

**Technical Paper Series
Macroeconomic Analysis and Tax Analysis Divisions
Congressional Budget Office
Washington, D.C.**

**THE EFFECT OF TAX-DEFERRED SAVINGS
PLANS ON HOUSEHOLD WEALTH
ACCUMULATION: EVIDENCE FROM THE
SURVEY OF CONSUMER FINANCES**

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March 1998

1999-4

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1. Introduction

Demographic trends and projections of continued slow productivity growth have led to significant concern about the viability of Social Security and Medicare in the next century. In addition to these gloomy predictions about public retirement programs, fundamental changes that are occurring in employer-sponsored pension coverage are also becoming a source of concern. Although the overall level of employer-sponsored retirement plan coverage in the U.S. has remained high during the last few decades, an increasing number of working families are now receiving their primary employer-sponsored retirement plan coverage through 401(k) and other voluntary Tax Deferred Saving Plans (TDSPs) rather than traditional Defined Benefit (DB) pensions. If combined employer and employee saving under a typical TDSP is less than employer saving under a typical DB plan, the outlook for national saving and retirement-income adequacy in the next century is even worse than current (gloomy) projections indicate.

Comprehensive analysis of how the switch from DB to TDSP coverage will affect saving requires a series of tests. The first step is to assess whether or not families eligible for TDSP coverage save more than families without TDSP coverage. If TDSP eligibility is found to raise saving, the second step is to ascertain whether or not the increase is large enough to account for the fact that TDSP assets are measured before tax, while other non-pension assets are measured after tax. Finally, even if TDSP eligibility is shown to increase net after-tax wealth, it remains to be shown that the amount of saving being done through TDSPs is equal to the saving that would

have occurred under traditional DB coverage.¹

This paper addresses only the first question--whether or not TDSP eligibility actually increases measured wealth accumulation. That first step seems straight-forward, and is obviously crucial in the overall analysis, but the answer is the subject of an intense debate between Poterba, Venti, and Wise (PVW: 1995, 1996) and Engen and Gale (EG: 1996).² Using data from the Survey of Income and Program Participation (SIPP), PVW find a strong, positive effect on saving in financial assets. But using the same data and a similar technique, EG find no increase (and possibly a decrease) in broadly-measured net saving by TDSP eligible families.

The debate over whether TDSP eligibility raises saving extends naturally from the similar debate about the effectiveness of Individual Retirement Accounts (IRAs) and other voluntary saving programs, and many of the contentious issues necessarily carry over. When TDSP plans began growing in popularity during the 1980s, they were perceived as mostly add-on arrangements to existing DB plans (Papke, Petersen, and Poterba (1993)). Thus, the basic test of TDSP effectiveness was the same as the test for IRAs: controlling for underlying differences in saving preferences, researchers tried to measure how many of the dollars flowing into TDSP

¹ For estimates of how retirement income adequacy varies across typical DB and 401(k)-type plans, see Samwick and Skinner (1995).

² The issues are also discussed in three summary papers in a recently published symposium about various incentives for saving in the tax code: see Hubbard and Skinner (1996), Poterba, Venti, and Wise (1995), and Engen, Gale, and Scholz (1996). For a theoretical discussion of the effectiveness of saving incentives in a life-cycle context, see Burman, Cordes, and Ozanne (1990).

accounts represented "new" saving that would not have been undertaken absent the program-- there was no concern about whether or not the saving in the TDSP was enough to replace what would have been saved in a DB. The critical issues when implementing these tests are (1) how to control for the underlying differences in saving preferences, and (2) how to measure whether flows into TDSP accounts are new saving or merely shuffling between existing accounts with net saving (and therefore consumption) unchanged.

On the first crucial issue, there is common ground between the opposing camps on how to identify saving effects. Both PVW and EG begin by specifying a relationship between the level of accumulated wealth and underlying saving determinants like income and demographics. Then, using data from two points in time, they test whether or not saving was higher for eligible families by allowing a dummy variable for eligibility in the second year to capture changes in wealth not accounted for by changes in income or the other underlying wealth determinants. The idea is that the eligible families in the later year were exposed to the TDSP plan for a longer period of time, and thus, if the program is effective, should have accumulated more wealth (after removing the effect of all other wealth determinants) than eligible families in the earlier year.³

The two sets of authors disagree strongly about the second critical issue, however-- exactly how to measure how much of the flow into TDSP accounts is actually new, net saving.

³ PVW conduct multiple tests of TDSP effectiveness--the approach described here is the one that both sets of authors agree on. PVW refer to this specific test as analysis of "like families."

PVW focus on two measures: they find an unexplained increase in total financial assets for TDSP-eligible families relative to ineligible families, and no unexplained decrease in non-TDSP financial assets. The first measure indicates a positive effect from TDSPs, and the second shows that the higher saving was not merely substituted out of other saving those families would have undertaken in the absence of the program.

The EG results on total financial and non-TDSP financial assets are similar to those found by PVW--they are using the same data sets and same basic technique. However, EG argue that the wealth concept used in the analysis should be broader, to incorporate the possibility of asset "shuffling" across other possible margins of substitution. For example, if families contribute to a TDSP account to gain the tax advantage, but turn around and increase the amount borrowed against their home so their consumption does not fall, the PVW measure will interpret their behavior as increased saving when their actions were merely offsetting. EG show that implementing their tests using broader measures of wealth reverses the PVW conclusion: saving is not higher, and indeed may be lower, for TDSP eligible families.

In this paper we implement the EG tests using more recent and more comprehensive data from the Survey of Consumer Finances (SCF). The data are more comprehensive both in terms of which assets are measured and the inclusion of high-wealth families. The SCF data also extend through 1995, whereas the SIPP data are only available through 1991. As in the PVW and EG studies, we generally find strong positive effects of TDSP eligibility on financial asset accumulation, and little evidence that the growth in TDSP assets was the result of shifting out of

other (non-TDSP) financial assets.

Our results on broader wealth measures differ sharply from the results found by EG using the SIPP, however. For example, in all of our specifications, we find no evidence of significant increases in debt for eligible families, and we do find that net worth (less housing that fell in value) did significantly increase for eligible families. When the sample is restricted to homeowners, we find significant effects for eligible families using even the broadest wealth measures.

Given the general result that TDSP eligibility seems to increase saving, it is important to note that the tests used here and in the previous studies are particularly sensitive to which time period is chosen for the analysis, probably because of fluctuations in relative prices and other macroeconomic shocks. This is evidenced by extremely different results between the two subperiods, 1989-1992 and 1992-1995. Not only do relative prices affect the wealth measures directly, it also seems that portfolio shuffling not associated with TDSP accounts may be affecting the results. This suggests caution about using the magnitude of the estimated effects to draw inferences about eligibility effects and especially for projecting future wealth accumulation.

In the next section we use data from the SCFs and the aggregate Flow of Funds Accounts (FFA) to analyze the growth in TDSP coverage (and drop in DB coverage), the rapid accumulation in TDSP accounts, and the portfolio shuffling in other parts of the aggregate household balance. In the third section we implement our tests of saving effects using variants of

the EG "between-group" and "within-group" difference-in-difference methodology. The last section concludes and identifies directions for future research.

2. Trends in Retirement Plan Coverage and Household Balance Sheets

In this section we use the 1989, 1992, and 1995 Survey of Consumer Finances (SCF) cross-sections and 1989 to 1995 Flow of Funds Accounts (FFA) household sector balance sheets to analyze recent trends in employer sponsored retirement plan coverage and household portfolios. The data indicate a significant shift from traditional DB pension coverage towards TDSP coverage, with little overall change in retirement plan eligibility. Also, there is a dramatic increase in the balances in both TDSP and traditional pensions, but that growth is somewhat offset by increased borrowing and other portfolio changes. In addition to motivating the tests of how TDSP eligibility affects saving (presented in the next section) the results here are also important for explaining why the results vary so much over the 1989-92 and 1992-95 subperiods. In particular, the effect of relative price changes on sub-period wealth changes is substantial.

Coverage under various retirement plans arrangements for SCF families is shown in Table 1.⁴ The top panel shows retirement plan coverage for all families--the covered fraction is

⁴ Coverage for a family is determined by plans associated with current jobs for both the head and spouse. Family coverage rates are much higher than individual coverage rates, because second earners are often ineligible for coverage or turn down voluntary coverage. Also, a family has combined DB/TDSP coverage if one spouse has just a DB plan and the other just a TDSP--thus the frequency of mixed coverage at the family level will be higher than the frequency of mixed coverage at the worker level.

less than half in any of the years, but that is because this group includes the retired and self-employed who do not have coverage associated with a current job. But among those with some form of employer-sponsored coverage, there has been a significant shift away from traditional DB coverage towards TDSP coverage.

The bottom half of the table shows more clearly the shift in pension coverage. The sample is restricted to the matched "working age" cross-sections we will be focussing on throughout the paper--families where the head is between ages 30 and 59, either the head or spouse (or both) is working, but neither the head or spouse is self-employed. In 1989, over 50% of these typical working-age families had some form of traditional DB coverage (28.8% DB only plus 23.4% DB/TDSP). By 1995, that fraction fell to about 34%, while the overall employer-coverage eligibility ratio was basically flat. At the same time, the fraction with some form of TDSP coverage rose from about 47% to near 60%--the fraction with only a TDSP plan increased the most, by over 13 percentage points. The fraction of eligible non-participants rose by 4.5 percentage points.

Table 2 shows how the growth in TDSP coverage varied across age and income groups. Overall, 12.2% more families in the working age cross-section were eligible for TDSPs in 1995 than in 1989. The growth was fairly uniform across age and income groups--the only groups that did not experience significant growth between 1989 and 1995 were the oldest (50 to 59), lowest income (less than \$10,000) and highest income (greater than \$100,000 in 1995 dollars). But coverage rates were already quite high for the oldest and highest income groups.

These trends in employer sponsored coverage suggest, if everything else were constant, that accumulated wealth in 1995 should be much higher than in 1989 even if the saving in TDSP plans simply replaced what would have gone into DB pensions. If people save as much in a TDSP plan as their employer would save on their behalf in a DB plan, then the fact that TDSP balances are measured in the SCF while DB balances held by employers are not included implies that measured household-level wealth should have risen.

Table 3 shows that accumulations in TDSP accounts were significant between 1989 and 1995. However, declines in the (inflation-adjusted) levels of other assets and increases in liabilities offset much of the growth, so that net worth actually fell in real terms. Focussing on the bottom half of Table 3, which shows balance sheet totals for the working age cross-sections, the growth in TDSP accounts was \$768 billion, while total net worth fell \$289 billion, because of declines in housing values and growth in liabilities. The third panel of Table 3 shows that the results are not driven simply by the high wealth families in the sample--if we exclude the top 5% of wealth holders in every cross-section (who hold almost 50% of the wealth) the same basic pattern of rapid growth in TDSP accounts offset by declines in housing and growth in borrowing emerges.

Table 3 is thus consistent with both the PVW and EG findings discussed in the introduction. As more families have become eligible, and TDSPs have been in place longer, there is a significant amount of saving flowing into the plans. The fact that non-TDSP assets also continued to grow indicates no obvious shuffling between TDSP saving and, for example,

household-level holdings of mutual funds or any other financial assets. However, there is also evidence that shuffling may be occurring at broader margins. In particular, the rapid growth in mortgage debt indicates that some of the growth in TDSP saving was offset by higher borrowing.⁵

Another perspective on trends in household-sector portfolios is shown in Table 4, which uses Flow of Funds Account (FFA) data from the Federal Reserve Board to break down the portfolio changes into net acquisitions and revaluations. Nominal balances of a given asset change if the household sector acquires more of that asset, or if the market price of that asset changes. In the FFA, financial assets, owned housing, and the equity value of non-corporate business are all subject to revaluation. In the 1989 to 1995 period, changes in the market value of these assets have been significant, and greatly affected the measured portfolio shifts that were presented in Table 3.

For example, the level of financial assets in the FFA (which includes some, but not all, TDSP balances as measured in the SCF--the rest are included with pensions) grew significantly in both the 1989 to 1992 and 1992 to 1995 subperiods. But the capital gains on financial assets (\$1,116 billion in the first subperiod, \$1,767 in the second) dominated net acquisitions (\$441 billion in the first subperiod, and \$226 in the second). The other interesting example is housing--although families acquired significant amounts of new housing (\$396 billion in the first subperiod, \$420 in the second) the weak growth in nominal housing prices led to little in the way

⁵ For a discussion of how taxes have affected mortgage borrowing in recent years, see Maki (1995).

of capital gains. Indeed, the real value of housing fell in both the SCF and FFA during the six year period because inflation outweighed the sum of nominal gains and net acquisitions.

Table 5 shows the effects of relative prices on portfolio balances in the two subperiods. The underlying inflation rates used to convert the nominal measures to reals were 13% between 1989 and 1992 and 8.5% between 1992 and 1995 (based on the CPI-U). Thus, real gains rates on all assets were significantly below the nominal rates in both periods, but further below in the first subperiod. Indeed, in the first subperiod, the real capital gains on financial assets were only 3.7%, but in the second period soared to 13.1%. All other assets fell in real terms during the first subperiod. In the second subperiod, only housing values continued to fall in real terms, albeit at a significantly lower rate.

Through the distortions caused by real relative price changes one can find interesting patterns emerging in Tables 3, 4, and 5. First, the growth in all types of retirement-oriented assets--pensions and life insurance plus the fraction of financial assets held in IRAs and TDSPs--stands out as a source of saving.⁶ Although households acquired significant housing and durables, also, they issued a lot of debt to match those acquisitions. This leads to an interesting possibility with respect to TDSPs--if the growth in retirement-oriented assets and debt occurred for the same families, there is evidence of portfolio shuffling at very broad margins. If, however,

⁶ Another way to say this is that most saving in the FFA is (on net) through some sort of tax-deferred arrangement. See Sabelhaus (1997) and Congressional Budget Office (1997) for more details.

the increases in debt occurred for both eligible and ineligible families, there is no evidence of shuffling. We turn to that question in the next section.

3. Estimating the Effect of TDSPs on Saving

In this section we describe and implement our tests of the impact of TDSP eligibility on saving using the 1989, 1992, and 1995 SCF data. The tests we use are variants of the EG "between group" and "within group" difference in difference estimators. We estimate the equations for several wealth measures and for various subsamples of the matched working age cross sections. Our results on financial assets are comparable to those in the PVW and EG studies using the SIPP data, but our findings on broader wealth components differ sharply. In particular, we find strong, positive effects of TDSP eligibility on wealth accumulation. However, we also find that the estimated effects vary significantly across the subperiods used in the analysis, which highlights the fact that relative price shocks and other macroeconomic factors may dominate estimated saving effects in the short run.

The two types of equations we estimate are referred to by EG as "between" and "within" group difference in difference estimators. The between group specification assumes that the relationship between a given wealth measure and exogenous determinants of wealth holdings (such as income and age) is the same for eligible and ineligible families. As in EG, the between group specification is,

$$(1) \quad W = \alpha + X \beta + \alpha_e E + \alpha_y YEAR + \alpha_{ey} E * YEAR + \varepsilon$$

where W denotes the level of wealth held by a given family, X is a vector of control variables including income, education, age, and other demographics, $YEAR$ is a time-dummy for the second cross-section (either 1992 or 1995, depending on which subperiod is being estimated), and ε is an error term.⁷ The coefficient α_e captures the fact that eligible families may have different wealth holdings at both points in time--for example, families that like to save may self-select into jobs where TDSP coverage is offered. The coefficient α_y captures the differences in wealth that occurred for both eligible and ineligible families over time, due to macroeconomic events or cohort effects.

The coefficient of interest in the between group equation is α_{ey} , which captures unexplained changes in wealth for eligible families between the first and second year. The coefficient is a difference in difference estimator--it measures how much wealth rose for eligible families relative to ineligible families over the time period, after having removed underlying differences between eligibles and ineligibles as well as underlying differences between the two time periods. In our tables of results, the only estimate reported is for α_{ey} .

⁷ The X vector includes dummies for six age groups, dummies for eight income classes, dummies for five education groups, and dummies for marital status, presence of second earner, white, male, and covered by DB plan.

Before presenting the results from the between group equations, it is worth noting how the "within group" estimator differs. The only difference is that the underlying determinants of wealth (age, income, etc.) are allowed to have different impacts on wealth at each point in time within each group. Rather than simply using a dummy to capture differences between eligible and ineligible families at each point in time, every coefficient in the wealth-determinant vector is allowed to vary. This is accomplished by interacting each variable in X with the eligibility dummy,

$$(2) \quad \begin{aligned} W &= \alpha + X \beta \\ &+ \alpha_e E + X * E \beta_e \\ &+ \alpha_y YEAR + \alpha_{ey} E * YEAR + \varepsilon \end{aligned}$$

As before, the coefficient of interest is α_{ey} , which measures the unexplained growth in wealth for eligible families relative to ineligible families between the two time periods.⁸

The results of estimating the between group equation for three time periods and two samples is shown in Table 6.⁹ The first three columns report values for all families in the

⁸ This specification is a slight variant of the EG within group estimator--they estimate versions of equation (1) for eligible and ineligible families with only the year dummies, and then compare the differences between those coefficients. The only difference between their specification and ours is that we impose the same error term distribution for eligible and ineligible families.

⁹ All of the equations are estimated by median (LAD) regression using STATA.

matched working age cross-section, while the fourth through sixth columns are for the bottom 95% of the wealth distribution in each period. The wealth restriction is imposed to try and replicate the results using SIPP, which does not have the high-income supplement like the SCF. The three sets of time periods are the six year period 1989-1995, and two three year subperiods 1989-1992 and 1992-1995.

Each entry in Table 6 is the estimate of α_{ey} for the given independent variable. Thus, for example, we find unexplained growth in financial assets of \$3,210 (with a t-statistic of 2.0) between 1989 and 1995 for TDSP-eligible families. That finding confirms the earlier results from SIPP--a positive and significant effect of eligibility on financial assets. The second row shows that financial assets less TDSP balances did not fall for eligible families--the coefficient is positive (\$211) but not significantly different from zero--this is also consistent with the results from SIPP, as both PVW and EG conclude that there is no apparent shuffling between TDSP and non-TDSP financial assets.

The results for other wealth measures in Table 6 show a mixed pattern of consistency with the EG findings using SIPP. Focussing on the 1989 to 1992 subperiod, which is closest to the 1987 to 1991 period in the EG study, we find evidence that financial assets less debt (net financial assets) grew for eligible families, while EG did not. However, that result disappears when we restrict the sample to the bottom 95% of wealth holders, which is more consistent with the SIPP sample. Similarly, we are able to replicate the significant declines in total net worth and net worth less TDSP balances in the EG study, when we focus on only the first subperiod.

One EG result we cannot replicate with the SCF data is the significant decline in net worth less TDSP balances and housing. EG argue that their finding on this dependent variable is the most damaging to the case for TDSP effectiveness, because the effect of declining real house values (Table 5) is removed. Thus, we cannot conclude as EG did that TDSPs are ineffective at raising saving. On the other hand, we cannot use the estimates in Table 6 to conclude that TDSPs raise saving, because the estimated positive effects are generally insignificant. For example, the estimated α_{ey} in the net worth minus housing equation (which should be positive and significant if TDSPs are effective, and is not polluted by declining home values) is positive, but the t-statistic is only 1.1 in the 1989-1992 subperiod, and even weaker for the entire six year period.

The ambiguity about TDSP effectiveness disappears when we move to the within group specification, as shown in Table 7. Like EG, we generally find larger and more significant estimated effects on financial assets than those based on the between group specification.¹⁰ In general, though, the similarities and disparities show up again. This time, however, the net worth less housing and net worth less housing and TDSP balance estimates are even more significant, and definitely at odds with the EG findings.

The estimates in Table 7 are also useful for focussing on the sensitivity of the results to

¹⁰ For example, in EG, the LAD coefficient on total financial assets is \$679 with a t-stat of 1.3 using the between group estimator, and \$1,734 with a t-stat of 4.9 using the within group estimator.

which time period is chosen. For example, the estimated effect in the total net worth equations reverses signs in the second subperiod, which is attributable to the fact that housing prices stopped collapsing (see Table 5). But at the same time, the estimated effect on total financial assets (and financial less TDSP, and financial less debt) grew significantly weaker in the second subperiod, which is the time period when real asset prices were growing much more strongly. One possibility (beyond the maintained hypothesis that TDSP eligible families intentionally accumulated less wealth than non-TDSP families in the second subperiod, all else equal) is that the assets held outside of TDSP accounts are more likely to be held in stocks which benefitted from the bull market. The data in the SCF do not allow us to investigate this possibility.

The last set of regressions in Table 8 are for homeowners only--the idea is to try and isolate a group of families who are more homogeneous in their saving propensities and especially portfolio composition.¹¹ The results are generally supportive of the findings using the entire sample, with stronger and more significant positive effects on wealth from TDSP eligibility, and a significant reduction in the time pattern. In particular, the general decline in net worth for eligible families in the first subperiod disappears, because TDSP and non-TDSP families alike all experienced the decline housing prices, rather than the effect being disproportionately on the TDSP eligible families. Still, the differential effects across time periods is pronounced: in particular, the results for net worth less owned housing and TDSPs is significantly negative in

¹¹ Both EG and PVW use the existence of IRAs as another approach to trying to isolate a more homogeneous saver group. That is valid for the time period they consider, but since TRA86 greatly restricted access to IRAs, many of the (unobserved) high-saving propensity families in our sample would not have had access.

the 1992-95 subperiod.

4. Conclusions

Because more and more families are now receiving their primary employer-sponsored retirement plan coverage through 401(k) and other tax-deferred saving plans (TDSPs) rather than traditional DB plans, estimates of how these plans affect behavior will play a prominent role in projections of future retirement income adequacy. In this paper we showed that TDSP eligibility does significantly raise household saving, even measured at the broadest possible margins of substitution. That reverses conclusions based on earlier research using other, less comprehensive, data sources.

We showed that TDSPs have passed the simplest effectiveness test: TDSP families saved more than non-TDSP families after controlling for other wealth determinants. The next test and clear direction for future research is to measure whether or not the higher TDSP saving is enough to overcome the loss in traditional pension plan benefits that has occurred. To do this, we need comprehensive measures of wealth (future pension benefits and household level wealth) to assess whether or the TDSP eligible (who would have been DB eligible in past decades) are making up for what their employers would have saved on their behalf.

5. References

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6. Data Appendix

This study uses data from the Survey of Consumer Finances (SCF) for the years 1989, 1992, and 1995. The SCF provides detailed information on household assets and liabilities, along with associated demographic characteristics. The SCF over samples high-income families to provide more accurate aggregate wealth data, and weights are assigned to each sampled household accordingly. To correct for missing data in the survey, each missing value is "imputed" five times by drawing from an estimate of the conditional distribution of the data. In the median regressions, we use only the third replicate for each family.

The definitions for the wealth categories used as dependent variables in Table 3 and in Tables 6 through 8 are largely drawn from the definitions provided by the Federal Reserve Board for use with the SCF (see Kennickell, et.al. 1997). Our only adaptation of their definition of total net worth is the addition of non-liquid pension balances to the total: the FRB defines pension contributions as assets only if they can be withdrawn or borrowed against; we include all account-type pension balances as part of net worth. All values are in 1995 dollars using the CPI-U; we inflated 1989 values to 1995 by a factor of 1.2266, and 1992 values to 1995 by 1.085.

The aggregate Flow of Funds Account (FFA) data used in Tables 4 and 5 comes from the Federal Reserve Board's Z1 release in September, 1997. The household balance sheets are in Table B100, and the asset revaluations used to measure capital gains and net acquisitions are in Table R100. The household sector totals have balances held by non-profit institutions (in Table L100A) removed to be more comparable with the SCF values.