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Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations

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Abstract
Do wealth shocks affect the health of the elderly in developed countries? The economic literature is sceptical about such effects which have so far only been found for poor retirees in poor countries. In this paper I show that wealth shocks also matter for the health of wealthy retirees in the US. I exploit the booms and busts in the US stock market as a natural experiment that generated considerable gains and losses in the wealth of stock-holding retirees. Using data from the Health and Retirement Study I construct wealth shocks as the interaction of stock holdings with stock market changes. These constructed wealth shocks are highly predictive of changes in reported wealth. And they strongly affect health outcomes. A 10% wealth shock leads to an improvement of 2-3% of a standard deviation in physical health, mental health and survival rates. Effects are heterogeneous across physical health conditions, with most pronounced effects for the incidence of high blood pressure, smaller effects for heart problems and no effects for arthritis, diabetes, lung diseases and cancer. The comparison with the cross-sectional relationship of wealth and health suggests that the estimated effects of wealth shocks are larger than the long-run wealth elasticity of health.

Key words: Retiree health, wealth shocks, stock market
JEL: G10; I10; J14

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1 Introduction

Richer people are healthier, happier and live longer. Little is known, however, about the causal mechanisms underlying this important correlation of wealth and health. Money might buy health, but health might also reversely affect expenditure and income generation. And third factors such as preferences or life events are likely to affect both simultaneously. The broad existing literature on the wealth-health relationship is skeptical about causal effect of wealth or wealth shocks on adult health in developed countries. So far such effects have been documented only for poor retirees in poor countries.[1] In this paper I exploit stock market fluctuations in the wealth of elderly US retirees as a source of exogenous wealth shocks. Contrary to the existing literature I find that wealth shocks strongly affect physical health, mental health and survival rates of elderly retirees in the US.

Over the past two decades every third retiree household in the US held part of its wealth in stocks. And these households invested on average about 20% of their overall remaining life-time wealth in such risky asset. As a consequence the booms and busts in the US stock market over the past 15 years generated dramatic unexpected gains and losses in the wealth of stock holding retirees. I analyze this natural experiment using rich micro-data from the Health and Retirement Study (HRS). The HRS is representative of the elderly US population and provides panel data on all wealth components including stock holdings as well as information on physical health, mental health and mortality.

I construct wealth shocks as the interaction of stock holdings with the stock market change. These constructed wealth shocks are highly predictive of changes in reported wealth. And they strongly affect the health of elderly retirees who are of average age 75 in the HRS. A 10% change in life-time wealth over a two year period is associated with a change of 2-3% of a standard deviation in four different health measures: a physical health index, self-reported health, mental health and the probability to survive to the next interview two years ahead. This means that among 100 retirees losing 10% of their remaining life-time wealth 2.5 will develop an additional health condition and one additional retiree will not survive the next two years (given a baseline 2-year mortality rate of 12%). The analysis of individual health conditions reveals a plausible pattern underlying the effect on physical health. Effects are strongest for hypertension, which we would expect to be most responsive in the short run. Smaller effects I find for heart diseases which are typically caused by high blood pressure. And there are no effects on arthritis, diabetes, lung disease and cancer which in general take more than two years to be affected by external factors. Compared to the cross-sectional relationship of wealth and health the estimated effects are large in magnitude.

For a causal interpretation of these estimates constructed wealth shocks must be inde-

[1] For reviews of the literature see Smith (1999), Deaton (2003), Cutler, Deaton and Lleras-Muney (2006), Cutler, Lleras-Muney and Vogl (2011).

pendent of any unobserved heterogeneity in health changes. Stock market changes are exogenous for the individual retiree but this is not the case for stock holdings. More educated, wealthier and more risk loving individuals typically hold larger fractions of their wealth in stocks. And the observation period covers only a limited number of on average slightly positive stock market changes. As a consequence constructed wealth shocks are likely to be correlated with unobserved determinants of stock holdings. For this reason I control separately for the fraction of wealth held in stocks. In other words, I compare health changes for individuals with the same amount of stocks at different points in the stock market cycle. One might still worry that results are driven by a correlation of the stock market with investor types or with the typical investor's health profile. Several robustness checks show that this is unlikely to be the case. This suggests that constructed wealth shocks indeed cause the observed changes in health.

For the interpretation of this relationship as the effects of wealth shocks on health it is further necessary to control for effects of the stock market or the macroeconomic environment that do not run through stock wealth. I argue that retirees without stocks are at least equally strongly affected by potential direct effects as those with stocks. I include time effects to absorb any macroeconomic shocks common to both groups.

Despite a broad existing literature positive effects of wealth shocks on elderly health have been documented so far only for poor retirees in Russia (Jensen and Richter 2003) and South Africa (Case 2004). As Cutler, Lleras-Muney and Vogl (2011) summarize in a recent literature review, "... [A] preponderance of evidence suggests that in developed countries today, income does not have a large causal effect on adult health". The most prominent papers providing this evidence can be summarized by three main approaches.

A first set of papers uses approaches related to Granger-causality (Adams et al. 2003; Smith 2005; Michaud and Van Soest 2008). Using the HRS data these papers show that wealth changes and lagged wealth conditional on socio-economic controls do not predict health changes at the micro-level. I discuss these studies in detail below. Their focus on a younger working-age population as well as measurement error in self-reported wealth and short-term responses in health seem to be likely explanations of why their results are different from the findings presented in this paper.

Another set of papers analyze aggregate time series of income and health at the state or cohort level (Ruhm 2000; Deaton and Paxson 2001; Deaton and Paxson 2004; Snyder and Evans 2006; Adda et al. 2009). None of these papers find evidence of a positive relationship of income changes and health changes at the macro-level. Endogeneity and measurement errors are less of an issue in aggregate data. Group averages are independent of individual-specific endogeneity and cancel out random measurement error.[2] However, aggregate income changes might be correlated with macro shocks that also have non-

[2] Aggregation at the state or cohort level is equivalent to instrumental variable estimation with a set of state or cohort dummies as instruments (Angrist and Pischke 2009).

income effects on health. This invalidation of the exclusion restriction, as the authors of these papers note, makes it difficult to infer causal effects from these findings.[3]

A third set of papers exploits lottery winnings as a source of exogenous variation in wealth (Lindahl 2005; Gardner and Oswald 2007; Apouey and Clark 2009). These papers find positive effect on mental health, while results are less conclusive for physical health. A general challenge of lottery studies are small sample sizes and in particular few observations of significant winnings. Further, only positive wealth shocks are observed.

In the present study I combine these different approaches. I merge the rich micro-data from the HRS with aggregate stock market changes to introduce a source of exogenous macro shocks. The interaction of these macro shocks with a micro-level measure of the exposure to these shocks, the amount of stock holdings, allows to better control for potential non-wealth effects of the macroeconomic environment. The resulting setup is in spirit a large-scale lottery framework that allows to analyze the causal effect of wealth gains and losses on elderly health in the US.

How plausible are the effects that I find? Should we expect positive physical health effects found for poor retirees in poor countries to carry over to wealthy retirees in the US? 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 less of an issue for stock holding US pensioners, who have enough money left to afford basic pills and food even after a considerable wealth loss. Further, Medicare covers the entire 65+ population in the US so that wealth shocks do not affect basic 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 consumption to affect health outcomes as dramatically as observed.

Other plausible channels are psychological factors such as happiness about pleasant trips that were not affordable before or financial worries and sadness about a lost fortune that had been accumulated as inheritance for the grandchildren. A broad literature in medicine, psychology and biology has documented effects of psychological stress on coronary artery diseases, clinical depression and mortality (Strike and Steptoe 2004). Positive emotions, on the other hand, were found to have positive effects on these health outcomes (for a review see Chida and Steptoe, 2008). In the HRS data I find strong wealth shock effects on high blood pressure and mental health and smaller effects on heart problems. This is exactly the kind of health response the bio-medical literature would predict if wealth shocks have an effect on psychological stress.[4]

[3]For a discussion of Ruhm (2000) and of Snyder and Evans (2006) see also Miller et al. (2009) and Handwerker (2008), respectively.

[4]The responsiveness of elderly mental health to income related shocks has also been documented by Grip et al. (2009).

The focus of this study on elderly retirees has several advantages. Compared to younger adults retirees have a lot of wealth and heterogeneity in wealth composition so there is a lot of wealth variation to exploit. Further, as they no longer participate in the labor market effects of stock market shocks running through labor demand are limited. That makes it easier to separate wealth shock effects from other confounding factors. Last, at an average age of 75 the analyzed retirees are closer to the margin of severe health problems (including death) than younger adults. This makes it more likely for effects of wealth shocks on latent health to become manifest in observable health outcomes.

However, caution must be exercised when extrapolating from my estimates to other settings. Effects are identified only for stock holding retirees who are on average wealthier, healthier and less risk-averse than those without stocks. Further the estimated effects might not be representative for younger adults who are in better physical shape and flexible in terms of their labor supply to compensate a given wealth shock.[5] Last, my estimates represent the short-term effects of wealth shocks. They might not be representative for the long-run effects of gradually accumulating wealth differences. The comparison with the cross-sectional relationship of wealth and health indeed suggests that the long-run wealth elasticity of health is smaller and more homogeneous across health conditions than the estimated causal effects of wealth shocks.

The remainder of this paper is organized as follows: Section II discusses the identification strategy, Section III describes the data, Section IV the empirical specification. Section V presents the findings and Section VI concludes.

2 Identification

This paper seeks to estimate the causal effect of wealth shocks on health. The difficulty of this task is the endogeneity of wealth. Wealth shocks might not only affect health, but health shocks are also likely to reversely affect expenditures and third factors might influence both wealth and health simultaneously. Further, wealth is typically measured with noise leading to attenuation bias. This measurement error problem tends to aggravate in first differences. For these two reasons the simple regression of health changes on wealth changes from observational data might not tell us a lot about the causal effect of wealth shocks on health outcomes.

The ideal experiment to solve the endogeneity problem 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

[5]Sullivan and von Wachter (2008), however, provide related evidence for younger adults. They show that exogenous job displacements dramatically increase the mortality hazard of male US workers during the years following the job loss. The authors interpret their findings to be consistent with job loss "causing acute stress, which may substantially raise the mortality hazard in the short term."

gains and losses for retirees owning stocks.[6] This natural experiment comes quite close to the ideal setting. As stock market changes are largely unpredictable for retirees without insider information holding stocks is equivalent to buying lottery tickets.

I construct stock market induced wealth shocks (hereafter *constructed wealth shocks*) as the interaction of the lagged fraction of life-time wealth held in stocks with stock market changes.

$$\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}} \tag{1}$$

where $s_{i,t-1}$ are past wave's stock holdings, $W_{i,t-1}$ is a measure of past wave's life-time wealth (see below) and $\frac{\Delta SP_t}{SP_{t-1}}$ the percentage change in the S&P500 stock market index between two waves. For example, an individual with 20% life-time wealth held in stocks in the past wave and a 50% stock market increase between the past and the current wave is assigned a 10% positive wealth shock.

To estimate the effects of wealth shocks on health outcomes I regress health changes directly on constructed wealth shocks while controlling for the main effects and demographic covariates:

$$\Delta H_{i,t} = \alpha + \beta\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}} + \gamma\frac{s_{i,t-1}}{W_{i,t-1}} + \vartheta_t + \delta X_{i,t} + \epsilon_{i,t} \tag{2}$$

where $H_{i,t}$ are different health measures, $\frac{s_{i,t-1}}{W_{i,t-1}}\frac{\Delta SP_t}{SP_{t-1}}$ are constructed wealth shocks, ϑ_t are year fixed effects and $X_{i,t}$ predetermined demographic controls. Health measures are regressed in first differences because wealth shocks can only explain changes but not past levels in health. Taking first differences therefore cleans the dependent variable of unexplainable variation while it does not reduced the number of observations since the construction of wealth shocks already requires a lag.

For the interpretation of β as the causal effect of wealth shocks on health two conditions must be satisfied. Constructed wealth shocks are causal only if they are independent of any unobserved heterogeneity in health changes. Further, their effect on health captured by β must run exclusively through changes in stock wealth.

[6] To my knowledge Coile and Levine (2006) have been the first to exploit this natural experiment. They analyze the impact of stock market movements on retirement decisions, comparing the effects of stock market movements on retirement for groups that are relatively more and less likely to hold stocks. I enhance their approach, using the exact fraction of wealth held in stocks instead of a binary indicator of stock market exposure which increases the power of the analysis.

2.1 Are constructed wealth shocks causal?

Stock market changes are largely unpredictable (for a review of the finance literature on market efficiency see Malkiel [2003]) and therefore random for the individual retiree. However, the observation period covers only a limited number of on average slightly positive stock market changes. As a consequence constructed wealth shocks are on average higher (more positive) for those with more stocks. At the same time stock holdings are not random. The richer, the more educated and the more risk loving typically hold larger fractions of their wealth in stocks. This results in a correlation of constructed wealth shocks with unobservable determinants of stock holdings. Regressing health measures in first differences cancels out unobserved heterogeneity that is constant over time. But determinants of stock holdings might not only correlate with health levels but also with health profiles over time so that first differences alone do not rule out potential endogeneity.[7] Therefore it is important to control separately for the lagged fraction of wealth held in stocks ($\frac{s_{i,t-1}}{W_{i,t-1}}$).

This means I compare health changes for individuals with the same amount of stocks at different points in the stock market cycle. Or in terms of the lottery analogy, I measure the health response to lottery winnings and losses conditional on the amount of lottery tickets bought. Still one might worry that the stock market is coincidentally correlated with the health profiles of the typical stock-owning retiree. Or investor types might change over time so that retirees with the same amount of stocks during a boom and during a bust might not be comparable. I analyze and discuss these potential issues in detail in the findings section when the main results are more easily at hand.

2.2 Are effects running exclusively through stock wealth?

Stock market changes might not only determine the valuation of stock holdings but also correlate with prices of other non-stock wealth holdings such as bonds or real estate. A way to test for such correlation is to look at the comovement of the stock market with the wealth of households that do not own stocks. Figure 1 compares the S&P500 with the coefficients from regressions of wealth changes on wave dummies for retirees with stocks and without stocks in the previous period. For retirees with stocks they follow the up's and down's in the S&P500.[8] But for retirees without stocks wealth changes are positive in all waves and seem uncorrelated with the stock market. More detailed regressions taking into account the precise month of interviews are presented in the Findings section. Again, the stock market is highly predictive for wealth changes of stock holders, while the effect on wealth for those without stocks is essentially zero. This suggests that there is not much

[7]For example, individuals who anticipate a health risk might want to reduce financial risks and redistribute their portfolio from stocks to safer assets. Or people with less education have more declining health profiles due to worst health behavior and at the same time hold less stocks due to less financial literacy. Given a limited number of on average positive stock market changes these examples of reverse causality or simultaneity would imply a positive correlation of constructed wealth shocks and health changes.

[8]Notice that the majority of respondents in the last wave face a lower S&P500 than at their previous wave's interview (this is also evident in Figure 2) and thus a negative average wealth change is what one should expect.

an effect of the stock market on non-stock wealth.

But the stock market or more broadly the macroeconomic environment might also affect health 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. As the sample is restricted to retiree households effects running through the individual's employment status are limited. But retirees might be troubled about their children becoming unemployed or their grand children not finding a job after graduating from high school. Further we could think of the provision of public goods that might depend on the macroeconomic environment and have a direct effect on pensioners' health. And retirees could be stressed and fearing social instability when hearing apocalyptic news about the economy in the media. However, it seems reasonable to assume that these direct effects are at least as strong for retirees who do not hold stocks as for those with stocks. Retirees without stocks tend to be poorer, less educated and more risk averse. If anything, they depend more on public goods, suffer more from bad news and their children are the first to get fired when it comes to mass lay-offs in a recession (Hoynes, Miller and Schaller, 2012). To control for potential direct effects in a conservative way I therefore include time fixed effects (ϑ_t).

Before describing the data and the final empirical specification in detail a few issues remain to be discussed.

2.3 Measurement and scaling issues

Changes in reported wealth are not only endogenous but also notorious for attenuation bias due to measurement error. Constructed wealth shocks help to minimize this kind of bias because they rely on levels instead of changes in self-reported wealth. Notice that the other component of constructed wealth shocks, changes in the S&P500, represent average stock market returns. Average returns do not account for individual portfolio compositions which are not observed in the data. However, the resulting measurement error in constructed wealth shocks is negatively correlated with actual returns but uncorrelated with constructed wealth shocks, i.e. the regressor of interest. This kind of measurement error – though occurring in the explanatory variable – only implies less precise estimates but no attenuation towards zero.

Constructed wealth shocks under- or overestimate actual wealth shocks if retirees' expectations of stock market returns systematically differ from zero. Luckily the HRS includes since 2002 a question about the likelihood that the stock market increases within the following year. Figure A.1 in the Appendix plots monthly averages for this question together with the S&P500. Expectations are strikingly low: even those with stocks expect on average only a 45-60% chance that the stock market will increase. Furthermore, expectations seem to be slightly correlated with the stock market. Following Dominitz and Manski (2007) I transform expected probabilities about stock market increases into expected stock market returns and adjust for them when constructing wealth shocks. As expectations are only marginal compared to actual stock market changes their inclusion decreases estimates only slightly. For better comparability of my results with other studies I therefore do not include expectations in the baseline regressions.

Constructed changes in stock wealth ($s_{i,t-1}\frac{\Delta SP_t}{SP_{t-1}}$) are divided, or rescaled, by a measure of life-time wealth ($W_{i,t-1}$), i.e. is the discounted sum of current wealth holdings and expected future pension income (see Data section for details). The rationale behind this rescaling is that the effect of a given wealth shock is likely to depend on the initial wealth level. A $50,000 loss might not be noteworthy for the very rich but is painful for the poorer. And what matters is not just what an individual possesses at the time of the shock but also what she expects to earn in the future. If she has high annual income and still many years to live a given wealth loss can be easily compensated by dissaving. Taking into account not just current wealth but also future income makes sense especially for retirees. They typically have constant pension income and a limited time horizon of remaining years to live. An additional advantage of rescaling by life-time wealth instead of current wealth is that life-time wealth has fewer zeros or negative values which have to be excluded from the analysis. Results, however, are not driven by the inclusion of life-time wealth. The overall effect pattern remains the same when rescaling wealth shocks by current wealth instead of life-time wealth.

3 Data

The data used in this study come from the waves 4 to 10 of the Health and Retirement Survey (HRS), covering the years 1998 to 2011.[9] The HRS is a biannual panel that started in 1992 with 12,654 individuals representing US adults of age 51 and older. In 1998 and 2004 new cohorts were added to keep the sample representative resulting in an extended sample of about 22,000 individuals. Moreover, in 1998 the fraction of individual retirement accounts invested in stocks, a variable that is central for my analysis, is introduced. Per household one so-called financial respondent is interviewed about her and the other family members' income and wealth holdings. Other questionnaire items such as health measures are reported by all household members. The sample of this study is restricted to financial respondents, who report wealth and stock holdings and non-zero retirement income in the previous wave, and their spouses if existent. Further I restrict the sample to singles and couples who were retired in the previous wave, i.e. either (i) both financial respondent and spouse were neither working for pay (i.e. neither working, nor part-time working, nor partly retired) nor unemployed or (ii) both considered themselves completely retired. The final regression sample consists of about 40,000 person-year observations, of which 20,000 refer to singles. The average age is 75.43 years, 63% of the sample are women and 82%

[9] The data is drawn from the RAND HRS file. Variables that are not included in the RAND file are added from the HRS raw data.

are white (see Table A.1 for further summary statistics).

The interview month is known, so that the HRS data can be matched to monthly stock market data from the Standard & Poor's 500 stock market index (S&P500).[10] Constructed wealth shocks are generated for financial respondents and matched to spouses. Interviews which start in one month and end in a later month are dropped as well as spouse interviews that are conducted in a different month from the financial respondent.

3.1 Wealth data

The HRS contains detailed information on income and wealth holdings. Financial information is reported in exact amounts and unfolding response brackets are offered if exact amounts are unknown. This study uses cleaned and partly imputed wealth data from the RAND HRS file. Current household wealth ($A_{i,t}$) consists of net housing wealth, 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, and debts. Pension plans such as 401(k)s are not reported for retirees in the HRS because these plans are usually cashed out or rolled over into an IRA upon retirement.

I construct a measure of life-time wealth ($W_{i,t}$) as the sum of current wealth and discounted expected future income.

$$W_{i,t} = A_{i,t} + E(\sum_{\tau=0}^{T-t} \frac{Y_{t+\tau}}{(1+r)^{t+\tau}}) \tag{3}$$

with $Y_{i,t}$ income and r the real annual interest rate. Current wealth and *past* earnings are well documented in the HRS. Fortunately, retiree income - consisting of pensions and annuities ($PIA_{i,t}$), old age social security ($SS_{i,t}$) and veteran benefits ($VetBen_{i,t}$) - can be expected to stay constant (in real terms) after the first receipt until the individual's end of life. Hence we can take past year's annual income from pensions, annuities, old age social security and veteran benefits as the expectation for future income.[11] Interest rate expectations (set to 3%) are assumed to stay constant as well. Further, the survival probability is needed. I calculate (τ)-year survival rates by age (t), gender (g) and 10-year birth cohort (c) using the SSA life tables.

$$W_{i,t} = A_{i,t} + (SS_{i,t} + PAI_{i,t} + VetBen_{i,t}) \sum_{\tau=1}^{T-t} \frac{E(S_{t+\tau}|t_i, g_i, c_i)}{(1+r)^{t+\tau}} \tag{4}$$

[10]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. However, using the Dow Jones Industrial Average, which represents only 30 companies delivers similar results.

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

Social security benefits pose a potential problem as there are financial incentives to delay take-up to age 65 (Coile et al. 2002). For retirees below age 65 who do not report receiving social security it is not clear whether they are postponing or whether they are not entitled to social security payments. I present robustness checks excluding all households with one or both spouses below age 65.

Different life expectancies within households, i.e. within couples, are a further complication. 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. Restricting the sample to singles in order to avoid this simplified life-time wealth formula for couples does not affect the pattern of the estimated effects (see robustness checks). The same holds true if I use current wealth ($A_{i,t}$) instead of life-time wealth to rescale wealth shocks.

A central ingredient for constructing wealth shocks 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 important to know the percentage of each IRA invested in stocks.

In 2006 and 2008 for each IRA the exact percentage invested in 'stocks and mutual funds' is reported. In the 1998 to 2004 waves 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.

Table 1 summarizes sample characteristics and main wealth measures per HRS wave (for further wealth summary statistics see Table A.2 in the Appendix). In 2004 younger than average cohorts are added, 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 who were retired in the previous wave and reported wealth, non-zero retiree income and stock holdings. 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 constructed 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.

The final two rows of Table 1 display average stock market changes between interviews and the resulting constructed wealth shocks. The booms and busts around the New Economy stock market bubble and the financial crisis, which are covered by the observation period, can be clearly seen. Averages of constructed wealth shocks per wave roughly resemble the average stock market change multiplied by the average fraction held in stocks in the previous period.

Figure 2 plots constructed wealth shocks and the S&P500 over time. Each circle represent one household and is placed at the month of the interview. Wealth shocks roughly range from -30% to +40%. These are dramatic changes. For a retiree who has about 10 years remaining to live a 10% loss in life-time wealth 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 or emergencies, consumption has to decrease by even more. Notice that these dramatic wealth shocks are constructed and might not correspond to changes in reported wealth of similar size. Their predictive power is assessed in the Findings section.

3.2 Health data

I use different health measures from the HRS as dependent variables: A physical health index, individual health conditions, self-reported health, self-reported change in health, a mental health index as well as survival to the next interview. For better comparability measures of bad health are inverted such that higher values of a measure always refer to better health. This means that a positive coefficient on wealth shocks always refers to an improvement in the respective health measure. For comparability of effect sizes across measures which are reported on different scales and represent health circumstances of different severity I also show results for 'probit-adapted' health measures following an approach by van Praag and Ferrer-i-Carbonell (2008). This approach yields effects in terms of standard deviations that additionally account for potential measure-specific non-linear scaling.[12] Summary statistics of original and transformed health measures are reported in the Appendix, Table A.3.

The physical health index equals the sum of conditions which have *ever* been diagnosed by a doctor according to the respondent. The HRS questionnaire includes seven physical health conditions: high blood pressure, heart disease, stroke, arthritis, cancer, diabetes,

[12] I 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 cut-off points implied by an ordered probit fitted on the raw sample fraction. Changes in these transformed health measures are then regressed via OLS on constructed wealth shocks and controls. Van Praag and Ferrer-i-Carbonell (2008) refer to this as 'probit-adapted OLS'.

and lung disease. These health conditions are also analyzed in separate regressions. In theory the wording of the question only allows for new ever-diagnosed conditions to appear but never to disappear. In the data, however, a significant number of people report a condition in one wave but neglects the same condition in a future wave. Including these cases tends to increase the significance of the results. It is therefore likely that such 'wrong' answers are not mere noisy but contain information about actual or perceived changes in the respondent's health. Individuals might understand the question wrongly (overlooking the 'ever') or repress the memory of a cured disease. One should therefore be aware that at least for a fraction of respondents these questions only indicate the current prevalence of a condition.

For self-reported health respondents 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. Self-reported changes in health are regressed directly in levels and not in first differences as the question already implies a health change.

The mental health index sums a subset of eight questions from the 20 question CES-D depression score, which has been developed to diagnose clinical depression. Six questions indicate 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. Two questions, that are subtracted from the index, indicate whether the respondent felt happy and enjoyed life, all or most of the time during the past week. Like the physical health index, the mental health index is inverted for regressions so that higher values indicate better mental health.

Deaths of survey participants are documented in so-called exit surveys in which 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. 'Survival', used as the dependent variable in the baseline regressions, indicates whether the respondent survives until the next interview. This means that survival from t to t+1 is regressed on wealth shocks from t-1 to t. Therefore only individuals up to wave 9as can be included in the survival regressions.

4 Empirical Specification

The identification strategy outlined above leads to the following empirical specification:

$$\Delta H_{i,t} = \alpha + \beta \frac{s_{h(i),t-1}}{W_{h(i),t-1}} \frac{\Delta SP_{m(i,t)}}{SP_{m(i,t-1)}} + \gamma \frac{s_{h(i),t-1}}{W_{h(i),t-1}} + \vartheta_t + \delta X_{i,t} + \epsilon_{i,t} \tag{5}$$

with indices:
i: Individual
$h(i)$: Household of (i)
t: HRS wave (biannual)
$m(i,t)$: Month of the interview of individual (i) in wave (t)

and variables:
$\Delta H_{i,t}$: Health outcomes
SP: Standard & Poor's 500 stock market index
s_{t-1}: Lagged stock holdings
W_{t-1}: Lagged life-time wealth
ϑ_t: Year dummies
$X_{i,t}$: Demographic controls: Dummies for gender (1), age group (12), cohort (10), race (2), degree (4), lagged region (4), and lagged marital status (7).

Changes in different health measures are regressed via OLS on the interaction of stock market changes with the lagged fraction of life-time wealth held in stocks (constructed wealth shocks) while controlling separately for the 'main effects', i.e. the lagged stock fraction and year dummies. I can additionally include the exact stock market change (which differs across individuals within the same year) and a dummy for no stock holdings to control for the main effects in a more flexible way. Including a full set of year x month dummies instead of year dummies leads to very similar results. Health outcomes and demographics vary at the individual level, wealth at the household level and the stock market at the monthly level. Standard errors are multi-level clustered by households and interview month (Cameron, Gelbach and Miller 2011).

Predetermined demographic controls such as age, gender, race or lagged martial status may be included to decrease the variance of the regression residual and thereby increase the precision of the estimates. The inclusion of demographic controls should not change the point estimate of constructed wealth shocks if the latter are (conditionally) independent.

5 Findings

5.1 Predictive power of constructed wealth shocks

Constructed wealth shocks are highly predictive of changes in reported wealth. As reported in column (1) of Table 2 the regression of changes in reported wealth on constructed wealth shocks and controls yields a highly significant coefficient of about 0.82. Including a large number of demographic controls hardly affects the estimate, resulting in a coefficient of 0.79. This means that a constructed wealth shock of 10% corresponds to a change in reported wealth by about 8%. As argued above, retirees are likely to adapt their consumption to wealth shocks. The estimated coefficient suggests that out of a 10% wealth shock 2% goes into consumption.[13] In column (3) and (4) of Table 2 the exact stock fraction is substituted by a dummy for stock holdings. Again stock market changes are highly predictive of wealth changes for those with stocks. A 10% change in the stock market leads to a 2.1% change in the wealth of stock holders.

Notice that the stock market effect on those without stocks (i.e. the coefficient on 'stock market change') is small and not significantly different from zero in all four columns of Table 2. This gives further support to the conclusion of Figure 1, that there is not much of an effect of the stock market cycle on the wealth of retirees without stocks. Further, the R^2 is extremely low despite the inclusion of a broad set of demographic controls. This indicates that reported wealth in first differences is a noisy measure. Despite this noise constructed wealth shocks do a good job in picking up actual changes in reported wealth. Let us now turn to the effects of these wealth shocks on health outcomes.

5.2 Effects of wealth shocks on health outcomes

Table 3 reports the baseline regressions of five health measures (rows) on constructed wealth shocks. Regressions in column (1) include as controls only the main effects, i.e. the lagged fraction of wealth held in stocks, a dummy for lagged stock ownership, the stock market change and year fixed effects. In column (2) a broad set of demographics is added. In column (3) dependent variables are standardized using Probit-adapted OLS so that estimates are in terms of standard deviations and thus comparable across health measures (see Data section). All estimates displayed in this and the following tables refer to the coefficient on constructed wealth shocks. A positive coefficient refers to a health improvement in the respective measure.

The regressions in the first column indicate a positive effect of constructed wealth shocks on all five health measures, ranging from 0.082 to 0.254. The effect is significantly different

[13] This implies a propensity to consume out of stock wealth of 20%. Compared to the literature that has found estimates ranging from 1-5% this seem very large (Poterba 2000). A possible explanation could be the old age of the sample. Consumption smoothing implies that the propensity to consume out of a given wealth shock increases with age. If you have less years to live a given shock has to be smoothed over fewer years. But the coefficient on wealth shocks might also be attenuated due to measurement error in the lagged stock fraction. The 20% estimate should probably not be overinterpreted.

from zero for all measures except for the self-reported change in health. Including a broad set of demographic controls in column (2) hardly changes any of the coefficients. This provides confidence that constructed wealth shocks are independent of unobserved heterogeneity. If the estimates were strongly affected by the inclusion of predetermined controls we should be worried about the exogeneity of constructed wealth shocks. The estimated effect on the physical health index indicates that a negative 10% wealth shock is associated with a deterioration of the index by about 0.025 units. In other words, among 40 retirees losing 10% of their life-time wealth one will develop an additional physical health condition. The effect on survival suggests that among 100 retirees suffering a 10% wealth shock there will be one additional death within the following two years. The estimates in column (3) show that in terms of standard deviations the significant effects are quite similar across health measures, ranging from 0.2 to 0.3.

In Table 4 I repeat these regressions separately for the seven health conditions from the physical health index. As in the previous regressions, all health conditions are transformed such that positive coefficients indicate a health improvement (i.e. a lower chance to develop the respective health condition). A problem of the analysis of various health conditions is that the chance of wrongly rejecting the null increases with every additional regression.[14] In the present setup, however, significant estimates would be more plausible for some health conditions than for others. Health changes are regressed on wealth shocks over a period of on average two years. Therefore estimated health shocks must be driven by diseases that are responsive to environmental factors and that do not take a lot of time to develop. The regressions in Table 4 reveal a strongly positive effect of wealth shocks on high blood pressure, a smaller effect on heart disease, but no significant effect on other health conditions. For arthritis, cancer, diabetes and lung disease there is also no joined significance in SUR models, neither for pairs nor for groups of three or four conditions. As in the regressions for health measures the inclusion of demographic controls hardly changes estimates. Standardized effects in column (3) further show that this overall pattern is not driven by differences in the baseline rate of these different health conditions, since effects are strongest for hypertension also in terms of standard deviations.

These heterogeneous effects across different physical health conditions are plausible (for a medical text book describing these conditions see Fauci et al. 2011). High blood pressure is the most responsive health problem in the short run and arises from both psychological stress as well as unhealthy nutrition and behavior. Moreover, high blood pressure is a cause for heart problems, so that a significant effect on heart problems is what one should expect given the strong effect on high blood pressure. Similarly, one might expect an effect on strokes, a condition that is caused by high blood pressure, too. Indeed, a slightly significant effect on strokes appears in some specifications but this effect is not robust. One reason might be that strokes are often fatal so that respondents decease before they could report this condition. In line with this reasoning the summary statistics in Table

[14] In general one can correct for this problem by either reducing the number of tests (as done above by summarizing conditions into one index) or by adjusting p-values (Anderson 2008).

A.3 show that strokes are the least observed condition even though strokes are among the leading causes of death (Fauci et al. 2011).

Effects on arthritis, diabetes, lung diseases or cancer would be less plausible. Arthritis is a chronic condition that takes more than a few years to develop and is unlikely to respond to psychological stress. Diabetes is driven by genetic disposition as well as by obesity. One could think of a response in body weight to stress, but such an indirect effect might take more than 1-2 years. And I do not find an effect of wealth shocks on body weight. Lung diseases are typically driven by smoking or unhealthy environments at work and take a long time to develop. Regarding cancer there is a psycho-medical literature discussing stress as a potential cause, but such effects remain highly controversial (Chida et al. 2008).

Looking at individual depression symptoms from the mental health index does not reveal a single driver such as hypertension for the physical health index (results reported in the Appendix, Table A.4). This is plausible. The mental health index does not 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.

Note that the effect on the two-year survival rate in Table 3 is exactly what we should expect given the effects on mental health and in particular on high blood pressure. High blood pressure related health problems are the leading cause of death in the Western world (Cutler, Deaton and Lleras-Muney 2006). And the sample of analyzed elderly, with average age 75, is already at the margin of death. 12% of the sample respondents do not survive the following two years (Table A.3; this death rate is also in line with US life tables). So it does not take a massive effect on latent health for them to be pushed over this threshold.

5.3 Effects by age, gender and degree of stock market exposure

Table 5 investigates the heterogeneity of effects across age and gender. The coefficients of wealth shocks interacted with the respective categories are displayed as well as the significance level of their difference. Overall, interaction terms are not estimated with much precision which is not surprising given that estimates in the overall sample are already quite noisy. But the effect heterogeneities that are strong enough to be detected are plausible.

The age interactions are strongly different in the survival regression. Wealth shocks affect survival rates for the elderly six times as much as for the younger group. The effect on the physical health index shows up with a similar age pattern. Effects are twice as large for the elderly. These differences are not significant, but the joint hypothesis of equality in both the survival and the health conditions regression can be rejected at the 5% level. For self-reported health and mental health no clear age differential arises. This pattern across health measures makes sense. Both mortality and health conditions show up in the data only if an individual is pushed over a certain health threshold. As the health distribution shifts with age towards worse health the density around this threshold increases with age. This means that we should observe a larger effect on mortality and health conditions for the elderly even if the effect on latent health is the same across age groups. Mental and self-reported health, on the other hand, are more continuous so that health deterioration over age does not automatically imply stronger effects on these measures.

There are no significant gender differences. Mental and self-reported health seem to be more affected for women which would be in line with the literature on gender differences in mental health but the estimated differentials are imprecise. These results do not imply that effects are the same for males and females but it seems that estimates are not driven by gender.

Table 6 explores the linearity of the effects. Instead of interacting stock market changes with the exact fraction of wealth held in stocks I include interaction terms with dummies indicating 1-10% and >10% wealth in stocks, respectively. If stock market effects increase with wealth held in stocks (i.e. stock market exposure) effects should be stronger for the latter interaction term. This is what the results in Table 6 indicate. Stock market changes affect retirees with more than 10% wealth in stocks two to eight times as much as retirees with 1-10% in stocks. Estimated effects for the latter group are small and therefore not significantly different from zero in most cases, but point estimates are positive for all health measures. Importantly, there is no effect of the stock market on retirees without stocks, as indicated by the coefficient on the stock market change main effect.

5.4 Are the effects of wealth shock on health outcomes causal?

The estimation results in Tables 3 to 6 show strong, robust and plausible effects of constructed wealth shock on physical and mental health outcomes of elderly retirees in the US. Since the empirical strategy exploits the randomness inherent in the stock market, interacted with the degree to which individuals hold retirement wealth in stocks, there is reason to believe that estimated effects are not simply driven by selection but reflecting a causal relationship. However, there are some alternative stories one could think of and ways to test them in the data.

One worry might be that the stock market correlates coincidentally with health profiles of those retirees who tend to hold a lot of stocks. A brief look at the stock market development over the observations period in Figure 1 suggests that this is unlikely to be the case. Positive and negative stock market changes follow each other and it is hard to imagine that health profiles of stock holders just happen to follow these ups and downs by chance. However, retirees with a lot of stocks might be more affected by the overall business cycle regardless of their stock market investment, for example if they tend to follow the news more closely and worry more about the country's economic future than retirees without stocks. I can test this hypothesis in the data by interacting stock holdings with the overall unemployment rate which reflects the business cycle better than the stock market. Alternatively, the stock market might for some reason affect wealthy retirees more than poorer retirees, regardless of actual stock holdings. To test this I interact the stock market change with the wealth fraction held in bonds, since wealthy retirees tend to hold larger fractions of their wealth both in stocks and bonds. Table 8 shows regressions with these two types of 'placebo shocks'. Despite the strong collinearity with the constructed wealth shock the placebo shocks do not consistently affect health outcomes when included separately. And the original wealth shock effect is robust and remains largely unchanged when I include all three shocks in horse race regressions in the third column of Table 8.

Another concern might be that retirees with the same fraction of wealth held in stocks at different points in the stock market cycle are not comparable. A retiree with 20% wealth in stocks at the beginning of a boom might be different from a retiree with 20% in stocks right before a crash. The observation period covers only a limited number of stock market changes so that there could be a spurious correlation of stock market changes with the type of investor. Also, individuals do not rebalance portfolios continuously. So a retiree with 20% in stocks who does not rebalance her portfolio will end up with 33% in stocks when the stock market doubles. One way to rule out such correlation of the stock market cycle with the type of investor as a potential driver is to instrument actual stock holdings with individuals' initial stock holdings in the first period. Initial stock holdings are constant over time for a given individual. Hence they are uncorrelated with where we are in the stock market cycle. Table A shows results from such 2SLS regressions. Point estimates and significance levels vary slightly compared to the baseline specification, but despite the loss of precision implied by this IV strategy the overall effect pattern carries over to this IV specification.

An alternative way to check whether estimated effects are driven by changes in investor types is the inclusion of predetermined demographic controls (Altonji et al. 2005). If the relationship of health changes and constructed wealth shock is driven by changes in the type of investors then the inclusion of controls like gender, age, education, and region of residence should change the coefficient on wealth shocks. As the comparison of the estimates in columns (1) and (2) in Tables 3 and 4 has shown, adding a wide range of demographic controls to the baseline specification hardly changes any of the estimates. But the included demographic controls might just be poorly measured proxies of the actual confounders. As Pischke and Schwandt (2012) show a more sensitive test to detect selection in the presence of measurement error is the inclusion of individual controls as dependent variable on the left-hand side of the regression equation. Table A.6 shows that none of these balancing regressions for various socio-economic controls yields significant wealth shock effects.

To sum up, it seems unlikely that a correlation of the stock market cycle with investors' health profiles or with investor types is driving the results. Notice that there is also no direct effect of the stock market on retirees without stocks, neither on wealth (Table 2) nor on health outcomes (Table 6). This suggests that constructed wealth shocks are indeed causing the observed changes in health and that effects are mainly running through stock wealth.

5.5 Effect size

How large are the estimated effects? A good way to assess the effect size is the comparison with the cross-sectional relationship of wealth and health. Regressing health on wealth in levels does not allow for a causal interpretation as the coefficient on wealth also reflects reverse causality and omitted third factors. But one would expect such endogeneity to bias the coefficient upwards. Such benchmark regressions provide an upper bound for the average causal effect of wealth on health in the sample, in particular if few additional controls are included.

Tables 8 and 9 compare the baseline effects of wealth shocks in column (1) with the cross-sectional relationship of health and ln household wealth in column (2). Column (3) shows the association of ln wealth with the change in health measures. There is only one benchmark estimate for self-reported changes in health and survival since these measures already refer to a health change. The estimates in the first two columns in Table 8 suggest that wealth shock effects on the physical health index and on self-reported health is of similar size as the cross-sectional relationship but it is 1.5 and 3 times larger for mental health and the survival probability, respectively. With respect to the physical health index this means that a 10% negative wealth shock leads to a similarly large health decline as the health gap that is associated with a 10% wealth difference in the data. Lose 10% of your life-time wealth as an elderly retiree in the stock market and you end up with a similar score on the physical health index as your neighbor who has been 10% poorer before.

Benchmark regressions for individual health conditions in Table 9 indicate that this is not yetthe whole story. While wealth shocks affect only particular conditions the cross-sectional wealth gradient is strongly significant and of similar size for all health conditions, except for cancer.[15] And for hypertension and heart disease the wealth shock effect is about twice the size of the benchmark gradient. This means that after a stock market induced wealth loss you will suffer more from hypertension and related diseases than your ex-ante poorer neighbor. But your neighbor is still more likely to have arthritis, diabetes and lung disease.

The differences between the baseline and cross-sectional estimates suggest that the effects of wealth shocks are different from the average causal effects of wealth on health in the sample. This seems plausible. Someone owning $500k can afford better health care

[15] For cancer the gradient is inverted meaning that richer people are more likely to have cancer. This reversal has been documented in other data sets but is so far largely unexplained.

and healthier consumption than somebody owning $300k which over time accumulates to a better health stock. This however is a different effect from losing $200k in a stock market crash, which involves high blood pressure and psychological factors such as stress and depression rather than just a slight change in health inputs. In light of the difference between these strong short-term effects and smaller but accumulating long-term effects of wealth differences the small coefficient in the survival benchmark regression makes sense. Survival does not contain any accumulated effect because those who died before are not observed. An elderly retiree who loses a lot of retirement wealth in the stock market will have a lower survival probability than the ex-ante poorer neighbor – given that this neighbor is still alive.

Notice that the comparison with the cross-section also provides further confidence that my estimates are not driven by a coincidental correlation of the stock market with the socio-economic status of stock market investors. If this were the case, we should observe a similar pattern of effects across health conditions as in the benchmark regressions. But the pattern is clearly different. Still one might worry that effects are driven by a correlation with the typical health *profiles* of investors. Possibly at older ages richer people tend to get more hypertension and related diseases simply because they have done well at younger ages. Column (3) in Tables 8 and 9 show the benchmark regressions of health changes on wealth levels. Again the pattern in these regressions is very different from the estimated wealth shock effects.

Another important effect size comparison are estimates from the existing literature. In an influential study, Smith (2005) uses a sample of employed individuals from the HRS and shows that changes in stock wealth conditional on socio-demographic controls do not correlate with changes in health. Since my sample consists of retirees with average age 75, these findings for employed individuals do not contradict my results. It is worth noting, however, that I obtain similar zero results as Smith (2005) if I substitute constructed wealth shocks by reported changes in stock wealth (Table A.7). These findings of zero effects are interesting because we would expect potential endogeneity left in stock wealth changes to bias the estimate up and not towards zero. A more severe problem than potential endogeneity – in particular for elderly retirees – might be measurement error in reported stock wealth. Regressions in Table 2 have shown that changes in overall wealth are quite noisy and this is likely to be the case as well for changes in stock wealth.

Another influential study using HRS data is Adams, Hurd, McFadden, Merrill and Ribeiro (2003). These authors develop an innovative approach related to Granger causality and find that lagged wealth conditional on a broad set of socio-economic variables is not Granger-causing changes in health for almost all health measures in the HRS. However, in a recent study Stowasser, Heiss, McFadden and Winter (2011) repeat the analysis of Adams et al. (2003) using the full range of data available in the HRS. In these extended data they reject Granger causality only for three out of 40 health conditions: for cancer, female lung disease and male hypertension. The rejection for hypertension, the condition for which I find strongest effects, could be explained by contemporaneous wealth shock effects. The approach of Adams et al. (2003) tests for a causal effect of lagged wealth on health changes. If it does not take long for hypertension to respond to a wealth shock then a lagged wealth shock might already affect lagged hypertension and no effect would be left in the first difference. If effects are not permanent, this could even imply an inverted effect on the first difference.

5.6 Alternative sample specifications and 2SLS regressions

Regressions in Table 10 show that results are robust against various changes in the sample specification. In column (2) all financial respondents and their spouses regardless of their employment status are included as long as some kind of retirement income is reported for the household. This increases the sample size by more than 50%, but coefficients remain largely the same. In column (3) only households are included in which both spouses are above age 64. This rules out the possibility that results are driven by the group of pre-retirement age pensioners who are typically selected into the sample through bad health. In column (4) only single households are included. In the final column the bottom quartile from the life-time wealth distribution is excluded, which again changes estimates only slightly.

In the baseline specification I regress changes in health directly on constructed wealth shocks. An alternative specification is a two-stage least squares regression with constructed wealth shocks as an instrumental variable (IV) for changes in reported wealth. In terms of an IV setup the baseline specification refers to the reduced form while the regression of reported wealth changes on wealth shocks reported in Table 2 is the first stage. Since the first stage coefficient in Table 2 is smaller one and the IV estimate equals the reduced form estimate divided by the first stage, we should expect a 2SLS estimation to inflate coefficients. The coefficients reported in Table 11 indeed are about 25% above the baseline estimates for each health measure.

Which estimate is more relevant, the IV or the 2SLS estimate? The 2SLS specification provides us with estimates that are scaled in terms of the average change in reported wealth associated with a given constructed wealth shock. But reported wealth is net of consumption. And as people tend to adapt their consumption to wealth shocks, changes in reported wealth tend to be systematically smaller than the original wealth shock. Hence changes in reported wealth are the residual change after smoothing, while constructed wealth shocks are a direct proxy for the actual wealth shock. From a policy perspective we are interested in estimates in terms of the actual wealth shock and not in terms of the wealth change that remains after people have adapted their consumption. This is why I choose the reduced from as the baseline regression.

5.7 Nutrition and health inputs

As discussed in the introduction calorie intake and health inputs are central mechanisms through which wealth affects health in poor countries ((Jensen and Richter 2003, Case 2004) but they might be less relevant for wealthy retirees in the US The HRS reports respondents' body mass index (BMI) and the number of doctor visits as well as out-of-pocket medical expenditure (OOP) which allows to directly test for the role of these potential mechanisms. Table A.8 in the Appendix shows that indeed wealth shocks do not significantly affect any of these three measures.

Notice, however, that there could be opposing effects at work that might cancel out in the regression. People might be cutting back on food expenditures as a response to a negative wealth shock. But 'cheaper calories' often come in the form of inferior food that remains stored in body fat to a greater extent than higher quality food. In this case cutting back on food expenditure might even increase people's BMI. The effect on health inputs is ambiguous, too. If wealth shocks make you sick you might end up going more not less to the doctor, even if this might imply higher OOP expenditures, e.g. because a premium health care coverage is not affordable anymore. Hence, the results in Table A.8 should not be interpreted as evidence that wealth shocks do not affect people's nutrition behavior or the optimal receipt of health inputs. However, it seems unlikely that these are the main mechanisms underlying the strong short-term effects of wealth shocks on physical and mental health that we observe in the data.

6 Conclusion

This paper provides evidence that wealth shocks have strongly positive effects on health outcomes of stock holding retirees in the US. A 10% wealth shock is associated with an improvement of 2-3% of a standard deviation in physical health, self-reported health, mental health and survival rates. Analyzing individual health conditions I find a strong effect on high blood pressure, smaller effects on heart diseases and no effect on arthritis, diabetes, lung disease and cancer. The analysis of interaction terms reveals that effects on physical health and mortality increase with age. The comparison with the cross-sectional relationship of wealth and health indicates that the estimated causal effects of wealth shocks are larger than the long-run wealth elasticity of health.

So far positive effects of wealth shocks on elderly health have been found only for poor retirees in Russia and South Africa. This paper is the first to document such effects for wealthy retirees in a wealthy country. I uncover these effects with a new measure to identify stock market fluctuations in the wealth of US retirees. This measure, the interaction of stock holdings with stock market changes, is of interest beyond the context of health economics. It could also be used to study, for example, the effects of unearned income on labor supply, savings and in particular on consumption.[16]

[16] See Coile and Levine (2006) for a study that uses a similar approach to analyze the effects of stock market movements on retirement (as discussed above).

The pattern of affected health conditions found in this study point to a story in which psychological factors play an important role. Psychological factors as central mechanism linking economic shocks and health outcomes are in line with the results of Sullivan and von Wachter (2008). They find strong mortality effects of lay-offs for displaced workers in the US and argue that psychological reactions are the most likely mechanism underlying these effects. These could be psychological reactions to the arrival of news about future consumption as well as reactions to actual changes in consumption. Applying the empirical strategy developed in this paper to data sets that allow to study consumption behavior in detail would be a promising path for future research. Of particular use would be consumption data in combination with information on individual stock portfolio compositions. Precise information on individual stock holdings allows to construct high-frequency individual-specific wealth shocks which would greatly increase the power of such analysis without the need of extended time series of stock market changes.

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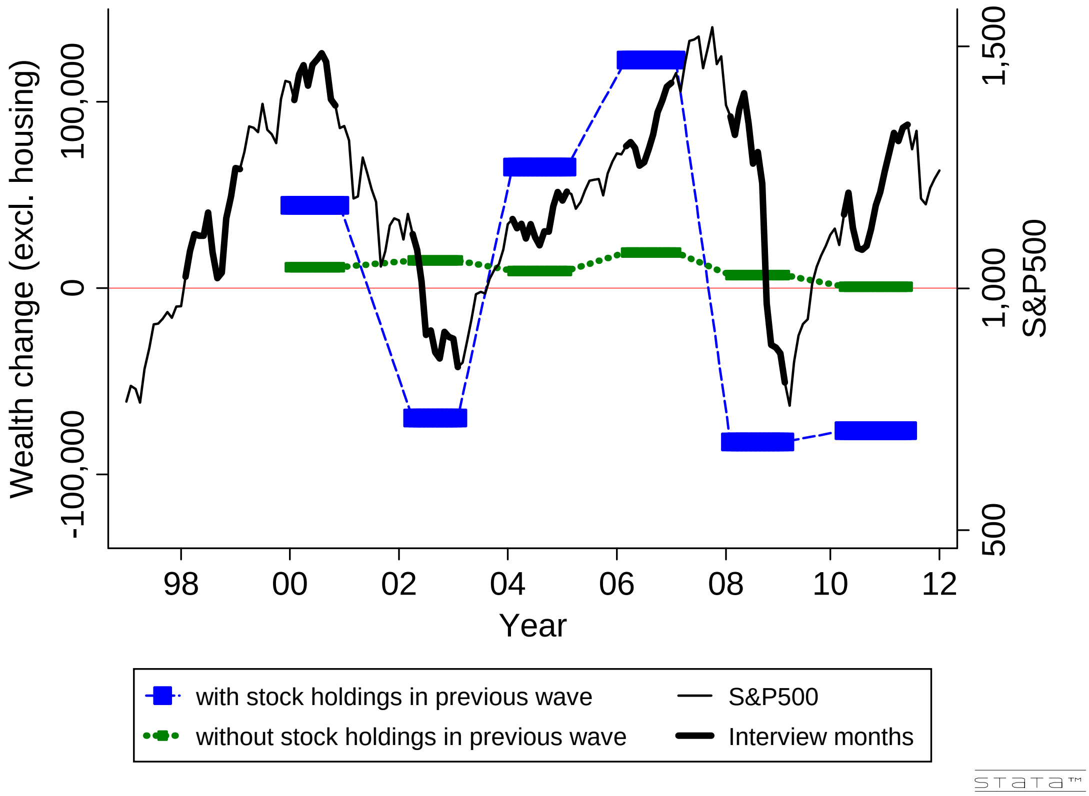
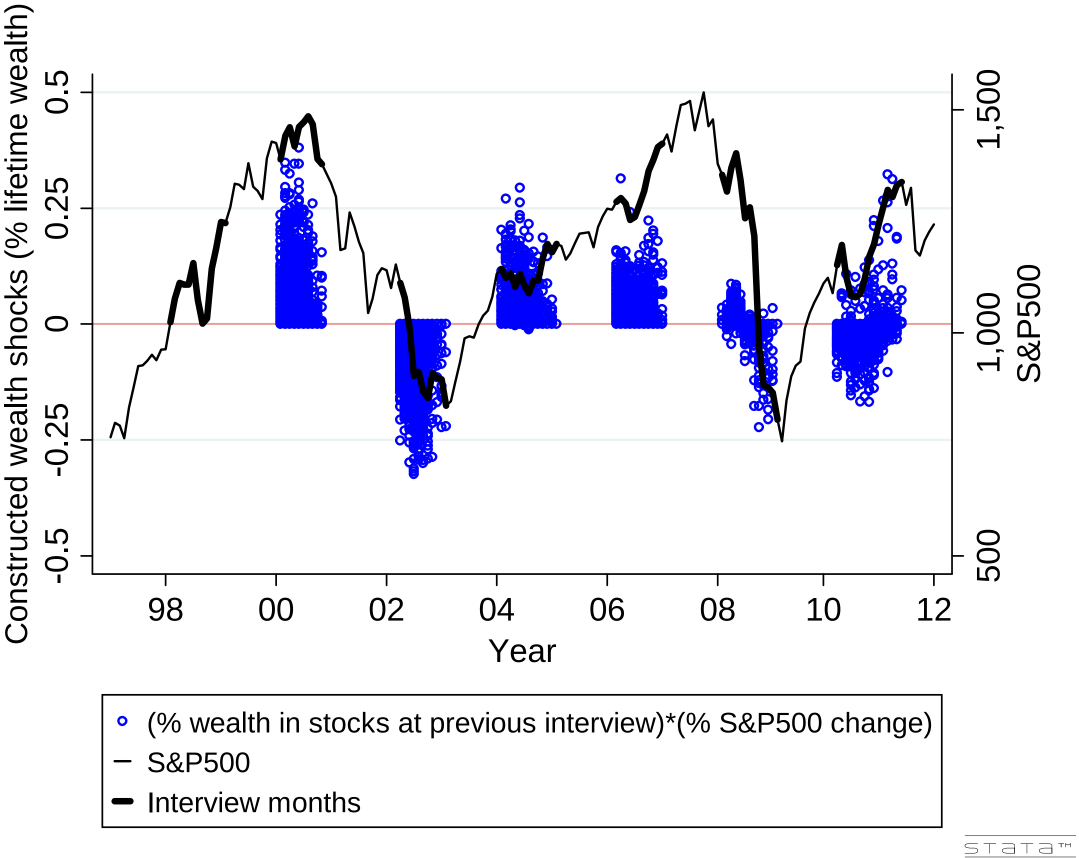
8 Tables and Figures

Figure 1: Changes in Reported Wealth and the S&P500



Average changes in reported wealth excluding housing wealth for retiree households with and without stocks in the previous period are plotted per HRS wave. The length of the bars indicates the time period in each wave over which interviews were conducted, as indicated by the bold sections of the S&P500 plot. There are more interviews at the beginning of each wave. Hence, in the last wave the majority of households faces a lower S&P500 than at the previous interview, in line with the average negative change in reported wealth (see also Figure 2). For further details on wealth measures and sample restrictions see the Data section.

Figure 2: Constructed Wealth Shocks and the S&P500



Constructed wealth shocks are plotted over time with the S&P500. Each circle represents the constructed wealth shock of one household and is placed in the figure at the exact month of the household's interview in t.

Table 1: HRS Sample Characteristics and Summary Statistics (Means) per Wave.

HRS wave	4	5	6	7	8	9	10
Year	1998-1999	2000-2001	2002-2003	2004-2005	2006-2007	2008-2009	2010-2011
Full HRS sample							
N	21,176	19,432	18,044	20,129	18,386	17,116	15,221
Age	65.9	67.1	68.4	66.6	68.0	69.2	70.5
% retiree households	0.55	0.58	0.61	0.55	0.59	0.60	0.64
Information '% of IRA in stocks'	3 categories	3 categories	3 categories	3 categories	exact %	exact %	exact %
Imputed % of IRA in stocks	0, 50, 100%	0, 50, 100%	0, 50, 100%	0, 50, 100%	exact %	exact %	exact %
Regression sample							
N	7,365	9,119	9,216	9,353	9,107	8,468	6,076
Current wealth (nominal USD)	249,782	289,939	320,886	356,201	445,105	437,027	366,217
Life-time wealth (nominal USD)	407,277	443,923	482,871	531,206	613,866	734,843	570,889
Fraction owning stocks	0.29	0.31	0.29	0.29	0.26	0.25	0.23
...those owning stocks							
N	1,745	2,070	2,060	2,051	1,902	1,715	1,170
Life-time wealth (nominal USD)	765,472	824,982	903,991	1,036,287	1,226,624	1,228,890	1,133,413
% life-time wealth held in stocks	0.19	0.20	0.19	0.20	0.21	0.22	0.22
S&P500 change since past interview	-	0.32	-0.32	0.15	0.16	0.01	-0.07
Constructed wealth shock	-	0.06	-0.06	0.03	0.03	0.00	-0.02
Constructed wealth shock (min.; max.)	-	0.00; 0.38	-0.32; 0.00	-0.01; 0.29	0.31; 0.00	-0.22; 0.09	-0.17; 0.32

Retiree households refer to singles or couples with neither working for pay nor being unemployed. The regression sample includes all households that were retired and reported their wealth, retiree income and stock holdings in the previous wave. Life-time wealth is the sum of current wealth and expected future discounted retiree income (see Data section). Waves 1 to 3 are excluded as there is no information on stock holdings in IRAs. Further wealth summary statistics are reported in the Appendix Table A.2.

Table 2: Regressions of Changes in Reported Wealth on Constructed Wealth Shocks.

Dependent Variable: Wealth change	(1)	(2)	(3)	(4)
Constructed wealth shock	0.819***	0.793***		
= %Wealth in stocks[t-1]	(0.173)	(0.175)		
x Stock market change				
D(Any stocks[t-1])			0.214***	0.213***
x Stock market change			(0.041)	(0.041)
Stock market change	0.057	0.041	0.045	0.029
	(0.127)	(0.126)	(0.128)	(0.128)
Main effects	✓	✓	✓	✓
Demographic controls		✓		✓
n	31,673	31,673	31,673	31,673
R^2	0.004	0.010	0.004	0.009

The dependent variable is the percentage change in life-time wealth. '*D(Any stocks[t-1])*' is a dummy indicating stock ownership in the previous wave. Main effects are the interaction terms and year dummies. Demographic controls are dummies for gender (1), age group (12), cohort (10), race (2), region (4), degree (4), and lagged marital status (7). Regressions include only one observation per household and year. For details on wealth measures see the Data section. Standard errors in parenthesis are multi-level clustered by household and interview month.

Table 3: Baseline Regressions of Health Measures on Wealth Shocks

Dependent variable ($\Delta > 0$: Health improvement)	OLS (1)	OLS (2)	Probit-adapted OLS (3)
Δ Physical Health Index	0.254***	0.252***	0.192***
n=35,739	(0.082)	(0.082)	(0.063)
Δ Self-reported Health	0.231*	0.252**	0.205*
n=41,693	(0.123)	(0.123)	(0.107)
Self-reported Change in Health	0.082	0.095	0.138
n=41,693	(0.080)	(0.084)	(0.122)
Δ Mental Health Index	0.645**	0.664***	0.298**
n=37,034	(0.252)	(0.253)	(0.130)
Survival	0.087*	0.109**	0.208**
n=34,380	(0.049)	(0.047)	(0.089)
Main effects	✓	✓	✓
Demographic controls		✓	✓
Standardized dependent variable			✓

The coefficient on constructed wealth shocks (*'%wealth in stocks[t-1] x stock market change'*) is displayed. A positive coefficient refers to a health *improvement*. 'Survival' indicates survival to the next wave (on average 2 years), thus not including respondents in the last wave. 'Probit-adapted OLS' yields effects in terms of standard deviations that are comparable across health measures. 'Main effects' are the lagged fraction of wealth held in stocks, a dummy for lagged stock ownership, the stock market change and year dummies. 'Demographic controls' are dummies for gender (1), age group (12), cohort (10), race (2), region (4), degree (4), and lagged marital status (7). Standard errors in parenthesis are multi-level clustered by household and interview month.

Table 4: Baseline Regressions of Health Conditions on Wealth Shocks

Dependent variable ($\Delta > 0$: Health improvement)	OLS (1)	OLS (2)	Probit-adapted OLS (3)
Δ High blood pressure	0.109***	0.107***	0.172***
	(0.040)	(0.039)	(0.063)
Δ Heart disease	0.066*	0.067*	0.115*
	(0.036)	(0.036)	(0.062)
Δ Stroke	0.011	0.013	0.028
	(0.025)	(0.025)	(0.052)
Δ Diabetes	-0.001	-0.005	-0.010
	(0.023)	(0.024)	(0.042)
Δ Cancer	0.028	0.029	0.055
	(0.020)	(0.020)	(0.038)
Δ Arthritis	0.040	0.039	0.063
	(0.046)	(0.046)	(0.074)
Δ Lung disease	0.001	0.001	0.002
	(0.021)	(0.021)	(0.041)
Main effects	✓	✓	✓
Demographic controls		✓	✓
Standardized dep. var.			✓

The coefficient on constructed wealth shocks (*'%wealth in stocks[t-1] x stock market change'*) is displayed. A positive coefficient refers to a health *improvement* in the respective dependent variable. Column (3) shows effects in terms of standard deviations that are comparable across health conditions.'Main effects' and 'Demographic controls' as in the previous table. N=35,739 in all regressions. Standard errors in parenthesis are multi-level clustered by household and interview month.

Table 5: Regressions of Health Measures on Wealth Shocks Interacted with Age and Gender

	Interaction category					
	Age			Gender		
			Δ			Δ
	<75	>=75	p-value	Male	Female	p-value
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Physical Health Index	0.152	0.313***	0.346	0.264*	0.236***	0.875
	(0.145)	(0.094)		(0.156)	(0.087)	
Δ Self-reported Health	0.339*	0.193	0.543	0.118	0.319*	0.535
	(0.182)	(0.162)		(0.232)	(0.176)	
Self-reported Δ Health	0.213*	0.013	0.176	0.019	0.151	0.367
	(0.120)	(0.111)		(0.113)	(0.110)	
Δ Mental Health Index	0.953***	0.490	0.321	0.250	0.940**	0.181
	(0.354)	(0.335)		(0.335)	(0.366)	
Survival	0.014	0.171***	0.022	0.119*	0.103**	0.824
	(0.049)	(0.062)		(0.070)	(0.048)	
Controls (interacted)						
Main effects		✓			✓	
Demographics		✓			✓	

The coefficients on constructed wealth shocks interacted with the two respective subgroups are displayed. 'Δ (p-value)' indicates the significance level of the difference between the two interacted coefficients. Positive coefficients refer to a health *improvement* in the respective dependent variable. Numbers of observations and (interacted) controls as in Table 3. The estimation method used is OLS. Standard errors in parenthesis are multi-level clustered by household and interview month.

Table 6: Linearity of Wealth Shock Effects

Dependent Variable	Δ Physical H Index (1)	Δ Self-rep. Health (2)	Self-rep. Δ Health (3)	Δ Mental H Index (4)	Survival (5)
Stock market change	-0.045	0.017	0.014	0.057	-0.016
(reference group)	0.036	0.062	0.035	0.156	0.024
D(1-10% stocks[t-1])	0.075*	0.030	0.063	0.034	0.006
x Stock market change	(0.044)	(0.080)	(0.047)	(0.097)	(0.019)
D(>10% stocks[t-1])	0.136***	0.068	0.027	0.240**	0.039**
x Stock market change	(0.029)	(0.045)	(0.025)	(0.108)	(0.018)
Main effects	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓

Coefficients of the interaction of stock market changes with dummies for lagged stock holding levels are displayed. Main effects: Dummies 1-10% and >10% wealth held in stocks in t-1 and year fixed effects. Demographic controls and numbers of observations as in Table 3. Standard errors are multi-level clustered by household and interview month.

Table 7: Inclusion of Placebo Shocks (Unemployment Rate Changes and Bond Holdings)

	Baseline (1)	Placebo u-rate (2)	Placebo bond (3)	Horse race (4)
Dep. var.: Δ Index of Health Conditions				
Wealth shock	0.252***			0.295**
	(0.082)			(0.151)
Placebo u-rate shock		-0.096		0.023
		(0.062)		(0.117)
Placebo bond shock			-0.519	-0.635*
			(0.388)	(0.386)
Dep. var.: Δ Self-reported Health				
Wealth shock	0.252**			0.218
	(0.123)			(0.213)
Placebo u-rate shock		-0.210***		-0.036
		(0.079)		(0.162)
Placebo bond shock			-0.020	-0.155
			(0.625)	(0.631)
Dep. var.: Self-reported Change in Health				
Wealth shock	0.095			-0.076
	(0.084)			(0.125)
Placebo u-rate shock		-0.056		-0.162
		(0.073)		(0.119)
Placebo bond shock			0.120	0.079
			(0.277)	(0.280)
Dep. var.: Δ Mental Health Index				
Wealth shock	0.664***			1.035***
	(0.253)			(0.327)
Placebo u-rate shock		-0.163		0.392*
		(0.164)		(0.219)
Placebo bond shock			1.990**	1.625*
			(0.935)	(0.958)
Dep. var.: Survival				
Wealth shock	0.109**			0.188*
	(0.047)			(0.100)
Placebo u-rate shock		0.000		0.090
		(0.052)		(0.108)
Placebo bond shock			0.106	0.071
			(0.170)	(0.168)
Main effects	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓

Wealth shocks: *%wealth in stocks[t-1] x stock market change*. Placebo u-rate shock: *%wealth in stocks[t-1] x unemployment rate change*. Placebo bond shock: *%wealth in bonds[t-1] x stock market change*. Main effects, demographic controls and numbers of observations as in Table 3. Standard errors are multi-level clustered by household and interview month.

Table 8: Benchmark Regressions of Health Measures on Ln of Life-time Wealth

	Baseline	Benchmark	
	$\Delta H_{i,t}$ on $\frac{\Delta S\&P_t}{S\&P_{t-1}} \frac{s_{i,t-1}}{W_{i,t-1}}$	$H_{i,t}$ on $lnW_{i,t}$	$\Delta H_{i,t}$ on $lnW_{i,t}$
Dependent Variable	(1)	(2)	(3)
Δ Physical Health Index	0.252***	0.211***	0.001
	(0.082)	(0.013)	(0.004)
Δ Self-reported Health	0.252**	0.335***	-0.014***
	(0.123)	(0.009)	(0.003)
Self-reported Δ Health	0.095	0.040***	
	(0.084)	(0.004)	
Δ Mental Health Index	0.664***	0.482***	-0.020***
	(0.253)	(0.017)	(0.007)
Survival	0.109**	0.030***	
	(0.047)	(0.002)	
Main effects	✓		
Demographics	✓		
Male, age, cohort, region		✓	✓

Column (1) shows the baseline estimates as in Table 3. Columns (2) and (3) show OLS regressions of health levels and health changes on ln life-time wealth, respectively. There are no level equivalents for 'self-reported change in health' and for 'survival'. Only gender, age, cohort and region controls are included in (2) and (3), hence life-time wealth proxies for overall socio-economic status. The inclusion of further controls decreases the coefficients on ln life-time wealth.

Table 9: Benchmark Regressions of Health Conditions on Ln of Life-time Wealth

	Baseline	Benchmark	
Dependent variable $H_{i,t}$	$\Delta H_{i,t}$ on $\frac{\Delta S\&P_t}{S\&P_{t-1}}\frac{s_{i,t-1}}{W_{i,t-1}}$	$H_{i,t}$ on $lnW_{i,t}$	$\Delta H_{i,t}$ on $lnW_{i,t}$
(Never had...)	(1)	(2)	(3)
High blood pressure	0.107***	0.048***	-0.002**
	(0.039)	(0.004)	(0.001)
Heart disease	0.067*	0.028***	0.000
	(0.036)	(0.004)	(0.001)
Stroke	0.013	0.030***	0.003***
	(0.025)	(0.003)	(0.001
Arthritis	0.039	0.033***	-0.006***
	(0.046)	(0.004)	(0.001)
Cancer	0.029	-0.024***	-0.001
	(0.020)	(0.004)	(0.001)
Diabetes	-0.005	0.060***	0.005***
	(0.024)	(0.004)	(0.001)
Lung disease	0.001	0.035***	0.002**
	(0.021)	(0.003)	(0.001)
Main effects	✓		
Demographics	✓		
Male, age, cohort, region		✓	✓

Column (1) shows the baseline estimates as in Table 4. Columns (2) and (3) show OLS regressions of health condition levels and changes on ln life-time wealth, respectively. Only gender, age, cohort and region controls are included in (2) and (3), hence life-time wealth proxies for overall socio-economic status. The inclusion of further controls decreases the coefficients on ln life-time wealth.

Table 10: Alternative sample specifications

Dependent Variable	Baseline (1)	Including non-retirees (2)	Excluding HH<65 (3)	Singles only (4)	Excluding poorest 25% (5)
Δ Physical Health Index	0.252***	0.216***	0.310***	0.435***	0.256***
	(0.082)	(0.080)	(0.081)	(0.114)	(0.071)
n	35,739	55,061	28,286	17,095	25,930
Δ Self-reported Health	0.252**	0.203	0.260*	0.120	0.207
	(0.123)	(0.133)	(0.136)	(0.204)	(0.134)
n	41,693	63,230	33,237	20,319	29,810
Self-reported Δ Health	0.095	0.089	0.052	0.048	0.036
	(0.084)	(0.066)	(0.097)	(0.148)	(0.094)
n	41,693	63,230	33,237	20,319	29,810
Δ Mental Health Index	0.664***	0.438**	0.768**	0.815*	0.711***
	(0.253)	(0.221)	(0.323)	(0.481)	(0.262)
n	37,034	56,892	29,240	17,747	27,450
Survival	0.109**	0.093***	0.100**	0.122	0.113**
	(0.047)	(0.035)	(0.050)	(0.089)	(0.049)
n	34,380	52,241	27,036	16,577	24,304
Main effects	✓	✓	✓	✓	✓
Demographics	✓	✓	✓	✓	✓

Comments as in as in Table 3. Column (2): non-retired individuals are included (as long as some kind of retirement income is reported for HH). (3): HH are excluded if either financial respondent or spouse or both are below age 65. (4): Only single HH included. (5): HH from quartile of the life-time wealth distribution are excluded.

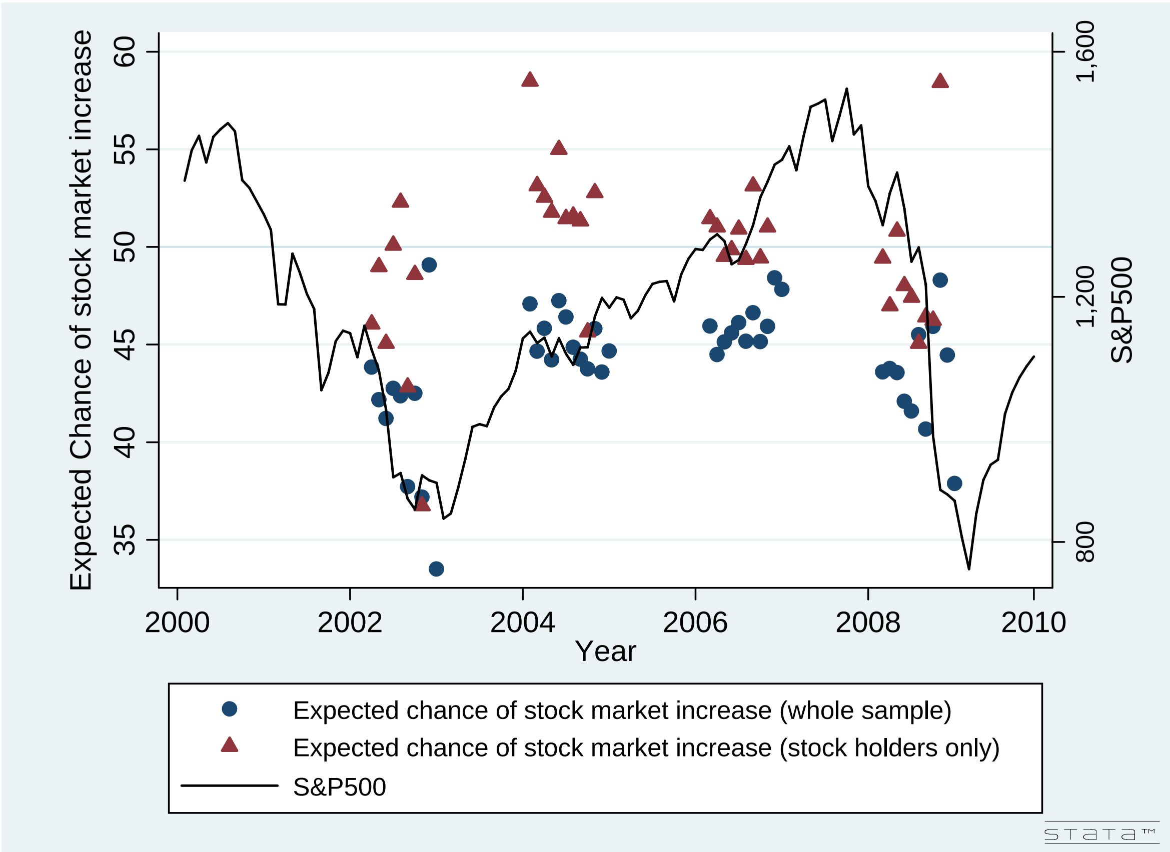
Table 11: Constructed Wealth Shocks as Instrument for Changes in Reported Wealth

	Baseline	2SLS
	$\frac{\Delta S\&P500}{S\&P500}\frac{s_{i,t-1}}{W_{i,t-1}}$ as regressor	$\frac{\Delta S\&P500}{S\&P500}\frac{s_{i,t-1}}{W_{i,t-1}}$ as IV for reported wealth changes
Dependent Variable	(1)	(2)
Δ Physical Health Index	0.252***	0.327***
	(0.080)	(0.123)
First stage F-statistic	-	22.37
Δ Self-reported Health	0.252**	0.312**
	(0.114)	(0.154)
First stage F-statistic	-	25.31
Self-reported Change in Health	0.095	0.118
	(0.089)	(0.107)
First stage F-statistic	-	25.31
Δ Mental Health Index	0.664***	0.780**
	(0.219)	(0.308)
First stage F-statistic	-	21.21
Survival	0.109**	0.137**
	(0.047)	(0.066)
First stage F-statistic	-	21.52
Main effects	✓	✓
Demographic controls	✓	✓

Column (1) displays the coefficients on constructed wealth shocks as in the Table 3. Column (2) displays the 2SLS coefficients on changes in reported wealth with constructed wealth shocks as instrument. First-stage F-statistics vary across health measures due to differences in the number of observations. For further comments see Table 3.

A Appendix

Figure A.1: HRS Expectations of an Increase in the Stock Market and the S&P500



Monthly averages of the following question in the HRS are plotted: 'By next year at this time, what is the percent chance that mutual fund shares invested in blue chip stocks like those in the Dow Jones Industrial Average will be worth more than they are today?' Averages for months with less than 25 responses are not displayed.

Table A.1: Summary Statistics Demographic Controls

Variable	Mean	Std. dev.	Variable	Mean	Std. dev.
Sex			*Education*		
Female	0.634		Years of education	11.659	3.390
			Less than high school	0.316	
Age			GED diploma	0.045	
Age	75.43	8.91	High-school graduate	0.324	
Age>75	0.522		Some college	0.177	
			College and above	0.138	
Race					
White	0.823		*Marital status (lagged)*		
African-American	0.142		Married	0.518	
			Partnered	0.016	
Region			Separated	0.013	
Northeast	0.165		Divorced	0.079	
Midwest	0.248		Separated/divorced	0.005	
South	0.408		Widowed	0.329	
West	0.179		Never married	0.032	

Standard deviations are omitted for binary variables. Further comments as in Table 1.

Table A.2: Summary Statistics Wealth Measures

Wealth measure	Symbol (1)	Mean (2)	Std. dev. (3)
Reported household wealth (nominal USD)	A_t	361,411	1,059,818
Change in reported household wealth (nominal USD)	ΔA_t	10,347	988,519
Household life-time wealth (nominal USD)	W_t	548,065	3,911,738
Relative change in reported household wealth	$\frac{\Delta A_t}{W_{t-1}}$	0.111	0.995
Fraction of life-time wealth held in stocks	$\frac{s_t}{W_t}$	0.064	0.145
Percentage change in the S&P500	$\frac{S\&P_t}{S\&P_{t-1}}$	0.049	0.223
Predicted wealth shocks	$\frac{s_{t-1}}{W_{t-1}} \frac{\Delta S\&P_t}{S\&P_{t-1}}$	0.002	0.035

For comments on sample restrictions see Table 1.

Table A.3: Summary Statistics of Health Measures.

	Original				Probit-adapted (standardized)	
	Levels		First difference		Levels	First difference
	Range (1)	Mean (Std. dev.) (2)	Range (3)	Mean (Std. dev.) (4)	Mean (Std. dev.) (5)	Mean (Std. dev.) (6)
Health measures						
Physical Health Index	[0;...;7]	4.669	[-5;...;5]	-0.231	0.000	-0.171
		(1.319)		(0.551)	(0.972)	(0.410)
Self-reported health	[0;...;4]	1.883	[-4;...;4]	-0.069	0.000	-0.060
		(1.121)		(0.938)	(0.958)	(0.806)
Self-reported change in health	[-1;0;1]	-0.243	-	-	0.001	-
		(0.599)			(0.870)	
Mental Health Index	[0;...;8]	6.340	[-8;...;8]	-0.017	0.000	-0.006
		(2.008)		(1.818)	(0.932)	(0.858)
Survival	[0;1]	0.885	-	-	0.000	-
		(0.319)			(0.609)	
Health conditions (*Never had...*)						
High blood pressure	[0;1]	0.350	[-1;0;1]	-0.047	0.000	-0.076
				(0.235)	(0.776)	(0.382)
Heart disease	[0;1]	0.664	[-1;0;1]	-0.047	0.000	-0.077
				(0.233)	(0.772)	(0.381)
Stroke	[0;1]	0.894	[-1;0;1]	-0.019	0.000	-0.037
				(0.149)	(0.595)	(0.287)
Arthritis	[0;1]	0.287	[-1;0;1]	-0.041	0.000	-0.068
				(0.233)	(0.753)	(0.387)
Cancer	[0;1]	0.819	[-1;0;1]	-0.026	0.000	-0.047
				(0.174)	(0.684)	(0.308)
Diabetes	[0;1]	0.773	[-1;0;1]	-0.030	0.000	-0.052
				(0.177)	(0.720)	(0.305)
Lung disease	[0;1]	0.873	[-1;0;1]	-0.021	0.000	-0.039
				(0.158)	(0.625)	(0.296)

Self-reported change in health and Survival refer to changes so that no first differences are constructed. Standard deviations are omitted for binary variables. For further comments see the Data section.

Table A.4: Regressions of Mental Health Index Items on Wealth Shocks

Dependent Variable ($\Delta > 0$: Mood improvement)	(1)	(2)
Δ Felt depressed	0.140*	0.143*
	(0.077)	(0.076)
Δ Felt sad	0.153**	0.154*
	(0.078)	(0.078)
Δ Everything is an effort	0.030	0.037
	(0.083)	(0.082)
Δ Sleep is restless	0.109	0.109
	(0.087)	(0.087)
Δ Felt alone	0.112	0.114
	(0.071)	(0.072)
Δ Could not get going	0.056	0.056
	(0.068)	(0.068)
Δ Felt happy	-0.003	0.000
	(0.068)	(0.068)
Δ Enjoyed life	0.043	0.044
	(0.054)	(0.053)
Main effects	✓	✓
Demographic controls		✓

The coefficient on constructed wealth shocks is displayed. A positive coefficient refers to an *improvement* in the respective dependent variable, e.g. feeling *less depressed* or feeling *more happy*. Further comments as in Table 3.

Table A.5: 2SLS Regressions with Initial Stock Holdings as Instrument for Actual Stock Holdings

Dependent Variable	Baseline (1)	Baseline IV sample (2)	$\frac{\Delta S\&P_t}{S\&P_{t-1}}[\frac{s_i}{W_i}]^{1998}$ as IV for constructed wealth shocks (3)
Δ Physical Health Index	0.252***	0.303***	0.392***
	(0.080)	(0.090)	(0.128)
n	35,739	21,953	21,953
Δ Self-reported Health	0.252**	0.261**	0.135
	(0.114)	(0.120)	(0.162)
n	41,693	25,969	25,969
Self-reported Change in Health	0.095	0.108	0.065
	(0.089)	(0.112)	(0.141)
n	41,693	25,969	25,969
Δ Mental Health Index	0.664***	0.456*	0.746***
	(0.219)	(0.248)	(0.277)
n	37,034	22,761	22,761
Survival	0.109**	0.095*	0.052
	(0.047)	(0.049)	(0.049)
n	34,380	22,814	22,814
Main effects	✓	✓	✓
Demographics	✓	✓	✓

The coefficient on constructed wealth shocks (*'%wealth in stocks[t-1] x stock market change'*) is displayed. Column (1) shows the baseline results. Column (2) repeats the baseline regressions in the IV sample. Column (3) reports coefficients from 2SLS regressions with wealth shocks based on the 1998 fraction of wealth in stocks as instrument Further comments as in Table 3.

Table A.6: Balancing regressions

Dependent Variable	Male (1)	African-American (2)	Age (3)	>12 years of education (4)	Region Midwest (5)
Constructed wealth shock	0.002	-0.028	0.685	0.089	-0.005
	(0.034)	(0.021)	(0.418)	(0.088)	(0.064)
Controls					
Main effects	✓	✓	✓	✓	✓
Demographics (excl. dep. var.)	✓	✓	✓	✓	✓

The coefficient on constructed wealth shocks in baseline regressions with individual controls as dependent variable is displayed. Demographic controls exclude (the category of) the dependent variable. All further comments as in Table 3.

Table A.7: Regressions of Health Measures on Changes in Reported Stock Wealth

	Specification of wealth shocks		
	Baseline	Using changes in *reported* stock wealth	
	$\frac{\Delta SP_t}{SP_{t-1}} s_{i,t-1}/W_{i,t-1}$	$(s_{i,t} - s_{i,t-1})/10,000$	$(s_{i,t} - s_{i,t-1})/W_{i,t-1}$
Dependent Variable	(1)	(2)	(3)
Δ Physical Health Index	0.252***	0.00005	0.002
n=35,739	(0.082)	(0.00009)	(0.010)
Δ Self-reported Health	0.252**	0.00012	0.046**
n=41,693	(0.123)	(0.00014)	(0.019)
Self-reported Change in Health	0.095	0.00003	0.006
n=41,693	(0.084)	(0.00004)	(0.014)
Δ Mental Health Index	0.664***	0.00006	-0.012
n=37,034	(0.253)	(0.00013)	(0.033)
Survival	0.109**	-0.00002	-0.006
n=34,380	(0.047)	(0.00002)	(0.009)
Main effects	✓	✓	✓
Demographic controls	✓	✓	✓

The coefficient on wealth shocks as defined at the top of each column is displayed. $\frac{\Delta SP_t}{SP_{t-1}}$ = percentage change in the S&P500; $s_{i,t}$ = stock wealth; $W_{i,t}$ = life-time wealth (see Data section). A positive coefficient refers to a health *improvement* in the respective dependent variable in terms of standard deviations. 'Main effects' are the lagged fraction of wealth held in stocks ($\frac{s_{i,t-1}}{W_{i,t-1}}$) and year dummies. 'Demographics' are age, age^2, age^3, dummies for gender and race, eight dummies for lagged marital status, five region dummies, five education dummies, and years of education. The estimation method used is OLS. Standard errors in parenthesis are multi-level clustered by household and interview month.

Table A.8: Regressions of Potential Mechanisms on Wealth Shocks

Dependent Variable	(1)	(2)
Δ BMI	-0.605	-0.554
	(0.387)	(0.399)
Δ Number doctor visits	-0.032	-0.031
	(0.035)	(0.035)
Δ OOP expenditure	0.014	0.013
	(0.012)	(0.012)
Main effects	✓	✓
Demographic controls	✓	✓

The coefficient on constructed wealth shocks is displayed. BMI is the respondent's body mass index. Number doctor visits refers to the respondent's doctor visits since the past interview. OOP refers to out-of-pocket medical expenditures. Further comments as in Table 3.

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