Wealth Shocks and Retirement Timing: Evidence from the Nineties

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I. Introduction

A timeless question asked by economist, policy maker and layperson alike, is whether higher wealth reduces labor supply. That is, *ceteris parabis,* do individuals with greater wealth consume more leisure? Although the normality of leisure is a general result in standard economic models, econometric support for it has not been as consistent. This may largely be due to two factors: measurement error in survey data, and the endogeneity of wealth and unearned income (Holtz-Eakin et al., 1993). In this paper, I focus on wealth effects among the population close to retirement age. I ask whether individuals who had unexpected and potentially substantial gains in wealth in the 1990s retired earlier because of these windfalls than they otherwise would have. Using shocks to retirement wealth that are arguably exogenous, I do find evidence of wealth effects.

The labor force participation rates of older men have fallen dramatically throughout the past century. However, preliminary evidence suggests that this decline has leveled off in the past ten years (Anderson, Gustman, and Steinmeier, 1999). Even if the steady state retirement rate has been stable for the past decade, there may be transitory changes in retirement timing in response to business cycle shocks or unexpected changes in individual level work incentives. In fact, as Figure 1 shows, labor force participation rates among men in their late 50s fluctuate quite a bit from year to year. In addition, as the structure of pension plans has changed, the incentives to retire at particular ages should be changing as well.

Since the late 1970s, the percent of workers with employer provided pensions has remained steady at about 50 percent (Clark and Quinn, 1999). These pension plans are often grouped into two categories: defined benefit (DB) plans and defined contribution (DC) plans. DB plans, often thought of as "traditional" pension plans, make payments to retired workers calculated using a formula based on years of service and salary over those years. The nominal benefit level is guaranteed. DC plans are accounts to which the worker and employer contribute. At a certain age or upon retirement, the worker may make withdrawals from the plan. Its value will depend on employee contributions, the employer’s matching rate, and the rate of return. In 1995, about 41 percent of private defined contribution fund assets were invested in equities, 15 percent were in bonds and the rest were in other items (EBRI, 1997).

A number of studies have linked the growth of (DB) pensions to the declines in the retirement ages of men during the second half of the 20th century (Stock and Wise, 1990; Samwick, 1998). As Figure 2 illustrates, DB plans have decreased relative to DC plans over the past 20 years. Unlike DB pensions, DC pensions do not provide workers with sharp incentives to retire at particular ages. Whereas DB pensions have non-linear accrual rates, DC accrual should be relatively smooth. Thus, the growth of DC pension plans could account for the "flattening out" of male retirement ages. Indeed, Friedberg and Webb (2000) find that individuals with DC pensions retire an average of two years later than individuals with DB plans.

An additional implication of the growth of DC pensions is that an increasing number of households face greater uncertainty regarding the exact amount of resources they will have as they approach retirement. While the rules of the pension plan may not suggest spikes in retirement at particular ages, variation in the value of accounts due to market performance may cause fluctuation in retirement timing due to wealth effects. This has not yet been examined in the literature.

Ameriks and Zeldes (2000) find that the fraction of retirement plan contributions invested in stocks has been increasing over the past decade. In addition, a growing share of households is investing their non-pension wealth in stocks. From 1989 to 1995, the share of households investing in stocks, either directly, or indirectly through mutual funds, increased from 32 percent to 41 percent (Diamond, 1999). While these returns may be relatively stable in the long run, the value of a portfolio at a given point in the near future is much more uncertain. The responsiveness of retirement timing to wealth shocks is therefore of growing interest, since an increasing portion of retirement resources are invested in risky assets. How will individuals react if stock prices unexpectedly rise or fall in the period close to their anticipated date of retirement? The effects will be of particular importance in the future if individuals are allowed to allocate a portion of their social security wealth in the assets of their choice.

Earlier studies of retirement timing have focused on the effect of Social Security, and have found conflicting results. Most of these studies test whether the decreases in male labor force participation rates can be explained by changes in Social Security benefits. The period most studied is 1969 to 1972, when real benefits increased unexpectedly by more than 20 percent for many workers. Using longitudinal data from the Retirement History Survey (RHS) Hurd and Boskin (1984) find that the benefit increase can explain a large amount of the acceleration of retirement in that period. However, Burtless (1986) and Hausman and Wise (1985), examine the same period with the same data, and find that the effects were very small. Krueger and Pischke (1992) estimate the effect of wealth shocks by exploiting Social Security changes in the opposite direction. Amendments to Social Security in 1977 substantially reduced benefits for individuals then ages 60 and younger. Again, despite the fact that the decreases in benefits were unexpected, labor supply of older men continued to decrease. A more recent study by Chan and Stevens (2000) finds that individuals' planned retirement ages do respond to "shocks" in pension plan incentives.

Studies of wealth effects on labor supply among workers of a wider age distribution have also failed to reach consensus. A number of early studies used survey data to estimate the effect of unearned income on hours worked by married men. Results varied from findings of a negative coefficient of varying magnitude on unearned income (Kosters (1966), Ashenfelter and Heckman (1973)), to a positive coefficient (Garfinkel (1973), Dickinson (1974)). Perhaps this is because unearned income, which was generally calculated as the difference between household income and husband's earnings, is still an endogenous measure. Several recent studies avoid the problem of endogenous wealth variation by making use of natural experiments. Imbens, Rubin, and Sacerdote (1999) survey lottery winners and find significant labor supply effects, both on participation and on hours, of winnings over $80,000 per year. However, they find that smaller winnings (less than $15,000 per year) are not associated with any significant changes in labor supply. Holtz-Eakin, Joulfaian and Rosen (1993) find that individuals in households that receive inheritances, particularly those that receive large inheritances, are more likely to leave the labor force and less likely to enter the labor force.

This paper contributes to both the literature on retirement timing and the literature on wealth effects by making use of a recent natural experiment – the "run up" of the stock market in the 1990s. I use data from the 1992 and 1998 waves of the Health and Retirement Study and a difference-in-differences methodology to compare retirement outcomes among individuals who should have had large windfalls to outcomes among different groups who should not have had the same gains. The paper proceeds as follows: in Section II I discuss the 1990s; Section III describes the methodology and the data; Section IV reports results; Section V discusses caveats and concerns, and Section VI discusses policy implications and concludes.

II. The Nineties

One of most discussed phenomena of the late nineties is the extraordinary performance of the stock market. Figure 3, which plots the year-end level of the Standard and Poor 500 Index, illustrates the gains made after 1994. The unprecedented price appreciation of public equity markets blessed many households with extraordinary capital gains that were both large and unexpected. French and Cheng (2000) estimate that for every dollar an individual invested in the stock market in 1994, they would receive $1.12 of an unexpected gain, in addition to a $0.70 expected gain.

Many of these gains went to individuals with defined contribution pension plans. Figure 4 plots aggregate DC balances between 1988 and 1999. Balances grew substantially during the late nineties. The graph shows that the difference between actual balances in 1999, and the balances one would expect based on the annual rate of change between 1988 and 1994 is $579 billion or 27 percent of actual 1999 balances. Because the number of individuals with DC plans has been growing, some of the increase in balances must be due to higher coverage and contributions, but some of it is due to capital gains. This suggests that those with DC pensions, especially those with a significant portion of their balances invested in stocks, likely had significant unexpected capital gains.

At the same time that the stock market was booming, labor force participation rates among those close to retirement age may have been increasing.[1] Thus, aggregate data on wealth and labor force participation provide no evidence that wealth shocks covary with withdrawal from the labor force. These aggregate data however, reflect that there are many other determinants of labor force participation – including changes in health, changes in Social Security incentives, and changes in wages driven by local labor market conditions. National unemployment rates, which were decreasing almost constantly from a high in June of 1992 of 7.8 percent to a low of 3.9 percent in September of 2000[2], likely led to increases in demand and thus higher wages for older workers. It is possible that even though overall retirement rates were declining, they may have been increasing for those individuals who had financial windfalls.

III. Empirical Methods

To test for a causal relationship between the capital gains experienced in the nineties and labor supply, it is necessary to have variation in capital gains that is exogenous. Because savings and portfolio composition vary with individual preferences and plans for retirement, this is often difficult to identify. The gains in the nineties were so large that it is hard to argue against the belief that some portion of the gains was unexpected. Thus, one might suggest a comparison of retirement and work patterns in the early nineties and late nineties as a test for wealth effects. Unfortunately this would not provide clean evidence, because of the confounding changes discussed above. The challenge for the researcher then, is to find exogenous variation in wealth shocks over the period that varies across individuals.

Ideally, we would have longitudinal survey data, which not only had detailed information about individuals' portfolios, but also recorded what returns individuals were expecting on their investments. Then one could observe the actual returns and consider the difference the unexpected gain. This is of course impossible. However, a good alternative is the Health and Retirement Study (HRS). Every two years since 1992, HRS

[1] See for example Walsh, M. "Reversing Decade-Long Trend, Americans Retiring Later in Life," New York Times. February 26, 2001.

has collected data on individuals born between 1931 and 1941, and their spouses. In addition, since 1998, it has interviewed individuals born between 1942 and 1947. The study has data on labor supply, pensions, income, assets, active savings and dissaving of assets, health, and a variety of demographic characteristics.[3] These data enable tests of wealth effects in at least several ways.

The first method, which I am currently pursuing but will not present in this paper, involves estimating the changes in net worth that are unexpected between waves of the study. This is done by subtracting the following from total changes in net worth: active savings as reported in the survey (e.g. contributions to 401(k) plans, sales or purchases of stocks, homes, etc.), and expected returns in different assets, which are calculated across the sample using historical data, rather than at the individual level. These unexpected changes in net worth can then be translated into changes in potential consumption over the retirement period, to get a measure of the magnitude of the wealth shock given the household's baseline level of resources. This method has several disadvantages, all stemming from reporting issues in survey data. First, individual responses of actual asset values may be reported with error. Individuals may not know the exact value of each asset they own, and even if they do, they may be unwilling to report it. When reported and imputed values for multiple assets are added together, and then combined with reported savings and dissaving, the actual measure of capital gain may have a great deal of noise relative to any predictive power it should have. The probability of falsely rejecting the null hypothesis that capital gains have no effect on retirement timing will be upward biased. A second limitation is that it is impossible to know what individuals' expected returns were. We can only infer that from historical data, and it is difficult to come up with expectation measures that vary with individual characteristics.

The second method of estimating wealth effects avoids some of the disadvantages of the first method discussed above, but it also lacks some of the benefits. This method involves identifying a treatment group that would have been affected by the change that

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[2] As reported on the Bureau of Labor Statistics website http://www.bls.gov.
[3] For a detailed description of retirement measures in the HRS see Gustman, Alan L., Mitchell, Olivia S., and Steinmeier, Thomas L. "Retirement Measures in the Health and Retirement Study." *The Journal of Human Resources* 30: S57-S83. For an overview of the HRS see Juster, F. Thomas, and Suzman, Richard. "An Overview of the Health and Retirement Study." *The Journal of Human Resources* 30: S7-S56.

is being studied, and a control group that would not have been affected by it. In the context of this paper, I would like to use a treatment group whose wealth rose unexpectedly as a result of the stock market, and a control group whose wealth did not. The relative benefit of this method is that it does not use actual measures of the wealth shock. Thus it avoids the measurement issues discussed above. A disadvantage is that it does not take advantage of the great deal of heterogeneity in wealth gains among the people in either of the groups.

One interesting source of variation is across pension plan type. Workers with DC plans, whose pension value depends directly on the stock market, should have had large unexpected increases in their pension wealth in the nineties, whereas workers with only DB plans, or without any pension should have experienced no unexpected increases in their retirement wealth. As a result, if wealth affects retirement timing, we would expect to see different patterns of retirement among workers with DC plans than workers without DC plans. In particular, in the late nineties, we would expect to see workers with DC plans retiring earlier than others.

This strategy assumes that once controlling for observable characteristics of an individual, a worker's pension plan type is exogenous – i.e. not influenced by the nineties economy, and not correlated with any other characteristics that would affect retirement timing. Because workers' choice of pension plan is generally determined with their choice of job, it is unlikely that the pension type of workers close to retirement in the nineties was influenced by events in the nineties. Unfortunately however, pension coverage is not randomly assigned. First, the likelihood of coverage from any type of pension increases with age, tenure, earnings, and firm size. Workers' job decisions likely reflect their preferences over salary, other forms of compensation, and mobility. Indeed, a large literature exists describing the incentives of firms to offer pensions, and workers' job decisions (Clark and Quinn, 1999). Thus a comparison of workers with DC plans to those with no pension, would be capturing many differences other than the 90s wealth gain.

A particular concern in comparing workers with DC pensions to those with DB pensions is that pension rules and accrual rates have a strong and independent effect on retirement timing. Because of the complex rules and accrual rates of DB plans, there are strong incentives to postpone retirement until a certain date, after which there are strong disincentives against continued work. As mentioned earlier, there is a substantial literature that suggests incentives of DB plans are responsible for increases in early retirement among men. If this is the case, estimates of a nineties wealth effect based on the difference in retirement outcomes of DB and DC participants would be biased. In particular, one might find that individuals with DB plans retire earlier than individuals with DC plans, and falsely fail to reject the null hypothesis that wealth has no effect on retirement timing.

One way to control for such differences in treatment and control groups is to look at the difference-in-differences in retirement rates between workers with DC pensions and those without them before and after the stock market boom. This would control for time invariant heterogeneity in retirement patterns between different types of workers, yielding unbiased estimates as long as there were no other unobserved changes occurring over the time period that differentially affected the retirement patterns of DC workers.

In this paper, I make use of two comparison groups – individuals who have no pension and individuals who only have DB pension plans. I compare retirement rates of workers with DC pensions relative to these groups, among individuals ages 55 to 60 in 1992 and in 1998 using the Health and Retirement Study (HRS). Because a new cohort of individuals born between 1942 and 1947 joined the study in 1998, it is possible to create mutually exclusive samples of individuals ages 55 to 60 in 1992 and 1998. The 55-60 year old respondents used in 1992 are members of the original cohort. In 1998, the 55-56 year olds are members of the new cohort, first interviewed in 1998, while the 57-60 year olds are members of the original cohort first interviewed in 1992.

Labor supply of individuals in 1992 should be a function of their information in 1992 and should not be affected by the increase in stock prices after 1994. On the other hand, labor supply of individuals in 1998 should be affected by wealth gains in the middle to late nineties. As discussed above, the gains should be greater for individuals with DC pensions than for others. Thus, by estimating the difference-in-differences in probabilities of retirement between DC workers and workers in the control group, in 1992 and 1998, $(Ret_{1998}^{DC} - Ret_{1992}^{control}) - (Ret_{1998}^{DC} - Ret_{1992}^{control})$, I test whether unexpected wealth gains increase the probability of retirement.

To get more precise estimates by controlling for observable individual characteristics that should affect retirement timing, I do this in a regression framework. I estimate the following linear probability model:

$$R_i = \alpha + \beta_1 X_i + \beta_2 Y1998_i + \beta_3 DB_i + \beta_4 NoPension_i + \beta_5 \left(Y1998_i \times DB_i\right) + \beta_6 (Y1998_i \times NoPension_i)$$

R equals one if the individual is retired and zero otherwise. *Y1998* is a variable equal to one if the individual is observed in 1998 and zero if they are observed in 1992, *DB* equals one if the individual has a *DB* pension plan and zero otherwise, and *NoPension* equals one if the individual does not have a pension. The reference group is made up of individuals with DC pension plans. *X* is a vector of observable characteristics including whether anyone in the household is eligible for retiree health benefits[4], marital status, self-rated health, race, ethnicity, education, and age. β_2 captures the time fixed-effect. β_3 and β_4 are the pension fixed-effects – they capture the time invariant effects on retirement of having only a DB pension plan, or of not having a pension at all, relative to having a DC pension. β_5 is the differences-in-differences estimate of a wealth effect when individuals with DB pensions are considered the "control" group. A negative and statistically significant estimate of it would allow me to reject the null hypothesis that wealth shocks do not affect retirement timing.

I consider various measures of retirement. There is little consensus in the field over the correct measure of retirement.[5] In this paper I present results when using two different definitions. The first considers an individual retired if they report in the HRS that they consider themselves retired. This definition considers individuals who have

[4] There is evidence that access to health insurance plays a large role in the decision to retire early (Gruber and Madrian, 1994). Unfortunately, because the 1998 HRS survey did not fully assess whether spouses would be covered by retiree health benefits, I cannot create a variable for individual retiree health benefits. However, a variable identifying whether either spouse is eligible should capture most of the variation across the sample.

[5] For a detailed discussion about retirement measures and their implications see Gustman, A. and Steinmeier, T. *Retirement Outcomes in the Health and Retirement Study* NBER Working Paper #7588, 2000.

retired from one job, but started another job retired as long as they themselves consider themselves retired. The second considers an individual retired if they identify as retired *and* are not working; it excludes individuals who identify neither as retired nor working. Both analyses exclude individuals who report their labor status as disabled.[6]

Although the HRS has administrative data on pensions for many of its initial respondents, in this paper I use the self-reported data. There is evidence that individual reports of their pension plan characteristics are often inconsistent with information from administrative sources (Gustman and Steinmeier, 2001). The primary reason I use self-reported data is that the administrative pension data is still being collected for respondents who were added to the sample in 1998. In addition, it may be better to use incorrect data that reflects the individual's incorrect beliefs, when studying whether individuals respond to retirement incentives or wealth shocks. In their study of responses to changes in pension incentives, Chan and Stevens (2000) justify their use of self-reported pension data for this very reason.

The following section presents descriptive statistics and regression results from the difference-in-differences analysis. All estimates use individual HRS sample weights and equations are estimated separately for men and women.

IV. Results

Descriptive statistics, by gender, for the 1992 and 1998 samples used in this study are in Table 1. Samples in 1998 are slightly smaller than those in 1992. The samples look similar between the two years in most ways. The average age in the samples, which are restricted to individuals ages 55 to 60, is 57. About 80 percent of the men are married and 70 percent of the women are married. About 15 percent of men and a slightly higher proportion of women report being in fair or poor health. There are slightly fewer blacks in the sample in 1998 but the percent Hispanic does not change much. For both men and women, education level is higher in 1998 than it is in 1992. Labor supply measures for

[6] I have estimated these equations with disabled individuals and the results on the variables of interest are similar.

men are roughly the same between the two years but more women work in 1998 than in 1992.

Pension coverage changes in several ways over the six-year period. The percent of women without a pension decreased 51 to 43 percent while coverage among men stays constant at 77 percent.[7] Over this period, coverage by DC pensions increased – from 38 percent to 46 percent among men and from 25 percent to 33 percent among women. Coverage by DB plans decreased among men from 39 percent to 31 percent and stayed about constant for women at 24 percent.

Table 2a provides regression results for men and table 2b has results for women. I will first discuss the results for men when using self-reported retirement as the dependent variable, presented in the first column. Some family eligibility for retiree health benefits and being in bad health both increase the probability that a man reports he is retired. Education less than high school graduation decreases the probability that he reports being retired.

The coefficient on *DB Pension* states that men with DB pensions were 17 percentage points more likely to be retired than men with DC pensions. This is not surprising given the early retirement incentives built into many DB pension plans. The coefficient on *No Pension* is also significant and positive, suggesting that men without a pension are 4 percentage points more likely to be retired than men with DC pensions. This is slightly surprising but is likely capturing the fact that not having a pension is likely correlated with lower wages. This finding is consistent with the belief that once controlling for wealth differences, high wage workers retire later than low-wage workers (Chan and Stevens, 2000). The coefficients on the control groups interacted with 1998 are the difference-in-differences estimates of a wealth effect. The magnitude of the effect depends on which group of workers is used as a control. The difference between DC and DB retirement rates decreased by 8 percentage points over the period, suggesting that wealth shocks led to an 8 percentage point increase in retirement. This effect is significant at the 5 percent level. The comparison with workers with no pensions

[7] This number is much higher than the overall population rate of coverage reported earlier from Clark and Quinn (1999) because of the age range of the population considered.

suggests the wealth effect is slightly smaller – 6.5 percentage points, and it is also significantly different from zero at the 5 percent level.

The second column reports estimates using the second definition of retirement – identifying as retired *and* not working. This analysis excludes individuals who are unemployed or out of the labor force for reasons other than retirement. The coefficients are virtually identical to those in the first column. Figure 5a illustrates these estimates graphically, by plotting the predicted proportion of white, married, 58 year old men who have completed college and are in good health who are retired, using coefficients from column one.

The results for women, in Table 2b are not as consistent across the two specifications. This is due to the different treatment of individuals who are not in the labor force in each equation. The first equation is estimated among all non-disabled women, regardless of their work history and the second excludes women who out of the labor force for reasons other than retirement. Thus, in the first set of results, the coefficient on *No Pension* is likely to be biased toward zero due to the fact that not having a pension is correlated with not ever being in the labor force, and women who have not had a strong attachment to the labor force are less likely to identify as "retired." For this reason, I prefer and discuss the second analysis, which excludes those women.

Like men, women with some family eligibility for retiree health benefits, in bad health, and of older ages are more likely to report being retired and not working. Married women are also more likely to be retired. Low educational attainment, which reduced the probability of early retirement for men, does not seem to have an effect on women. Hispanic women are less likely to be retired.

For the most part, the coefficients on the pension variables are similar to the ones estimated for men. The level difference in retirement probabilities between women with DC pensions and women with DB pensions is slightly smaller than it was for men – 13 percent versus 18 percent. While the difference-in-differences estimates of wealth effects are the same sign as they were for men, they are slightly smaller. The estimate when using women with no pensions as the control group is not significantly different from zero at the 5 percent level although it is at the 10 percent level.

V. Discussion

The estimates provide evidence that positive wealth shocks lead to early withdrawal from the labor force. There are several reasons to be cautious in the interpretation of the results.

The first concern is that although my identification strategy exploits variation in wealth gains due to arguably exogenous pension plan type, it ignores the variation in other forms of household wealth – IRAs, stocks, bonds, housing, etc. This should not bias my coefficients unless other wealth holdings were correlated with pension plan type. If we assume that households diversify their portfolios, then pension type will necessarily be correlated with other wealth holdings. Because DB pensions guarantee a fixed stream of income after one retires, and the value of DC pensions fluctuates with the value of the stocks or bonds in which they are invested, individuals with DB pensions may hold riskier assets in their portfolios. For example, individuals who have invested in stocks through their 401(k) DC pension plan may be less likely to hold stocks independently than individuals who have DB pensions. Then individuals with DB pensions would have greater capital gains in their private holdings than individuals with DC pensions. This would bias my results to zero.

The bias however, is likely to be small for several reasons. First, among individuals in the HRS, there do not seem to be substantial differences in portfolio shares (of non pension assets) across pension type. Figures 6a and 6b graph portfolio shares for individuals by pension type, in 1992 and 1998. In 1992, the mean share of a household wealth allocated to stock was 6 percent among individuals with DC plans and 5 percent among individuals with DB plans only or with no pension plans. Bond shares were almost identical. Individuals with DC pension plans had a slightly higher mean share in IRAs, which is the opposite of what one might expect. Individuals without DC pensions allocate more of their assets to business and real estate and the subset with no pension allocates less of their assets to home equity. The shares in 1998 are similar except all three groups have a greater mean share in stocks and the difference in business and home shares seems to have decreased. In addition to this suggestive evidence that portfolio shares do not vary by pension type, numerous studies have found that 401(k) contributions do not crowd out overall private savings (Poterba, et al, 1995).

A greater concern with the D-D result is related to the composition of the treatment and control groups. Because coverage by DB plans has been declining since the 70s and coverage by DC plans has been increasing, it is possible that the composition of my treatment and control groups changed over the period. Much of the shift occurring in the nineties was among younger workers – i.e. new workers were much less likely to be offered coverage through a DB plan than previously. However, many companies that had DB pensions did convert policies to DC policies. If the companies that converted were ones with "weaker" DB plans, the DB plans that "survived" would be the generous ones. Thus, it is possible that the 55-60 year old workers in the 1992 DC group are workers with relatively "weak" pensions. This would bias my results toward a positive finding.

The similarities in portfolio shares across groups and over time suggest that there have not been big changes in the composition of these groups, at least with respect to portfolio composition. Friedberg and Webb (2000) examine characteristics of DC and DB pension holders in the 1992 HRS and find that people with different pensions are "strikingly similar." Non-pension assets and earnings are almost identical, and education levels are very close to one another. Additional evidence of this is that when they estimate retirement regressions as a function of pension type, adding job characteristics such as tenure and industry does not change results. The fact that early DC holders were similar to DB holders suggests that DC holders in 1992 are *not* dominated by individuals whose weak DB pension plans had been converted. Nevertheless, I run some additional tests to see if characteristics of the treatment and control groups changed over time.

I estimate OLS regressions of the same form I used for the retirement equations, with log net worth and education as left-hand side variables. Here, net-worth excludes any pension wealth, so I am testing whether there were changes across the treatment and control groups over time with respect to wealth outside of the pension wealth gains assumed in this paper. If the coefficient on the interaction term of year and pension type is statistically significant, I would reject the hypothesis that the characteristics of the groups stayed constant over time. Again, the coefficient on the pension dummies captures time invariant differences between the groups – we may expect those to be statistically significant, although Friedberg and Webb (2000) do not find such differences across pension type among individuals with pensions. Table 3 reports these results. The results provide no evidence that the composition of these groups has changed with respect to education or wealth, for either gender. They do show that men and women without any pension coverage, and men with DB pensions only, have fewer years of education than individuals with DC plans.

VI. Conclusion

Many papers in the 80s and 90s studied the effects of DB pension characteristics and the trend towards earlier retirement. Friedberg and Webb (2000) show that this trend may be reversing slightly as DC plans become more common and DB plans less common. This is because DC plans have smooth accrual and thus no work disincentives. Despite the lack of non-linearities in accrual, DC pension plans may have their own effect on retirement rates. The results of this paper suggest that the effect may be a simple wealth effect. Individuals whose DC balances are higher than anticipated may retire earlier simply because they can afford to. Individuals whose balances tumble (as some have in the past year) may chose to postpone retirement a bit because they feel financially constrained. Indeed, the regression results showing that the difference between DB early retirement rates and DC early retirement rates shrunk by 7 to 8 percentage points over the nineties suggest it may be too early to conclude that the shift to DC pensions will lead to increases in the age of retirement.

The difference-in-differences results presented in this paper provide preliminary evidence consistent with a wealth effect. Individuals with DC pensions stood to gain substantially in the nineties. The increase in early retirement among men with DC pension between 1992 and 1998 suggests that they may be consuming more leisure in response to this wealth gain. The fact that retirement rates did not change for other men – those with DB pensions only and those with no pensions, makes it unlikely that this increase in early retirement is due to other societal changes. Unfortunately, such a reduced form approach does not allow for much of a conclusion on the magnitude of individual responses to wealth shocks. Ongoing work estimating retirement equations as a function of actual unexpected gains should provide more detailed evidence.

As a greater share of retirement income flows from risky assets like 401(k) plans invested in stocks, the retirement security of households is subject to greater uncertainty. Allowing households to invest a share of their Social Security wealth in assets of their choice will increase this resource uncertainty further. Thus, evidence of wealth effects and the simple existence of large wealth shocks have clear implications for public policy and Social Security. While the shocks in the nineties were positive wealth shocks, many individuals recently experienced negative wealth shocks as the stock market tumbled. These affected not only current workers who were planning their retirement, but also retired workers whose consumption depends on the balances in their 401(k) plans and IRAs, and Social Security. If workers responded to pension gains in the nineties by retiring early as the results in this paper find, how are they responding to huge pension losses now that they are retired? Data collected in the 2000 HRS and to be collected in the 2002 HRS will provide information on how these early retirees are adjusting to negative wealth shocks.

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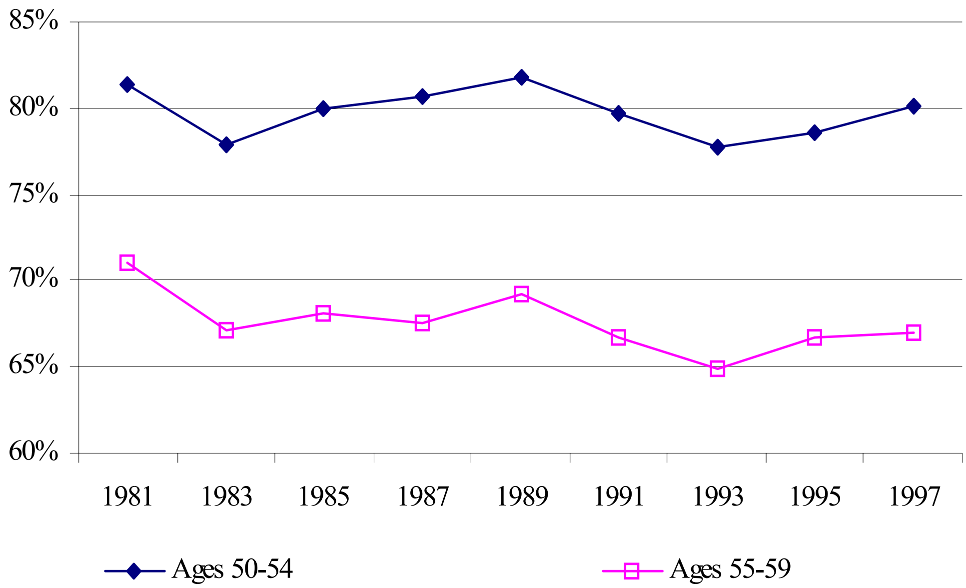
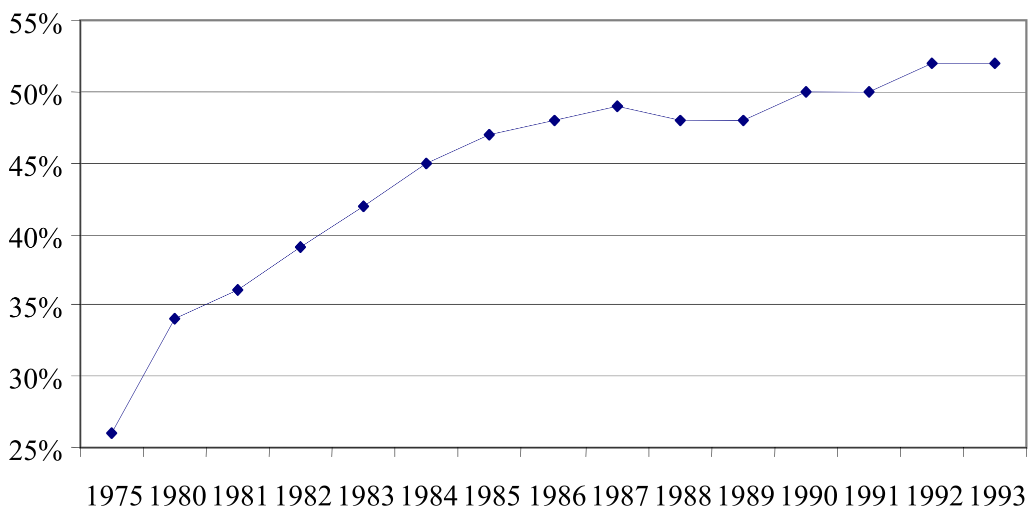
Figure 1: Percent of Men Working Full Time



Source: March Current Population Survey (CPS)

Figure 2: Percent of Individuals with Pensions with DC Pension



Source: Employee Benefit Research Institute

Figure 3: Year End Level of Standard and Poor 500 Index

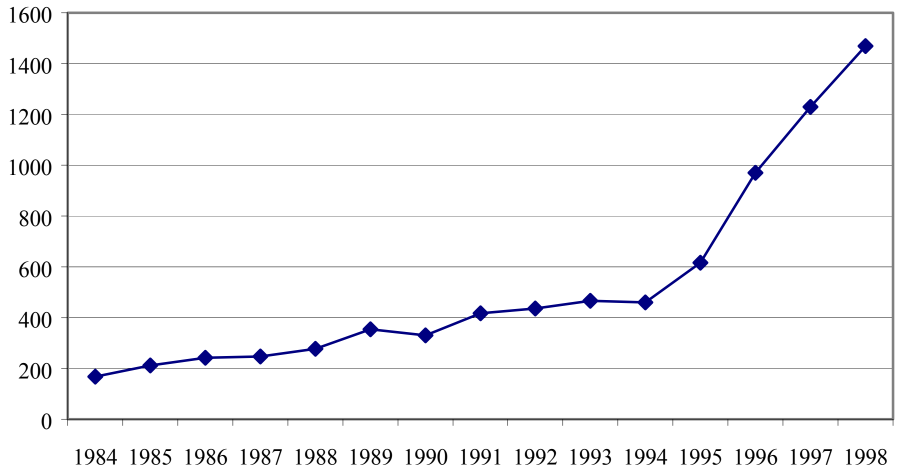
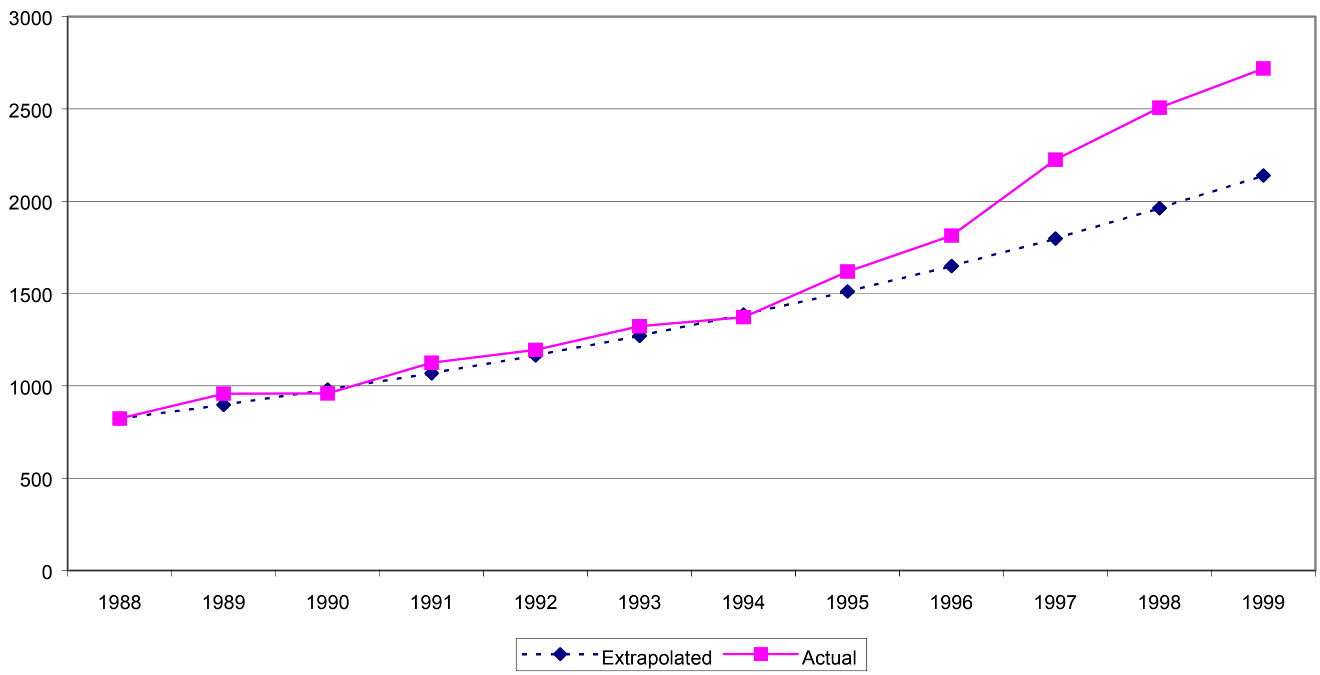


Figure 4: Aggregate DC Balances, billions of $1999

Actual and Extrapolated based on pre-1994 average changes



Source: Board of Governors of the Federal Reserve System: Flow of Funds Accounts of the United States, Third Quarter 2000

Table 1
Descriptive Statistics

	Men				Women			
	1992 (n=2118)		1998 (n=1794)		1992 (n=2448)		1998 (2176)	
	Mean	Std.	Mean	Std.	Mean	Std.	Mean	Std.
Ret. Health Ins	0.611	0.488	0.492	0.500	0.590	0.492	0.503	0.500
Married	0.825	0.380	0.793	0.405	0.728	0.445	0.696	0.460
Bad Health	0.141	0.348	0.147	0.355	0.171	0.377	0.182	0.386
Black Race	0.086	0.280	0.052	0.223	0.095	0.294	0.074	0.262
Hispanic	0.050	0.218	0.059	0.235	0.062	0.241	0.059	0.237
HS Drop	0.234	0.423	0.180	0.384	0.250	0.433	0.188	0.391
College Grad	0.436	0.496	0.501	0.500	0.335	0.472	0.432	0.495
Age	57.46	1.71	57.19	1.73	57.42	1.71	57.13	1.74
Working	0.843	0.364	0.866	0.341	0.625	0.484	0.692	0.462
Self Report Retired	0.150	0.357	0.161	0.367	0.098	0.297	0.108	0.311
DC Pension	0.376	0.484	0.458	0.498	0.248	0.432	0.328	0.470
DB Pension Only	0.392	0.488	0.310	0.463	0.244	0.429	0.238	0.426
No Pension	0.232	0.422	0.232	0.422	0.508	0.500	0.433	0.496

Table 2a
Linear Probability Model Results for Men

Variable	*Dependent Variable=1 if Self Reports as Retired* Coefficient (Robust SE)	*Dependent Variable=1 if NOT currently Working and Reports Retired* Coefficient (Robust SE)
Ret. Health Ins	0.1658*	0.1701*
	(0.0126)	(0.0130)
Married	-0.0209	-0.0269
	(0.0165)	(0.0175)
Bad Health	0.0682*	0.0744*
	(0.0185)	(0.0196)
Black Race	0.0075	0.0083
	(0.0195)	(0.0206)
Hispanic	-0.0059	-0.0014
	(0.0190)	(0.0210)
HS Drop	-0.0421*	-0.0441*
	(0.0161)	(0.0168)
College Grad	-0.0015	-0.0017
	(0.0144)	(0.0148)
age55	-0.1209*	-0.1266*
	(0.0212)	(0.0219)
age56	-0.1035*	-0.1086*
	(0.0211)	(0.0217)
age57	-0.0825*	-0.0859*
	(0.0211)	(0.0216)
age58	-0.0303	-0.0340
	(0.0232)	(0.0239)
age59	-0.0140	-0.0107
	(0.0235)	(0.0245)
Year=1998	0.0883*	0.0917*
	(0.0175)	(0.0180)
No Pension	0.0439*	0.0516*
	(0.0171)	(0.0181)
DB Only	0.1735*	0.1786*
	(0.0192)	(0.0198)
No Pension*1998	-0.0645*	-0.0736*
	(0.0254)	(0.0267)
DB Only*1998	-0.0789*	-0.0849*
	(0.0300)	(0.0306)
Constant	0.0481	0.0543*
	(0.0262)	(0.0274)
n	3,907	3,740
R-Sq	0.1201	0.1225

* Denotes Statistical Significance at the 5% level.

Table 2b
Linear Probability Model Results for Women

Variable	*Dependent Variable=1 if Self Reports as Retired* Coefficient (Robust SE)	*Dependent Variable=1 if not currently Working and Reports Retired* Coefficient (Robust SE)
Ret. Health Ins	0.0943*	0.1507*
	(0.0100)	(0.0139)
Married	0.0145	0.0375*
	(0.0109)	(0.0128)
Bad Health	0.0261	0.0611*
	(0.0139)	(0.0202)
Black Race	0.0069	-0.0105
	(0.0140)	(0.0169)
Hispanic	-0.0449*	-0.0620*
	(0.0115)	(0.0172)
HS Drop	0.0089	0.0145
	(0.0136)	(0.0192)
College Grad	0.0177	0.0137
	(0.0113)	(0.0140)
age55	-0.1031*	-0.1424*
	(0.0180)	(0.0229)
age56	-0.0948*	-0.1289*
	(0.0188)	(0.0244)
age57	-0.0966*	-0.1262*
	(0.0176)	(0.0233)
age58	-0.0657*	-0.0883*
	(0.0190)	(0.0252)
age59	-0.0440*	-0.0699*
	(0.0196)	(0.0252)
Year=1998	0.0547*	0.0701*
	(0.0185)	(0.0201)
No Pension	-0.0029	0.0617*
	(0.0141)	(0.0192)
DB Only	0.1252*	0.1332*
	(0.0210)	(0.0224)
No Pension*1998	-0.0358	-0.0471
	(0.0220)	(0.0283)
DB Only*1998	-0.0676*	-0.0748*
	(0.0308)	(0.0326)
Constant	0.0609*	0.0408*
	(0.0210)	(0.0264)
n	4,620	3,419
R-Sq	0.0724	0.0941

* Denotes Statistical Significance at the 5% level.

Figure 5a: Predicted Proportion of Men who report being Retired

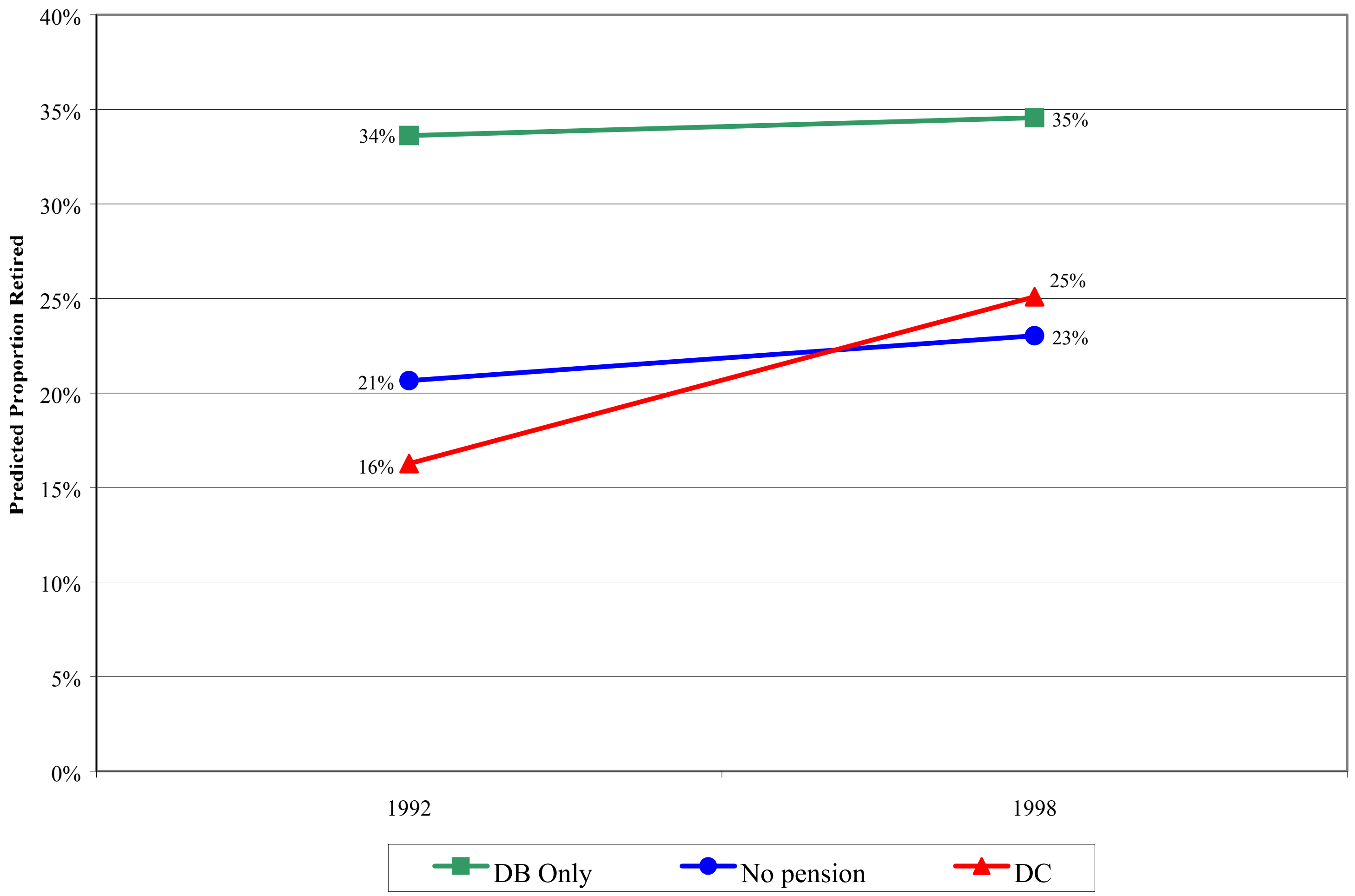


Figure 5b: Predicted Proportion of Women who report being Retired

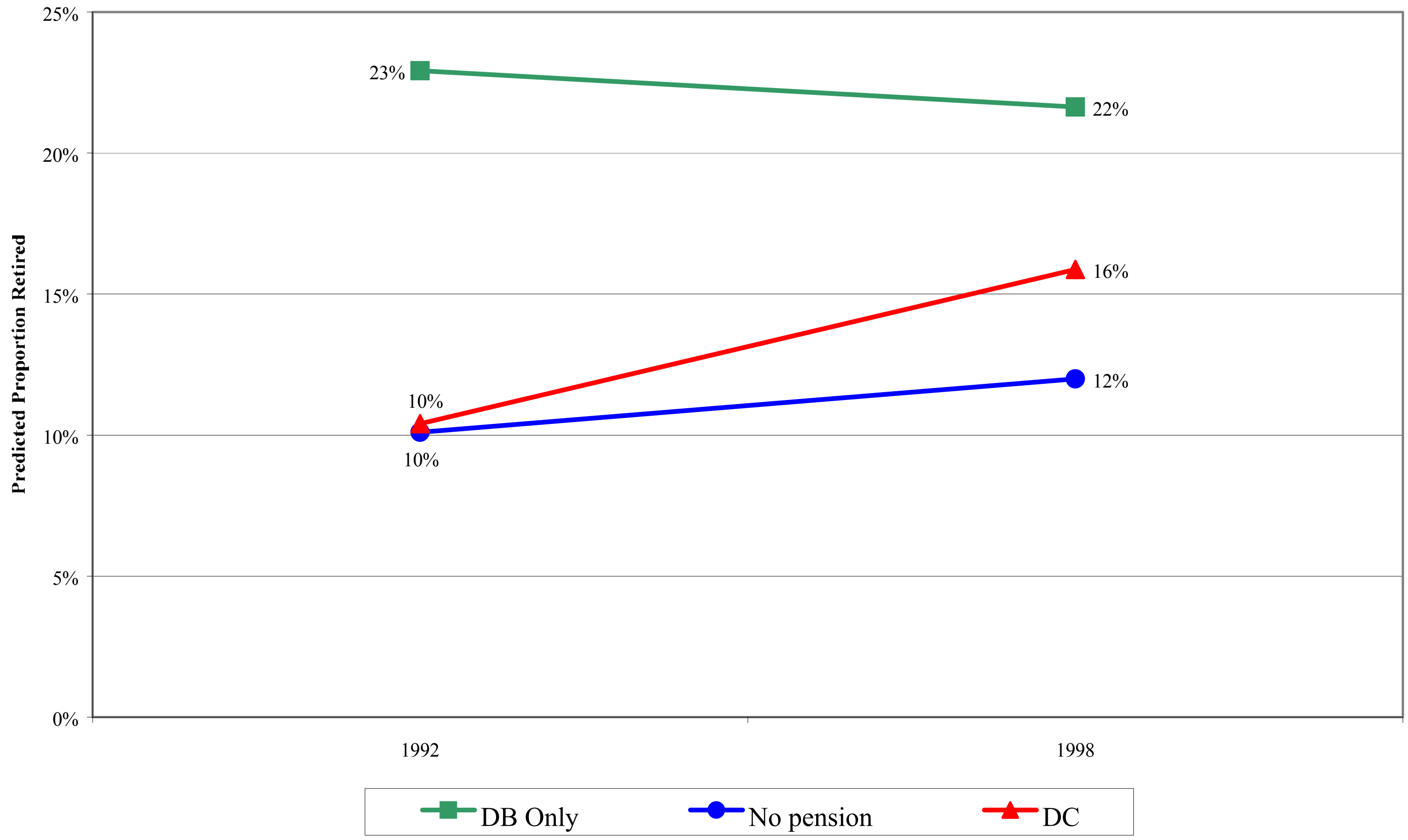


Figure 6a: Portfolio Shares by Pension Type, 1992

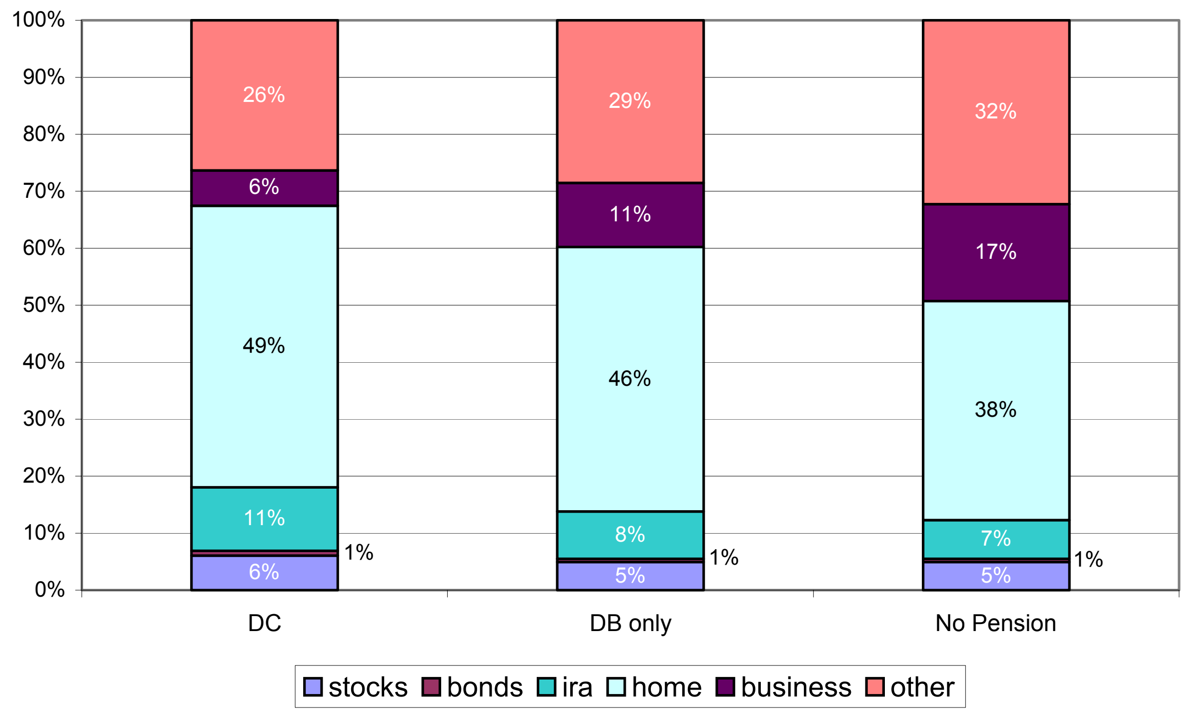


Figure 6b: Portfolio Shares by Pension Type, 1998

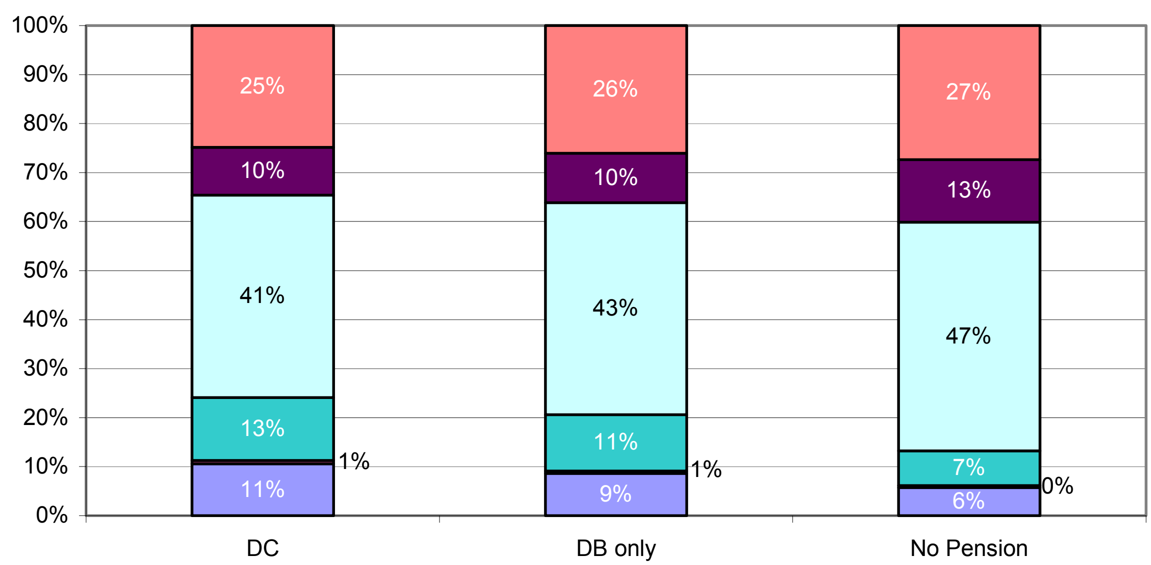


Table 3
Regression Results: Testing for Compositional Changes

	Men		Women	
Variable	Log Net Worth	Education	Log Net Worth	Education
Married	0.6825*	0.0172*	1.0920*	0.1156*
	(0.0805)	(0.1654)	(0.0660)	(0.1196)
Bad Health	-0.4544*	-1.4979*	-0.8135*	-1.2651*
	(0.0739)	(0.1894)	(0.0811)	(0.1086)
Black Race	-0.9453*	-1.5704*	-0.9342*	-0.6879*
	(0.0966)	(0.1813)	(0.0906)	(0.1337)
Hispanic	-1.2622*	-3.0705	-0.8584*	-3.4464*
	(0.1347)	(0.8680)	(0.1195)	(0.2318)
age55	-0.1526	0.2626	-0.2028*	0.2975*
	(0.0967)	(0.1837)	(0.0821)	(0.1393)
age56	-0.2022*	0.8633*	-0.1663*	0.5852*
	(0.0827)	(0.3844)	(0.0839)	(0.2934)
age57	-0.0877	0.3758*	-0.3082*	0.0454
	(0.0798)	(0.1651)	(0.0844)	(0.1329)
age58	-0.0773	0.2924	-0.1093	0.1620
	(0.0851)	(0.1667)	(0.0797)	(0.1283)
age59	-0.0690	-0.0296	-0.0738	-0.1107
	(0.0842)	(0.1747)	(0.0818)	(0.1316)
Year=1998	0.1451*	0.7211*	-0.0224	0.4090*
	(0.0714)	(0.2590)	(0.0827)	(0.1425)
No Pension	-0.0769	-1.4770*	-0.2309*	-1.2114*
	(0.1010)	(0.1906)	(0.0760)	(0.1263)
DB Only	-0.0940	-0.2021	-0.1217	0.1060
	(0.0663)	(0.1497)	(0.0837)	(0.1446)
No Pension*1998	-0.0917	0.2110	0.1382	0.0644
	(0.1466)	(0.5184)	(0.1149)	(0.1877)
DB Only*1998	-0.0321	-0.2680	0.1653	0.4109
	(0.1082)	(0.3085)	(0.1251)	(0.4599)
Constant	11.7654*	13.2384*	11.5794*	13.0440*
	(0.1005)	(0.2422)	(0.0926)	(0.1730)
n	3,721	3,874	4,282	4,605
R-sq	0.1184	0.0931	0.1896	0.1618