker savings comparisons by publicizing the distribution of their employees' contribution rates. An inexpensive experiment could be run along these lines, by actually asking a treatment group of firms to publicize this information at the workplace.

There may also be important sources of systemic variation in the tax-deferred instruments under study. For example, the Canadian retirement savings system adopted a new set of rules in 1992: workers who did not make the maximum allowable contribution in a given year could now carry forward the difference, enabling them to contribute more than the maximum in subsequent years.[15] Standard economic theory suggests that this policy change should increase steady state contribution levels (since the choice set has been expanded), while behavioral theories, which emphasize self-control problems and procrastination, would predict a deterioration in asset accumulation.

Beyond institutional and environmental variation, it may also be productive to exploit variation in household demographics. Households whose heads have relatively low levels of education will be more sensitive to many of the behavioral effects described above. Social demonstration and learning effects will be strongest for households who do not have extensive financial knowledge and preexisting active investment strategies. Likewise households with limited budgeting experience and lots of "unpredictable" expenses—for example, young households with children or new home owners—may be most susceptible to the overoptimism biases discussed above and hence will be most likely to increase their accumulation rate as a result of 401(k) availability.

The behavioral approach also suggests that empiricists focus more attention on little-studied psychological variables like the gap between savings intentions and savings outcomes (Bernheim 1995). Is this gap smaller for 401(k) eligible savers? Is this gap smaller for employees who receive free financial advice at the workplace? Standard economic theories cannot explain the existence of this gap, let alone its likely variation with environmental variables. By contrast, behavioral theories are based on the existence of such gaps and have much to say about the mechanisms that open and close them.

15. I am indebted to Gary Engelhardt for pointing out this set of issues to me.

This short list of empirical examples is guided by the unifying principle that it may be possible to specifically test the behavioral microfoundations of IRA and 401(k) efficacy. Such tests will be useful in determining whether—and, if the answer is affirmative, why—these instruments work.

Conclusion

Most of the behavioral analysis that I have reviewed implies that tax-deferred retirement instruments will raise net national savings. However, I want to conclude with four notes of caution. First, as I have repeatedly emphasized, behavioral arguments sometimes work against IRA and 401(k) efficacy (e.g., target saving).

Second, almost all of the behavioral effects reviewed above will be at least partially offset by asset shifting. For example, even if 401(k)s provide an excellent commitment device for actors with poor self-control, 401(k)s will only raise accumulation if this commitment device is not a close substitute for pre-existing commitment devices like home equity or mental accounts. Likewise, even if 401(k)s generate social demonstration or competition effects, which elevate contribution rates, this may simply draw away accumulation from other less public savings categories.

Third, many of the strongest behavioral arguments in support of tax-deferred investment vehicles apply only to 401(k)s. For example, IRAs generate less commitment, since they are rarely funded with a preannounced automatic deposit scheme. IRAs function less well as a channel factor, since they are not associated with workplace solicitation. IRAs generate fewer peer group effects, since they are less likely to be discussed with coworkers. Finally, IRAs benefit less from overoptimism effects than do 401(k)s, since 401(k) allocations are preannounced before the resolution of uncertainty.

Behavioral economics emphasizes learning, which leads to a fourth note of caution. In a simple laboratory experiment, subjects are usually given a chance to participate in mock trials of a game before any rewards are actually at stake or any data is recorded. Once the game is played "for real," the experimenters almost always repeat the game for several rounds—usually at least 10. In most games, the play at round 1 looks very little like the play 10 rounds later. It takes subjects a long time to learn how to play, despite the extraordinary simplicity of almost all laboratory experiments.

By comparison, the 401(k) experiment is effectively little more than 10 years old. Moreover, most of the empirical data that Poterba, Venti, and Wise have analyzed to date is from the first half of this experiment, when relatively sophisticated investors presumably represented a large share of the participant pool. Note that these sophisticated investors would be the least likely to exhibit many of the behavioral effects outlined in this essay. Hence, the Poterba-Venti-Wise results may be biased *against* finding 401(k) efficacy. On the other hand, whatever behavioral effects have arisen may weaken over time, as investors eventually learn how to shift assets optimally, or learn how to subvert the commitment properties of 401(k)s. Will savers eventually grow quite comfortable borrowing against their 401(k)s, just as they have grown more comfortable taking out home equity credit lines? Further complicating the picture, the 401(k) experiment has coincided with over a decade of abnormally high equity returns; 401(k) popularity could depend on a booming stock market. It is probably necessary to conclude that the existing empirical analysis of 401(k) effectiveness may tell us little about steady state responses to this new asset category.

Nevertheless, the prospects for 401(k)s and similar tax-deferred retirement instruments seem bright. Social psychology research has identified a core set of features of successful behavioral interventions. The 401(k) seems to have been designed by someone who intuitively or formally understood those lessons.

References

Ainslie, George. 1992. *Picoeconomics: The strategic interaction of successive motivational states within the person.* Cambridge: Cambridge University Press.

Akerlof, George. 1991. Procrastination and obedience. *American Economic Review Papers and Proceedings* 81:1–19.

Alicke, Mark D. 1985. Global self-evaluation as determined by the desirability and controllability of trait adjectives. *Journal of Personality and Social Psychology* 49:1621–30.

American Express. 1996. A commonsense guide to personal money management.

Aronson, Elliot, and Michael O'Leary. 1983. The relative effectiveness of models and prompts on energy conservation: A field experiment in a shower room. *Journal of Environmental Systems* 12:219–24.

Asch, S. E. 1951. Effects of group pressures upon the modification and distortion of judgment. In *Groups, leadership, and men,* ed. H. Guetzkow. Pittsburgh: Carnegie Press.

———. 1952. *Social psychology.* New York: Prentice-Hall.

———. 1955. Opinions and social pressure. *Scientific American,* November, pp. 31–35.

———. 1956. Studies of independence and conformity: A minority of one against a unanimous majority. *Psychological Monographs* 70 (9, Whole no. 416).

Banc One drops credit card tied to 401(k). 1996. *New York Times,* 26 December.

Barsky, Robert B., F. Thomas Juster, Miles S. Kimball, and Matthew D. Shapiro. 1997. Preference parameters and behavioral heterogeneity: An experimental approach in the Health and Retirement Study. *Quarterly Journal of Economics* 112:537–80.

Bayer, Patrick J., B. Douglas Bernheim, and John Karl Scholz. 1996. The effects of financial education in the workplace: Evidence from a survey of employers. NBER Working Paper no. 5655. Cambridge, Mass.: National Bureau of Economic Research, July.

Bernheim, B. Douglas. 1995a. Do households appreciate their financial vulnerabilities? An analysis of actions, perceptions, and public policy. In *Tax policy for economic growth in the 1990s,* 1–30. Washington, D.C.: American Council for Capital Formation.

———. 1994b. Personal saving, information, and economic literacy: New directions for public policy. In *Tax policy for economic growth in the 1990s,* 53–78. Washington, D.C.: American Council for Capital Formation.

———. 1996. Rethinking saving incentives. Stanford Economics Working Paper no. 96–009. Stanford, Calif.: Stanford University.

Bernheim, B. Douglas, and Daniel M. Garrett. 1996. The determinants and consequences of financial education in the workplace: Evidence from a survey of households. NBER Working Paper no. 5667. Cambridge, Mass.: National Bureau of Economic Research, July.

Borgida, Eugene, and Richard E. Nisbett. 1977. The differential impact of abstract vs. concrete information on decisions. *Journal of Applied Social Psychology* 7:258–71.

Brown, Jonathan D. 1986. Evaluation of self and others: Self-enhancement biases in social judgments. *Social Cognition* 4:353–76.

Bryan, J. H., and M. A. Test. 1967. Models and helping: Naturalistic studies in aiding behavior. *Journal of Personality and Social Psychology* 6:400–407.

Camerer, Colin, Linda Babcock, George Loewenstein, and Richard Thaler. 1997. Labor supply of New York City cabdrivers: One day at a time. *Quarterly Journal of Economics* 112:407–42.

Camerer, Colin, and Teck-Hua Ho. 1996. Experience-weighted attraction learning in games: A unifying approach. Pasadena: California Institute of Technology. Working paper.

Campbell, Jennifer D. 1986. Similarity and uniqueness: The effects of attribute type, relevance, and individual differences in self-esteem and depression. *Journal of Personality and Social Psychology* 50:281–94.

Cartwright, Dorwin. 1949. Some principles of mass persuasion: Selected findings of research on the sale of United States War Bonds. *Human Relations* 2:253–67.

Engen, Eric M., William G. Gale, and John Karl Scholz. 1994. Do saving incentives work? *Brookings Papers on Economic Activity* 1:85–180.

———. 1996a. The effects of tax-based saving incentives on saving and wealth. NBER Working Paper no. 5759. Cambridge, Mass.: National Bureau of Economic Research.

———. 1996b. The illusory effects of saving incentives on saving. *Economic Perspectives* 10:113–38.

Erev, Ido, and Alvin E. Roth. 1997. Modeling how people play games: Reinforcement learning in experimental games with unique, mixed strategy equilibria. Pittsburgh, Pa.: University of Pittsburgh. Working paper.

Fields, W. C. n.d. *The temperance lecture.* Proscenium Records.

Fischhoff, Baruch, and Ruth Beyth. 1975. "I knew it would happen": Remembered probability of once-future things. *Organizational Behavior and Human Performance* 13:1–16.

Frank, Robert H. 1985. *Choosing the right pond.* New York: Oxford University Press.

Gourinchas, Pierre-Olivier, and Jonathan A. Parker. 1996. Consumption over the life-cycle. Cambridge: Massachusetts Institute of Technology. Mimeograph.

Herrnstein, Richard J. 1997. *The matching law: Papers in psychology and economics.* Cambridge, Mass.: Harvard University Press.

Hoch, Stephen J., and George Loewenstein. 1991. Time-inconsistent preferences and consumer self-control. *Journal of Consumer Research* 17:429–507.

Hubbard, R. Glenn, and Jonathan S. Skinner. 1996. Assessing the effectiveness of saving incentives. *Economic Perspectives* 10:73–90.

Hubbard, Glenn, Jon Skinner, and Stephen Zeldes. 1995. Precautionary saving and social insurance. *Journal of Political Economy* 103:360–99.

Irwin, Francis W. 1953. State expectations as functions of probability and desirability of outcomes. *Journal of Personality* 21:329–35.

Ito, Takatoshi. 1990. Foreign exchange rate expectations: Micro survey data. *American Economic Review* 80:434–49.

Jacobs, R. C., and D. T. Campbell. 1961. The perpetuation of an arbitrary tradition through several generations of a laboratory microculture. *Journal of Abnormal and Social Psychology* 62:649–58.

Kidd, John B. 1970. The utilization of subjective probabilities in production planning. *Acta Psychologia* 34:338–47.

Kidd, John B., and J. R. Morgan. 1969. A predictive information system for management. *Operational Research Quarterly* 20:149–70.

King, G. R., and A. W. Logue. 1987. Choice in a self-control paradigm with human subjects: Effects of changeover delay duration. *Learning and Motivation* 18:421–38.

Kirby, Kris N. 1997. Bidding on the future: Evidence against normative discounting of delayed rewards. *Journal of Experimental Psychology* 126:54–70.

Kirby, Kris N., and R. J. Herrnstein. 1995. Preference reversals due to myopic discounting of delayed reward. *Psychological Science* 6:83–89.

Kirby, Kris N., and Nino N. Marakovic. 1995. Modeling myopic decisions: Evidence for hyperbolic delay-discounting within subjects and amounts. *Organizational Behavior and Human Decision Processes* 64:22–30.

———. 1996. Delay-discounting probabilistic rewards: Rates decrease as amounts increase. *Psychonomic Bulletin & Review* 3:100–104.

Laibson, David I. 1996. Hyperbolic discount functions, undersaving, and savings policy. NBER Working Paper no. 5635. Cambridge, Mass.: National Bureau of Economic Research.

———. 1997. Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics* 112:443–77.

Langer, Ellen J. 1975. The illusion of control. *Journal of Personality and Social Psychology* 32:311–28.

Larwood, Laurie, and William Whittaker. 1977. Managerial myopia: Self-serving biases in organizational planning. *Journal of Applied Psychology* 62:194–98.

Leventhal, Howard, Robert Singer, and Susan Jones. 1965. Effects of fear and specificity of recommendation upon attitudes and behavior. *Journal of Personality and Social Psychology* 2:20–29.

Lewin, Kurt. 1952. Group decision and social change. In *Readings in social psychology,* ed. G. E. Swanson, T. M. Newcomb, and E. L. Hartley, 459–73. New York: Holt.

Loewenstein, George, and Nachum Sicherman. 1991. Do workers prefer increasing wage profiles? *Journal of Labor Economics* 9:67–84.

Lovallo, Dan, and Colin Camerer. 1996. Overconfidence and excess entry: An experimental approach. California Institute of Technology Working Paper no. 975. Pasadena: California Institute of Technology.

Marks, Rose W. 1951. The effect of probability, desirability, and "privilege" on the stated expectations of children. *Journal of Personality* 19:332–51.

McArthur, Leslie Zebrowitz. 1981. What grabs you? The role of attention in impression formation and causal attribution. In *Social cognition: The Ontario symposium,* vol. 1, ed. E. T. Higgins, C. P. Herman, and M. P. Zanna, 201–46. Hillsdale, N.J.: Erlbaum.

Millar, A., and D. J. Navarick. 1984. Self-control and choice in humans: Effects of video game playing as a positive reinforcer. *Learning and Motivation* 15:203–18.

Miller, Dale T. 1976. Ego involvement and attributions for success and failure. *Journal of Personality and Social Psychology* 51:1208–17.

Navarick, D. J. 1982. Negative reinforcement and choice in humans. *Learning and Motivation* 13:361–77.

O'Donoghue, Ted, and Matthew Rabin. 1997a. Doing it now or later. Evanston, Ill.: Northwestern University. Working paper.

———. 1997b. Incentives for procrastinators. Evanston, Ill.: Northwestern University. Working paper.

Perloff, Linda S., and Barbara K. Fetzer. 1986. Self-other judgments and perceived vulnerability to victimization. *Journal of Personality and Social Psychology* 50: 502–10.

Poterba, James M., Steven F. Venti, and David A. Wise. 1996. How retirement saving programs increase saving. *Economic Perspectives* 10:91–112.

Rachlin, Howard, and David I. Laibson, eds. 1997. *The matching law: Papers in psychology and economics by Richard J. Herrnstein.* New York: Russell Sage Foundation; Cambridge, Mass.: Harvard University Press.

Rankin, Deborah M. 1993. How to get ready for retirement: Save, save, save. *New York Times,* 13 March, p. 33.

Read, Daniel, George Loewenstein, Shobana Kalyanaraman, and Adrian Bivolaru. 1996. Mixing virtue and vice: The combined effects of hyperbolic discounting and diversification. Pittsburgh, Pa.: Carnegie-Mellon University. Working paper.

Riess, Marc, Paul Rosenfeld, Valerie Melburg, and James T. Tedeschi. 1981. Self-serving attributions: Biased private perceptions and distorted public descriptions. *Journal of Personality and Social Psychology* 41:224–31.

Rohrer, J. H., S. H. Baron, E. L. Hoffman, and D. V. Swinder. 1954. The stability of autokinetic judgment. *Journal of Abnormal and Social Psychology* 49:595–97.

Ross, Lee, and Richard E. Nisbett. 1991. *The person and the situation: Perspectives of social psychology.* New York: McGraw-Hill.

Roth, Alvin E., and Ido Erev. 1995. Learning in extensive-form games: Experimental data and simple dynamic models in the intermediate term. *Games and Economic Behavior* 8:164–212.

Rushton, J. Phillipe, and Anne C. Campbell. 1977. Modeling, vicarious reinforcement and extroversion on blood donating in adults: Immediate and long-term effects. *European Journal of Social Psychology* 7:297–306.

Samuelson, William, and Richard J. Zeckhauser. 1988. Status quo bias in decision making. *Journal of Risk and Uncertainty* 1:7–59.

Schelling, Thomas C. 1984. The intimate contest for self-command. *Choice and consequence: Perspective of an errant economist,* 57–82. Cambridge, Mass.: Harvard University Press.

Schultz, Ellen E. 1995. More workers are cracking their nest eggs. *Wall Street Journal,* 17 August.

Shapiro, Matthew D., and Joel Slemrod. 1995. Consumer response to the timing of income: Evidence from a change in tax withholding. *American Economic Review* 85:186–200.

Sherif, M. 1937. An experimental approach to the study of attitudes. *Sociometry* 1: 90–98.

Sicoly, Fiore, and Michael Ross. 1977. Facilitation of ego-biased attributions by means of self-serving observer feedback. *Journal of Personality and Social Psychology* 35:734–41.

Simmons Market Research Bureau, Inc. 1996. The 1996 study of media and markets. New York: Simmons Market Research Bureau, Inc.

Snyder, Melvin L., Walter G. Stephan, and David Rosenfield. 1976. Egotism in males and females. *Journal of Personality and Social Psychology* 33:435–41.

Solnick, J., C. Kannenberg, D. Eckerman, and M. Waller. 1980. An experimental analysis of impulsivity and impulse control in humans. *Learning and Motivation* 2:61–77.

Stephan, Walter G., Davis Rosenfield, and Cookie Stephan. 1976. Egotism and attribution. *Journal of Personality and Social Psychology* 34:1161–67.

Svenson, Ola. 1981. Are we all less risky and more skillful than our fellow drivers? *Acta Psychologia* 47:143–48.
Taylor, Shelley E., and Jonathan D. Brown. 1988. Illusion and well-being: A social psychological perspective on mental health. *Psychological Bulletin* 103:193–210.
Taylor, Shelley E., and Susan T. Fiske. 1978. Salience, attention, and attribution: Top of the head phenomena." In *Advances in experimental social psychology,* vol. 11, ed. L. Berkowitz, 249–88. New York: Academic Press.
Thaler, Richard H. 1985. Mental accounting and consumer choice. *Marketing Science* 4:199–214.
———. 1990. Saving, fungibility, and mental accounts. *Journal of Economic Perspectives* 4:193–205.
———. 1994. Psychology and savings policies. *American Economic Review* 84: 186–92.
Thaler, Richard H., and H. M. Shefrin. 1981. An economic theory of self-control. *Journal of Political Economy* 89:392–406.
Twain, Mark. (1899) 1906. Following the equator. In *The complete works of Mark Twain,* vol. 13, 9–10. New York: Harper.
Weinstein, Neil D. 1980. Unrealistic optimism about future life events. *Journal of Personality and Social Psychology* 39:806–20.
Winston, G. C. 1980. Addiction and backsliding: A theory of compulsive consumption. *Journal of Economic and Behavior Organization* 1:295–324.
Zakay, Dan. 1983. The relationship between probability assessor and the outcomes of an event as a determiner of subjective probability. *Acta Psychologia* 53:271–80.
Zeckhauser, Richard J. 1986. Comments: Behavioral versus rational economics: What you see is what you conquer. *Journal of Business* 59:435–49.
Zuckerman, Miron. 1979. Attribution of success and failure revised, or: The motivational bias is alive and well in attribution theory. *Journal of Personality* 47:245–87.