Is Wealth Causing Health? Evidence from Stock Market Induced Wealth Shocks.

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Abstract

This paper investigates the effect of wealth shocks on retiree health, mental health and mortality. Stock market fluctuations over the past 18 years are exploited as a natural experiment that generated considerable gains and losses in the wealth of US retirees. Using data from the Health and Retirement Study I find that a 10% wealth increase leads to an improvement of 1.5-3% of a standard deviation in each of four broad physical and mental health measures including mortality. Effects are heterogeneous across different physical health problems, with most pronounced effects for the incidence of high blood pressure, smaller effects for heart problems, strokes and cancer, and no effects for arthritis, diabetes and lung diseases. These results suggest that wealth shocks strongly affect retiree health in the US and that psychological factors seem to be an important channel through which these effects are delivered.

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1 Introduction

It is a well-established fact that richer people are healthier, happier and live longer. However, it is little understood to which extent this relationship is driven by a causal effect of wealth on physical health, mental health and mortality. Money might buy health, but health also might reversely affect income and expenditure. And third factors, such as education or ability, are likely to influence both wealth and health simultaneously. The existing literature on the wealth-health relationship finds little evidence for a causal effect of wealth on adult health in developed countries and tends to emphasize reverse causality running from health to income.[1] Contrary to these findings, this paper provides evidence that wealth shocks have a strong positive causal effect on physical health, mental health and mortality of wealthy retirees in the US.

To identify the causal effect of wealth on adult health, this paper exploits stock market induced shocks in the wealth of US retirees. Over the past two decades about one-third of US retiree households held part of their wealth in stocks. And for stock-holders the fraction of life-time wealth held in stocks amounted on average to about 20%. As a consequence the booms and busts in the US stock market over the past two decades generated considerable unexpected gains and losses in the wealth of stock holding retirees. Using data from the Health and Retirement Study (HRS) I predict wealth shocks by the interaction of individual stock holdings with stock market changes and analyse their effect on various health measures. I find that a 10% change in life-time wealth over a two year period is associated with a change of 1.5-3% of a standard deviation in four different health measures: A health problems index, self-reported health, mental health and the probability to survive two more years. Analysing individual health problems indicates a strong effect on hypertension, smaller effects on heart diseases, strokes and cancer, and no effect on arthritis, diabetes and lung disease. These effects are surprisingly large. The comparison with the overall wealth-health relationship in the sample suggests that estimated effects are of equal size or even larger then the overall relationship.

Wealth shocks are predicted by the interaction of the fraction of wealth held in stocks in the past period with the stock market change between the past and the current period. To interpret their association with changes in health as causal effects of wealth on health, predicted wealth shocks must satisfy two condition. They must be independent of any unobserved heterogeneity and their effect on health must exclusively run through changes in stock wealth.

As stock holdings are likely to be endogenous I control separately for the fraction of wealth held in stocks. In other words, I compare health changes for individuals with the same fraction of wealth held in stocks at different points in the stock market cycle. One concern might be that a retiree with 20% of her wealth in stocks right before a boom might not be

[1] For reviews of the literature see Smith (1999), Deaton (2003), Cutler, Deaton and Lleras-Muney (2006), Cutler, Lleras-Muney and Vogl (2010).

comparable to a retiree with 20% in stocks right before a bust. To rule out that estimates are driven by such correlation of unobservables with the stock market cycle I present regressions in which the individual stock fraction is instrumented by the individual's stock fraction in the first wave or by the average stock fraction over the whole sample. Both initial and average stock fractions are independent of were we are in the stock market cycle.

The exclusion restriction, that predicted shocks affect health exclusively through the change in stock wealth, might be invalidated for several reasons. Stock market changes could be correlated with changes in other wealth components such as house prices. Such correlation would bias the wealth shock coefficient because predicted wealth shocks would systematically under- or overestimate actual wealth shocks. Looking at changes in reported wealth for retirees with and without stocks in the previous period, however, indicates that this is unlikely to be the case. While reported wealth of stock holding retirees strongly correlates with the changes in the stock market, the correlation for those without stocks is essentially zero. The exclusion restriction would further be invalidated if the stock market or more generally the macroeconomic environment had non-wealth effects on retiree health. I argue that retirees without stocks are at least equally strongly affected by such direct effects as those with stocks. Therefore I include time effects to absorb any macroeconomic shocks common to both groups.

Several robustness checks give support that estimated effects actually represent causal effects of wealth shocks on health. Further, effects are quite homogeneous across age, gender and negative vs. positive shocks. Only for mortality effects are especially strong for retirees above age 75 and if shocks are negative.

The evidence of positive effects of wealth shocks on health presented in this paper is in contrast with much of the existing literature. Adams et al. (2003), Smith (2005) and Michaud and Van Soest (2008) find that lagged wealth, income and other components of SES do not predict changes in health for elderly in the US. Ruhm (2000) analyses mortality-unemployment state-level time series finding that higher unemployment is associated with lower mortality. Deaton and Paxson (2001) and Deaton and Paxson (2004) look at mortality-income cohort times series for the US and the UK, respectively. Their results indicate that periods of greatest mortality declines do not match with periods of greatest income increases. Snyder and Evans (2006) compare cohort mortality around the so called 'Social Security notch'[2] finding that the notch generation which received lower Social Security had slightly lower mortality.

It is, however, difficult to derive causal inference about the effects of wealth on health from these studies. For the first group of papers, which employes an approach closely related

[2] In the 1970s a calculation error lead to unexpected increases of Social Security benefits for the 1910-1921 birth cohorts. The error was corrected in 1977, abruptly decreasing benefits and generating the so called 'Social Security notch'.

to granger causality, it is not clear whether lagged income or wealth is truly exogenous.[3] Also wealth and income at the individual level are notoriously subject to measurement error which makes it -in the absense of an instrument- hard to distinguish a zero effect from attenuation bias. The second branch of papers tackles endogeneity using aggregate income variation that cancels out unobserved heterogeneity at the individual level. But aggregate shocks that generate or correlate with such aggregate income variation are likely to have also non-income effects on health.[4] This invalidation of the exclusion restriction, as the authors of these papers note, makes it difficult to infer a zero or negative causal effect of wealth on health from these findings.

So far only a few papers identify income shocks at the individual or household level. Jensen and Richter (2003) look at the pension crisis in Russia during which many retirees were not paid their pensions for an extended period of time. They find that for affected pensioners, who faced on average an income decrease by 24%, nutrition and use of health care decreases while the likelihood to die over the next two years increases by 5%. Case (2004) analyses the effect of a pension reform in South Africa that lead to a large and unexpected income increase for black and coloured pensioners. She finds positive health effects, which run through improved nutrition, living conditions and psychological factors such as a reduction of stress. Lindahl (2005) uses lottery winnings in Sweden as source of exogenous income variation estimating that a 10% income increase improves self-reported health by 4-5% of a standard deviation. Sullivan and von Wachter (2008) find that exogenous job displacements increase the mortality hazard of male US workers by 50-100% during the years following the job loss, while long-run earnings decrease by 15-20%. These estimates are similar to the mortality effects found in this study, though they are likely to be driven to some part by non-income effects of unemployment on health.

Should we, however, expect positive effects found for poor retirees in developing countries or displaced workers in the US to carry over to wealthy, stock holding US retirees? Health inputs like medical treatment, medication or mere calorie intake might be affected by wealth shocks for poor retirees in Russia or South Africa. But this is probably not the case for stock holding US pensioners, who have even after a considerable wealth loss enough money left to affort basic pills and burgers. Further medicare covers the entire 65+ population in the US so that wealth shocks do not affect health insurance coverage unlike for displaced workers. Consumption of healthy food and purchase of a healthy environment could be more responsive determinants of retiree health in the US than basic health inputs. But two years might not be enough time for luxury goods to become manifest in physical health outcomes.

[3]"[T]here are influences between income and health that run in both directions, and the lags can be as long as a human lifetime." (Deaton 2003, p. 121). For further discussion see Mealli and Rubin (2003), and Cutler (2005).

[4]For example, the cause of death most (negatively) affected by the unemployment rate in Ruhm (2000) are care accidents, which is probably due to less traffic in times of less economic activity rather than a better driving style of unemployed people. Similarly, changes in cohort mortality over the time period analysed by Deaton and Paxson (2001) and Deaton and Paxson (2004) were probably dominated by technical innovation and new diseases (e.g. Aids).

A more plausible channel would be psychological factors such as happiness about pleasant trips that were not affordable before or stress and sadness about a lost fortune that was planned as an inheritance for the grand children. Such channel would be in line with much of the literature in medicine, psychology and biology, that emphasizes psychological factors as important determinants of mental and physical health. There is broad agreement that stress causes depression and coronary artery diseases (for a review see Strike and Steptoe 2004), which in turn increase the likelihood of other severe diseases such as heart diseases and strokes and eventually death. Moreover various studies have suspected stress to be a driver for certain cancer types (for a review see Chida et al. 2008). There is also some agreement that positive emotions have a positive effect on health (for a review see Chida and Steptoe 2008).[5] This paper finds strong wealth shock effects on high blood pressure and mental health and smaller effects on heart problems, strokes and cancer. Given the circumstances and the timing of the wealth shocks as well as the findings from the bio-medical literature, this seems to be a plausible effect pattern.

The focus of this study on retirees is both an advantage and a limitation. Compared to younger adults retirees have a lot of wealth and heterogeneity in wealth composition so there is a lot of variation to exploit. And as they do no longer participate in the labour market effect of stock market shocks running through labor demand as well as labor supply responses are limited. That makes it easier to separate wealth shocks from other confounding effects. Further the elderly are closer to the margin of severe health conditions than younger adults so that wealth effects on health are more likely to become manifest in measurable health outcomes. On the downside, estimates of wealth shock effects on retiree health might not be comparable to effects of wealth on health for younger adults that participate in the labor market. The latter are more flexible and in better physcial shape and therefore likely to be less affected by wealth shocks. The results of Sullivan and von Wachter (2008) indicate, however, that this is not necessarily the case.

Section II describes the data set I use, while Section III discusses the identification strategy in detail that leads to the empirical specification in Section IV. Section V presents the findings and Section VI concludes.

[5]For example, in experiments healthy subjects were exposed to rhinovirus or influenza virus and those who scored ex-ante higher on optimism and happiness scales were less likely to get infected (Cohen et al. 2006).

2 Data

The data used in this study comes from the Health and Retirement Survey (HRS). The HRS is a biannual panel starting in 1992 with 12,654 individuals representing US adults aged 51 - 61 in 1992 (born during the years 1931 - 1941). In 1998 and 2004 new cohorts were added to keep the sample representative for those of age 51 and older. This study uses 9 HRS waves (1992 - 2008) covering over 170,000 person-year observations of about 30,000 individuals from 20,000 households. The analysed sample is restricted to singles and couples that were retired in the previous wave (i.e. both respondent and spouse - if existent- were neither working nor unemployed) and provided information on wealth, stock holdings and retirement income. The final regressions sample uses about 90,000 person-year observations. Of these, 40,000 refer to a single response per household-year (no spouse existent) while the remaining are responses of two spouses (two observations per household-year). The interview month is known, so that the HRS data can be matched to monthly stock market data from the Standard & Poor's 500 (S&P500).[6]

a. Wealth data

The HRS contains detailed information on income and wealth holdings.[7] Wealth and individual income is reported by one 'financial respondent' per household and matched to the spouse if existent. Therefore wealth varies only between households while health measures and other individual variables differ between spouses. Financial information is reported in exact amounts and unfolding response brackets are offered if exact amounts are unknown.

a.1 Current household wealth

Household wealth consists of: net housing wealth[8]; real estate wealth; vehicles; business wealth; individual retirement accounts (IRAs); stocks and mutual funds; checking and savings accounts; cds, savings bonds and treasury bills; bonds; other savings; debts.

a.2 Life-time wealth

Life-time wealth (W_t) is the discounted sum of current wealth and future income.

$$W_t = A_t + \sum_{\tau=0}^{T-t} Y_{t+\tau} \frac{S_{t,\tau}}{(1+r)^{t+\tau}}$$

A_t is current wealth, $Y_{t+\tau}$ future earnings, r the real annual interest rate and $S_{t,\tau}$ the individual survival rate. Current wealth and *past* earnings are well documented in the HRS. Fortunately, retiree income - consisting of pensions, annuities, old age social security and veteran benefits - can be expected to stay constant after the first receipt until the

[6]Interviews which start in one month and end in a later month were dropped as well as spouse interview's that were not conducted in the same month as for the financial respondent.

[7]This study uses cleaned and partly imputed wealth data from the 'RAND Income and Wealth Imputations' file. Variables that are not included in the RAND file were added from the HRS raw data.

[8]Housing wealth does not include secondary homes, as this information is not available in wave 3.

individual's end of life (receipts stay constant in real terms). Hence for retirees we can simply take past year's annual income from pensions, annuities, old age social security and veteran benefits as the expectation for future income.[9] Interest rate expectations (current values taken from the SSA report) are assumed to stay constant as well. (τ)-year survival rates are constructed by gender (g), 10-year birth cohort (c) and age (t) using the SSA life tables.[10]

$$W_t = A_t + (SS_t + PAI_t + VetBen_t)\sum_{\tau=1}^{T-t} \frac{S_{t,\tau,g,b}}{(1+r)^{t+\tau}}$$

Social security benefits pose a potential problem as their receipt does not depend on labor force status and even though people can claim social security at age 62 (if entitled) there are financial incentives to postpone social security take-up to age 65 (Coile et al. 2002). Thus for retirees below age 65 who do not report to receive social security it is not clear whether they are postponing or whether they are not entitled to social security payments and life-time wealth cannot be constructed. Fortunately the HRS includes a question about future expected social security payments, so that it can be tested whether results are sensitive to the exclusion of retirees who do not yet get social security but expect payments in the future.

Another complication are different life expectancies within households, i.e. within couples. Typically wives can expect to survive their husbands, but it would be demanding to calculate all different survival constellations and the corresponding exact survivor benefit amounts. For simplicity a couple's life-time wealth is calculated by applying the couple's mean life expectancy to the sum of the couple's total annual income.

$$W_t = A_t + (SS_t + PAI_t + VetBen_t + SS_t^s + PAI_t^s + VetBen_t^s)\sum_{\tau=1}^{T-t} \frac{S_{t,\tau,g,b} + S_{t,\tau,g,b}^s}{2(1+r)^{t+\tau}}$$

Restricting the sample to singles in order to avoid this simplified life-time wealth formula does not affect the pattern of the estimated effects, only standard errors increase due to the decreased sample size.

a.3 Stock holdings

A central ingredient of the measure of wealth shocks used in this study is the amount of stock holdings. Direct stock holdings are well documented in each wave, but they do not include stocks held in IRAs. Retirees often hold considerable fractions of their wealth in (often various) IRAs. To calculate the total amount of stock holdings it is therefore

[9] The HRS usually reports monthly (past month's) income which is multiplied by 12 to obtain future annual income.

[10] One might worry about the direct influence of age, gender and birth cohort trough the survival rate on predicted wealth shocks, which are rescaled by life-time wealth. However, results are robust against different specifications of the survival rate. Even setting it to equality (taking only next year's income instead the whole life-time sum) does not change results qualitatively.

important to known the percentage of each IRA invested in stocks.

In 2006 and 2008 for each IRA the exact percentage invested in 'stocks and mutual funds' (i.e. not in interest-earning assets) is reported. In 1998 - 2004 three categories indicate whether IRAs are invested 'mostly in stocks', 'mostly in interest-earning assets', or 'about evenly split'. I translate these categories into 100%, 0%, and 50% invested in stocks, which results in roughly the same investment distribution in 2004 as for the exact information in 2006 and 2008. The assumption of a stable investment distribution between 2004 and 2006/2008 for US IRAs is checked with data from the Survey of Consumer Finances (SCF), a US representative triennial survey with about 22,000 households per wave. The SCF reports exact information on the IRA fraction invested in stock for 2004 and 2007. The cumulative distribution function does not change significantly between SCF 2004 and SCF 2007, indicating that IRA investment distributions in the US were indeed stable over that period.

For the three initial HRS waves, 1992 - 1996, no information is available on IRAs invested in stock. In order not to lose these entire waves, IRAs in these years are assigned the average IRA stock investment rate of the year 1998 (52%). This adds a considerable amount of noise and results should be tested against the exclusion of these waves. Notice that replacing a regressor by its mean does not lead to 'classical' measurement error, ie. error uncorrelated with the true value and correlated with the included regressor. Instead the resulting error is correlated with the true value but not with the regressor.[11] Therefore estimates are unbiased, only standard errors are increased.

a.4 Wealth summary statistics

Table 1 summarizes the number of observations, age, retiree rates and main wealth measures. In 1998 older than average cohorts are added and younger cohorts in 2004, leading to discontinuous jumps in these measures. Retiree rates increase with age, but even at age 70 for 30% of the households at least one spouse is still in the labor force. The fourth and fifth row show the information available on the fraction of IRAs invested in stocks and the respective imputed values. The regression sample includes all households that in the *previous* wave (i) were retired and (ii) reported wealth, retiree income (for the construction of life-time wealth) and stock holdings. Hence there are no observations in the first wave and the wealth information in wave 1 refers to the number of observations in wave 2. In the regression sample on average about half the life-time wealth is held in current wealth and about 1/3 of all households hold at least some stocks. Since wealth shocks are predicted for households with stocks, these are the 'treated'. They are on average twice as wealthy as retirees without stocks and hold about 20% of their life-time wealth in stocks.[12] For a

[11] Think of the error as the residual from the regression of the true value on a constant; the constant is the mean and orthogonal to the residual

[12] In the first three waves the fraction of households with stocks is inflated due to the assumption that any IRA is invested 52% in stocks. For the same reason stock holdings in these waves are artificially low because many poor households with small IRA accounts that in reality do not own any stocks are included in the group of stock holders.

discussion of wealth changes of these different groups see the next section (Figure 4 and Tables 3, 4).

b. Stock market data

To predict the average gains and losses in stock portfolios, stock holdings are interacted with changes in the Standard and Poor's 500 (S&P500) index. The S&P500 is the weighted average of 500 of the biggest actively traded companies in the US and therefore represent a broad indicator of the US stock market.[13] Figure 1 shows the S&P500 index and periods in which HRS interviews were conducted marked by boxes. As it can be seen, the HRS interviews cover well the two major boom and bust periods the US economy experienced over the past 28 years.

Interview months are known so that the HRS data are not matched to only one S&P500 value per wave but on a monthly basis, resulting in 108 instead just 9 distinct matched S&P500 values. Even more stock market variation results for individual first differences. Since individuals are interviewed in different months each wave, the change in stock market prices depends on both the month of the current as well as the month of the past interview. The roughly 45,000 individual first differences in the regression sample are matched to about 900 distinct first differences in stock prices. The distribution of these first differences for the S&P500 is shown in Figure 2. However, HRS respondents probably do not exactly know the current value of the S&P500. If we assume that respondents know the value rounded by 50, then we are left with only 36 distinct S&P500 changes in first differences (about the number of different bars in Figure 2).

Figure 3 plots predicted life-time wealth shocks and the S&P500 over time. Wealth shocks roughly range from -40% to +40%. These are dramatic changes. For a retiree who has about 10 years remaining to live a 10% loss in life-time wealth already equals the amount of planned expenditures for a whole year. If she is smoothing consumption, she will have to spend 10% less than planned every month until the end of her life. If a fixed part of her wealth is planned for inheritance, consumption has to decrease by even more. Notice that predicted wealth changes and the S&P500 comove by construction, therefore the co-movement in Figure 3 does not tell us anything about the accuracy of the predictions, i.e. whether the stock market actually affects wealth of stock owning retirees (for a comparison with reported wealth changes see below).

c. Health measures

As dependent variable this study uses different health measures from the HRS: An index of health problems, self-reported health status, a mental health index and mortality. For better comparability of the different measures I follow an approach of van Praag and Ferrer-i-Carbonell (2008) and assign to the categories of each measure the expected value of a standard normal variable conditional on being between the category's lower and upper

[13]The Dow Jones Industrial Average, for example, includes only 30 companies. However, using the Dow Jones instead of the S&P500 delivers similar results.

cut-off points implied by an ordered probit fitted on the raw sample fraction (van Praag and Ferrer-i-Carbonell refer to this as 'probit-adapted OLS'). Standardising the measures by subtracting the mean and dividing by the standard deviation leads to very similar results.[14]

c.1 Index of health problems

The health problem index equals 8 minus the sum of indicators for whether a doctor has *ever* told the respondent that she had a particular disease. The eight included diseases are high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, psychiatric problems, and arthritis. An 8 refers to a respondent who never had any of these diseases diagnosed while a 0 indicates that every disease had been diagnosed at some point. Respondents who report the diagnosis of a health problem in the past but not in the current wave are excluded. First differences are equal or smaller zero.

c.2 Self-reported health and self-reported changes in health

For self-reported health respondent are asked to rate their current health as poor, fair, good, very good or excellent. An additional question, self-reported changes in health, asks whether compared to the previous interview health is worse, the same, or better.[15]

c.3 Mental health index

As a mental health index I use the inverse of the CESD depression score: 6 plus 2 'positive' indicators minus 6 'negative' indicators (i.e. 0=worse, 8=best). The negative indicators measure whether the respondent experienced the following emotions all or most of the time during the past week: felt depressed, everything is an effort, sleep is restless, felt alone, felt sad, and could not get going. The two positive indicators measure whether the respondent felt happy and enjoyed life, all or most of the time during the past week.

c.4 Mortality

Deaths of survey participants are documented since the third wave (1996). In so-called exit surveys a proxy respondent (usually a surviving family member) is interviewed about time and circumstances of the death. Thus deaths are well documented and not just one possible reason for an observed panel attrition. Table 2 lists the number of documented deaths in the HRS and the regression sample over time. In 1994 -1995 additional and on average much older cohorts (the AHEAD sample) were added, explaining the increase in documented deaths between 1995 and 1996. For 2008 so far only a fraction of all deaths have been documented. 'Survival', used as dependent variable in the baseline regressions, indicates whether the respondent survives until the next interview and it is regressed on past wealth shocks. Hence the mortality regression sample includes those who were retired and reported their wealth (incl. stock holdings) at least two periods prior to their death.

[14] The advantage of probit-adapted OLS over standardising is that it takes into account a possibly unbalanced distribution of the sample over the measure's different categories.

[15] Wave 1-6 offers 5 categories to rate the health changes: much worse, somewhat worse, same, somewhat better, much better. For comparability with wave 7-9 the first two and the last two categories are recoded as worse and better, respectively.

3 Identification

This paper seeks to estimate the causal effect of wealth on health. The challenge of this task is the endogeneity of wealth. Wealth does not only affect health but in turn is as well affected by health, and third factors might influence both simultaneously. The ideal experiment to solve the problem of endogeneity and to measure the causal effect of wealth on health would be a lottery that randomly assigns wealth losses and gains to people and measures their health before and some time after the assignment.

This paper exploits the booms and busts of the US stock market over the past two decades as a natural experiment that generated considerable wealth gains and losses for those owning stocks. For a causal interpretation it is required that these stock market induced wealth shocks are conditionally independent ('as good as randomly assigned') and that they affect health only through the change in stock wealth ('exclusion restriction').

The data come from the Health and Retirement Survey (HRS) which interviews individuals every second year in detail about their wealth and different measures of health. The interview date is known so that observations can be matched with stock market data on a monthly basis. Given that interview dates are independent of the stock market (or of respondent characteristics, or both), stock market changes between two interviews of the same individual are random.[16] But stock market induced wealth changes are determined by the interaction of stock market changes with stock holdings and stock holdings are probably not randomly assigned. Therefore it is necessary to control for stock holdings separately. This means that I compare retirees with the same fraction of wealth in stocks over different points in time. One might worry, though, that people with the same fraction in stocks before a boom and before a crash are not comparable. A retiree with 20% wealth in stocks at the beginning of a boom might not just be lucky but better informed than a retiree with 20% in stocks right before a crash. Note that such predictability of stock market returns would contradict well-established evidence of market efficiency from the finance literature (for a review see Malkiel 2003). To rule out that results are driven by stock market predictability I show robustness checks in which I instrument stock holdings by average or initial stock holdings, which are both uncorrelated with were we are in the stock market cycle.

The exclusion restriction requires that stock market induced wealth shocks affect health exclusively through stock wealth. First, stock market changes might correlate with house prices or the valuation of other wealth holdings. Due to HRS data restrictions I cannot include regional information on house prices. But I can compare changes in reported wealth

[16] Serious health conditions might cause interview delays (Adams et al. 2003, p. 12) which could be a problem if stock market changes are correlated with the length of the time interval. I include the number of months between interviews as a control to take care of this potential endogeneity.

for those with and without stocks. The exclusion restriction would be invalidated if wealth holdings of those without stocks were affected by the stock market. Figure 4 shows that this is not the case, comparing the S&P500 with the coefficients from regressions of wealth changes on time dummies for retirees with stocks (bars) and without stocks (circles) in the previous period. Wealth changes for retirees without stocks are positive in all waves and seem uncorrelated with the stock market. For retirees with stocks they follow the up's and down's in the S&P500, especially after wave 4 when t-1 stock holdings are measured more precisely. Table 3 shows the regression underlying Figure 4 (for regressions using individual changes instead of wave aggregates see below).

Further, stock markets (or more broadly the macroeconomic environment) might affect health also through non-wealth channels. For example, a macroeconomic environment in which stock markets collapse might have negative effects on the individual's employment which would probably not only affect her wealth but also directly her health. To rule out such labour market interactions the sample is restricted to retired households. But there might still be direct effects of stock markets on retiree health, e.g. stress and fears of social instability due to bad news in the media. I include time effects to absorb any macroeconomic shock that is independent of stock holdings and therefore common to retirees with and without stocks. Notice that direct health effects of stock market news might be different for these two retiree groups as those without stocks are on average poorer and probably more risk averse. However, I would expect retirees without stocks to be - if anything - more vulnerable to bad news about the economy which would bias results downwards, making it harder to find significant effects.[17] To show that results do not depend on this assumption I present robustness checks excluding those without stocks.

Finally, predicted wealth shocks could under- or overestimate actual wealth shocks if retirees' expectations of stock market returns systematically divert from zero. The last four HRS waves include stock market expectations which indicate a slight correlation with the stock market. As a consequence net-of-expectations wealth shocks tend to be slightly higher than predicted wealth shocks. However, as expectations are only marginal compared to actual stock market changes (ranging from about -6% to +6% compared to stock market changes of -100% to +100%) their inclusion decreases estimates only slightly (results not yet reported in this draft). The overall effect pattern and the main conclusions remain unchanged, while it makes my results less comparable to other studies that do not include expectations.

The identification strategy outlined above leads to a straight-forward setup: The interaction of stock holdings with stock market changes identifies the causal effect of wealth once I control for the main effects. Holding constant the portfolio composition ensures that wealth shocks are randomly assigned and controlling for time effects absorbs macro shocks that might affect health through other channels besides stock wealth. To decide

[17]For example, poorer retirees might be more dependent on public goods like social housing or food programs.

for the best empirical specification a few issues remain to be discussed.

First, the effect of a wealth change on health is likely to depend on the initial wealth level. A $50,000 loss might not be noteworthy for the very rich but is very painful for somebody poor. And what matters is not just current wealth but life-time wealth, i.e. the discounted sum of current wealth holdings and future income. If you still have many years to live any wealth shock can be smoothed over more periods. And if you can expect to earn a lot in the future a given wealth shock can be better compensated by dissaving. Therefore I divide, or rescale, predicted wealth shocks by life-time wealth to estimate more homogeneous -and more meaningful- average effects over different wealth groups. Note that rescaling just by current instead of life-time wealth would overestimate the relative size of shocks for retirees with high pensioner income and exclude those with zero or negative current wealth.

Second, wealth shocks are predicted by the interaction of past period's fraction of wealth held in stocks with the stock market change since then, ignoring any wealth information from the current period. Why not instead taking reported wealth in both the past and the current period and instrument the change by the stock market changes times stock holdings? Such instrument would be misleading. Reported wealth is net of consumption, thus the more people react to a given shock by adapting their consumption the smaller is the change in reported wealth. The first stage in such instrumental variable (IV) setup would be biased towards zero, artificially inflating the IV estimate.

However, it is unlikely that people entirely compensate the overall wealth shock between two interviews. Therefore I can test to which extent predicted wealth shocks actually predict changes in reported wealth (this is the 'attenuated first stage'). Figure 4 has already suggested that wave aggregates of reported wealth correlate with the stock market for stock owning retirees but not for those without stocks. I repeat this exercise in Table 4 using individual data instead of wave aggregates. In column 1 and 2 percentage changes in life-time wealth are regressed on the interaction of stock market changes with a stock ownership dummy while controlling for main effects. In column 3 and 4 stock market changes are interacted with the exact fraction of wealth held in stocks (representing the measure of predicted percentage wealth shocks), again controlling for the main effects. As the measurement of stocks in IRAs is noisy in the first waves, I repeat both regressions excluding these waves.

The coefficients on the interaction terms are strongly significant in all four columns. The stock market main effects which represent the effect on the wealth of retirees without stocks is close to zero and -if anything- slightly negative. These results indicate that a 10% change in the stock market leads to an average change of 1%-2% in reported life-time wealth for those with stocks in the previous period. Further, a 10% predicted wealth shock is associated with a change of about 7.7%-8% in reported life-time wealth. This shows that reported wealth is strongly correlated with predicted wealth shocks though with an elasticity smaller than one which could be interpreted as evidence of consumption smoothing. For those without stocks there is not much of an effect, giving further support to the conclusion from Figure 4, namely that stock market effects through other wealth components are not a big issue.

Finally, choosing retirees as study population has advantages beyond ruling out labour market interactions. Retirees have a lot of wealth and wealth heterogeneity, so there is a lot of variation to exploit. Further it is relatively easy to compute retiree life-time wealth since pension and annuity income, social security and veteran benefits typically stay constant in real terms until death. And the HRS as ideal data set in terms of variables mostly consists of retirees.

4 Empirical Specification

The identification strategy outline above leads to a straight-forward empirical specification.

$$\Delta H_{i,t} = \alpha + \beta \frac{\widehat{\Delta W}_{h(i),t}}{W_{h(i),t-1}} + \gamma \frac{s_{h(i),t-1}}{W_{h(i),t-1}} + \vartheta_t + \delta X_{i,t} + \epsilon_{i,t} \qquad (1)$$

$$\text{with} \quad \frac{\widehat{\Delta W}_{h(i),t}}{W_{h(i),t-1}} = \frac{s_{h(i),t-1}}{W_{h(i),t-1}} \frac{SP_{m(i,t)} - SP_{m(i,t-1)}}{SP_{m(i,t-1)}}$$

with indices:
i: individual
t: HRS wave (biannual)
$m(i,t)$: month of the interview of individual (i) in wave (t)
$h(i)$: household of (i)

and variables:
H: different health measures (normalised as explained in the Data section)
SP: Standard & Poor's 500 stock market index
s: stock holdings
W: life-time wealth $= A_t + (SS_t + PAI_t + VetBen_t) \sum_{\tau=1}^{T-t} \frac{S_{t,\tau,g,b}}{(1+r)^{t+\tau}}$
ϑ: time dummies
X: age, age^2, years of education, dummies for gender (2-1), race (3-1), region of residence (5-1), degree (5-1), past wave's marital status (8-1), and number of months between interviews

Changes in different health measures are directly regressed on the interaction of stock market changes with the fraction of life-time wealth held in stocks. The fraction of wealth in stocks is included separately (person main effect) to guarantee conditional independence of predicted wealth shocks. Year dummies (a flexible specification of stock market main effects) capture common macro shocks that directly affect health. Health measures are regressed in first differences and further controls are included to reduce the regression residual and thereby increase the precision of the estimates. However, these further controls (except for number of months)[18] should not change point estimates if predicted shocks are conditionally independent. Notice that taking the first difference does not reduce the number of observations since past wave's stock holdings and stock market changes already require one lag. Further one must not add controls which could be affected by wealth shocks ('bad controls' in the language of Angrist and Pischke (2009)). Therefore

[18]'Number of months' are included to control for potential interview delays due to health problems.

the lagged instead of the current marital status is included.

For better comparability the different health measures are normalised (see Data section) and re-coded such that changes are interpreted in terms of standard deviations and positive changes always refer to a health improvement. OLS regressions on these transformed values ('probit-adapted OLS') allow to compare effect sizes across the different health measures, representing effects as moving within the sample distribution rather than across differently scaled categories. Standard errors are clustered by individuals as first differences are likely to be serially correlated. Clustering at the level of households, interview dates or stock market changes leads to lower standard errors.

5 Findings

Table 5 presents the baseline results. Different health measures (rows) are regressed on predicted wealth shocks and a cumulative number of controls (columns). The final column shows results with all controls excluding the first waves for which the fraction of stocks in IRAs is not known. All estimates in this and the following tables display the coefficient on wealth shocks in the different specifications. Positive coefficients refer to health improvements in terms of standard deviations of the different health measures.

The bivariate regressions in the first column indicate a strongly significant relationship of predicted wealth shocks with the health problem index, the self-reported change in health and survival, while there is a weakly significant correlation with self-reported health and no significant relationship with mental health. Including initial stock holdings in the second column does not change this pattern dramatically, indicating that the coefficients in the first column are not driven by average differences of people with different fractions of their wealth held in stocks.[19] When time dummies are included in the third column, all coefficients except for mental health decrease by one half, suggesting that stock market changes come along with macroeconomic shocks which also affect the health of those without stocks. The coefficient in the mental health regression, on the other hand, increases and becomes significant. This might indicate that the mental health of those without stocks is reversely affected by stock market changes while there is not much of an effect for stock owners. However, repeating the first two regressions in the sample restricted to year>1999 results in significant positive effects for stock owners. Hence the insignificant results when including the first waves should not be overinterpreted. Adding further additional controls in column 4 seems to increase the precision of the estimates in some cases but does not change the overall pattern. This is what we should expect if wealth shocks conditional on stock holdings are truly exogenous. Excluding waves 1992-1996 in column 5 (we are left with waves 2000-2009 as the 1998 wave is used as a lag) changes coefficients

[19]This is not surprising as for the predicted wealth shocks these stock holdings are interacted with positive as well as negative stock market changes, so that any bias would be reverted if interacted with negative changes. Also the dependent variable is regressed in first differences canceling out any individual fixed effect driving stock holdings.

somehow but again the overall pattern remains the same. The fact that in column 5 more precise information on stocks in IRAs is used (for the first waves an average of 52% stock share for IRA is assumed) should increase the precision of the estimates (as explained in the Data section). But at the same time the sample size is decreased, driving up standard errors.

To sum up, the baseline regressions in Table 5 suggest that the predicted wealth shocks have a significant positive effect of similar size on the health problems index, self-reported health, mental health and mortality. Coefficients in column 5 indicate that a +10% shock in life-time wealth is associated with an increase of about 1.5% to 3% of a standard deviation in the different health measures. These effects are not driven by person nor by time effects and therefore can be interpreted as the causal effect of wealth shocks on health. The inclusion of various controls does not affect the overall pattern nor the exclusion of the first waves.

The baseline regression setup relates health changes to wealth shocks over a one to three year period. If the estimated effects represent causal effects of wealth on health, they must be driven by diseases that are responsive to environmental factors and that do not take a lot of time to develop.

To get an idea of which health problems are driving the estimated effect, Table 6 shows results for the decomposed health problem index, using the individual health problems of the index as the dependent variables in separate regressions. Estimates refer to the coefficients of wealth shocks in the full specification (as Table 5, column 4 and 5). As in the previous regressions, each dependent variable is normalised such that positive coefficients indicate a health improvement (i.e. a lower chance to get a new health problem) in terms of standard deviations. The regressions reveal a strong and positive effect of wealth shocks on the incidence of high blood pressure (ie. a negative effect on the probability to get high blood pressure) and effects of about half the size for heart diseases, psychiatric problems and - surprisingly - cancer; in specifications with less 'other controls' the coefficient for strokes is significant, too. The incidence of arthritis, diabetes and lung disease, on the other hand, is not affected. These heterogeneous effects across different diseases seem reasonable. High blood pressure and psychiatric problems are plausible drivers for a short to medium term effect of wealth shocks on the health of wealthy retirees that are covered by health insurance (medicare). High blood pressure arises from both psychological stress as well as unhealthy nutrition and behavior. Moreover, high blood pressure is a cause for heart problems (and strokes), but not for diabetes, cancer, lung diseases or arthritis. Therefore a positive effect on heart problems and strokes is what one should expect given the strong effect on high blood pressure. The effect of wealth shocks on cancer is in line with effect of stress on cancer incidence that is supposed by the epidemiological literature.[20] When the first waves are excluded in the second column standard errors go up and

[20]Note that those with positive wealth shocks might smply tend to do better cancer checkups which increases the likelihood of cancer diagnosis even if the risk of cancer might be lower than for those with

all coefficients except the one on high blood pressure become insignificant. The central message from this table is that wealth shocks have a strong effect on high blood pressure, a small effect on diseases which are caused by high blood pressure as well as on cancer, but no effect on effect on arthritis, diabetes and lung disease. Further it seems that large sample sizes are needed to detect these smaller effects.

Table 7 shows results for the individual items of the mental health index, which indicate the absence of the respective negative emotion or the presence of the respective positive emotion "over much of the time during the past week". Hence a positive change indicates the appearance of a positive emotion or the disappearance of a negative emotion compared to the previous interview, and vice versa. Results indicate weakly significant effects on feeling depressed and lonely, while the coefficients on the other items are smaller but still in all cases the right direction. No single item seems to be the single driver of the effect on the overall index, as one should expect. The mental health index does not - unlike the health problems index - represent a list of different diseases but a collection of symptoms associated with clinical depression. Any single symptom is not necessarily a sign of depression but what makes it a mental health problem is having many of the symptoms at the same time. Therefore it is not surprising that the significant estimate for the mental health index is not driven by single items but rather represents their joint significance.

How big are the estimated effects? To answer this question it is insightful to compare them with a simple benchmark regression of the respective health measure on wealth and some controls. The resulting coefficient will be driven not only by the causal effect of wealth on health but also by reverse causality and any simultaneous effect from omitted variables. As one would expect reverse causality and omitted variables to have a positive influence, the coefficient from such benchmark regression should be larger than the estimated causal effect.

The equivalent to the predicted wealth shocks are percentage changes in reported lifetime wealth (used as dependent variable in Table 4). Table 14 shows in column 1 the estimated causal effects from the baseline regressions and in column 2 the coefficients from a regression of health changes on relative changes in reported wealth and age, gender and race as controls.[21] Surprisingly the coefficients in column 2 are very small and not even significantly different from zero for the health problems index, the mental health index and survival. An explanation for smaller estimates could be attenuation due to measurement error. Self-reported income and especially wealth are notorious for measurement errors. And as retirement income is quite stable and wealth is regressed in first differences, much of the variation might represent mere measurement error biasing the estimates towards

negative shocks. However, one would expect to see such checkup effect also for other diseases like high blood pressure.

[21] Notice that in the baseline regressions additional controls (besides the main effects) are included only to improve the precision of the estimates, as predicted wealth shocks should be conditionally independent of these controls. This is probably not true for wealth in the benchmark regression, Therefore only a few, clearly exogenous controls are included. However, if more controls are added, benchmark estimates become even smaller.

zero (Angrist and Pischke 2009, chapter 5.1.).

Potential measurement errors are less of a problems in a level-regression. The level-equivalent to the regression in column 2 is the regression of health levels on the logarithm of life-time wealth.[22] The resulting coefficients in column 3 are much larger than in the second column and significantly positive in all cases. Notice that this increase might not only be due to less measurement error but probably partly caused by fixed effects that were canceled out in first differences. However, estimates are still only slightly larger than the estimated causal effects and even smaller in the survival regression. This seems to be at odds with a positive bias due to reverse causality and omitted variables.

One explanation could be that predicted wealth shocks underestimate actual wealth shocks due to non-stock wealth that correlates with the stock market. But in this case reported wealth of those without stocks should correlate positively with the S&P500 which is not the case as shown above (see Tables 3 and 4). Another reason could be that life-time wealth is overestimated, which would make predicted wealth shocks (which are rescaled by life-time wealth) appear smaller than they actually are. However, artificially underestimating the life-time wealth formula, e.g. even by assuming that people live only one more year does not alter a lot the size of the estimated causal effect relative to the respective benchmark regression.

A more plausible explanation is that predicted wealth shocks correctly represent actual wealth shocks but the resulting causal effect on health might not be representative for the average causal effect of wealth on health in the sample. Much of the causal effect of wealth on health in the benchmark regression should come from the long-term wealth elasticity of health. Someone owning $500k can afford better health inputs than somebody owning $300k which over time accumulates to better health outcomes. This might however be a very different effect from losing $200k in a stock market crash, which involves high blood pressure, stress and depression rather than just a slight change in health inputs.

Analysing individual health problems in Table 9 provides evidence in favor of such wealth shock interpretation. While the estimated causal effect of wealth shocks differs across health problems (between zero and 0.17), the benchmark relationship is quite homogeneous, ranging from 0.05 to 0.1 (except for cancer, see below). This means that for high blood pressure the benchmark relationship amounts to only one third of the estimated causal effect while it is of similar size for heart disease and psychiatric problems. For other health problems like arthritis and diabetes the estimated causal effect is zero but the correlation is significantly positive (and hence larger). Cancer stands out with a negative wealth relationship, indicating that richer people are less protected from cancer diagnosis (controlling for age, gender and race), while the estimated causal effect is positive. These

[22] Percentage changes are equivalent to the logarithm only for small changes and there are wealth shocks up to +/-50% included. However, regressing predicted wealth shocks as logarithmic instead of percentage changes (i.e. $ln(\widehat{W_t}/W_{t-1})$ instead of $\widehat{\Delta W_t}/W_{t-1}$) results in very similar estimates.

results suggest that the estimated effects of wealth shocks are indeed of a different quality and not just an inflated proxy for the average causal effect of wealth in the sample.

Tables 10 - 13 present various robustness checks, again using the baseline specification for better comparability. Table 10 compares the baseline results with IV regressions in which predicted wealth shocks are instrumented by the 1992-2006 average stock holdings (column 3), the 1998-2006 average (column 4) and initial 1998 stock holdings (column 5). These instruments are uncorrelated with where we are in the stock market cycle and therefore rule out a potential endogeneity due to stock market predictability (see Identification section). For the health problems index and for mental health coefficients become slightly larger in the IV setup while they slightly decrease in the other regressions. Standard errors increase throughout, which is what one should expect.[23] Given that coefficients only change slightly and mostly remain significant despite this increase in noise suggests that the baseline findings are not driven by stock market predictability.

Specification checks in Table 12 indicate that results are not driven by the comparison of wealthy stock owning retirees with those who are poor or who do not own stocks. When excluding the bottom life-time quartile or those without stocks, coefficients only change slightly and remain mostly significant despite increasing standard errors due to decreased sample size. In the last two columns I repeat the baseline regressions using a simplified life-time formula representing the sum of current wealth and only one year of pension income (instead the discounted sum of all future expected income streams). Estimated coefficients decrease since predicted wealth shocks are rescaled by a smaller life-time wealth, but significance levels and the overall pattern do not change.

Heterogeneity checks in Table 13 show that effects are quite homogeneous across age, gender and negative vs. positive shocks. Only for mortality effects are significantly different between subsamples. Mortality for retirees above age 75 is stronger affected than for those below 75. This seems plausible given that the elderly are closer to the margin of death while this is not the case for mental health or self-reported health. Further negative wealth shocks seem to have a more devastating impacts on mortality than the positive effects of positive shocks of equal size. This could be in line with concepts like loss aversion, though it remains unclear why the same pattern is not observed for the other health measures.

6 Conclusion

This paper investigates the effect of wealth shocks on health by regressing health changes over a two-year period on stock market induced wealth shocks over the same period. I find

[23] In both IV-setups information is lost by using proxies instead of actual values for stock holdings. And in column (5) I am additionally losing many observations, all those who do not report stock holdings in 1998, in particular the cohorts that were added in 2004.

significant positive effects of similar size on physical health, self-reported health, mental health and mortality. A 10% change in life-time wealth is associated with a change of about 1.5-3% of a standard deviation in these health measures.

Effects are heterogeneous across different physical health problems, with most pronounced effects for the incidence of high blood pressure, smaller effects for heart problems, strokes and cancer, and no effects for arthritis, diabetes and lung diseases. This pattern suggests that psychological factors play an important role in the transmission of effects from wealth shock to retiree health. Estimated effect sizes are of equal size or larger than the overall wealth-health relationship. This suggests that effects are larger than the average causal effect of wealth on health in the sample. Effects are quite homogenous across age, gender and types of wealth shocks. Only for the mortality regressions there is a significantly stronger effect for the elderly and if wealth shocks are negative.

There are three main conclusions. First, wealth shocks have a causal effect on physical health, mental health and mortality of wealthy retirees in the US. Second, effects too large to represent the average causal effect of wealth on health in the sample. Third, psychological factors seem be a major channel through which these effects.

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8 Tables and Figures

Table 1: Summary Statistics Wealth

HRS wave	1	2	3	4	5	6	7	8	9
year	1992-1993	1994-1995	1996-1997	1998-1999	2000-2001	2002-2003	2004-2005	2006-2007	2008-2009
# observations	12,605	11,389	11,150	22,419	20,770	19,521	21,349	19,676	18,434
age	55	57	59	67	68	69	67	69	70
% retiree households	22%	30%	38%	63%	66%	70%	64%	68%	70%
information '% of IRA in stocks'	none	none	none	3 categories	3 categories	3 categories	3 categories	exact %	exact %
imputed % of IRA in stocks	52%	52%	52%	0, 50, 100%	0, 50, 100%	0, 50, 100%	0, 50, 100%	exact %	exact %
regression sample									
# observations	-	941	1,899	2,611	8,334	7,910	8,532	8,464	7,942
current wealth	198,660	244,801	278,882	272,641	320,652	343,968	395,071	487,048	494,830
life-time wealth	416,623	541,103	591,140	490,127	558,341	595,344	665,405	774,073	863,006
% with stocks (excl IRAs)	29%	34%	33%	31%	33%	31%	30%	28%	27%
% with stocks (incl IRAs)	38%	46%	47%	32%	34%	33%	32%	31%	30%
…those with stocks (incl. IRAs)									
# observations	-	401	975	1,369	3,089	3,125	3,181	3,216	2,872
current wealth	375,552	404,323	455,005	520,935	580,899	625,063	769,296	927,367	929,626
life-time wealth	650,643	780,726	842,988	795,207	873,734	936,483	1,098,133	1,277,645	1,343,069
stock holdings (incl IRAs)	89,232	98,677	129,586	195,696	220,003	199,866	301,739	356,956	319,659
% life-time wealth in stocks	11%	12%	12%	19%	20%	19%	20%	20%	21%

Notes: New cohorts added in 1998 and 2004; retiree households refer to singles and couples where both respondent and spouse -if existent- were neither working nor unemployed; the regression sample includes all households that in the previous wave were retired and reported their wealth, retiree income (for the construction of life-time wealth) and stock holdings - hence there are no observations in wave 1 and the wealth information in wave 1 refers to the num-ber of observations in wave 2; life-time wealth is the sum of current wealth and future discounted retiree income (see Data section).

Figure 1: S&P500 with indicated HRS observation periods

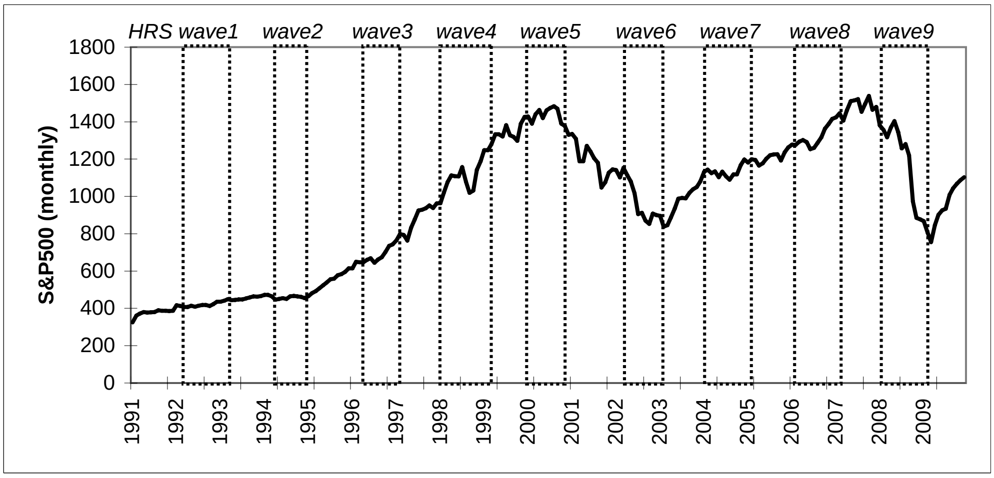
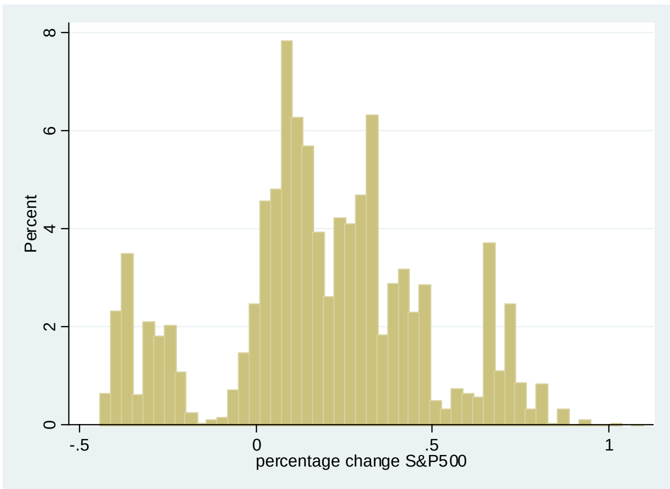
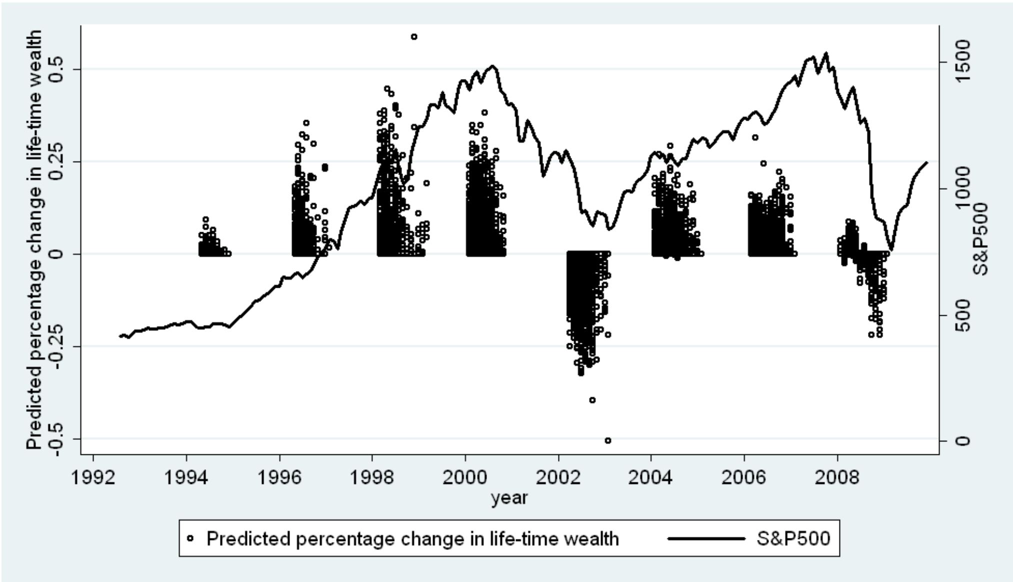


Figure 2: Distribution of Changes in S&P500



The distribution of percentage changes in the S&P500 between two interviews of the same household is displayed. Values around -0.4, for example, refer to interviews that were conducted in wave 6 or 9 at a time when the S&P500 was 40% below its value at the households's previous interview.

Figure 3: Predicted Wealth Changes and the S&P500 over Time



Predicted wealth shocks ('past interview's wealth fraction held in stocks'*'percentage change in the S&P500') over time are plotted with the S&P500. Each circle represents one household and is placed in the figure at the exact month of the household's interview.

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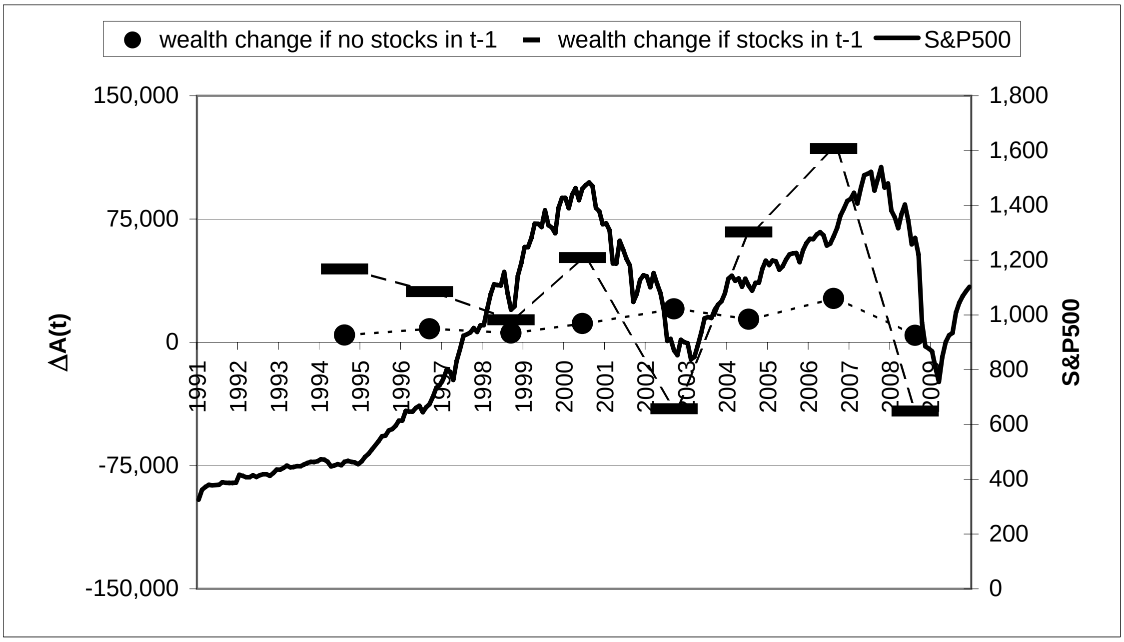
Table 2: Summary Statistics Deaths in the HRS

HRS wave	1		2		3		4		5		6		7		8		9	
year	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06	07	08	09
# observations	12,605		11,389		11,150		22,419		20,770		19,521		21,349		19,676		18,434	
# deaths	-	-	45	124	437	488	542	637	574	615	566	555	592	581	569	566	343	-
age at death	-	-	61	63	78	79	79	79	78	79	80	81	80	80	81	80	81	-
regression sample																		
# observations included	-		873		1,802		2,468		7,954		7,625		8,210		8,171		-	
# deaths included	-	-	8	17	26	32	42	70	154	414	325	395	343	437	335	425	242	-

Notes: Deaths are regressed as a 'Survival' dummy that indicates survival to t+1, i.e. it is 0 if the individual deseases before the next interview and 1 otherwise. This survival dummy is regressed on wealth shocks between t-1 and t, so that for each observation in the regression sample one lag and one future observation is required. For that reason there are no regression observations in wave 1 and wave 9. The final row shows how many deaths in each year are included in the sample (accounted by survival=0). For each included death, the deceased respondent was retired and reported her wealth and health in t-1 and t, and deceased before the t+1 interview. For example, deaths in 1994 are included if the individual was retired and reported wealth and health in 1992 and 1994, but in 1996 a proxy respondent reported that the individual died in 1994 (obviously after the interview).

Figure 4: S&P500 and Reported Wealth Changes over Time



Coefficients from Table 3, columns 1 and 2 are plotted; ΔA(t)= changes in reported current wealth.

Table 3: Reported Wealth Changes over Time

	Dep. Var.: ΔA_t without stocks in t-1	with stocks in t-1
wave2	4,837	44,625
	(5,170)	(29,007)
wave3	8,214	30,839**
	(7,364)	(15,246)
wave4	5,660	13,687
	(4,011)	(17,191)
wave5	11,406***	51,491***
	(2,566)	(15,246)
wave6	20,365**	-40,480**
	(9,155)	(17,881)
wave7	14,041	67,154*
	(9,656)	(35,675)
wave8	26,665***	117,832**
	(4,056)	(59,493)
wave9	4,258	-41,819
	(8,111)	(51,207)
n	34,626	

A_t=reported current wealth; coefficients on wave dummies interacted with an indicator of stock ownership in t-1 are displayed; no constant included; these coefficients are plotted in Figure 4.

Table 4: Regressing Wealth Changes on Stock Market Changes (column 1, 2) and on Predicted Wealth Changes (3, 4).

Dep. Var.: $\Delta W_t/W_{t-1}$	(1) full sample	(2) year>1999	(3) full sample	(4) year>1999
$\Delta SP_t/SP_{t-1}$*$D\text{stocks}_{t-1}$	0.133***	0.222***		
	(0.040)	(0.054)		
$D\text{stocks}_{t-1}$	-0.040***	-0.036***		
	(0.013)	(0.013)		
$\Delta SP_t/SP_{t-1}$*$[s_{t-1}/W_{t-1}]$			0.777***	0.800***
			(0.122)	(0.148)
s_{t-1}/W_{t-1}			-0.222***	-0.216***
			(0.041)	(0.042)
$\Delta SP_t/SP_{t-1}$	-0.044	-0.038	-0.047*	-0.018
	(0.031)	(0.040)	(0.025)	(0.034)
constant	0.174***	0.169***	0.176***	0.171***
	(0.009)	(0.009)	(0.008)	(0.008)
n	34,619	30,926	34,619	30,926

W_t= life-time wealth (see 'Data' section); SP_t = S&P500 stock market index; $D\text{stocks}_{t-1}$=(stockholdings$_{t-1}$ > 0); s$_{t-1}$=stockholdings$_{t-1}$; Sample: households retired in t-1; 3 observations with $\Delta\% W_t > 5000\%$ excluded; (standard errors) clustered by household.

Table 5: Baseline Regressions

Dep. Var.	(1)	(2)	(3)	(4)	(5)
Δ Index of Health Problems	0.256***	0.253***	0.154***	0.149***	0.166***
	(0.045)	(0.047)	(0.053)	(0.053)	(0.060)
n=36,502					33,870
Δ Self-reported Health	0.162*	0.263***	0.106	0.121	0.211*
	(0.083)	(0.088)	(0.100)	(0.100)	(0.122)
n=46,220					40,827
Self-reported Change in Health	0.775***	0.665***	0.358***	0.338***	0.215*
	(0.090)	(0.091)	(0.102)	(0.101)	(0.114)
n=46,244					40,849
Δ Mental Health Index	-0.135	-0.088	0.218*	0.233*	0.312**
	(0.101)	(0.106)	(0.120)	(0.120)	(0.144)
n=38,932					36,550
Survival	0.391***	0.280***	0.141*	0.154**	0.204**
	(0.062)	(0.067)	(0.079)	(0.073)	(0.090)
n=36,804					31,710
controls					
past wave's stock fraction		✓	✓	✓	✓
time dummies			✓	✓	✓
other controls				✓	✓
restricted to year>1999					✓

The coefficient on wealth shocks ($\widehat{\Delta W}_t/W_{t-1}$, equation 1) is displayed; a positive coefficient refers to a health improvement in the respective dependent variable; all dependent variable (including 'Survival') are standardized as explained in the data section; $\Delta X = X_t - X_{t-1}$; 'Surival' is 0 if respondent deceases before (t+1) and 1 otherwise; the estimation method used is OLS ('probit-adapted' OLS); standard errors, in parethesis, are clustered by individual; 'other controls' are: age, age^2, years of education, dummies for gender, race, initial marital status, degree and # months since previous interview.

Table 6: Individual health problems

Dep. Var.	(1)	(2)
ΔHealth Problem Index	0.149***	0.166***
	(0.053)	(0.060)
	decomposed	
Δhigh blood pressure	0.168***	0.169***
	(0.050)	(0.058)
Δheart disease	0.098**	0.082
	(0.050)	(0.060)
Δstroke	0.073*	0.071
	(0.043)	(0.053)
Δarthritis	0.023	0.037
	(0.057)	(0.064)
Δcancer	0.081*	0.071
	(0.044)	(0.053)
Δdiabetes	0.010	-0.037
	(0.031)	(0.036)
Δlung disease	0.039	0.036
	(0.032)	(0.035)
Δpsychiatric problems	0.079**	0.058
	(0.036)	(0.043)

The coefficient on wealth shocks ($\widehat{\Delta W}_t/W_{t-1}$, equation 1) is displayed; a positive coefficient refers to a health improvement in the respective dependent variable; all dependent variables are standardized as explained in the data section; $\Delta X = X_t - X_{t-1}$; the estimation method used is OLS ('probit-adapted' OLS); standard errors, in parethesis, are clustered by individual; 'other controls' are: age, age^2, years of education, dummies for gender, race, initial marital status, degree and # months since previous interview.

Table 7: Individual items of the mental health index

Dep. Var.	(1)	(2)
ΔMental Health Index	0.233*	0.312**
	(0.120)	(0.144)
	decomposed	
Δfelt depressed	0.193*	0.194
	(0.102)	(0.124)
Δeverything is an effort	0.162	0.095
	(0.107)	(0.128)
Δsleep is restless	0.082	0.131
	(0.123)	(0.148)
Δfelt alone	0.193*	0.251**
	(0.105)	(0.126)
Δfelt sad	0.155	0.204
	(0.113)	(0.133)
Δcould not get going	0.048	0.101
	(0.118)	(0.141)
Δfelt happy	0.041	0.055
	(0.101)	(0.120)
Δenjoyed life	0.053	0.132
	(0.088)	(0.104)
controls		
past wave's stock fraction	✓	✓
time dummies	✓	✓
other controls	✓	✓
restricted to year>1999		✓

The coefficient on wealth shocks ($\widehat{\Delta W}_t/W_{t-1}$, equation 1) is displayed; a positive coefficient refers to a health improvement in the respective dependent variable; all dependent variables are standardized as explained in the data section; $\Delta X = X_t - X_{t-1}$; the estimation method used is OLS ('probit-adapted' OLS); standard errors, in parethesis, are clustered by individual; 'other controls' are: age, age^2, years of education, dummies for gender, race, initial marital status, degree and # months since previous interview.

Table 8: Comparison with unconditional health-wealth relationship in the sample

Dep. Var.: H_t measure	(1) ΔH_t on $\widehat{\Delta W}_t/W_{t-1}$ (Table 5, (4))	(2) ΔH_t on $\Delta W_t/W_{t-1}$	(3) H_t on lnW_t
Health Problems Index	0.149***	0.001	0.177***
	(0.053)	(0.002)	(0.009)
SRH	0.121	0.018***	0.286***
	(0.100)	(0.005)	(0.007)
SRΔH	0.338***	0.015***	-
	(0.101)	(0.004)	-
Mental Health	0.233*	0.005	0.230***
	(0.120)	(0.005)	(0.007)
Survival	0.154**	-0.002	0.054***
	(0.073)	(0.004)	(0.003)
controls			
past wave's stock fraction	✓		
time dummies	✓		
other controls	✓		
age, age^2, gender, race		✓	✓

Coefficients on $\widehat{\Delta W}_t/W_{t-1}$ (column 1), $\Delta W_t/W_{t-1}$ (2), and lnW_t (3) are displayed; all waves included; (2) and (3) are estimated for the same sample as (1); there is no estimate for $SR\Delta H$ in (3) as $SR\Delta H$ is only defined in first differences.

Table 9: Unconditional health-wealth relationship for individual health problems

Dep. Var.: H_t measure	(1) ΔH_t on $\widehat{\Delta W}_t/W_{t-1}$ (Table 6, (1))	(2) H_t on lnW_t
Health Problem Index	0.149***	0.177***
	(0.053)	(0.009)
decomposed		
high blood pressure	0.168***	0.057***
	(0.050)	(0.007)
heart disease	0.098**	0.060***
	(0.050)	(0.007)
stroke	0.073*	0.058***
	(0.043)	(0.005)
arthritis	0.023	0.051***
	(0.057)	(0.006)
cancer	0.081*	-0.040***
	(0.044)	(0.006)
diabetes	-0.010	0.081***
	(0.031)	(0.006)
lung disease	0.039	0.077***
	(0.032)	(0.005)
psychiatric problems	0.079**	0.103***
	(0.036)	(0.006)
controls		
past wave's stock fraction	✓	
time dummies	✓	
other controls	✓	
age, age^2, gender, race		✓

Coefficients on $\widehat{\Delta W}_t/W_{t-1}$ (column 1) and lnW_t (2) are displayed; column 1 repeats the baseline results of Table 6; (2) is estimated for the same sample as (1).

Table 10: Instrumenting predicted wealth shocks ($\frac{\Delta S\&P_t}{S\&P_{t-1}} * \frac{s_{t-1}}{W_{t-1}}$) by the interaction of S&P500 changes with the individual's average stock fraction ($\frac{\Delta S\&P_t}{S\&P_{t-1}} * [\frac{s}{W}]^{average}$) or the individual's 1998 stock fraction ($\frac{\Delta S\&P_t}{S\&P_{t-1}} * [\frac{s}{W}]^{1998}$)

	baseline (Table 5)		IV $[\frac{s}{W}]^{average}$		IV $[\frac{s}{W}]^{1998}$
Dep. Var.	(1)	(2)	(3)	(4)	(5)
Δ Index of Health Problems	0.153***	0.169***	0.228***	0.243***	0.248***
	(0.053)	(0.060)	(0.064)	(0.072)	(0.087)
n	36,320	33,701	36,270	33,663	23,033
Δ Self-reported Health	0.121	0.211*	0.019	0.117	0.002
	(0.100)	(0.122)	(0.118)	(0.145)	(0.183)
n	46,220	40,827	46,143	40,773	28,153
Self-reported Change in Health	0.338***	0.215*	0.269**	0.104	0.064
	(0.101)	(0.114)	(0.119)	(0.129)	(0.157)
n	46,244	40,849	46,168	40,795	28,170
Δ Mental Health Index	0.233*	0.312**	0.266*	0.385**	0.403*
	(0.120)	(0.144)	(0.144)	(0.173)	(0.208)
n	38,932	36,550	38,904	36,522	24,965
Survival	0.154**	0.204**	0.117	0.184*	0.078
	(0.073)	(0.090)	(0.086)	(0.105)	(0.117)
n	36,804	31,710	36,746	31,670	23,340
controls					
past wave's stock fraction	✓	✓			
average stock fraction			✓	✓	
1998 stock fraction					✓
time dummies	✓	✓	✓	✓	✓
other controls	✓	✓	✓	✓	✓
restricted to year>1999		✓		✓	✓

The coefficient on predicted wealth shocks ($\widehat{\Delta W}_t/W_{t-1} \equiv \frac{\Delta S\&P_t}{S\&P_{t-1}} * \frac{s_{t-1}}{W_{t-1}}$) is displayed; the estimation method used is OLS in columns 1 and 2, and 2SLS in columns 3-5; the IV in (3) is $\frac{\Delta S\&P_t}{S\&P_{t-1}} * [\frac{s}{W}]^{average}_{92-06}$, in (4) is $\frac{\Delta S\&P_t}{S\&P_{t-1}} * [\frac{s}{W}]^{average}_{98-06}$, and in (5) is $\frac{\Delta S\&P_t}{S\&P_{t-1}} * [\frac{s}{W}]^{1998}$. Further comments as in Table 5.

Table 11: Alternative sample specifications

Dep. Var.	(1) baseline	(2) excl. if age<65	(3) excl. if $s_{t-1} = 0$	(4) excl. poorest quartile
Δ Index of Health Problems	0.149***	0.186***	0.108*	0.146***
	(0.053)	(0.069)	(0.064)	(0.055)
n	36,502	25,879	14,651	28,225
Δ Self-reported Health	0.121	0.226	0.146	0.103
	(0.100)	(0.141)	(0.119)	(0.103)
n	46,220	31,520	18,072	34,894
Self-reported Change in Health	0.338***	0.157	0.289**	0.220**
	(0.101)	(0.128)	(0.122)	(0.106)
n	46,244	31,544	18,074	34,904
Δ Mental Health Index	0.233*	0.264	0.215	0.218*
	(0.120)	(0.163)	(0.144)	(0.126)
n	38,932	27,918	15,754	30,246
Survival	0.154**	0.197*	0.167*	0.149**
	(0.073)	(0.109)	(0.088)	(0.076)
n	36,804	24,149	14,696	27,604
controls:				
past wave's stock fraction	✓	✓	✓	✓
time dummies	✓	✓	✓	✓
other controls	✓	✓	✓	✓

Comments as in Table 5.

Table 12: Subsample heterogeneity

	(A) age			(B) gender			(C) sign of shocks		
Dep. Var.	<=75	>75	Δ (p-value)	male	female	Δ (p-value)	shocks≤ 0	shocks≥ 0	Δ (p-value)
Δ Index of Health Problems	0.103	0.211**	0.287	0.187**	0.126*	0.525	0.076	0.171*	0.670
	(0.066)	(0.082)		(0.078)	(0.067)		(0.200)	(0.093)	
Δ Self-reported Health	0.100	0.152	0.788	0.044	0.179	0.453	-0.054	0.030	0.823
	(0.116)	(0.167)		(0.140)	(0.128)		(0.341)	(0.169)	
Self-reported Change in Health	0.272**	0.204	0.718	0.207	0.452***	0.178	0.165	0.251	0.844
	(0.127)	(0.151)		(0.144)	(0.128)		(0.388)	(0.197)	
Δ Mental Health Index	0.252*	0.210	0.858	0.141	0.310*	0.430	0.136	0.317*	0.694
	(0.145)	(0.193)		(0.163)	(0.158)		(0.427)	(0.190)	
Survival	0.072	0.333**	**0.079***	0.192*	0.116	0.560	1.022***	0.198	**0.034****
	(0.059)	(0.144)		(0.110)	(0.086)		(0.364)	(0.128)	
controls (interacted):									
past wave's stock fraction	✓			✓			✓		
time dummies	✓			✓			✓		
other controls	✓			✓			✓		

Coefficients on predicted wealth shocks ($(\widehat{\Delta W}_t/W_{t-1})$), interacted with the two respective subgroups, are displayed; 'Δ (p-value)' indicates the significance level of the difference between the two coefficients; all controls are interacted with the subgroups; all waves included; dependent variables are transformed as in the baseline regressions; further comments as in Table 5.

Table 13: Specification of Wealth Shocks

	baseline	alternative rescaling		using *reported* stock wealth changes	
	(1)	(2)	(3)	(4)	(5)
Dep. Var.	$\frac{\Delta S\&P500}{S\&P500} * s_{t-1}/W_{t-1}$	$\frac{\Delta S\&P500}{S\&P500} * s_{t-1}/A_{t-1}$	$\frac{\Delta S\&P500}{S\&P500} * s_{t-1}/10,000$	$(s_t - s_{t-1})/W_{t-1}$	$(s_t - s_{t-1})/10,000$
Health Problems Index	0.149***	0.097***	0.001	0.003	-0.000005
	(0.053)	(0.034)	(0.002)	(0.007)	(0.000033)
n	36,502	33,434	36,502	36,502	36,502
SRH	0.121	0.002	0.005	0.032**	0.000092
	(0.100)	(0.063)	(0.003)	(0.015)	(0.000068)
n	46,220	42,011	46,220	46,220	46,220
SRΔH	0.338***	0.150**	0.012***	0.010	0.000009
	(0.101)	(0.067)	(0.004)	(0.015)	(0.000050)
n	46,244	42,036	46,244	46,244	46,244
Mental Health	0.233*	0.095	0.005	-0.005	0.000010
	(0.120)	(0.082)	(0.006)	(0.018)	(0.000073)
n	38,932	35,831	38,932	38,932	38,932
Survival	0.154**	0.074*	0.002	-0.014	-0.000032
	(0.073)	(0.045)	(0.003)	(0.016)	(0.000049)
n	36,804	33,504	36,804	36,804	36,804
controls:					
past wave's stock holdings	✓	✓	✓	✓	✓
time dummies	✓	✓	✓	✓	✓
other controls	✓	✓	✓	✓	✓

In column 3 and 4 the poorest quartile is excluded; in 5 and 6 retirees without stocks in (t-1) are excluded; in 7 and 8 the life-time wealth sum (see Data section) is set to one, thus constructing life time wealth as the sum of current wealth and just one annual income. Further comments as in Table 5.

Table 14: Retirees vs. Non-retirees

	(1) baseline	(2)	
	$\frac{\Delta S\&P500}{S\&P500} * \frac{s_{t-1}}{W_{t-1}}$	$\frac{\Delta S\&P500}{S\&P500} * \frac{s_{t-1}}{A_{t-1}}$	
Dep. Var.	retiree HH's	retiree HH's	non-retiree HH's
Health Problems Index	0.166***	0.107**	0.002
	(0.060)	(0.042)	(0.015)
n	33,870	26,328	13,137
SRH	0.211*	0.042	0.061**
	(0.122)	(0.088)	(0.026)
n	40,827	31,563	14,652
SRΔH	0.215*	0.122	0.017
	(0.114)	(0.089)	(0.025)
n	40,849	31,581	14,658
Mental Health	0.312**	0.131	0.032
	(0.144)	(0.108)	(0.026)
n	36,550	29,073	13,944
Survival	0.204**	0.137**	0.003
	(0.090)	(0.067)	(0.006)
n	31,710	24,478	11,327
controls			
past wave's stock fraction	✓	✓	✓
time dummies	✓	✓	✓
other controls	✓	✓	✓
restricted to year>1999	✓	✓	✓

Retiree HH's are definded as singles (or couples) that are (both) retired or out of the labor foce. For non-retiree HH's at least one HH member is employed or unemployed. A positive coefficient refers to a health improvement in the respective dependent variable; all dependent variable (including 'Survival') are standardized as explained in the data section; $\Delta X = X_t - X_{t-1}$; 'Surival' is 0 if respondent deceases before (t+1) and 1 otherwise; the estimation method used is OLS ('probit-adapted' OLS); standard errors, in parethesis, are clustered by individual; 'other controls' are: age, age^2, years of education, dummies for gender, race, initial marital status, degree and # months since previous interview.